

PAUL PELLIOT

# NOTES ON MARCO POLO

I

OUVRAGE POSTHUME

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## AVANT-PROPOS

*L'œuvre de Paul Pelliot, qui fait l'objet de cette publication, fut entreprise il y a bien des années; on peut même dire que son auteur y travailla dès les débuts de son activité scientifique; c'est en 1904 qu'il la commença, dans ses Deux itinéraires de Chine en Inde à la fin du VIII<sup>e</sup> siècle, en faisant des rapprochements entre les renseignements de Marco Polo sur la Chine du Sud et les données des sources chinoises. A maintes reprises, il commenta le texte de Marco Polo dans ses cours du Collège de France, notamment de 1918 à 1930 et de 1936 à 1939; il en fit l'objet de nombreuses communications savantes et donna plusieurs notes à Henri Cordier quand celui-ci réédita le Marco Polo de Yule.*

*C'est devant les insuffisances du travail de Yule et de Cordier qu'il décida avec A. C. Moule, spécialiste des relations sino-occidentales au Moyen Âge, d'entreprendre une nouvelle édition de Marco Polo, tenant compte de nombreuses variantes apportées par des manuscrits que Yule n'avait pas connus, et accompagnée d'une introduction et d'un commentaire. En 1928, le professeur L. F. Benedetto avait publié à Florence, sous le titre Marco Polo, Il Milione, une nouvelle édition du texte du manuscrit franco-italien 1116 de la Bibliothèque Nationale, en y joignant des variantes et surtout des fragments conservés dans d'autres versions; les principaux provenaient d'un manuscrit latin découvert par lui à la bibliothèque Ambrosienne de Milan, qui était la copie fragmentaire d'un manuscrit dont, au XVIII<sup>e</sup> siècle, le cardinal espagnol Zelada avait possédé un exemplaire plus complet dont on ignorait le sort; d'autres fragments provenaient de la version italienne publiée au XVI<sup>e</sup> siècle par Ramusio, et qui représente une tradition différente.*

*Pelliot et Moule intéressèrent à leur projet l'éminent collectionneur et érudit Sir Percival David qui entreprit lui-même de rechercher le fameux manuscrit Zelada, dont on savait qu'il était peut-être à la bibliothèque du chapitre de la cathédrale de Tolède. Après bien des difficultés, Sir Percival David parvint à l'y retrouver en 1933. C'est alors que A. C. Moule entreprit d'établir une version anglaise en faisant figurer dans son texte les variantes en italique, avec indication en manchette du manuscrit dont chacune provenait; il la publia en 1938 et, peu après, le texte du manuscrit Zelada. Ces deux volumes parurent à Londres sous le titre Marco Polo, The Description of the World. Ils portaient les deux noms de Moule et de Pelliot. La version synthétique de Moule avait en effet nécessité un travail considérable de critique pour établir la forme correcte des noms propres et des termes techniques que Marco Polo avait consignés. C'est en principe pour justifier le choix qui avait été fait que Pelliot rédigea les notes qui sont publiées ici. A. C. Moule s'était réservé d'en écrire un certain nombre; elles ont été publiées en 1957 à Cambridge sous le titre Quinsai, with other Notes on Marco Polo.*

*Les notes de Pelliot prirent peu à peu un développement considérable; ce n'était plus un commentaire de Marco Polo, c'était une collection de monographies souvent monumentales où se développa l'ampleur extraordinaire de son érudition. Le fait qu'il s'agissait de notes l'incita à inclure dans son texte tout ce qui normalement aurait pu être relégué dans des notes au bas des pages, ce qui explique la rédaction parfois un peu compliquée de l'ouvrage.*

*La majeure partie des notes de Pelliot fut imprimée à Londres en placards, en 1939 et 1940, au fur et à mesure de leur rédaction. Un bon nombre des notes prévues à l'origine n'étaient pas encore écrites, mais il fut jugé préférable d'imprimer tout ce qui existait, quitte à incorporer dans l'ensemble les notes qui pourraient être rédigées et imprimées par la suite; certaines ne purent l'être, soit que Pelliot ait jugé bon de revoir ses matériaux pour les compléter, soit qu'il ne les ait pas rédigées à cause de la guerre; une soixantaine de notes de Pelliot restèrent manuscrites. Pendant le conflit, les plombs furent fondus et l'impression resta inachevée, puis Pelliot mourut en 1945. Devant les difficultés considérables que présentait une nouvelle impression, Sir Percival David et A. C. Moule jugèrent bon de surseoir. A. C. Moule pensait qu'il fallait publier un ensemble complet de notes, en demandant aux spécialistes de rédiger celles qui manquaient, et en revisant celles de Pelliot afin qu'elles fussent à jour. C'était un travail énorme dont la réalisation présentait de grandes difficultés.*

*Plusieurs années passèrent. Finalement, en 1953, le Comité assumant la publication des œuvres posthumes de Pelliot délégua M. Paul Demiéville, membre de l'Institut, professeur au Collège de France, auprès de Sir Percival David et de A. C. Moule, pour leur proposer de procéder en France à la publication des notes de Pelliot dans l'état où elles étaient. Peu après, par la générosité de Sir Percival David, auquel appartenaient les documents, ceux-ci parvenaient à Paris. Chargé par le Comité d'en assurer la publication, j'ai rassemblé les notes par ordre alphabétique, ordre prévu dès l'origine, et entrepris de les mettre au point matériellement en complétant certaines références, en comblant quelques lacunes, mais sans toucher à leur contenu; elles formeront deux volumes dont le dernier sera pourvu d'une bibliographie et d'un index. Avec la collaboration de MM. James Hamilton et A. W. Macdonald, tous deux attachés au Centre national de la Recherche scientifique, qui ont bien voulu m'aider à revoir le texte écrit par Pelliot directement en anglais, et à corriger les épreuves, j'ai pu mener à bien cette tâche. C'est grâce au patronage de l'Académie des Inscriptions et Belles-Lettres, et à l'aide du Centre national de la Recherche scientifique et de l'Imprimerie nationale, que peut enfin paraître ce magnus opus du grand orientaliste français, le travail le plus important qu'il ait jamais écrit.*

*Paris, le 20 mars 1958.*

Louis HAMBIS.

# ABRÉVIATIONS

## DES TITRES DES OUVRAGES ET DES PÉRIODIQUES

### OUVRAGES

- B**..... BENEDETTO (L. F.), *Marco Polo, Il Milione*, Firenze, 1928.
- B<sup>1</sup>**..... BENEDETTO (L. F.), *Il Libro di Messer Marco Polo, cittadino di Venezia detto Milione ecc.*, Milano-Roma, 1932.
- BB**..... BALDELLI-BONI (Conte Gio. Batt.), *Il Milione di Marco Polo*, Firenze, 1827, 2 vol.
- Be et Ber**..... BEREZIN (I. N.), *Istoriya Mongolov sočinenie Rašid'Eddina* (Trudy VOIRAO, V, VII, XIII et XV, Saint-Pétersbourg, 1858, 1861, 1868, 1888), 4 vol.
- Bl**..... BLOCHET (E.), *Djami el-Tévarikh, Histoire générale du monde par Fadl Allah Rashid ed-Din*, tome II, Leiden-London, 1911.
- Br**..... BRETSCHNEIDER (D<sup>r</sup> E.), *Mediaeval Researches from Eastern Asiatic Sources*, London, 1888, 2 vol.
- Ch**..... CHARIGNON (A. J. H.), *Le Livre de Marco Polo*, Pékin, 1924-1926, 3 vol.
- EI**..... *Encyclopédie de l'Islam*, Leyde, 1913-1936, 4 vol.
- Fe et Fe<sup>1</sup>**..... FERRAND (G.), *Relations de voyages et textes géographiques arabes, persans et turks relatifs à l'Extrême-Orient du VIII<sup>e</sup> au XVIII<sup>e</sup> siècle*, Paris, 1913-1914, 2 vol.
- Ha<sup>1</sup>**..... HAMMER-PURGSTALL (J. VON), *Geschichte der Ilchane*, Darmstadt, 1842-1843, 2 vol.
- Ha<sup>2</sup>**..... HAMMER-PURGSTALL (J. VON), *Geschichte Wassaf's*, Bd. I, Wien, 1856.
- Ha<sup>3</sup>**..... HAMMER-PURGSTALL (J. VON), *Geschichte der Goldenen Horde*, Pest, 1840.
- Hobson-Jobson<sup>2</sup>** YULE (H.) et BURNELL (A. C.), *Hobson-Jobson. A Glossary of Colloquial Anglo-Indian Words and Terms*, London, 1903.
- HR**..... HIRTH (F.) and ROCKHILL (W. W.), *Chau Ju-kua*, St.-Petersburg, 1911.
- LS**..... LE STRANGE (G.), *The Lands of the Eastern Caliphate*, London, 1905.
- Mi**..... MINORSKI (V.), *Hudūd al-'Ālam, 'The Regions of the World'*, London, 1937.
- Mo**..... MOULE (A. C.), *Christians in China before the Year 1550*, London, 1930.
- Oh**..... D'OHSSON (Baron C.), *Histoire des Mongols*, Amsterdam, 1852, 4 vol.
- Pa**..... PAUTHIER (G.), *Le Livre de Marco Polo citoyen de Venise*, Paris, 1865, 2 vol.
- Pe**..... PENZER (N. M.), *The Most Noble and Famous Travels of Marco Polo*, London, 1929.
- Pel**..... PELLIOT, *Les Mongols et la Papauté*, ext. *Revue de l'Orient chrétien*, 3<sup>e</sup> Série, t. III [1922-1923], n<sup>os</sup> 1 et 2, p. 3-30; t. IV [1924], n<sup>os</sup> 3 et 4, p. 225-335; t. VIII [1931-1932], n<sup>os</sup> 1 et 2, p. 3-84.
- RR**..... RICCI (Aldo) and ROSS (Sir E. Denison), *The Travels of Marco Polo* (transl. into English from the text of L. F. Benedetto), London, 1931.
- WANG Hui-tsu<sup>1</sup>**. *Leao Kin Yuan san-che t'ong-ming lou* (ed. Kouang-ya chou-kiu).

- WANG Hui-tsu<sup>2</sup>. *Yuan-che pen-tcheng* (ed. 15th *kouang-siu*).  
*Wy*..... WYNGAERT (A. VAN DEN), *Sinica-Franciscana, Itinera et relationes fratrum minorum saeculi XIII et XIV*, t. I, Quaratchi, 1929.  
*Y*..... YULE (Henry), *The Book of Ser Marco Polo* (ed. Cordier), London, 1921, 2 vol.  
*Y<sup>1</sup>*..... YULE (Henry), *Cathay and the Way Thither* (ed. Cordier), London, 1913-1916, 4 vol.  
*YS*..... *Yuan che* (ed. Kouo-tseu-kien de Nankin des Ming).  
*Z et Zu*..... ZURLA (Placido), *Di Marco Polo e degli altri Viaggiatori Veneziani piu illustri dissertazioni*, Venezia, 1818-1819, 2 vol.

## PÉRIODIQUES

- BEFEO*..... *Bulletin de l'École Française d'Extrême-Orient*.  
*BSOS*..... *Bulletin of School of Oriental Studies*.  
*JA*..... *Journal Asiatique*.  
*JNCB*..... *Journal of the North China Branch of the Royal Asiatic Society*.  
*JRAS*..... *Journal of the Royal Asiatic Society*.  
*TP*..... *T'oung Pao*.  
*Trudy VOIRAO* *Trudy vostočnago otdeleniya imperatorskago arkheologičeskago obščestva*.  
*ZOMG*..... *Zeitschrift der Deutschen Morgenländischen Gesellschaft*.  
*ZVOIRAO*.... *Zapiski vostočnago otdeleniya imperatorskago arkheologičeskago obščestva*.

# NOTES ON MARCO POLO

BY

PAUL PELLIOT

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## 1. ABACAN

*abaccatum* Z  
*abachan* FB  
*abagatan* V  
*abaiam* TA<sup>3</sup>

*abatam* VA  
*abatan* F, FA, TA<sup>1</sup>, VB, VL  
*abatar* LT

*abatham* P  
*abattan* L  
*abbaccatan* R

The name is doubtless corrupt. PAUTHIER (*Pa*, 544) thought that « Abacan » was certainly the general whose name he wrongly read as 阿刺罕 A-tz'ü-han; but it is A-la[朶]han, as SCHLEGEL justly remarked in 1898. In « Abacan », SCHLEGEL saw a clerical error for « Alacan », and this correction has been considered as possible, or probable, in *Y*, II, 596; III, 103; *RR*, 410; *B*<sup>1</sup>, 437. CHARIGNON (*Ch*, III, 123) takes this solution for granted, and gives the credit of it to the « ancient Jesuits », but without authority. Palaeographically, the corruption is possible, and we have cases like *babisci* for *balisci* in PAGNINI's Pegolotti (cf. *Y*<sup>1</sup>, III, 149, 154).

The original form of the name transcribed A-la-han in Chinese can be ascertained. There is a biography of A-la-han in *YS*, 129, 3 *b-4 b*; an account of his life is also given in the inscription written by Yü Chi concerning his son El-temür (*Tao-yüan hsio-ku lu*, ed. *Ssü-pu ts'ung-k'an*, 24, 1-6); both have been critically combined by T'ü Chi, 93, 2 *b-4 b*. A-la-han was a Jalair, the son of 也柳干 Yeh-liu-kan (\*Yälügän); and Rašidu-'d-Din (*Bl*, II, 576) speaks of a general الاتان Alaqañ, son of جيلكه بهادر Jilügä-bahadur; the alternation of *y-* and *j-* seems due to the fact that the same sign marks these two sounds at the beginning of words in Uighur-Mongol writing, but the two names are the same (with *i* [= *e*]  $\sim$  *ä*, and the quiescent final *-n* usual in Mongolian). So we can be sure that, in the present case, A-la-han is to be understood as Alaqañ; GAUBIL's « Argan » (also « Hargan », « Algan »; cf. *Hist. de Gentchiscan*, 161, 169, 191-192; *Y*, II, 261) is a wrong restitution.

Yälügän was killed in battle at Yang-chou in 1258, and Alağan, born in 1233, succeeded him in command of the Mongol «wing» troops (諸翼 *chu-yi*; cf. the 37 *yi* troops enumerated in YS, 13, 5 *b*; I suppose that it is the Mongol name of Alağan's unit which follows his own name in Rašid, but I do not know how to restore it). Alağan took part in the battles at the time of Qubilai's accession to the throne, later, in 1262, against Li T'an, and later still against the Sung (cf. *JNCB*, 1927, 10). In 1281, he was placed in command of 400,000 Mongol troops (read «140,000» with T'u Chi?) to invade Japan, but died in Ning-po. The mention of Alağan's name in Rašidu'd-Din shows that the fame of his favour with Qubilai had reached Persia; moreover, his father may have been at some time in Persia. But much remains to be examined on this subject. T'u Chi, 102, 10 *b*, maintains that the 也里可 Yeh-li-k'o of YS, 132, 1 *a*, and the surely identical 也里怯 Yeh-li-ch'ieh of YS, 135, 5 *a*, are other spellings of Alağan's name; phonetics, however, seem to exclude such an identification. But 阿里罕 A-li-han in YS, 129, 8 *a*, must be, as has been remarked by WANG Hui-tsu<sup>1</sup>, 18, 5-6, another form of Alağan (the second syllable is not accentuated, and its vowel was practically slurred). On the other hand, Rašid's various and sometimes conflicting statements (at least in BEREZIN's edition and translation) about the several Yälügän or Yelugä of Jalair origin (cf. *Ber*, I, 36, 38, 84; III, 145) have never yet been discussed in connection with Mongol and Chinese texts.

Alağan's official biography is not always in agreement with the *pên-chi* or with the chapter on Japan. For instance, as to the number of troops, these other texts speak of 100,000, not of 400,000. It is well known that many of the biographies of YS were compiled at the beginning of the Ming dynasty from funerary inscriptions and notices of private origin; but the *pên-chi*, as a rule, are more reliable. We learn from the latter (YS, 11, 6 *a*) that, on account of Alağan's illness, an Imperial edict of July 13, 1281, named in his place 阿塔海 A-t'a-hai (\*Ataqai?) as commander-in-chief of the troops sent against Japan, and that Alağan died on July 22 in the same year. Farther on, it says, under the date September 11, 1281: «As Alağan is dead, the order has been given that A-t'a-hai and others should garrison the Three Ports, and that A-t'a-hai should go and seize the remaining brigands at sea.» One month later (YS, 11, 6 *b*), A-t'a-hai asked to use the garrison of the Three Ports for an action against bandits in Fukien, but the request was refused. A-t'a-hai (1234-beginning of 1290) has a short biography in YS, 129, 4 *b*-5 *a*, supplemented in T'u Chi, 93, 1-2 (his name is sometimes written A-ta [答]-hai, and once, by mistake, T'a-hai; cf. WANG Hui-tsu<sup>2</sup>, 46, 4 *b*). There is in YS, 129, no mention of the edict of 1281, but only of a later appointment, in 1283; T'u Chi has supposed there was an error of date in the biography. I am not certain that this is quite right. In spite of his appointment in July 1281, A-t'a-hai does not seem to have then done anything against Japan, and wanted only to march against Fukien. It was only on January 1, 1283, that A-t'a-hai was ordered to take over the 300 sea-junks which were at the disposal of Fan Wên-hu (YS, 12, 5 *a*, where we have the wrong form A-la[東]-hai; this passage has not been noticed by WANG Hui-tsu, nor by T'u Chi. There are other cases in YS of confusion between *la* and *t'a* or *ta*; Fan Wên-hu is Polo's «Vonsamcin», *q. v.*); and the biography may allude to a fresh edict in the beginning of 1283. On the other hand, there are strange gaps in A-t'a-hai's biography, as has already been noticed by T'u Chi. This may be due to the fact that A-t'a-hai went through

some official troubles, which were passed over in the family notice used later by the compilers of *YS*. Preceding the texts on the naval campaign of the summer of 1283, the *YS*, 12, 7 a, has a text according to which, on February 19, 1281, «A-t'a-hai was sent to garrison Kuča». It is by no means certain that we are not here concerned with another A-t'a-hai, although there is no mention of two A-t'a-hai in WANG Hui-tsu<sup>1</sup>; but it is also possible that our A-t'a-hai suffered then a temporary disgrace, although the measure sending him to Chinese Turkestan was not carried out. After the punitive expeditions against Japan were abandoned, A-t'a-hai helped in preparing a fleet to go to the rescue of the troops engaged in Champa and Tonking, fought against Nayan, and on his death, was granted high posthumous honours. Apart from the above indications, approximate translations of Chinese texts may be found in *Pa*, 540-543, and *Ch*, III, 127-132. Many details could be added from the *Gazetteers*, for instance from the *Yang-chou fu chih*.

To conclude, Alaqañ died before he could take any real part in the campaigns against Japan. It might naturally be supposed that «Abacan» is corrupted not from Alaqañ, but from «Atacai» = \*Ataqai, who at least sent his ships to sea, and perhaps went to sea himself, but the correction would be more difficult palaeographically, and Alaqañ is the better known of the two. On the whole, I incline to the belief that «Abacan» stands for «Alacan» (Alaqañ and Fan Wên-hu are named side by side in 1280, *YS*, 11, 1 b, just as we have «Abacan» and «Vonsamcin» in Polo), although Polo must have mixed him up to some extent with A-t'a-hai; but I do not feel sure enough to introduce «Alacan» into the text. Whatever the truth may be, no tragic fate ended Alaqañ's or Ataqai's life, no more than Fan Wên-hu's (see «Vonsamcin»). As usual, Polo, accurate as to the main events and the names, is wrong when he comes to the specific part played by each individual.

## 2. ABAGA

*abaga* F, Fr, t, FA,  
L, LT, TA<sup>3</sup>, V, Z

*abagan* FA  
*abagha* TA<sup>1</sup>, TA<sup>3</sup>

*abbaga, albaga* Lr  
*aghaba* TA<sup>3</sup>

The name is Mong. Abaya, which means «uncle»; Rašidu-'d-Dīn (for instance in QUATRE-MÈRE, *Hist. des Mongols*, 56) writes اباقا Abāqa and اباقا Abaqa, with the notation of -γ- as -q- so common in the transcriptions of Mongolian names at that time; in the same way, Waśśāf (*Ha*<sup>2</sup>, 98, etc.) writes اباقا Abaqa. In Syriac, we find «Abya» (BUDGE, *The monks of Kūblāi Khān*, 88, 144). The Chinese transcriptions are 阿不哥 A-pu-ko (\*Abuga = Abaya), 阿不合 A-pu-ha (\*Abuya), 阿八合 A-pa-ha and 阿八哈 A-pa-ha (Abaya); see WANG Hui-tsu<sup>2</sup>, 42, 3 a. The forms in -pu- are due to the unaccented nature of the second syllable, the vowel of which was slurred; there are many similar instances. The form with -u- is met with also in the West: cf. «Abouha» for Abaya in *Gestes des Chiprois (Hist. des Crois., Arm., II, 843)*.

Abaya was Hülägü's eldest son, and succeeded his father, who died on February 8, 1265



(cf. QUATREMÈRE, *Hist. des Mongols*, 417); he himself died on April 1, 1282 (*Ha*<sup>1</sup>, I, 313); on the circumstances of his death, cf. QUATREMÈRE, *Hist. des Sultans Mamlouks*, II, 48.

Abaya remained in fairly close touch with Qubilai, and the Chinese texts mention him on several occasions. As these texts are not indicated in *Br*, II, 17, it is not without interest to collect them here :

1. (YS, 107, 7 b) : The prince (or « king », *wang*) A-pa-ha (Abaqa) was the son of Hsü-lieh-wu (Hülägü).

2. (YS, 203, 4 b) : « A-lao-wa-ting ('Alā'u-'d-Dīn) was a man from 木發里 Mu-fa-li in the Western Countries (Hsi-yü). In the eighth year *chih-yuän* (1271), Shih-tsu (= Qubilai) sent envoys to fetch makers of catapults (*p'ao-chiang*) from the Prince of the blood A-pu-ko (Abaya). The prince obeyed the edict by sending 'Alā'u-'d-Dīn and I-ssü-ma-yin (Isma'il). The two men with their families went post-haste to the capital (= Peking) where they were provided with an official residence. They first made a large catapult and set it up in front of the Fives Gates... » (Isma'il was from 旭烈 Hsü-lieh). The whole text has been translated by MOULE (*JNCB*, 1927, 12-13), but without identification of Prince A-pu-ko. This is important inasmuch as it has a bearing on the famous siege of Hsiang-yang (see « Saianfu ») and the alleged part played there by the Polos : the Mussulman catapults were used for the siege of Hsiang-yang at the end of 1272 or in the beginning of 1273 (cf. MOULE, *ibid.*, 10; *B*<sup>1</sup>, 433, gives November 1272, which seems too early). It is a pity that, besides the original forms Mu-fa-li and Hsü-lieh, MOULE should have added the arbitrary forms substituted by Ch'ien-lung's commissioners in the 18th cent., as well as possible geographical values of these last such as Mosul and Shiraz. The result is that Mosul and Shiraz have been adopted in *Pe*, 229; but they are impossible if we start from the original Mu-fa-li and Hsü-lieh. MOULE has also given Rašidu-'d-Dīn's account of the siege of Hsiang-yang, where we read : « Before that time, there was no big Frankish mangonel in Cathay (منجنیق فرنگه *manjaniq-i Färängä*; this is the emendation proposed in *Bl*, II, 513, for منجنیق قونجه *manjaniq-i qumgäh* of *Oh*, II, 391, which had puzzled YULE, *Y*, II, 168; the emendation is the more probable if we remember that Frankish mangonels are mentioned in Arabic by Nowairi [cf. QUATREMÈRE, *Hist. des Mongols*, 136], and that *farangi* came to mean a 'gun' in Čay. Turkish, in Telugu and in Chinese of about 1550-1600 A. D.; cf. PAVET DE COURTEILLE, *Dict. turc-oriental*, 388; YULE, *Hobson-Jobson*<sup>2</sup>, 352; *Ming shih*, 325, 9 a; *BEFEO*, IX, 671; *Br*, II, 316; that does not mean, in spite of *Bl*, II, 513, that these mangonels were of Italian origin). From this kingdom, Tālib the mangonel-maker, who was from Ba'albek and Damascus, went there, and his sons 'Abu-bäkr and Ibrāhīm and Muḥammad and his assistants made seven great catapults... » It is evident that both texts relate to the same event, and MOULE has already pointed out that the names of Abu-bäkr and Ibrāhīm are given in YS, 203, 4 b-5 b, as the names of Isma'il's two sons. As to Rašid's Muḥammad, I have more hesitation in identifying him, as MOULE suggests, with 'Alā'u-'d-Dīn's grandson Muḥammad-šāh. Hsü-lieh, Isma'il's native place, has been said to be Shiraz (which is certainly wrong), or the name to mean « belonging to Hülägü » (*Br*, I, 274), or to be Herat (*Y*, II, 167), or « Hillè » (*Bl*, II, 514). It is true that the same characters Hsü-lieh are used for Hülägü (see « Ulaü »), and that Herat (Harī) was known to the Chinese at the beginning of the Ming under a name Ha-lieh which is not very far removed

from Hsü-lieh. But, in my opinion, the only plausible solution is the Alep proposed as an alternative to Shiraz by MOULE (p. 13). If Ṭālib, as it seems, is another name of Isma'īl (curiously enough, D'OHSSON, *Oh*, II, 391, names the sons, but not the father), Rašīd, it may be noticed, is not sure of his origin, as he mentions both Ba'albek and Damascus. But we may admit that the man came from that region; and Hsü-lieh is a fairly accurate transcription of حلب Haleb, Alep; I do not think that we here have to take into account the «Areg» which designates Ba'albek in Steph. Orbelian (cf. PATKANOV, *Istoriya Mongolov*, I, 33). Under 1296, a text of YS, 19, 3 b, mentions, just before «the gunner» Isma'īl, a name 別馬里思丹 Pieh-ma-li-ssü-tan the final of which is certainly -stān and refers to a country; but the rest of the name remains obscure, and it is not even certain that there is a connection between Pieh-ma-li-ssü-tan and Isma'īl. As to 'Alā'u-'d-Din's native place Mu-fa-li, its identification with Mosul has been accepted by numerous authorities, from QUATREMÈRE (*Hist. des Mongols*, 155) to BLOCHET (*Bl*, II, 513). But I am much more in favour of BRETSCHNEIDER's «Moferin» (*Br*, I, 273), which is also YULE's «Miafaraḳain» (*Y*, II, 167), that is to say Maifārkāt; the people of Maifārkāt were good catapult-makers (cf. QUATREMÈRE, *ibid.*, 360, 369). In 1282, the Chams also used «Mussulman» (Hui-hui) guns against the Chinese, and so did probably the Burmese in 1300 (cf. YS, 210, 3 b; BEFEO, IX, 676; T'U Chi, 92, 7 b-8 a).

3. (YS, 210, 7 a) : «From Ch'üan-chou (see «Çaiton»), to reach that kingdom [of Ma'abar; see «Maabar»], there are about 100,000 li. From that kingdom to the city of the great prince (*ta-wang*) A-pu-ha (Abaya), by sea, with a favourable wind, it is possible to arrive in about 15 days». Abaya's city must be Bagdad.

4. (YS, 8, 1 a) : «In the tenth year [*chih-yüan*], the first month, . . . on the day *chi-mao* (February 14, 1273), an Imperial edict sent 扎朮呵 Cha-shu-k'o (\*Jajurya?), 押失塞 Ya-shih-han (\*Yašyan?; or Jaju-qaya and \*Šiqan?) and 崔杓 Ts'ui Shao, with 100,000 ounces of gold (? 100,000 ounces of silver) to the prince A-pu-ha (Abaya), in order to buy drugs in the Lion's Kingdom (Shih-tzü-kuo, Ceylon)». I know nothing of these three men.

5. (YS, 85, 2 a) : «Originally, more than 7,000 families of hunters and falconers had been gathered by T'ai-tsu (Chinghiz-khan) in the various districts, and put under the authority of the great prince Hsü-lieh (Hülägü). The office in charge of them was first established in the second year *chung-t'ung* (1261). In the twelfth year *chih-yüan* (1275), the great prince A-pa-ha (Abaya) sent an envoy with a memorial [asking] that they should be under the direct authority of the Court; they were attached to the Ministry of War.» For the later fate of these people in China who belonged to the *ilkhans* of Persia, see «Caçan». The various branches of Chinghiz-khanids seem to have kept their appanages in China proper almost to the end of the dynasty.

6. (*Yüan tien-chang*, 57, 16 a-17 a) : This is a long text dated January 27, 1280, relating to Mussulmans who had brought gerfalcons from Baryu and who used to slay sheep according to the rules of their religion (for this text, closely connected with Polo's and Rašīd's, see «Bargu»).

7. (YS, 11, 1 b) : On March 31, 1280, silk and paper-money were given, among others, to subordinates of the Prince A-pa-ha (Abaya).

## 3. ABASCE, and see INDIE

*abas, abbas* Z  
*abasa, abasia (cor.), abassa,*  
*abasse, abaste, bassa* V  
*abasce* F, L, TA<sup>1</sup>, Z  
*abascia* LT, P; R  
*abascie* TA<sup>1</sup>, TA<sup>3</sup>  
*abase, abasee, basce* F  
*abasia* F, Fr, t, FA, FB

*abassaia, abassia* VL  
*abasyam* G  
*abasye* FB  
*abaxia* V  
*abassaie* FAR  
*abbasie, albasie* FA  
*abissia, abixia* VB

*absce* L  
*albaxia* VA  
*bascia* P  
*bascie, nabascie* TA<sup>1</sup>  
*nabasce* TA<sup>1</sup>, TA<sup>1r</sup>, TA<sup>3r</sup>  
*nabascia* TA<sup>3</sup>  
*abisscini* R

I have hesitated between «Abasce» and «Abascie», but have come to the conclusion that the more probable form is «Abasce», with the use of *-sce* as a simple notation of *-š* (cf. «Chescemir»). The original is Arabic Ḥabaš, «Abyssinian». While the weak *h-* has been hardened into *c-* by Polo in such words as «Cogacin» and «Curmos», the stronger *h-* has been dropped by him in «Abasce» as in «Avarium». This is not easy to explain, since Polo also drops the weak *h-* in «Ulau». European usage concurs with Polo when we write «Abyssinia» and «Aleppo». For «Abasce», cf. Y, II, 431; YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Abyssinia» and «Hubshee»; DAMES, *Barbosa*, I, 36, 39; EI, s. v. «Abyssinia». Marignolli's «Abasty» is certainly a misreading for «Abascy», practically the same form as in Polo (cf. Y<sup>1</sup>, III, 222-223; Wy, 532).

The name «Ethiopia», given by V as another name of «Abasce», was used sometimes in a looser way. It is nevertheless surprising that, in the summary we have of part of a letter written by Giovanni da Montecorvino from «Cambaliech» (see «Cambaluc» = Peking) on February 13, 1306 (cf. GOLUBOVICH, *Bibl. bio-bibl.*, II, 140), it should be said that envoys came to Brother Giovanni from Ethiopia, asking him to go there or to send missionaries, since from the days of Matthew the Evangelist and his disciples these people had not been instructed in the faith of Christ. VAN DEN WYNGAERT (Wy, 355), in agreement with MOULE (*JRAS*, 1924, 556), was of opinion that our Ethiopia was not meant, but some region in Asia. It seems clear, as already suspected by YULE (Y<sup>1</sup>, III, 7), that the «envoys» saw Montecorvino in India, not in China, but I am not certain that this «Ethiopia» is to be looked for in Asia, or even in Socotra, as YULE supposed after ASSEMANI. The mention of S. Matthew is of some importance. He is said by the *Breviary* to have preached in «Ethiopia», and, just in the same way as Montecorvino speaks of S. Matthew on the subject of «Ethiopia», Marignolli names him in his paragraph on «Abasty», i. e. «Abascy», which is certainly Abyssinia. So Montecorvino, I think, probably believed that the «envoys» came from this last country. The same may hold good for what Jourdain Cathala says of Ethiopia in a letter written at Tana (near Bombay) in 1323, and the mislocation of Ethiopia in the would-be Marino Sanudo map (which is, in fact, Paolino da Venezia's; cf. GOLUBOVICH, *Bibl. bio-bibl.*, II, 84-85) is of no greater account than the fact of the letter addressed on December 1, 1329, by Pope John XXII (not «Alex. II» as in Y, II, 432)

«Magnifico viro Imperatori Aethiopum», to give him notice of the departure of Jourdain Cathala, bishop of «Columbum» in India (cf. GOLUBOVICH, *ibid.*, II, 432; III, 357-358). There was regular intercourse by sea between Abyssinia and the western coast of India, and I rather think that, by Ethiopia, Montecorvino and Jourdain Cathala really meant Abyssinia. But the location of «Ethiopia» in Jourdain's letter was misunderstood in Avignon, and this led to the strange letter of 1329. Moreover, a similar letter was addressed on September 11, 1329, to the Emperor of Cathay, who was of course still farther from Persia and India than Abyssinia (cf. A. MERCATI, *Monum. Vaticana veterem diocesim Columbensem... respicientia*, Rome, 1923, 8 v°, p. 16; Mgt. MERCATI, in agreement with GOLUBOVICH, is in favour of looking for Jourdain's and John XXII's «Ethiopia» in India). Cf. also MOULE's opinion Vol. I, 435-436.

## 4. ACBALEC MANGI

City		
[ <i>acbalec mangi</i> ] L	<i>ambalec magi</i> TA <sup>1</sup>	<i>cinelech</i> VL
<i>achilechimangi</i> P	<i>anbalet mangi</i> TA <sup>3</sup>	<i>cinelech mangi</i> VLr
<i>acmelchamangi</i> LT	<i>arcamalec</i> FB	<i>machare emelegamin</i> VB
<i>acmelec mangi</i> F, VA	<i>atiualet</i> FA	<i>machase emelegamin</i> V
Province		
<i>abalet magy</i> G	<i>acbaluch mançi</i> Z	<i>alchilechimangi</i> P
<i>abel mangi</i> VB	<i>achalec mansy</i> FAt	<i>ambalet mangi</i> TA <sup>1</sup>
<i>acbalac mangi</i> Fr, t, VA	<i>achalet manzi</i> FA	<i>anbalet mangi</i> TA <sup>3</sup>
<i>acbalec mangi</i> F, L	<i>achbaluch mangi</i> R	<i>cinelech mangi</i> VL
<i>acbalech mangi</i> LT	<i>achebelach mandi</i> V	<i>ebeleh manzi</i> VB
<i>acbalet manzy</i> FB		

Explained in RAMUSIO's text as meaning «the White City on the border (*de' confini*) of Mangi». It is plainly a Persian construction, Aq-baliq-i-Manzi, «the White City of Manzi», so called to distinguish it from the Aq-baliq of Cathay, which is Chêng-ting-fu. I have kept the *balec* of F, though I might equally well have preferred the *baluc* of R and Z on the analogy of Cambaluc, where all good mss. have *u*; a Turkish *t* is easily heard as *u* by foreigners (see also «Camul»). Opinions have varied about the identity of this place. It has escaped former editors that Rašīdu-'d-Dīn (*Bl*, II, 598) mentions, in the «kingdom» of Tangut, which was then the appanage of Mangala's son Ananda, the following places: Kinjanfu (= Hsi-an-fu), Qamju (Kan-chou), Uraqai (I read ارقى instead of the editor's ارزى), Halājan (see «Calacian») and اق باليق Aq-baliq. BLOCHET sees here Polo's «Acbaluc Mangi», and I think he is right.

To account for the Turkish name, it is useless to look, with KLAPROTH and PAUTHIER, for a Chinese name containing the word «white»; there is no «white» in the Chinese name of the other Aq-baliq, Chêng-ting-fu. The only important place which fits Polo's indications is 漢中 Han-chung on the Han River. I have little doubt that Aq-baliq is the Turkish name of Han-chung; YULE (*Y*, II, 35) has suspected it, and I agree here for once with CHARIGNON (*Ch*, II, 187).

Yet there is a difficulty. Aq-baliq is mentioned by Rašidu-'d-Dīn among the cities of Tangut, that is to say of Shàn-hsi and Kansu, which is no part of Manzi. It might be suggested that Aq-baliq being the Turkish form the Mongolian equivalent of which is Čayan-balyasun, Rašid's Aq-baliq may perhaps be the Čayan-balyasun of 甘肅州 Kan-su-chou named in the Mongol period by the 大元馬政記 *Ta-Yüan ma-chêng chi*, 8 a. I do not believe that the obscure Čayan-balyasun of Kan-su-chou can be taken into account here. Han-chung (then named 興元 Hsing-yüan) was, under the Mongol dynasty, in Shàn-hsi (consequently in Tangut) just as it is now (cf. YS, 60, 2b), and this must be Rašid's Aq-baliq, thus identical with Polo's; neither of them is in Manzi (see «Mangi»). But here intervenes RAMUSIO's explanation of the name, no doubt going back to Polo himself (the crucial words «de les confin dou Mangi» are already in F, and have equivalents in VA, L, V, VB) : in spite of the literal value of the name, it is said to mean «the White City on the border of Mangi». In other words, Polo does not make it a city of Manzi, but to distinguish it from the other Aq-baliq in Cathay, Persian-speaking people called Han-chung «the Aqbalıq of Manzi» because it was contiguous to Manzi. We must only admit that Polo expressed himself in a loose way when he also said of «Sindufu», i. e. Chêng-tu in Ssü-ch'uan, that it was «de le confin dou Mangi». Did Polo use «confin» with the double value of Arab. *ḥudūd*, or of Chin. 界 *chieh*, «boundary» and «territory»?

It is remarkable that Rašidu-'d-Dīn, who uses the Mongol form Čayan-balyasun when speaking of Chêng-ting-fu, gives the Turkish form Aq-baliq when he refers to Han-chung. In my opinion, the reason is that the accounts of the campaigns of Chinghiz-khan and Tului, which are at the basis of all mentions of Chêng-ting-fu in Rašid, have been translated by him from the chronicle in Mongolian which, translated into Chinese, still exists under the title of *Shêng-wu ch'in-chêng lu*. And this is also the reason why, in his accounts of these campaigns, Rašid uses scholarly transcriptions of Chinese names, while in other parts of his book he uses the popular forms then current among Persians (see «Pianfu», «Taianfu», «Saianfu»).

I attach no value to the difference in F and other mss. between «Acbalect Mangi», name of the province, and «Acmelec mangi», name of the capital; Polo must have used one and the same form in both cases. The same alternation, equally valueless, occurs between «melic» and «belic» (see «Melic»).

## 5. ACHBALUCH

### *achbaluch* R

This paragraph appears only in RAMUSIO, but its authenticity is beyond doubt. YULE (Y, II, 14) has already given some good reasons for identifying «Achbaluch» with 正定府 Chêng-ting-fu (not «Ch'êng-ting fu» as in *Pe*, 343, 350, and *B*<sup>1</sup>, 437), called under the Mongols 眞定府 Chên-ting-fu. CHARIGNON's objections (*Ch*, II, 163, 187) are a marvel of false reasoning, and it is to

be regretted that they should have caused hesitation in *B*<sup>1</sup>, 437. «Achbaluch» is the pure Turkish Aq-baliq, and means «White City» (*baliq*, *balty*, is not Mongolian in spite of *Y*<sup>1</sup>, II, 14, and *Ch*, II, 162, 163). YULE's opinion about the names given by the Mongols to Imperial residences is devoid of significance (in spite of STRAHLENBERG; cf. *Y*, II, 35), but the same author uses a more solid argument when he remarks that the Mongol equivalent of Aq-baliq would be Čayān-balyasun, and that Rašidu-'d-Dīn speaks of «Chaghan Balghasun which the Chinese call *Jintsinfu*». In favour of the equivalence of the last form to Chên-ting-fu (Chêng-ting-fu), YULE quotes the itinerary copied by Mir 'IZZET ULLAH in 1812, which gives (*JRAS*, No. xiv [1847], 308) «Jig zing fu 吉京府», corrected by YULE to «Jingdzinfu». This has to be abandoned. The itinerary of 1812 gives only modern forms, although the copy is very faulty, and we must almost certainly read 京定府 Jīngdīngfū. But the text, or rather the texts, of Rašidu-'d-Dīn remain, and I can quote four different passages, in *Bl*, II, 216 (where the form 京定府 Jīndīn-fu of his two mss. has been arbitrarily altered to 京定府 Čīng-din-fu by the editor), in Rašidu-'d-Dīn's «Life» of Chinghiz-khan (*Ber*, III, 21, 29; Persian text, 33, 47, «Čayān-balyasun, which in the language of the Chinese is called 京定府 Jīng-zīn-fū»; the Chinese parallel texts of the campaign mention here Chên-ting-fu), and finally in Rašid's unpublished «History of China», a manuscript translation of which I owe to the kindness of Dr. R. LEVY. We should expect Jīng-din-fu or Jīndin-fu in all passages of Rašid; but Rašid, owing to different sources, has often two spellings, one more scientific, the other more popular. It is possible that the *t*- of *ting*, heard of course by the Mongols as a sonant, had received a spirant palatalization, somewhat analogous to the one which, out of Chin. 頂子 *ting-tzū*, «knob of official cap», has given to-day *jingzǎ* in Turkī and in Mongolian.

Šāh-Rūh's envoys passed in 1420 through 塞丁府, which has been read «Sadinfur» and corrected into «Sadinfu»; REINAUD and YULE (cf. *Y*, I, 278, 285) long ago proposed to see here also Chêng-ting-fu. The *sad* often renders a palatal č, there is no vowel written in the first syllable, and I think the real reading is very probably 塞丁府 Šīndīn-fu = Chên-ting-fu.

In *Bl*, II, 448, 449, Rašidu-'d-Dīn mentions 塞喀巴達 \*Sämkä (?) - [or \*Šämkä-]bahadur in connection with Čayān-balyasun; Sämkä(?)-bahadur seems to be an epithet of 史天澤 Shih T'ien-tsê (BLOCHET's attempts to explain this last purely Chinese name through Mongolian and Manchu are futile), although I find no trace of it in Shih T'ien-tsê's biography (*YS*, 155, 4 b-7 a; T'U Chi, 78, 1-6). It is perfectly true that Shih T'ien-tsê was early in command at Chên-ting, where he came back to die in the beginning of 1275; but Rašid is mistaken when he gives at this point his account of the submission of Čayān-balyasun under Mongka.

RAMUSIO, our only source here, writes «Achbaluch». Now, Fra Mauro, in 1459, mentions on his map a city «Hacbaluch» near a city «Zouza» (*Zu*, 36; HALLBERG, 224, 236, where «Gouza» is not the form actually given on the map). «Hacbaluch» is certainly a wrong form of «Achbaluch», and it cannot be an abbreviation for «Acbaluc Mangi», since «Acbaluc Mangi» is also mentioned by Fra Mauro. But, if we note that «Zouza» is very near the abnormal forms «Gouza» and «Çonça?» peculiar to R and Z respectively for «Giogiu», the inference may be drawn that Fra Mauro knew, among others, a manuscript of Polo very close to the one which gave to RAMUSIO his «Achbaluch» and which is also represented, in an abridged form, by Z.

6. ACMAT<sup>1</sup>

*achmac, achmach bailo R*

RAMUSIO, our only source for this chapter, writes «Achmach Bailo» and «Achmac». I have corrected them to «Acmat», taking -c for a misreading of -t, but after much hesitation, since such changes of finals are also always possible in popular speech. The original name is of course Aḥmad. Polo has a final -t when he speaks of another Aḥmad (see «Acmat»<sup>2</sup>) and the confusions between t and c are so frequent in mediaeval mss. that I think we can safely adopt «Acmat». For the second element, see «Bailo».

Aḥmad, in Chinese 阿合馬 A-ha-ma, a man from Bānākāth (south-west of Tashkend) according to Mussulman writers, was Qubilai's most powerful minister when he was assassinated (April 10, 1282). In addition to his biography in YS, 205 (and in T'u Chi, 106, 1-6), there are countless mentions of Aḥmad and his family in YS and other Chinese works of the time. Rašīdu-'d-Dīn's account and most of the Chinese texts relating to Aḥmad's murder have been translated by MOULE in JNCB, 1927, 1-28; this is not the place to undertake a fresh comparison of them all. I wish only to point out a few additional facts:

1. Although 和尚 *ho-shang* means «Buddhist monk» and, as such, was commonly employed at the time in «vulgar» language as the equivalent of Turk. and Mong. *toin*, it was also then very often used as a personal name, either alone in the case of non-Chinese people, or with a surname prefixed in the case of Chinese. WANG Hui-tsu<sup>1</sup>, 18, 9-10, distinguishes 13 men called Ho-shang in YS alone, and, for others with a Chinese surname, including Kao Ho-shang, feels unable to decide whether Ho-shang is here a personal name or is to be understood as indicating that they were monks.

2. In the case of Kao Ho-shang, Aḥmad's biography (YS, 205, 3 b) calls him a 妖僧 *yao-sēng*, «monk of black arts», and Yü Chi (cf. JNCB, 1927, 34) «the monk of black arts, the *p'u-sa* (*bodhisattva*) Kao»; but Yü Chi was only ten years old in 1282, and knew the story second-hand. That Kao Ho-shang was some sort of a magician is confirmed by an earlier text (YS, 11, 1 a; cf. JNCB, 1928, 257), which says: «The 17th year *chih-yüan*, ... the second month, on the day *i-hai* (March 12, 1280), Chang I said: 'Kao Ho-shang exercises magical practices; he can bring devils (*kui*) to serve as soldiers and [can also] reduce the enemy from afar.' An order was issued that Ho-li-ho-sun (Qoryosun) should take soldiers and with Kao Ho-shang go to the northern borders.» Chang I was a man of note, a colleague of Aḥmad as *p'ing-chang* from 1270 to at least 1275, and it is of some importance to find him interested in Kao Ho-shang already in 1280, as he is one of those who were executed as being party to Aḥmad's murder.

3. As MOULE has justly observed (cf. also JNCB, 1928, 257), it is difficult not to admit that the Kau *finjan* of Rašīdu-'d-Dīn is Kao Ho-shang; but Kao Ho-shang was not *finjan*, that is to say a *p'ing-chang*, and nothing shows that he had anything to do with the siege of Hsiang-yang, while

Rašid attributes there a conspicuous role to Kau *finjan*. There can be no doubt that many instances of confusion have crept into Rašid's story. They may perhaps be partly accounted for in the following way : (a) There was a man called Ho-shang who is mentioned in the accounts of the famous siege, although I doubt whether he was notorious enough to have been heard of by Rašid ; but we are not in a position to speak definitely on that point. (b) Kao Ho-shang, as is shown by the text mentioning the order of March 12, 1280, really had some connection with the army. This is alluded to in Aḥmad's biography when it says : «At that moment, the monk of the black arts Kao Ho-shang, as his magical practices had had no effect while with the army, came back » (the translation in *JNCB*, 1927, 22, is not accurate). Rašid may have had a distorted echo of Kao Ho-shang's campaign with Qoryosun (on whom cf. *JNCB*, 1927, 23). (c) Although Kao Ho-shang was not a *p'ing-chang*, a *p'ing-chang* was really mixed up in the plot, and paid for it with his life.

4. RAMUSIO's Cenchu (see «Cenchu») being the *ch'ien-hu* Wang Chu, his Vanchu (= *wan-hu*, see «Vanchu») must be Kao Ho-shang. Now this title of *wan-hu* was hereditary, and an administration of a *wan-hu* belonging to a man called Ho-shang is mentioned in *YS*, 86, 6 b; but I am not yet in a position to say whether Polo, rightly or wrongly, may have had that *wan-hu* in mind when he calls «Vanchu» (= *wan-hu*) the man who must be Kao Ho-shang.

As to the authorship of that chapter, I think that MOULE (*JNCB*, 1927, 28) still gave too much weight to MURRAY's adverse arguments. I agree with *B*, CLXI, and with *Pe*, 202, that the chapter can have come from no one but Polo himself.

7. ACMAT<sup>2</sup>

*acamat* FA<sup>t</sup>  
*achomach* [soldan]  
*Acmat*, *Acmath* Z  
*acolmat* FA<sup>4</sup>

*acomant* F, Ft  
*acomar* Ft  
*acomat* F, Fr, t, FA, L  
*alcamat* FA<sup>t</sup>

*archomac* LT  
*chomach* (cor.), *chomas* V  
*il Soldano* TA<sup>1</sup>, TA<sup>3</sup>

All editors, including *B*<sup>1</sup>, 437, have kept the «Acomat» of F; but I see no reason not to prefer the «Acmat» and «Acmath» of Z, which is the form that a man knowing Persian as Polo did must have used, and which agrees with the «Achmach» (read «Achmath») given by RAMUSIO for the other Aḥmad, Qubilai's minister (see «Acmat<sup>1</sup>»). For the difficulties raised by a third name, see «Rucnedin Acmat».

The Aḥmad here in question was one of Abaya's younger brothers, and he assumed power after Abaya died on April 1, 1282 (not 1281, as in *B*<sup>1</sup>, 437). He is said to have been baptized in his youth under the name of Nicholas, but later on converted himself to Islam, and took the name of Aḥmad (Hethum says wrongly «Mahomet Can», *Hist. des Crois.*, Arm., II, 186-187). His real Mongol name has long been a matter of doubt, because the texts hesitate between Neküdär (or Negüdär;



see «Negodar») and Tākūdār (or Tägūdār); Muḥammad-khan QAZWĪNĪ gives strong arguments in favour of Tägūdār (cf. his edition of Jūwainī I, XLIII and نَب), which is the form used in 1307 by Hethum («Tagodar» in *Hist. des Crois.*, Arm., II, 185; see also «Caraunas»). Abaya had named his son Aryun (see «Argon<sup>1</sup>») as his successor, but Aryun had to wait before he could vindicate his rights. The fight then began, continuing with varying fortunes, as in Polo's account, and ended in Aḥmad's murder. The «very great and beautiful plain» of Vol. I, 458, is Aq-ḥōjah, to the east of Qazwīn. Aḥmad reigned from May 6, 1282, to Aug. 10, 1284. In Y, II, 474, Aryun, seeing Aḥmad prisoner, is made to exclaim «Morio», a word used by the Mongols when successful in archery competitions; but *Ha*<sup>1</sup>, I, 358, actually gives «Mordio»; without the Arabic spelling, I dare not attempt an explanation.

The names of Aḥmad appear as «Tangodar» (variant «Tagodar»), «Nicole» and (wrongly) «Mahomet Can» in Hethum the historian (*Hist. des Crois.*, Arm., II, 185, 312). Vardan and Steph. Orbelian write «Takudar» (PATKANOV, *Istoriya Mongolov*, I, 25, 49). The Latin translation of Aryun's letter of 1285 to Honorius IV gives «Ameto» (CHABOT, *Hist. de Mar Jabalaha III*, 190). On Aḥmad's tomb (?) at Kašān, cf. *TP*, 1930, 439, and see here «Caraunas».

As to the names Nāgūdār and Tägūdār, I have not yet found any Nāgūdār in Mongol or Chinese texts; but a Salji'ut 帖古迭兒 T'ieh-ku-tieh-êrh is mentioned in T'u Chi, 41, 6 a, from an inscription (he does not appear in the corresponding biography of YS), and from this Chinese transcription, we know for certain that the correct form is Tägūdār, in agreement with Hethum's «Tagodar», and not «Tākūdār» as has been generally assumed.

Aḥmad-Tägūdār seems to have been brought up in Mongolia (see «Caraunas»); in such a case, his baptism, if he really was baptized in his youth, must have taken place there.

### 8. ACRE

*acchon, achon* LT  
*acon* Z  
*achre* VA

*acon* L, P, P<sup>5</sup>; G  
*acre* FA, FB, L, O, VB, VL;  
R

*acri* F, L, TA<sup>1</sup>, TA<sup>3</sup>  
*agri* Lr  
*anchona* V

«Acre» is the French form, «Acri» the Italian, «Achon» the Latin. The name already occurs in the Bible as 'Akko (*Judges*, I, 31); it is 'Akka in Arabic. I have preferred the French form, since we admit that Polo's book was written by Rustichello in French, and we have retained all Western names in French forms.

Acre, or Saint-Jean d'Acre, often called Ptolemais, conquered by the Crusaders on May 26, 1104, was for almost two centuries the main port of landing for the «passage» to the Holy Land. It fell again into the hands of the Mussulmans on May 28, 1291.

## 9. ADAM

*adaam* L  
*adam* F, FA, FB, LT, TA<sup>3</sup>, V, VA, VB, Z; G, R  
*adamo* TA<sup>1</sup>, TA<sup>2</sup>; R  
*adan* F, V  
*addam* LT

On Adam's Peak, cf. *Zu*, 51; *Y*, II, 320-322; *Fe*, 688; DAMES, *Barbosa*, II, 117; HALLBERG, 6-7; *BEFEO*, IV, 358-359; *HR*, 73-75; *TP*, 1915, 379; 1933, 433.

The name of Adam is written 阿談 A-t'an (\*Â-d'âm) in a Nestorian work of about the 8th cent. (cf. SAEKI, *The Nestorian Documents*, Tôkyô, 1937, 213, and Chin. text, 56); 阿耽 A-tan in the Sino-Jewish inscriptions of 1489, 1512 and 1663 (TOBAR, *Inscr. juives*, 36, 57, 65); and 阿旦 A-tan by Ma Huan in the first half of the 15th cent. (both *tan* were pronounced *tam* until about 1400-1450, and, from the 11th to 12th cent., a Chinese unaspirated *t*- served to render a foreign *d*-).

While associating the name of Alexander the Great with the wonders of Adam's Peak, Ibn Baṭṭūṭāh does not ascribe to Alexander the making of the chains spoken of by Polo; YULE quotes such an ascription only from a Persian poet of the 15th cent. But this last form of the legend must have existed earlier; Mufazzal knows it in the middle of the 14th cent. (cf. BLOCHET, *Moufazzal*, 697; the note is irrelevant). Western travellers also heard of it; it occurs on Fra Mauro's map (*Zu*, 51).

## 10. ADEN

*adam* FA<sup>t</sup>, LT  
*adan* F, L, V, Z  
*adem* P, VA, VB, VL; R  
*aden* F, Fr, t, FA, FB, L, V (cor.), VB, Z  
*adenti* TA<sup>1</sup>, TA<sup>2</sup>  
*adom*, *adon* G  
*andar* V  
*arbe* TA<sup>2</sup>  
*arden*, *dan*, *denti*, *edenti*, TA<sup>1</sup>  
*edem* LT, P, VA; G  
*eden* F, LT

Polo deals with Aden (Ar. 'Aden) from hearsay, but I am not inclined to believe in the occasional confusion between Aden and Adel to which YULE refers, though with some diffidence, in *Y*, II, 433. Cf. also *EI*, s.v. «'Aden» (very scanty); HALLBERG, *L'Extrême-Orient*, 8-10; L. DAMES, *Barbosa*, I, 53-58; *Fe*, 688. The name of Aden, 阿丹 A-tan, does not appear in Chinese texts before the first quarter of the 15th cent.; cf. DUYVENDAK, *Ma Huan re-examined*, 59-62; *TP*, 1933, 343, 420-422 (where I have shown that, in 1421 [or 1416-1419?], the name of the golden coin then in use at Aden was known under the Western name of *fulūri*, «florin»); *TP*, 1935, 287. For a valuable description of Aden in Arabic, then unpublished, cf. FERRAND, in *JA*, 1921, II, 320. Important native sources on Aden have been recently edited by Oscar LÖFGREN, *Arabische Texte zur Kenntniss der Stadt Aden im Mittelalter*, Leipzig-Upsal, 1936.

It may be partly due to the obscurities of Polo's second-hand information on Aden that

two different places called Aden occur on some old maps, as for instance on that of Martin Behaim.

The name of 'Aden is not traced back with certainty beyond the Middle Ages; but R. DUSSAUD, in *Rev. de l'Hist. des Relig.*, CVIII [1933], 43, 47-48, has shown that it was possibly mentioned, in the 5th cent. B.C., by Ezekiel, XXVII, 23.

## 11. AGIUL

*aguil* F  
*anguil* VB

*angul* R  
*eguil* FA, FB

*giel* V

Although admitting that the name can be read «Agiul» in the best ms. (F), BENEDETTO (*B<sup>1</sup>*, 438) has kept «Aguil» in his edition, and so has RICCI (*RR*, 211), but it is obviously «Agiul» that is correct (*RR*, 410). It is said in *Y*, II, 138, that «Agul» was the *name* of a prince, father of Nayan (see «Naian»), and this has been interpreted in *RR*, 410, and *B<sup>1</sup>*, 437, as meaning that the prince «Agul» was the man who fought against Li T'an. *B<sup>1</sup>*, 437, adds that, according to CHARIGNON, our «Agiul» is «Arcu», the son of Uriyangqadai. YULE was right in saying that the names are the same, that is to say not «Agul», but أجول Aju (cf. *Bl*, II, 94), but the individuals are different. On the other hand, CHARIGNON was equally right when he said that we have here to deal with Uriyangqadai's son, but he only gives the Chinese form 阿朮 A-shu (= Aju); «Arcu» itself does not exist (it is not even one of the valueless forms introduced by Ch'ien-lung's commissioners).

If I say that the names are the same, it is partly on the authority of Polo's «Agiul». Rašidu-'d-Dīn, who mentions our A-shu, calls him simply أجو Aju (*Bl*, II, 449), and the Chinese form A-shu (once 兀朮 Wu-shu in *YS*, 127, 1*b*) does not permit us in itself to decide between \*Aju and Aju; on the other hand, Aju, father of Nayan, is called in Chinese 阿朮魯 A-shu-lu (*YS*, 107, 3*b*). But Waśśāf (*Ha<sup>2</sup>*, 40; *Oh*, II, 397), who also speaks of our A-shu, calls him أجون Ajun, easily miswritten for Aju, and so supports indirectly Polo's «Agiul». There are several A-shu and several A-shu-lu named in *YS* (cf. WANG Hui-tsu<sup>1</sup>, 17, 10*a-b*), and it is possible that the real name of all of them is Aju. Unfortunately, I do not know the origin of the name, nor its meaning. The question is made still more obscure by the form given to the name of Uriyangqadai's son in Rašid's account of Mongol and Turkish tribes; there that son is called اوجوقان Ujuqan (in the doubtful passage missing in *Ber*, I, 146, and thus restored in the introductory remarks to the Persian text, p. XIV) and اوجو Uju (p. 197). The first labial vowel falls in with one of the Chinese transcriptions. It looks as if the name actually was \*Aju, also pronounced \*Uju; Waśśāf's «Ajun» would show the instable paragogical -n of Mongol endings, and Polo's «Agiul» would only provide one more example of a copyist's final flourish mistaken for an -l or -r. In such a case, «Agiul» would have nothing to do with the other well-attested name Aju.

Whatever may be the true name, the A-shu mentioned in Li T'an's biography (and in *YS*,

5, 2*a*; cf. *Ch*, III, 13), the «Agiul» of Polo, is certainly, as CHARIGNON has said, the son of Uriyangqadai, son himself of the great Sübötai. His biography is in *YS*, 128 (also in *T'u Chi*, 91). In 1253-1255, he had fought in Yün-nan under the eyes of his father; later on, he took part in the siege of Hsiang-yang (see «Saianfu») and in the rest of the campaign against the Sung. Early in 1276, he was sent against the rebel princes of the North, and returned victorious in the winter of the same year. Soon afterwards, he started another campaign against Qarā-ḥōjo, in the course of which he died, at the age of 53 («54» *more sinico*; according to *YS*, 11, 4*b*, he died in Beš-baliq, and the Court heard of his death in the beginning of 1281; his dates must thus be 1227-1280). His participation in the fight against Li T'an is expressly mentioned: «In the third year *chung-t'ung* (1262), he marched against Li T'an under the command of Prince Pai-ch'u (Baiču?) and of T'ieh-ko (\*Tägä?).» On the last two names, see «Mongatai».

## 12. AIGIARUC

*agiaint* FA  
*agyanie* FB  
*agiarme* TA<sup>1</sup>

*agiaruc* F  
*argialchucor* LT  
*ayaruc* Z

*aygiarne* TA<sup>3</sup>  
*chaizenich* V

In Turkish, *Ai-yaruq* means «Moonshine», as Polo says, and is quite a normal name for a woman (cf. *Y*, II, 465-466). In *TP*, 1930, 272, 440, I have pointed out that the spelling of F and others seemed to be based on a pronunciation *Ai-ḡaruq* of «Kirghiz» type. The «Ayaruc» of Z may be a copyist's slip for «Agiaruc». There are in the Polian mss. other names where -i- and -gi- alternate (cf. for instance «Caraian» and «Caragian» in F), but these occur almost exclusively where there is really a -ḡ- in the original name (see «Caragian»), and I suppose it is also the case here.

Qaidu's daughter is known to Eastern writers only under her true name of Qutulun (*Ha*<sup>1</sup>, II, 143-144; *Bl*, II, 9), but even with them her history has a tinge of romance, and it is quite possible that the by-name *Ai-yaruq* clung to her popularly. In *Bl*, II, 9, قۇتۇلۇن Qutulun's name is followed by a term چىغان which the editor has corrected to چىغان; so the name seems to be Qutulun Čayan, but no reason is given for the emendation; BARTHOLD, in MINAEV's *Marko Polo*, 315, had retained the original reading of Rašid's ms. \*Qutulun Čaya. In his Appendix, however, BLOCHET (*Bl*, II, App. 16) quotes from the *Mu'izzu'l-Ansāb* a passage according to which Qaidu had two daughters, Qutulun Čayan and Qutučin Čayan («Koutlough Tchaghan» of *Bl*, I, 165 has no value). Qutulun is a woman's name in -lun, derived from *qutuq*, «fortune», and of the same type as for instance Nomolun or Tämülün; Qutučin is also a feminine form derived from the same word.

To what YULE said of the life of Qutulun, it may be added that her father, suspected of loving her not as a father, finally married her to one of the high officials of his household; she had herself chosen her husband and bore him two sons (cf. BARTHOLD, *loc. cit.* 315; *Bl*, I, App. 16, where the name of Qutulun's husband is doubtful).

## 13. ALAINS

*alains* FB, FC<sup>1</sup>*alani* F, P, VA, VB; G, R*alau* VL*allani* VA*alguni (christiani)* V

The Alans, an Iranian speaking people, make their appearance as « Alani » (later sometimes « Halani » Greek Ἁλανοί), in classical literature in the first half of the 1st cent. A.D.; the confederation of their tribes seems to have extended, then or somewhat later, from north of the Aral Sea to the basin of the Don.

The name occurs also in Chinese texts at an early date. It has often been said that, according to the *Hou-Han shu* (5th cent.), the name of the kingdom of 奄蔡 Yen-ts'ai, said to represent Aorsoi, was changed to that of 阿蘭那 A-lan-na (ROCKHILL, *Rubruck*, 88; Y<sup>1</sup>, III, 186; IV, 271; « A-lan-ya », which occurs twice, is a slip or a misprint in *Br*, II, 87). But, first, the equivalence Yen-ts'ai = Aorsoi is far from evident phonetically (cf. *TP*, 1923, 132). Again the *Hou-Han shu* has no A-lan-na, only A-lan-liao [聊], which has passed into *Ts'ê-fu yüan-kuei*, 961, 3 b, and *T'ung chih*, 196, 25 b. The reading A-lan-na, quoted by Abel RÉMUSAT (*Nouv. Mém. asiat.*, I, 239) from *Wên-hsien t'ung-k'ao* (338, 4 a), goes back in fact to Tu Yu's *T'ung-tien* (compiled from 766 to 801; 193, 2 a), from which it has passed into *T'ai-p'ing yü-lan* (793, 9 a), *T'ai-p'ing huan-yü chi* (183, 7 a) and *Wên-hsien t'ung-k'ao*; it is clearly a case of clerical error, one way or the other. But the *Hou-Han shu* copies here the *Wei lio* of the first half of the 3rd cent. A.D., and the *Wei lio* has only « the kingdom of Yen-ts'ai which is also called A-lan ». As the *Wei lio* names also a kingdom of 柳 Liu, it is clear, as CHAVANNES pointed out already in 1905 (*TP*, 1905, 559; 1907, 195), that it is that Liu which, in the form *liao*, is an erroneous addition to A-lan in the *Hou-Han shu*, and the A-lan-na of the *T'ung-tien* is in its turn a corruption of the false name A-lan-liao. We must read A-lan = Alani, in all passages. As to the change of name from Yen-ts'ai to A-lan, I take it to mean that, about the beginning of the Christian era, the supremacy in the Aral region had passed from the Yen-ts'ai tribes to the Alans proper. In the same way texts tell us that the name of the country later became Su-t'ê, i. e. Soydak (the « T'ê-su » still quoted by CHARPENTIER, *ZDMG*, 1917, 365, is a late interversion of characters); this does not mean that the Alans and the Sogdians are identical.

On the authority of DE GUIGNES, *Hist. gén. des Huns*, II, 279, and of VIVIEN DE SAINT-MARTIN, *Et. de géogr. anc.*, Paris, 1850, I, Les Alains, 109-110, CHARPENTIER (*loc. cit.* 361) says that, according to Chinese texts, the A-lan were so called « from the Altai mountains, from which they came originally », and does not shrink from bringing in also the Manchu word *alin*, « mountain ». But there is not one word in Chinese texts as to the origin of « A-lan »; DE GUIGNES and VIVIEN DE SAINT-MARTIN have simply, and vainly, tried to account for the sentence in Ammianus Marcellinus, « . . . Halani . . . , ex montium appellatione cognominati », by « Altai » or « alin ».

Apart from the *Wei lio* (and the *Hou-Han shu* copying the *Wei lio*), I know only of one independent mention of the A-lan in Chinese texts before the Mongol period. It is true that the

*T'u-shu chi-ch'êng*, Pien-i-tien, 67, quotes a text on the « Kingdom of A-lan » from the *Wei shu*, 101, 7 a, which would also seem to provide a mention of the name in the 4th-6th cent. A. D. But the location it gives does not fit that of the Alans. As the parallel text in the *Pei shih* (96, 8 b) writes 可蘭 K'o-lan, and as this last form is confirmed by a citation from an independent source in *T'ai-p'ing yü-lan*, 797, 19 a, there can be no doubt that K'o-lan (\*Qalan, \*Qaran?) is correct; the Alans are out of the question (cf. HALOUN, in *ZDMG*, 1937, 275). It is a little later, for about 600 A. D., that the *Sui shu* (84, 8 b; copied also in *Pei shih*, 99, 9 a) mentions the A-lan as living east of Fu-lin (< Früm = Rüm, the Byzantine Empire); although these A-lan are included in a list of nomad tribes which are mainly of Turkish origin, the Alans are certainly meant.

The name A-lan of the Alans occurs only once more in Chinese, on the map of the *Ching-shih ta-tien* of c. 1330, from which it passed into *YS*, 63, 16 a; the form is there a double one, 阿蘭阿思 A-lan A-ssü, Alan-As (« A-su » is a slip in *Br*, II, 88). But the map is really copied from a Western original; Alan is not the name under which the Alans were then known to the Mongols and the Chinese; the form A-ssü itself, although found occasionally in Chinese texts, is also rather a Western one, representing As (it is used twice in *YS*, 63, 16 a; cf. *JA*, 1920, I, 166; also in *YS*, 18, 4 b); but the *Secret History of the Mongols* (§§ 262, 270, 274) uses Asut, a regular Mongol plural from As, and it is this Asut which is correctly rendered by the Chinese transcription 阿速 A-su, occurring hundreds of times in the Yüan period (BRETSCHNEIDER [*Br*, I, 305; II, 99] is wrong in saying that Asut is a plural of « Asu »; « Asu » does not exist; CHARPENTIER, *loc. cit.*, 362, who still believes that A-su remains unexplained, is mistaken when he attributes also the form A-su to Rašidu-'d-Dīn, and, in the *u* of A-su [= Asut], a specifically Mongol form, sees an « evident » parallel with Ptolemy's Ἀσιῶται). An abnormal spelling 阿宿 A-su is used once, in 1316 (*YS*, 25, 4 a); an adjectival form in -tai, 阿速帶 A-su-tai, \*Asutai (< \*Asutai?), occurs once, in 1283 (*YS*, 12, 9 b), and is also known as the personal name of various individuals; 阿蘇 A-su in *JA*, 1920, I, 168, is a slip (the *YS* has here the usual form).

The two names « Alan » and « As » for the mediaeval Alans are well attested; « Alani sive Assi », says Plan Carpine (*Wy*, 89); Rubrouck, while still west of the Don, speaks of the « Alani qui ibi dicuntur Aas », and a little later of the « Alani sive Aas » who live in the high mountains of the south, together with the Circassians (*Wy*, 191, 199; the reading « Acias » of some mss. is corrupt, and moreover would not give the « Akas » mentioned in QUATREMÈRE, *Hist. des Mongols*, 70, *Pa*, 487, and *Mo*, 260, 261, still less the « Aq-As », « White As », which I have seen somewhere, but cannot trace). On the other hand, nobody has ever doubted that descendants of at least one branch of the great Alan nation are to be found to-day in the Iranian Ossets of the Caucasus (in the neighbourhood of the Dariel Pass), so called from the Georgian form Owset'i of an original Ows, the same name as As or « Aas », آس Ās or آس Ās in Mussulman mediaeval writers (the Georgian final, despite T'u Chi, 160, 17 b, has of course nothing to do with the Mongol plural in -ut of Asut). I cannot discuss here to what extent we must, strictly speaking, make a distinction between Alan and As in the history of the Caucasus.

Opinions are at variance as to the earliest appearance of the name As. MARQUART (*Osteurop. und Ostasiat. Streifzüge*, 167) has proposed to identify with As the last part of names like Doh-sās and Tūlās (or Tūwalās), so that the name Ās would already be attested in the first half of

the 9th cent.; MINORSKY (*Mi*, 445, 456, 457) favours this view. BARTHOLD (*EI*, s. v. «Allān») has doubted the correctness of MARQUART's interpretation, and says that the name As, or Al-Ās («The Ās»), appears only in the Mongol period, and only in Oriental sources, while missionaries and travellers speak only of the Alans (this is wrong, as is shown by the above-quoted passages of Plan Carpine and Rubrouck). But everybody admits that the name Yasy (plural of Yas), used in Russian chronicles when referring to the middle of the 10th cent., is the very name of the As or Ossets, and this carries it back well before the Mongol period. The same can be said of the «As» in Al-Bīrūnī (*Mi*, 481) and of probable mentions of the As in the ᾿Αζία of Constantine Porphyrogenet (cf. *Mi*, 445) and in a term of the letter of the Khazar ruler discovered in the Cairo Genizah (cf. KOKOVCOV, in *Žurn. minist. nar. prosv.*, Nov. 1913, 163).

The name has been said to be much more ancient still. DE GUIGNES, and after him VIVIEN DE SAINT-MARTIN (105-106, 154-159, 178), have thought that it was the original of Yen-ts'ai, and also of 安息 An-hsi (misread by them A-hsi, «A-si», and moreover confounded by VIVIEN with the purely Chinese name 安西 An-hsi of T'ang times). Moreover, VIVIEN saw also the Ossets in the «Essedones» of Pliny, and believed in the great age of the Georgian mentions of the Ows (or Ossets). Yen-ts'ai may or may not be Aorsoi, but it has certainly nothing to do with Ās; and 安息 An-hsi transcribes Arsak, the Arsacide kingdom; the identification of the «Essedones» is arbitrary; the Georgian chronicles are fraught with anachronisms. It would thus seem that this part of VIVIEN's monograph may be dismissed without more ado, if it were not that VIVIEN has come to certain conclusions which are still too readily accepted. Believing the name of As, Ows, to stand for the Ossets of the Caucasus from very ancient times, and admitting at the same time that the same name was transcribed, when the Central Asian Alans are referred to, as Yen-ts'ai and An-hsi, VIVIEN has supposed that the real tribal name had been superseded by Alan, a name derived from a word meaning «mountain» similar to *alin* in Manchu and given to the As by some Eastern neighbour. This is exactly the position adopted, after VIVIEN, by CHARPENTIER in 1917, simply replacing the exploded Yen-ts'ai and An-hsi by the Chinese name 烏孫 Wu-sun and by the ᾿Ασιῶται and Asiāni of classical writers (*loc. cit.*, 364-365). The identification of the Wu-sun and the real value of ᾿Ασιῶται or Asiāni are two of the moot problems of Central Asian history, and I do not intend to discuss them here. But I wish to say explicitly that there is no basis whatever in Chinese texts for the would-be substitution of a foreign appellation «Alan» for a former native name «As» or «\*Os», nor anything in them that connects the Wu-sun with the Alans.

On the contrary, it seems that «Alan» represents the original name of that Iranian confederation. It is well known that the Caucasian Ossets are so called by their neighbours, but that they call themselves «Irōn», *i. e.* Iranians, from the same root as Aryan. Beyond Sogdiana, Herodotus speaks of the Scythian ᾿Αρειῶται, where the Achaemenid inscriptions mention the Haraiva. Now GAUTHIOT (*Grammaire sogdienne*, III) has already remarked that, according to a law discovered by ANDREAS, \*-ry- became -l- in the language of those Northern Iranians, so that these tribes, called originally «Arya-» (cf. ᾿Αριαυ.), became normally Ala-, Alani (the same has been said since by MARQUART himself in W. DOEGEN, *Unter fremden Völkern*, 1925, 380-381); their name is the same as the native name «Irōn» of the Ossets.

A good bibliography on the Alans is given in *Mi*, 444; the main Chinese texts relating to the Alans («Asut») of the Mongol period have been summarized or translated in *Br*, II, 84-90, by MOULE in *JRAS*, 1917, 21-26, and in *Mo*, 260-264, and, in Russian, by A. I. IVANOV, in *Khristianskii Vostok*, II (1913), 281-300. The old Alan confederation no longer then existed, and the Alans of the Chinese texts, the Asut, are Caucasian Alans; they are mentioned first in connection with the conquest of their country, and afterwards, on many occasions, because they formed, after deportation, an important part of the Imperial Guard.

The *Secret History* of 1240 mentions a town of Mākāt (§ 270) or Mägät (§§ 274, 275). In *JA*, 1920, I, 168, I have supposed it to be identical with the «city of 蔑怯思 Mieh-ch'ieh-ssü (\*Mäkäs) of the A-su (Asut)» which was conquered by Mongka in the winter of 1239-1240, after a siege of three months (*YS*, 2, 3*b*; T'u Chi, 47 2*a*, corrects it to 1238-1239); the name is written 滅怯思 Mieh-ch'ieh-ssü in *YS*, 122, 6*a*; 麥怯斯 Mai-ch'ieh-ssü in *YS*, 128, 6*a*; 麥各思 Mai-ko-ssü (\*Mägäs) in *YS*, 132, 4*a*; it occurs also in an inscription of the 牧菴集 *Mu-an chi*, 13, 8*b*; but that important document is useless as we have it, since all foreign names in it have been «corrected» by the Ch'ien-lung commissioners; cf. also *ibid.*, 19, 10*b*. I had since noted the phonetic similarity of \*Mäkäs, \*Mägäs, with معص Ma'as or مغان Mayaś, given in the 10th cent. by Mas'ūdī (II, 42) as the name of the Alan capital (cf. MARQUART, *Osteurop. Streifz.*, 165); recently, MINORSKY (*Mi*, 446) has also, independently, thought of a connection between the two names. I had further quoted, as representing probably the same name, the مکس Mäkäs of Rašidu-'d-Dīn; MINORSKY (*Mi*, 446), who cites other passages from Jūwainī, thinks that this last name is more likely to apply to the Mokša, a Mordvan tribe; without being dogmatic, I still incline to my former view. The question is further complicated by the mention, in *YS*, 2, 4*a*, of a 木柵山寨 *mu-cha shan-ch'ai* in the region of the Asut (A-su; absurdly altered into Aqsu by the 18th cent. commissioners). «*Mu-cha shan-ch'ai*» can be understood either as «the mountain stronghold of *Mu-cha*» (this is the view taken by T'u Chi, 4, 14*a*, but his interesting note is marred by an error which makes of «Mas'ūdī» himself the name of an Alan city), or as «the palisaded mountain stronghold»; I favour this last interpretation (*mu-cha*, «palisade», occurs for instance in *YS*, 8, 9*b*), and think that this unnamed stronghold may also be our Mäkäs or Mägäs. Of Mäkäs, all I can say is that T'u Chi, 47, 2*a*, I do not know on what authority, makes it back on the mountains and face the sea (perhaps T'u Chi has applied to Mäkäs what is said later of the capital of the kingdom of A-su in the *Ming shih* [cf. *infra*]; the information would then be irrelevant).

Although some of the Alans were still opposing the Mongols when Rubrouck went to Mongolia, a great part of the nation, with their princes, had then already been carried to the Far East to serve with the Mongol armies. Nevertheless Marignolli is mistaken when he speaks of Alan troops in Chinghiz-khan's army (*Wy*, 543); the date of 1229 given by DEVÉRIA (*JA*, 1896, II, 432) seems to be also too early; troops were levied against the As (A-ssü) in the autumn of 1230 according to 秋閒集 *Ch'iu-chien chi*, 51, 7*a*, but this date must be wrong, and is corrected to 1235 by T'u Chi, 47, 1*b*. I doubt even that the enrolling in the Imperial Guard of half the troops of the Alan prince Arslan should be dated 1236-1237 (DEVÉRIA, 432); 1239-1240 or 1238-1239 are perhaps more likely. Anyhow, the Alan corps lasted until the end of the Yüan



dynasty. Marignolli (*W*y, 526) gives an estimate of 30,000 Alains in China. The «Right Asut Guard» had been created in 1272; the «Left Asut Guard», in 1310 (YS, 86, 6b-7a; 99, 3a). Towards the end of the Mongol dynasty, the 庚申外史 *Kêng-shên wai-shih*, or «Unofficial history of Shun-ti» (ed. *Hai-shan-hsien-kuan ts'ung-shu*, I, 22; ed. *Pao-yen-t'ang pi-chi*, I, 30-31), speaks of the revolt of the «Red Army» in 1367 and of 6,000 Asut who were sent to quell it; the author adds : «The Asut are Green-eyed Moslems (綠睛回回 *lû-ching Hui-hui*).»

Polo speaks of a massacre of Alan troops when Bayan took Ch'ang-chou in 1275, and T'u Chi (160, 17a) has said that Polo's information was a valuable addition to the Chinese texts on the history of Ch'ang-chou; PAUTHIER (*Pa*, 486) has explained, as a retaliation for that massacre, the savage treatment the inhabitants of Ch'ang-chou suffered at the hands of Bayan when the city was stormed. But I have already remarked in *TP*, 1914, 641-642, that Polo's memory must here have failed him. The massacre of the Alains took place not at Ch'ang-chou, south of the Yang-tzû, but north of the river, at 鎮巢 *Chên-ch'ao*, the modern *hsien* of 巢 *Ch'ao* (formerly of Lü-chou-fu, and now of the *tao* of An-ch'ing, Anhui). My view has been accepted in *Y*, III, 96; *Y*<sup>1</sup>, IV, 271; *Mo*, 140-141; *Ch*, III, 69; *RR*, 415; *B*<sup>1</sup>, 441. But, in my paper of 1914, I simply stated the bare facts, without mentioning the sources, and my theory would seem to receive a fatal blow from a passage of the *pên-chi* of Qubilai in T'u Chi, 7, 31a-b, where we read : « [In 1275.] . . . the fifth moon, . . . on the day *jen-ch'ên* (June 17, 1275), the military governor of P'ing-chiang, Liu Shih-yung, the *tien-shuai* Chang Yen, the military governor Wang An-chieh, [all officials] of the Sung, assailed and put to death the Asut (A-su) garrison of Ch'ang-chou and chose Yao Yin to conduct the affairs of [Ch'ang-]chou, which came again under the rule of the Sung.» But T'u Chi's text is a combination of YS, 8, 10a (*pên-chi* of Qubilai) and of YS, 127, 4b (biography of Bayan), with one important exception : the two texts do not name the Asut. We have seen that T'u Chi considered Polo's text as providing new material for the history of Ch'ang-chou; it is perfectly clear that he has added the Asut in the present passage of the *pên-chi* on the sole authority of Marco Polo; in fact, he says so himself in a note to his biography of Bayan (90, 6b).

Bayan's biography (YS, 127, 4b) and the *P'ing Sung lu* (ed. *Shou-shan-ko ts'ung-shu*<sup>2</sup>, 2, 2b-3b; there are here minor discrepancies of dates and names between the YS and the *Sung shih*; cf. T'u Chi, 90, 6b) confirm that Ch'ang-chou had first submitted (on April 8, 1275, according to *Sung shih*, 47, 4a), then revolted again (on June 17, according to YS, but on June 2, in *Sung shih*, 47, 5a); the Sung officers held the city again for half a year. On the day *jen-wu* of the 11th moon (December 4, 1275), Bayan arrived himself outside Ch'ang-chou, which the Mongols had surrounded with a wooden palisade (the *čäpär* of QUATREMÈRE, *Hist. des Mongols*, 336-337) and a large and deep moat. Twice Bayan tried to prevail upon the inhabitants of the city to surrender, pledging himself to spare the inhabitants, and then threatening them with wholesale slaughter if they disregarded his offer. No answer came. Finally, on the day *chia-shên* (December 6, 1275; the date of December 6, 1275, is also given in *Sung shih*, 47, 7b), the infuriated general ordered the assault, and was the first to plant his red flag on top of the city wall; the city was sacked and the inhabitants butchered.

But there is in all these texts no mention of the Asut, while there are several concerning Chên-ch'ao; I shall quote the main ones :

α. YS, 132, 4*b*, biography of Ang-chi-êrh, a Hsi-Hsia man (not an Alan as is said in *Mo*, 141) : Ang-chi-êrh's father had submitted to Chinghiz-khan with his men in 1221, and Ang-chi-êrh succeeded in the command of these Hsi-Hsia troops, to which others of the same nation were added. When Bayan passed south of the Yang-tzü, he ordered Ang-chi-êrh to remain in the north and to pacify the western half of the region between the Huai and the Yang-tzü. «The [seat of the] military district of Chên-ch'ao submitted and was garrisoned by Asut troops. The population would not tolerate their harshness. The [former Sung] commander [of the military district] (*tu-t'ung*), 拱福 Hung Fu, killed the whole [Asut] garrison and revolted. Ang-chi-êrh attacked and stormed the city and captured [Hung] Fu. . . »

β. YS, 132, 1*a* [and cf. T'u Chi, 102, 11*a*], biography of the Asut Hang-hu-ssü : «In 1270, the son of Hang-hu-ssü, A-t'a-ch'ih, helped in conquering the important pass of Wu-ho-k'ou (in the district of Wu-ho, Anhui). In 1274, he helped in conquering the prefectures of 松江 Sung-chiang and others (I suspect that the text is altered and I propose to read 沿江 *yen chiang*, 'along the Yang-tzü'; Sung-chiang is the region in which lies Shanghai, far from Anhui) and garrisoned Chên-ch'ao. The population would not accept the orders. The Sung general Hung Fu, who had submitted, resorted to trickery, and taking advantage of an occasion when [A-t'a-ch'ih] was drunk, he killed him. Shih-tsu (= Qubilai) expressed compassion for [A-t'a-ch'ih's] death and granted to his family 500 taëls of silver, 3,500 strings of cash in paper money and [the ownership of] 1539 families of Chên-ch'ao ».

γ. YS, 132, 2*b* [and cf. T'u Chi, 102, 12*a*], biography of the Asut Yü-wa-shih : Yü-wa-shih's father, Yeh-lieh (Eliya?), who had been appointed chiliarch in the army of the Asut, «helped in conquering Hsiang-yang (see 'Saianfu'), then helped in conquering the cities along the Yang-tzü. The Sung governor (*an-fu*) Hung [Fu], after he had submitted, revolted again; he induced [Yeh-lieh] to enter the city, and, at a banquet, profiting from his [Yeh-lieh's] drunkenness, he killed him. . . Yü-wa-shih succeeded his father as chiliarch of the army of the Asut, and followed the minister Bayan in the pacification of the Sung. He was granted [the ownership of] 2,052 families of Chên-ch'ao.»

Of course, it might be supposed that another massacre of drunken Alans, of which there is no trace in history, took place when Ch'ang-chou revolted; but the coincidence is hardly possible, and the texts quoted above leave no doubt, in my opinion, that the Alans were killed at Chên-ch'ao, not at Ch'ang-chou. Polo has wrongly connected with Ch'ang-chou an incident which occurred elsewhere; the rebellion of both cities after they had submitted and the phonetic similitude between Chên-ch'ao and Ch'ang-chou are probably responsible for the confusion.

The date of the massacre at Chên-ch'ao cannot be 1274, as is stated in *Mo*, 262; T'u Chi, 102, 12*a*, has adopted 1276, which is not quite certain either. Hung Fu's stubborn resistance and death at Chên-ch'ao are narrated in his biography in the *Sung shih*, 451, 2*b*, but without any allusion to the Asut and without precise dates. According to the *pên-chi* of the *Sung shih*, 47, 4*b*, 5*a*, after the seat of the military district of Chên-ch'ao had submitted to the Mongols, it was reconquered by Hung Fu in 1275, 4th moon, on the day *jên-yin* (April 28), and, as a consequence,

the Sung Court gave Hung Fu official authority over the military district of Chên-ch'ao in the 5th moon, on the day *i-hai* (May 31). In *YS*, 9, 3 *a*, Hung Fu's rebellion and his death are all told together under the *wu-wu* day of the 2nd moon of 1276, *i. e.* March 9, and this is certainly why T'u Chi decided in favour of 1276. But the indications of the *pên-chi* of the *Sung shih* are here very precise. If there is no mention there of Hung Fu's capture and death, it is because in March 1276 the Sung dynasty had almost fallen, and communication between Chên-ch'ao and Hang-chou was no longer possible. I believe the Asut were massacred when Hung Fu revolted on April 28, 1275, but Hung Fu held on for a whole year before coming to a dire end himself on March 9, 1276. In a text relating to the Asut guard, *YS*, 99, 3 *a*, it is said that «in the 23rd year *chih-yüan* (1286), the Asut army attacked in the south Chên-ch'ao and suffered heavy losses». The date is much too late, and an error must have crept into the text. The most natural correction (more natural though in Arabic figures than with Chinese characters) would be to read «13» instead of «23», and this would agree with the apparent 1276 of the *pên-chi* in *YS*; but the author of the note may have thought of the whole incident together as is done in the *pên-chi*; or the error «23» may stand for «12».

A text of the *Hsü t'ung-chien kang-mu* adds to the accounts of the siege of Ch'ang-chou by Bayan given in the *YS*, the *Sung shih* and the *P'ing Sung lu*, a detail which has not passed into T'u Chi, but to which attention has already been called in *Pa*, 485-486 : after forcing the inhabitants of the suburbs of Ch'ang-chou to carry earth and build a wall of approach to the city, Bayan killed and burnt them and used the boiling fat of the corpses to manufacture fire-missiles which were thrown to set alight the wooden «chevaux-de-frise» of the wall battlements 煎油作礮焚其牌杈; the text is not very clear, but PAUTHIER's rendering is certainly inaccurate). Pauthier insists that such a cruel deed can only be explained as a retaliation for the death of the Alans. The Alans, in my opinion, have nothing to do with this case, but it is true that Bayan might have wished to take revenge after Ch'ang-chou had rebelled and had resisted the efforts of the Mongols for more than five months. The account given in the *Hsü t'ung-chien kang-mu* must be derived from the *Ch'ien-t'ang i-shih*, a work of the Yüan period (ed. *Wu-lin chang-ku ts'ung-pien*, 7, 7 *b*; cf. *Mo*, 141), and it is certainly correct, but the cruel device must not be regarded as due simply to delirious wrath. YULE (*Y*, II, 181) has already alluded, in his commentary on the present passage, to a text of Plan Carpine on the attack of a fortified place by the Mongols (*Wy*, 83) : «. . . And if they cannot have it in this manner, they throw quick fire; even they use sometimes to take the fat of the men they kill and to throw it liquefied upon the houses. And wherever fire comes over that grease, it burns almost inextinguishably. . . » Bayan's action at Ch'ang-chou provides the best commentary to this text of Plan Carpine.

Chên-ch'ao remained, apparently to the end of the Mongol dynasty, under the baleful influence of the Alan massacre. But the texts relating to the changes in its official status are contradictory (cf. *YS*, 14, 1 *b*; 59, 8 *b*; 99, 3 *a*; WANG Hui-tsu<sup>2</sup>, 8, 6 *a*; 24, 10 *a*); originally a military district (*chün*) of the Sung, it seems, under Qubilai, to have been first a myriarchy called Chên-ch'ao-fu, which was degraded to a *chou* in 1286 under the name of Ch'ao-chou, and finally became a simple district known as Ch'ao-hsien in 1291.

The As, or Asut, or Alans, were certainly Christians, as Polo says after Benedict the Pole

(*Wy*, 137) and Rubrouck (*Wy*, 191-192, 199), and before Marignolli (*Wy*, 526). No sound objection can be based on the significance of the term «Green-eyed Hui-hui», used in the second half of the 14th cent. in reference to the Asut by the author of the *Kêng-shên wai-shih* (cf. *supra*). Although «Hui-hui», technically speaking, means «Mussulman», it has often been used in a loose manner; the Jews are called «Shu-hu Hui-hui» (= Juhut Hui-hui; cf. G. KUUN, *Codex Cumanicus*, 161; > Ĵufud in BLOCHET, *Moufazzal*, 701; Osm. Ĵufut) in ch. 30 of the *Yüan tien chang*, alongside of the «Mu-su-man Hui-hui» (= Musulman Hui-hui), who are the Mussulmans; in modern times, the Jews are sometimes called «Lan-mao Hui-tzŭ» («Blue-cap Mussulmans»; cf. WYLIE, *Chinese Researches*, Historical, 7). In a passage cf. *YS*, 8, 1a, the Nestorian Syrian Ai-hsieh ('Isa) is simply called a «Hui-hui» (cf. *Mo*, 230). Towards 1600, Matteo RICCI found «Hui-hui» used as a common designation for Mussulmans, Jews and Christians (cf. TACCHI-VENTURI, *Opere storiche del P. M. Ricci*, I, 87, 470).

Rubrouck adds that the Alans followed the rites of the Greek Church, used Greek texts and had Greek priests; he is of course speaking of the Alans of Southern Russia, the only ones he could have met. In *Etudes*, t. 124 [1910], 19-20, J. BRÜCKER has explained by the former adherence of the Alans to the Greek Church the surprising fact that, according to WADDING's edition, in his *Annales Minorum*, of the letters of Montecorvino, the future Archbishop of Khanbaliq taught the forty young boys he had bought over to read not only Latin, but also Greek (*litteris Latinis, & Graecis*). I am afraid this ingenious argument cannot be maintained. Although GOLUBOVICH, in his collation of the Chigi ms. with Wadding's edition, says nothing of it (*Bibl. bio-bibliogr.*, II, 131-134), the words «et Graecis» are not given in the Chigi ms., which GOLUBOVICH believes to be the one used by Wadding, nor do they appear in the Paris ms. (cf. MOULE, in *JRAS*, 1914, 547; 1921, 86); but one can only wonder why they should have been added by Wadding.

Whatever the case may be, and although Montecorvino, who speaks at length of the conversion of the Nestorian Prince George (see «George»), does not even name the Alans in his letters of 1305 and 1306, we have to admit that the Alans in China, cut off from their native country and left without a clergy, converted themselves later to the Roman creed which Montecorvino preached in Peking. The decisive proof is provided by the letter sent from Peking to the Pope in 1336, signed by certain Alan princes whose names really appear as such in Chinese history, and in which the Alans beg the Pope to send a new Archbishop, since the death of Montecorvino had already left them for some years without a pastor (cf. *Mo*, 252-254, 261-264). I may add that the Andrew the Frank who carried the letter of the Alans, and who is left unidentified in *Mo*, 252, is certainly Andalò di Savignone, as has already been said in 1878 by DESIMONI, followed by HEYD (*Hist. du commerce*, II, 218) and by GOLUBOVICH (*Bibl. bio-bibliogr.*, IV, 255).

The question of possible mentions of the Asut or Alans in native Chinese texts after the Mongol period is not solved. The *Ming shih* (332, 10a) has a notice on the kingdom of A-su (the name is written with the characters used in Mongol times for the Asut); it has been translated by BRETSCHNEIDER in *China Review*, v, 179, but without any attempt at identification. The kingdom of A-su is said there to be «near Samarkand and Mecca»; it is a vast country; its capital

backs on a mountain and faces a stream which flows south into the sea; there are plenty of fish and salt. The people till the ground; they are charitable and peaceful. In 1419, the ruler 牙忽沙 Ya-hu-sha (\*Ya'qūb-šāh) sent an envoy who offered horses and various products of the country. But A-su could not regularly send tribute, owing to the great distance. In 1463, China dispatched an envoy to that kingdom, but finally A-su did not send tribute again. West of A-su, on an island in the sea, there is the kingdom of 沙哈魯 Sha-ha-lu, which sent an embassy under Yung-lo (1403-1425); Sha-ha-lu is a transcription of the name of Šāh-Ruḥ in *Ming shih*, 332, 6a and one would think of Šāh-Ruḥiya on the Syr-Daria; the fact that Šāh-Ruḥiya has an independent notice (cf. *China Review*, v, 119) would be no decisive objection; but the location and the description hardly fit. The *Ming hui-tien* (quoted in *T'u-shu chi-ch'êng*, Pien-i-tien, 86) gives the quantities of victuals delivered every three days to the 120 men who were attached to the embassy of 1419.

It may be that Alan traders formed the «embassy» of 1419; at that time, many Alans had become Mussulmans, so that we should not be surprised at the apparently Mohammedan name of their ruler (moreover there are Christians named Ya'qūb). But the question is more intricate. In a Ming itinerary, A-su, written in the same way, certainly represents Aqsu in Chinese Turkestan (cf. *China Review*, v, 234). The name of Aqsu (Turk. Aq-su, «White water») does not seem to have been known in Mongol times, but it was in use at the end of the 14th cent., as is shown by the accounts of the campaigns of Tamerlane (cf. *Br*, II, 232-235). Now, in the 菽園雜記 *Shu-yüan tsa-chi* of Lu Jung (ed. *Shou-shan-ko ts'ung-shu*<sup>2</sup>, 5, 5; on Lu Jung's work, written in the second half of the 15th cent., cf. *TP*, 1933, 282), there is a paragraph on embassies projected in 1463 to be sent to various countries but which eventually were not sent (there is here a discrepancy with the *Ming shih* quoted above); the list of countries is given as follows: Herat, Samarkand, Kašyār, A-su, Turfan, Ha-mi (Qomul), Pai-chia-ssü-lan(?). From the very order of the enumeration, Aqsu is more likely to be referred to here than the Alan country. Provisionally, I incline to the view that the A-su of 1419 may well be Asut, *i. e.* Alans, but that the A-su to which an envoy was sent in 1463 is Aqsu, and that the identity of the transcription of both names is responsible for a confusion between the two countries in the *Ming shih*.

But this concerns only the Asut or Alans of the Caucasus. As for the Asut who had been deported into Northern China, I think that, after the fall of the Yüan dynasty, they lasted for three more centuries, although they had become more and more mongolized. In «Sanang Setsen», we find several mentions of a Mongol tribe the name of which is transcribed «Assod» by SCHMIDT (*Gesch. der Ost-Mongolen*, 145, 147, 153, 169, 171, 201, 205); but, in Mongol spelling, SCHMIDT's «Assod» is exactly the name which I read Asut. Aruqtai, who played an important part in Mongol history in the first half of the 15th cent., was an Asut (he is the A-lu-t'ai of Chinese texts; cf. GILES, *Biogr. Dict.* no. 3; *Br*, II, 163). When the Emperor Ching-t'ai was taken prisoner by the Mongols in 1449, the Mongol tradition has it that he was given an Asut wife, by whom he had a son (cf. SCHMIDT, *loc. cit.*, 169, 171, 380, and GOMBOEV, *Altan tobči*, 173). In the 17th cent., the Asut are still named among the tribes which constitute the empire of Dayan-khan (cf. MOSTAERT, *Textes oraux ordos*, Peking, 1937, p. VII). I know of no later mention of the Asut.

How long they remained Christians is not known. In the first years of the 17th cent., Ricci still heard of Christians living outside of the Great Wall. Most of them probably were Öngüt (see «Giorge» and «Ung»), but perhaps some were the direct descendants of the Christian Alans of Polo and Marignolli.

## 14. ALANIE

*alanai* TA<sup>1</sup>*alania* L, TA<sup>3</sup>, Z*alanie* F

In Mongol times, the true land of the Alans was in the Eastern Caucasus, but the name was often extended to a region north-west of the Caspian Sea (see «Alains»). In Hethum, in the chapter on Georgia, «Alanie» is mentioned as the name of the Caucasian Alburz (*Hist. des Crois.*, Arm., II, 129, 268). Hethum's editors are mistaken when they also see the name of the Alans («a nation of Turkish origin»!) in their author's «Aloen», which in fact represents the Caucasian Albania (Alvan > Ayvan); cf. *ibid.* II, 128, 139, 268, 276, 593, 628. On Albania (Pers. Ran and Aran, Arab. Arrān, «Aran» of Jourdain Cathala, ed. CORDIER, 93), cf. MARQUART, *Ērānšahr*, 116-119. On Fra Mauro's map, «Alana» (not «Alano» as in HALLBERG, 14) is marked as a town far to the north-east of the Caspian Sea; it seems probable that «Alanie» is here meant, but with a wrong location. The «Alania» of the Catalan Map, although also located north of the Caucasus, is more in agreement with the accounts of Plan Carpine and Rubrouck.

## 15. ALAODIN

*alaodim* LT, VA*alaodin* F, L*alaodyn* L*alaudin* VB*alodan* P*aloadin* FA, FB; R*alodim* VA*alodin* TA<sup>3</sup>, VA*aloodyn* TA<sup>1</sup>*laudin* V

‘Alā’u-’d-Dīn Muḥammad, the seventh Grand Master of the Alamūt «Assassins», born in 1212, had succeeded his father in November 1221 (BROWNE, *Lit. Hist. of Persia*, II, 207; not «1220» as in II, 456); but he was not the last Grand Master. Murdered in 1255, during the Mongol campaign against him, he was succeeded by his son Ruknu-’d-Dīn Ḥuršāh; it was the latter who surrendered to the Mongols in 1256. Soon after he was assassinated, probably when he was being sent from Persia to Mongka. Cf. *Oh*, III, 174-201; *Y*, I, 146; *Br*, I, 114, 116, 135, 136. The notice on «‘Alā’al-Dīn Muḥammed b. Hasan» in *EI* says he was born in «609 (1210)» and succeeded in «618 (1220)»; these equivalences are errors for 1212 and 1221 respectively.

## 16. ALAU (c. 9)

*alau* TA<sup>3</sup>, LT

This is the name, in TA<sup>3</sup> and LT, of the city where Nicolò and Matteo Polo left Qubilai's envoy. It is very probably the result of a clerical error, as explained by BENEDETTO (*B*, 246).

17. ALEXANDRE<sup>1</sup> (THE GREAT)

*alesander* Z  
*alesandro* TA<sup>3</sup>, V, VA, VB  
*allessandro* R  
*alexander* LT, P, Z; G

*alexandre* F  
*alexandro* VA, VL  
*alixandre* F, FA, FB  
*allesandro, allejandro* VA

*allessandro* TA<sup>1</sup>, VB  
*alexander* L; G  
*alyxandre* FA

On his epithet of Dū-'l-Qarnāin, « the Two-Horned », see « Çulcarnein ».

Alexander's name occurs several times in Polo's text. His information about the Macedonian conqueror is not derived from genuine historical sources, but from stories heard in the East and perhaps from some mediaeval *rifacimenti* of that fabulous history of Alexander which is known as the work of the Pseudo-Callisthenes. In two cases (see « Gate of Iron » and « Lone [Dry] Tree »), Polo formally refers to the « Book of Alexander ». The world-wide popularity of the Pseudo-Callisthenes in hellenistic and mediaeval times is aptly illustrated in YULE's Introduction (I, 113-115). Yet I would like to add a few remarks.

Polo left Venice when he was about fifteen, to return only two or three years before he dictated the account of his travels, so that he could certainly not have been well versed in Western literature. Of course, he had heard of Alexander in Mussulman countries, where the same stories circulated about him as in Europe, but the question may be asked whether the express mentions of the « Book of Alexander » are due to him or the professional recaster of romances, Rustichello. In the account of his outward journey, Polo speaks, when dealing with Eastern Persia, of the « arbre seul que les cristiens appellent l'arbre seche » (*B*, 32); but in the latter part of his book, we read of « l'arbre sol que en livre d'Alexandre est appelé l'arbre seche » (*B*, 222). This second mention belongs to the chapters where the fastidious and stereotyped descriptions of battles are generally attributed to Rustichello. Now the legend of the Dry Tree was well known among Christians in the East, where Polo may have heard it, and a repetition of the same sentence in another part of the book would have been nothing abnormal. But that the « Book of Alexander » should take the place of the « Christians » the second time is the more surprising since the « Dry Tree » does not occur in the Pseudo-Callisthenes, nor in most mediaeval versions derived from it, but only in additional sections sometimes joined to the Romance of Alexander, like the *Væux de Paon*. The mention of the « Book of Alexander » in the present case would perhaps be easier to explain if we assume that Rustichello intervened here.

The other case is no less embarrassing, though for another reason. The story of the nations walled in by Alexander at the eastern end of the Caucasus is an old tale, actually given in the Pseudo-Callisthenes and all the versions derived from it (see « Gate of Iron » and « Gog and Magog »). But Polo's text, taken at its face value, would imply that in the « Book of Alexander » the tribes shut in by Alexander's wall were called « Tartars »; this Polo qualifies by saying that there were no Tartars in Alexander's time, but Comans and the like (which is almost as wrong). Now, the Tartars were sometimes connected in the 13th cent. with the people « whom Alexander the Great shut up in the Caspian mountains », for instance by Richer of Senones (cf. ZARNCKE, *Der Priester Johannes*, II, 22), by the Emperor Frederic II, and in Polo's time by Ricold de Montecroce (cf. *Y*, I, 56-57); but I can find no version of the Romance of Alexander where the Tartars are mentioned; such a version, if it should exist, could not of course be older than c. 1240. Rustichello cannot well be held responsible for the present passage, where the remark on the Comans can only have been made by Polo himself. My impression is that Polo had heard of the Romance of Alexander, although he had never read it, and on the strength of statements which connected the Tartars with the shut-up tribes, erroneously attributed the mention of the Tartars to the Romance itself. If it be the case here, something of the same sort may have occurred with the Dry Tree, the mention of which was perhaps attributed to the Romance by mistake and not because it occurred in the additional matter inserted in some late version.

That most of Polo's information on Alexandre, derived from popular accounts heard in the East, should tally with the legends of the Pseudo-Callisthenes is not surprising if we remember that almost all the Eastern traditions relating to Alexander are indirectly derived from versions of the Pseudo-Callisthenes itself. There was practically no direct souvenir of Alexander in Persia or in India, not to speak of China where he had never been heard of before the Middle Ages. It was long believed that the acquaintance of the Mussulman world with the Pseudo-Callisthenes was due to some Arabic version of the early Mussulman period. But NÖLDEKÉ, in his remarkable *Beiträge zur Geschichte des Alexanderromans* (*Denkschr. der kais. Ak. d. Wiss.*, Ph.-hist. Cl., vol. 38, Vienna, 1890), has shown that there must have been first a now lost Pahlvi version of the early 7th cent., from which a Syriac translation which is still in existence was made at an early date. This is of some moment when we now pass on to India.

Alexandria is sometimes mentioned in Indian literature (see « Alexandre<sup>2</sup> »), but Alexander the Great left in India still less of a souvenir than in Persia. The name « Alikasudara » or « Alikyasudala » in the inscriptions of Aśoka renders Alexander, but refers to a later individual, either Alexander of Epirus or Alexander of Corinth. S. LÉVI was of opinion, however, that, in the middle of the 7th cent., the name of Alexander appeared as « alasa-Çaṇḍakośa », « lazy Çañḍakośa », in Bāṇa's romanced biography of King Harṣa, the *Harṣacarita* (cf. *Mémorial Sylvain Lévi*, 414). LÉVI considered his opinion confirmed by the fact that Bāṇa reproached « alasa-Çañḍakośa » for not having entered Strīrajyam, the Kingdom of Women. This, according to LÉVI, could be no other than the story of the Amazons which, in the Pseudo-Callisthenes, marks the last stage of Alexander's advance before he turns back towards the West. « So Bāṇa's witty allusion », says LÉVI, « is the only sign, but a sufficient one, which proves that the Romance of Alexander was known in India, among scholars and at the royal Courts, in the 7th cent. of our era. »



But there is more. Hsüan-Tsang, who was Harṣa's guest in 643 and left India in 644, speaks of the « Western 'Kingdom of Women' » (西女國 Hsi Nü-kuo), which lay to the south-west of the kingdom of Fu-lin (Fröm = Röm, the Roman Orient). According to the Chinese pilgrim, there were only women in that country, some of the precious productions of which were sold in Fu-lin. « That is why the king of Fu-lin sends every year men who have sexual intercourse with those women . . . » In that Kingdom of Women, LÉVI saw « the famous kingdom of Amazons ruled by the Queen Candace [this is not quite correct; Candace was not a queen of the Amazons], which has taken such an important place in the development of the Romance of Alexander ». « The yearly sending of human stallions to the kingdom of the Amazons », LÉVI adds, « is not mentioned, as far as I know, in any of the numerous versions of the Romance; but it frequently appears in the literature of the *Physiologus* and of the *Mirabilia*. The Chinese monk must have noted a version of the tale which had undergone some contamination, or perhaps he himself is responsible for a contamination between two tales he had heard. »

Without maintaining that LÉVI has conclusively proved his case, I am prepared to accept the bulk of his deductions. A weak point of his argument is that, as he has himself stated (p. 422), there are several Kingdoms of Women mentioned in Chinese historical literature, as well as in Indian and in Mussulman sources (see « Female [Island] »). While many tales of the *Physiologus* have found their way to India and the Far East (or sometimes may have originated there and been carried to the West), it is moreover somewhat risky to assume a double origin for Hsüan-tsang's brief account. On the other hand, LÉVI said nothing as to the date when the Romance of Alexander could have reached India or as to the channels through which it may have passed. We may perhaps add more precise indications on these points.

The Kingdom of the Amazons, or Kingdom of Women, is well known in the Mussulman versions of the legend of Alexander. Its most detailed exponent is no other than Firdausi. Alexander first sends to the Kingdom of Women a philosopher from Röm, and afterwards himself reaches the country, the main city of which was called روم Härüm (cf. MOHL, *Livre des Rois*, 8vo ed. v, 206, 208; SPIEGEL, *Die Alexandersage bei den Orientalen*, Leipzig, 1851, 28; NÖLDEKE, *loc. cit.* 51). The name Härüm is unexplained. Although it was not adduced in SCHAEDEER's *Iranica* (1934), phonetically it may very well be a doublet of Röm, Rüm, and represent the form with an initial *h-* which is known in Armenian, has passed to Fröm in Middle-Iranian and is the original of Ch. Fu-lin. In Firdausi's time, this form would be a survival, but its curious misapplication to a city of the Kingdom of Women would find some sort of explanation in the relations which, according to Hsüan-tsang, existed between Röm and the Kingdom of Women. If Hsüan-tsang really owed his information to an Iranian source which had reached the basin of the Ganges, that lost source may have also spoken of the men annually dispatched to the Kingdom of Women by the king of Röm.

The question of date remains. If a Romance of Alexander corresponding to the Pseudo-Callisthenes was known in India in time to leave traces in the writings of Bāṇa and Hsüan-tsang, it must have reached India in 630-640 at the latest. On the other hand, the only likely channel for a transmission of the Greek romance to India is Pahlvi. But NÖLDEKE has shown that the Pahlvi translation of the Pseudo-Callisthenes can neither be earlier than the end of the 6th cent.,

nor later than the first quarter of the 7th, so that the date we must postulate for the transmission to India is possible. We must only admit, if LÉVI's theory be correct, that the popularity of the romance in India was as rapid as it was short-lived.

### 18. ALEXANDRE<sup>2</sup> (CITY)

*alesandria* TA<sup>3</sup>, V, VB, Z  
*allessandria* VB; R  
*alexandre* F

*alexandria* L, LT, P, VA, VL, *allessandra* TA<sup>1</sup>  
 Z *allexandria* L; G  
*alixandre* FA, FB

Alexandria. The form «Alexandre», constant in F, is corroborated by «Alixandre» in all the French mss. of Hethum (*Hist. des Crois.*, Arm., II, 232).

On the name of Alexandria in Indian literature, cf. in the first place S. LÉVI's paper of 1934, reprinted in *Mémorial Sylvain Lévi* (Paris, 1937, 413-423). LÉVI concurs with the opinion I first upheld in 1914 (*JA*, 1914, II, 413-417) that the Alasanda of the *Questions of King Menander* was the Egyptian Alexandria. Moreover, *ālisandaga*, the name of a bean, and *ālakandaka*, a name of the coral, must be nouns derived from Alexandria.

In Chinese Buddhist texts, the Chinese version of the *Questions of King Menander* gives a form 阿荔散 A-li-san (\*·Ā-ljiē-sân), nearer to the Greek original for the vowel of the second syllable than Pāli Alasanda. LÉVI (*loc. cit.* 418) also thought he had found the name of Alexandria in the Chinese version of Nāgārjuna's commentary on the *Prajñāpāramitā*; but he elicited it through a correction which I hold as very doubtful.

Apart from Buddhist texts, I proposed in *TP*, 1915, 690-691, to identify with Alexandria of Egypt the name 梨軒 Li-hsüan (\*Liei-χjæn), 犁軒 Li-kan (\*Liei-kân), etc., known in China from the end of the 2nd cent. B. C. Although others entertain different views, I still think that the equivalence is substantially correct. It remains doubtful whether, in the first half of the 3rd cent. A. D., the name of Alexandria underlies the transcriptions 遲散 Ch'ih-san (\*D'i-sân) and 烏遲散 Wu-ch'ih-san (\*Uo-d'i-sân) of the *Wei lio*; cf. HIRTH, *China and the Roman Orient*, 181-182 (but the equivalence has gained in probability now that we know for certain that 烏弋山離 Wu-i-shan-li [\*Uo-jæk-sän-ljiē, still more anciently \*O-djæk-sän-ljia], certainly renders the name of another Alexandria; cf. *ZDMG*, 1937, 252; *TP*, 1938, 148). Chao Ju-kua, writing in 1225, has a whole paragraph on 暹根陀 O-ken-t'o (\*Āt-kæn-d'â), and describes its Pharos with the wonderful mirror (*HR*, 146-147; cf. LE STRANGE, *Nuzhat-al-Qulūb*, transl., 239-241); this last transcription is made from the Arabic form Iskandariya.

### 19. ALINAC

A name not given by the mss. and introduced conjecturally by BENEDETTO (cf. *B*<sup>1</sup>, 437); it appears as «Alinac» in *RR*, 380, but «Alinek, Alinaq» in the *Index*, 411. BENEDETTO may be right,

as the «melic» (*q. v.*) meant by Polo must be Alināq, killed in 1284 (cf. *Y*, II, 474). But the text is surprising. Polo says that the «melic» of whom he has spoken and who has been killed «avoit a non Soldan» and was the greatest lord after Aḥmad. Already during the second half of the Abassid caliphate, the title of *sulṭān*, originally borne only by the caliph, had commonly been usurped by any powerful chief (cf. BECKER, in *Der Islam*, VI, 350 sq.). It is quite possible that Polo should have heard of Alināq as «the Sulṭān», and not known his true name. Or he may have confused Alināq with Sulṭān *idāji*, who had great power under Aryun and was finally killed on March 4, 1291, three days before Aryun himself died (cf. *Oh*, IV, 56-57).

## 20. ALTAI

*achai, chai* VB  
*alcai* F, VL  
*alcay* FA, FB, L  
*alcaym* G  
*alchahy* LT

*alchai* TA<sup>3</sup>, VA, VL  
*alchaim* TA<sup>3</sup>  
*alchay* LT, P, TA<sup>1</sup>  
*allai* VA

*altai, batai* V  
*altay* R  
*dalcay* FB  
*elcay* FA

The name is of course that of the Altai mountains, but it is not used in the sense of Altai proper, either Northern or Southern. Moreover, Polo never went to any Altai. In the text, the name of Altai first occurs, after the paragraph on Qaraqorum and the history of Chinghiz-khan, as that of a great mountain where all the «great lords» («grant seignors») of the line of Chinghiz-khan are taken to be buried. The second time, Polo, having told what he had to say about the god and the laws of the Tartars, takes leave of «Qara-qorum and Altai — where the [Great Lords of the] Tartars are buried —» to speak of tribes still more to the north. It is evident that Polo means by Altai a mountain north of the Gobi; YULE (*Y*, I, 247) had too much common sense to think otherwise, and *B*<sup>1</sup>, 438, has misunderstood him when he makes him look for Polo's Altai «immediately north of the Great Wall near Kalgan». Without too much precision in his mind, Polo certainly means, by Altai, the traditional place of Chinghiz-khan's burial, somewhere near the sources of the Onon and the Kerulen. Whether Qubilai and his successors were also buried there is another question, which however need not be here taken into account, as Polo left China before Qubilai's death. For the difficult problem of Chinghiz-khan's death and burial, see «Caagiu».

YULE (*Y*, I, 247) has said that the name of Altai was used in a rather loose way by «Sanang Setsen», but he is not quite correct. In «Sanang Setsen», it is said (SCHMIDT, 103) that Chinghiz-Khan died when his camp was on the southern side of the «Altan-ḥan» (not «Altun-ḥan» as in YULE; *altun* is Turkish), on the banks of the Qara-mōrān (Yellow river). «Altan-ḥan» is Mongolian, and means «Gold Lord[-Mountain]», but the legend carries us far to the south-west of the Khingian YULE speaks of, and, unless we correct «Altan» to «Altai», there is nothing to prove that

«Sanang Setsen» here refers to any Altai. We must rather suppose that, in this passage, «Sanang Setsen» has in view the «Altan mountain» («Altan-alin»; *alin* is the Manchu word for «mountain») of D'ANVILLE's Map of Chinese Turkistan, 3rd sheet, at the north-western angle of the great bend of the Yellow river, also called Altan tebši (cf. MOSTAERT, *Textes oraux ordos*, LXIX, and POPOV, *Men-gu-yu-mu-czi*, 153). On the other hand, in another passage, the same Mongol chronicler (SCHMIDT, 109) says that Chinghiz-khan was buried between the northern side of the «Altai-ḡan» and the southern side of the «Käntäi-ḡan». Here, with a rather hazy geographical conception on the part of «Sanang Setsen», we rejoin Polo's text inasmuch as the Altai is really named in connection with Chinghiz-khan's burial.

This is not the oldest Mongol notion of the name of Altai. In the *Secret History* of 1240, the name of Altai occurs several times (§§ 144, 158, 161, 177, 194, 196, 198, 205) and is always applied to what we now call Southern Altai, which is in Western Mongolia. Such is also the use of the name in Rašidu-'d-Dīn when he gives the Altai mountains as one of the limits of the Naiman territory (cf. *Oh*, I, 425; *Ber*, I, 2, 108; II, 112; III, 3, 127; Rašidu-'d-Dīn uses Altai alone, or بزى التاي Buzurg Altai, or ياكه التاي Yäkä Altai, both meaning «Great Altai» in Persian and in Mongolian). The same Altai is meant when YS, I, 5*b*, says that Tayang-khan came from the 按臺 An-t'ai and camped at the Khanggai mountains, or when it sums up the history of the campaign led by the future Emperor Wu-tsung in 1300-1301 against Qaidu (YS, 22, 1*a*; see «Caidu»), in the course of which, Wu-tsung having reached the An-t'ai (Altai) mountains, the Naiman submitted to him (parallel texts in YS, 119, 10*a*, and 132, 3*a*, use the Chinese name Chin-shan of the Altai). From all these texts, we see that Polo's use of Altai for mountains in North-Eastern Mongolia must have been of popular, not official, origin, although «Sanang Setsen» gives us the proof that this undue extension of the name was still current several centuries later.

It would be interesting to study again the much debated problem of the possible relation of Altai (Altai) to Turk. *altın*, *altun*, Mong. *altan*, all meaning «gold», and to investigate the different values of the corresponding Chinese name of Chin-shan, «Gold Mountains» (the phonetic relation of Altai to *altan* might be of the same order as that of Alašai to Alašan [see «Calacian»]; Chinese texts have sometimes Altan for Altai; cf. POPOV, *Men-gu-yu-mu-czi*, 442, 464, and the index, p. 25, for the double name of a lake Altan-nōr or Altai-nōr). But that would greatly overstep the limits of the present note (cf. VLADIMIROV, in *Doklady Ross. Ak. Nauk*, 1929, B, 170). I wish only to call attention to three points. 1st : Kāšyari, who does not mention any Altai, gives «Altunqan» as the name of a mountain in the land of the Uigurs (BROCKELMANN, 240). 2nd : When the *Ming shih*, in its biographies, says of a number of people that they were originally «Tatar [= Mongols] of the Chin-shan», it is clear that this Chin-shan does not refer to our Altai, but to mountains in Eastern or South-Eastern Mongolia. 3rd : While Chin-shan is of early occurrence in Chinese texts (cf. provisionally CHAVANNES, *Doc. sur les Tou-kiue*, 338) and although we find an Altun-yiš in the Orkhon «runic» inscriptions, there is no mention of the Altai nor of any «Gold Mountain» in the *Hudūd al-'Ālam*. But BARTHOLD is mistaken when he says (*EI*, s. v. «Altai») that the name of Altai seems to appear first «at the time of the domination of the Kalmuks»; he has apparently forgotten all the mentions of it in the 13th and 14th cents.

## 21. AMBERGRIS

*ambra* L, LT, P, TA<sup>1</sup>, TA<sup>3</sup>,  
VB, VL; R  
*ambracan*, *ambracano* R

*ambre* F, FA, FB, TA<sup>3</sup>  
*ambrum* P, Z  
*anbra*, *anbran* V

*ambre* F  
*ambro* VA  
*walrodis* G

In BENEDETTO's Italian translation of Polo, «*ambra*» is mentioned in the chapters intitled «Tebet», «Males Island», «Scotra», «Mogedaxo», and «Çanghibar» (*B*<sup>1</sup>, 187, 351, 352, 357, 361). But the English versions by RICCI and ROSS and by MOULE speak of «amber» in the chapter on Tibet and of «ambergris» in all other cases. The modern common usage in Italian and in French is to employ «*ambra*», «*ambre*» indiscriminately for both products. The question may be raised, however, of the extent to which this confusion prevailed in Italy in Polo's time. For the sake of clarity, I wish to state at the start that, although «amber» (*ambrum*) is mentioned in the chapter on Tibet only by Z, I accept it as a genuine statement going back to Polo himself, and also that general historical data preclude the possibility of understanding *ambrum* as «ambergris» in regard to Tibet (on the use of amber in Tibet, cf. B. LAUFER, *Historical jottings on amber in Asia*, in *Mem. of the Amer. Anthropol. Assoc.* I [1907], 211-244, especially 230-231). On the other hand, «ambergris» is clearly meant in all the other passages.

In his admirable *Histoire du commerce du Levant au Moyen-Âge*, HEYD seems to have taken it for granted that all mediaeval mentions of «*ambrum*», «*ambra*» «*ambre*», etc., at least in Italy and in France, refer to «ambergris» (cf. his Index, and especially II, 571-574). The case is simple enough with Oriental sources (when they are clearly rendered), since the Oriental names for «amber» and for «ambergris» are fundamentally different; and it is certainly remarkable that in FERRAND's *Relations de voyages*, the scores of mentions of «*ambre*» in the translations of Arabic and Persian texts should always be interpreted as «ambergris». In his recent edition of Pegolotti (pp. 412-413), EVANS questions the truth of HEYD's too absolute views on «ambergris». For EVANS, *ambra*, may be both «amber» and «ambergris», and it is only «*ambracanno*» which he considers as «ambergris» definitely. In one case, EVAN's argument is decisive: Pegolotti speaks of *ambra*, while the corresponding Latin version of the privilege, in 1317, gives *ambre vulgariter dicte bernsteen*; Flemish *bernsteen*, Germ. *Bernstein*, never meant «ambergris», but only «amber»; moreover a privilege issued in Flanders is in itself more likely to speak of Baltic «amber» than of «ambergris» of the Indian Ocean. It is more difficult to decide in other cases. EVANS says that both *ambra concia* and *ambracanni* (plural; this is the only mention of the word in Pegolotti) occur side by side in one passage (p. 78) and therefore cannot be identical. I agree; but *ambra concia* means «finished *ambra*», «worked *ambra*»; *ambracanno* might be the name of the same material, but in a raw or unfinished state. When EVANS says «I suspect that when Pegolotti referred to amber perforated for beads he meant yellow amber», I was at first ready to concur with him, until I noticed that in the documents studied by G. LUDWIG in *Italienische Forschungen*, I [1906], 293-294, out of thirty odd «paternostri» mentioned between 1512 and 1557, only one was of *ambra zalo* («yellow amber»), and all the others of *ambracan*. In Polo's

inventory (Vol. I, 556), there are two mentions of «peroli danbro [= d'ambro]». RAMUSIO, in his version of Polo, speaks once of *ambra* (57 D) and three times of *ambracano* (57 D, 58 C). On Fra Mauro's map, the word is written *ambracan* (not «ambrazan» as in *Zu*, 52, and HEYD, II, 572). There is no doubt that «ambergris» is always meant, and it is the only value attached by Italian dictionaries to *ambracani*, in Venetian *ambracan*. The origin of *ambracano* is not clear. There is no foundation in the tradition that it is the name of an African fish; and *ambrum canum*, «white amber[gris]», suggested by TOMMASEO and BELLINI, lacks textual support.

Moreover, the double meaning which is given to «ambra» by Polo and at least once by Pegolotti and which is more common in texts and documents from Northern Europe, would have been deducible, even if it were not attested otherwise, since it is necessary to account for the change in the meaning of the word «amber». It is well known that our «amber», the yellow fossil amber, was called *succinum* in Latin; *succin* is still used in French occasionally. The West also used «carabe», borrowed through an Arabic channel from Pers. کهربا *kahrubâ*, «amber» (lit. «straw-attracting»); Cf. DOZY, *Glossaire*<sup>2</sup>, 247; LOKOTSCH, *Etym. Wörterbuch*, No. 1004; *Romania*, 1909, 139; LAUFER, *Sino-Iranica*, 521-522. As to the word «amber», it was borrowed in the 11th cent. from Arab. 'anbar, which does not mean «amber», but «ambergris» (for a possible Somali etymology of the Arab. 'anbar, cf. *JA*, 1925, I, 172). «Amber», as it is used in English, is historically a misnomer.

Polo's information on «ambergris», especially with the additional matter provided by Z, is by far the most accurate to be found in mediaeval sources, and even in modern times, down to the last century. Scientists are now agreed that «ambergris» is an intestinal concretion of the sperm whale, or cachalot, *Physeter macrocephalus*, and that the oil and the white waxy substance known as spermaceti («whale sperm») are obtained from the cavities of the huge head of the same animal. I deal with the oil and spermaceti under «Capdoille». Here I shall confine myself to some remarks on «ambergris», mainly based on Chinese sources.

The first known mention of «ambergris» in Chinese texts occurs in the *Yu-yang tsa-tsu* of c. 860, where the 阿末 *a-mo* perfume is listed as a product of the country of 撥拔力 Pa-pa-li (\*Puât-b'wât-liək), \*Barbarig, a Middle-Persian form of Barbara, Berbera, the Somali Coast (cf. *HR*, 128); the text has been copied into the *Hsin T'ang shu*, 221 B, 8 b. *A-mo* (\*'a-m'ât, with -t > -d/-r) certainly transcribes 'anbar, as is said in *HR*.

In the *Hsi shih chi* of 1263, a paragraph is devoted to «ambergris», which reads as follows (WANG Kouo-wei's edition, 9-10; BRETSCHNEIDER's translation in *Br*, I, 152-153, is extremely inaccurate): «The 撒八兒 *sa-pa-êrh* comes out of the Western Ocean (*hsi-hai*); it must be the sperm (遺精 *i-ching*) of the sea-turtle (搗玳 *tai-tai*, an unusual combination, perhaps faulty for 玳瑁 *tai-mei*). The bonitoes (蛟魚 *chiao-yü* = 鮫魚 *chiao-yü*; cf. *TP*, 1933, 416-418; it is the *tunnus*, 'tunny', of Z; the bonito is allied to the tunny) eat it and vomit it; after years it conglomerates. Its price equals that of gold. The false product is made of rhinoceros excrements.» This name *sa-pa-êrh* (miswritten 撒巴爾 *sa-pa-êrh* [a «Ch'ien-lung» transcription?] in *HR*, 237, from where I repeated it in *TP*, 1933, 436) was equated by BRETSCHNEIDER to «*sahabiri*» or rather شَاهَبَرِي *šāhbarī*, a misreading of شَاهَبَوِي *šāhboī*, «royal scent», one of the Persian

epithets of «ambergris» (cf. also *HR*, 237). In *TP*, 1933, 436, I questioned the likelihood of a Chinese transcription being based on such a misreading (but, by a most unfortunate slip, I then adduced a Ming form 撒白值 *sa-po-chich* which actually refers to «amber», not to «ambergris»; *sa-po-chich* must be the Arab. *sabaf*, «jet», which has given Span. *azabache*, Port. *azeviche*; cf. *DOZY*, *Glossaire*<sup>2</sup>, 221). I now think that *sa-po-êrh* is only one of the many corrupt forms which have crept into the present text of the *Hsi shih chi*, and that we must correct *sa*[撒]-*pa-êrh* to *an*[俺?]-*pa-êrh*, the very name 'anbar of «ambergris». As a matter of fact, although only in Ming times, 俺八兒 *an-pa-êrh* (\**ambar*) occurs three times as a transcription of 'anbar (cf. *TP*, 1915, 159; 1933, 420; the three mentions, however, are derived from one and the same original).

There was much discussion among mediaeval Arabic and Persian scholars as to the origin and nature of «ambergris»: some saw in it the outflow of a submarine spring, others a dew which, emerging from the rocks, flowed into the sea and there coagulated; others yet maintained that it was the excrements of an animal. The upholders of the spring or of the dew hypotheses easily accounted for the presence of «ambergris» in the intestines of birds or of fish: birds and fish were so fond of «ambergris» that they greedily swallowed it, although it choked and killed them (cf. *DEVIC*, *Le pays des Zendjs*, 188-194). But, for the capture of the «fish» who has swallowed lumps of «ambergris», Abū Zayd Ḥasan of Sirāf, c. 916, gives certain information which, although less detailed, agrees somewhat with that of Polo (*FERRAND*, *Voyage du marchand arabe Sulaymān*, 132-133): «Ambergris is also found floating on the sea in lumps of considerable weight; sometimes these lumps are more or less the size of a bull. When the fish called *tāl* (?) sees a lump of ambergris, it swallows it; but when the ambergris reaches the stomach of the fish, the fish dies of it and floats on the water. There are people who keep a look-out in boats, knowing the season when the fish swallow ambergris. So, when they get sight of a fish floating on the water, they draw it to the coast with iron harpoons which are driven into the back of the fish and to which strong cords are attached. Then they open the stomach of the fish and take out the ambergris. . . » The most noticeable difference between Abū Zayd and Polo is that the former speaks of *dead* fish harpooned by fishermen. Neither of the authors was an eye-witness of the capture. Moreover, Abū Zayd had heard from an eye-witness how fishermen had cut into pieces one of these «fish» which had been stranded in the vicinity of Sirāf. I think that Abū Zayd's error is due to an instinctive disbelief in the possibility of fishermen capturing with harpoons and killing the huge monsters which had swallowed «ambergris». Some similar feeling is probably responsible for Polo's statement that the «whales» are inebriated with tunny brine and so do not feel the harpoons driven into their backs.

Birds and dragons have also been connected sometimes with «ambergris» in Mussulman lore. The theory which makes «ambergris» the dung of birds was heard from the «Moors» by Barbosa at the beginning of the 16th cent., and still is, according to *THORNE*, the opinion of the natives of the Malabar coast (*DAMES*, *Barbosa*, I, 106-107). But it occurs half a century before Barbosa, on Fra Mauro's map, where, close to the Maldive Islands, we read the following notice (*Zu*, 52): «This line of islands which gird the Indian Ocean . . . is inhabited by various species of birds. In that sea ambergris (*ambracan*) is found. The people of those [parts] say that it (*i.e.*

the ambergris) is produced in these islands and that the sea in washing the shores carries it afterwards with the currents into the Indian Sea. Others believe that it is the sperm of the whale. It must be noticed that when sailors see the birds of the aid islands, it appears to them that they have come too near their coasts and they move away, because beyond that there is so deep a darkness that the ships who would happen to enter there could no more advance or go back, and it is known from experience that those who have gone there have perished.» Mauro's source for the present passage is not known. It may be that there is here some dim connection between the deep darkness of the islands and the traditions relative to the birds so big that in their flight they mask the sun (see «Ruc»). Or it may have something to do with the thick air breathed out by dragons, which is mentioned in the Chinese texts quoted below. A French *Lapidaire* of the early 12th cent. speaks of «ambra» as of a stone produced by a sea-fish or by the breath of whales (cf. Ch.-V. LANGLOIS, *La connaissance de la nature et du monde*, 1927, 34, taking into account p. 29, n. 1). A Chinese text of 1520, doubtless copying an earlier authority, speaks of the birds who pick up the spittle (= ambergris) vomited by dragons (*TP*, 1915, 392).

The dragon theory is the one that was current in the Far East. I know only of one exception: «ambergris» has been sometimes called in Japanese 鯨糞 *gei-fun*, «whale excrements», and the term may go back to some local Chinese trade-name, although I cannot trace it; it seems more probable that *gei-fun* was coined in Japan, perhaps under direct Mussulman influence. All Chinese texts, from Sung times downwards, employ only one name for «ambergris», that is 龍涎 *lung-hsien*, «dragon's spittle» (*lung-hsien* is the usual pronunciation, but *lung-yen* is also possible; PHILLIPS [*JNCB*, xx, 221] and ROCKHILL [*TP*, 1915, 158-159] transcribe *lung-yen*; the Sino-Japanese *ryūyen* is also based on a pronunciation *lung-yen*; another form of the term, 龍泄 *lung-hsieh*, with the same meaning, is listed in the dictionaries, but I do not know of any example of it in ancient texts). The term *lung-hsien* occurs in Chinese as early as the 9th cent. (cf. *P'ei-wên yün-fu*), but without any specific meaning. The first mention of it that I can trace with the designation of the «ambergris» occurs in one of Su Shih's poems (1036-1101), and the first datable description of the product is the one given in 1178 by Chou Ch'ü-fu in his *Ling-wai tai-ta* (*Chih-pu-tsu-chai ts'ung-shu* ed., 7, 9a). As it was copied almost *verbatim* in 1225 by Chao Ju-kua, it can be said to have been translated in *HR*, 237: «the fishermen (? 鮫人 *chiao-jên*; the term generally designates a kind of 'mermaid', but can be used metaphorically for a 'fisherman'; it is surprising, however, that such a poetical term should occur in the present text) gather it.» Chao Ju-kua also altered Chou Ch'ü-fei's text by suppressing, before the remark that the «ambergris» has no scent in itself, the words: «As I had to go to P'an-yü (= Canton), I saw it (*i. e.* 'ambergris').» As a matter of fact, all the rest of that passage is not merely hearsay information, but the result of Chou Ch'ü-fei's personal observations while he was at Canton.

A notice on «ambergris» occurs in a 香譜 *Hsiang p'u*, «Repertory of perfumes», quoted, perhaps at second-hand, by the modern dictionary *Tz'ü yüan*. This is not the anonymous Sung *Hsiang p'u* in 2 ch., available for instance in the *Hsüeh-chin t'ao-yüan*. But it may be the *Hsiang p'u* in 4 ch. compiled by 陳敬 Ch'ên Ching probably at the end of the Sung dynasty (it has a later preface of 1322, by another scholar; cf. *Ssü-k'u* . . ., 115, 28 a-b), which



I do not have at my disposal. The quotation given in the *Tz'ü yüan* offers no original information.

The next Chinese source of importance has not yet been made use of by Western scholars; it is the notice on «ambergris» compiled by 張世南 Chang Shih-nan in his *游宦紀聞 Yu-huan chi-wên* of 1232 (*Chih-pu-tsu-chai ts'ung-shu* ed., 7, 4b-5b). I think it is worth translating here. The text is as follows: «Of all perfumes, ambergris (*lung-hsien*) is the most appreciated. At Kuang-chou (Canton), its market value per ounce is not under a hundred thousand (coins); the second quality itself is worth fifty to sixty thousand (coins); it belongs to the [class of] goods which are a State monopoly among the barbarians (cf. DEVIC, *Le pays des Zendjs*, 189, on the handing over of stranded 'ambergris' to the local authorities in Arabia). It is produced in the country of the Ta-shih (Arabs; see 'Arabie'). Near the coast of the sea, when there is a cloudy vapour that hangs over the land lying between mountains, one knows at once that a dragon is asleep below. It (*i. e.* the vapour) lasts half a year, or two, [or] three years; men in turns keep it under observation and wait until the cloud dissipates; then they know that the dragon is gone. They go and look, and are certain to obtain 'ambergris' (lit. 'dragon-spittle'), either five or seven ounces, or more than ten ounces; and they divide (the quantity) equally according to the number of men who have kept observation. If they did not do it equitably, there would be a fight between them and murder. Others say that dragons coil in great numbers on big rocks in the ocean; while lying, they let out spittle; the fish assemble and swallow it; and when the natives see it, they instantly dive and take it. Others again say that in the great ocean there is a whirlpool, at the bottom of which there is a dragon. His spittle gushes out and, heated by the sun, coagulates in pieces which the wind drifts floating to the shore. People then take it and hand it over to the officials. I have questioned the perfume-makers (合香人 *ho-hsiang-jên*) of Ch'üan[-chou] (see 'Çaiton') and Kuang-chou (Canton) and they have said: 'When ambergris is added to perfumes, it can present the scent of camphor and musk; even after several decades, the scent is still there'. The statements of the 嶺外雜記 *Ling-wai tsa-chi* (= *Ling-wai tai-ta*) are as follows: 'Ambergris («dragon spittle») is produced in the [country] of the Ta-shih (Arabs). In the Western Ocean, there are many dragons. When one falls asleep while resting on a rock, his spittle floats on the water, and on accumulation it hardens. The fishermen (*chiao-jên*) gather it as a most valuable substance. When it is fresh, its colour is white; after some time, it becomes purple (紫 *tzü*; not «red» as in *HR*, 237); after a long time, it becomes black. Others again say that the white ambergris is like 百藥煎 *po-yao-chien*, with unctuous stripes; the black one is inferior to it; it is like 五靈脂 *wu-ling-chih*, but brilliant; its scent is almost rancid (nothing of the present sentence occurs in the *Ling-wai tai-ta*; at the same time, the text shows that the comparison with the *po-yao-chien* and with the *wu-ling-chih*, which I had first found in a work of 1520 [*TP*, 1933, 415], and afterwards in another of 1388 [*TP*, 1936, 222], actually goes back to Sung times; the *wu-ling-chih* is the dung of the bat; the *po-yao-chien* has not been identified). It resembles pumice-stone, but is lighter. Some say that it has an extraordinary scent, others that its smell is rank [or that it] can bring out the scent of other perfumes, but all that is erroneous. [Ambergris] does not improve or spoil [other] perfumes, it has merely the power of preserving their fumes together. If one in mixing perfumes employs genuine ambergris, when [the prepared perfume]

burns a blue smoke floats in the air and gathers without dissipating. The guests can use scissors to divide the thread of the smoke. The reason for that is that the ambergris still retains some of the virtue of that dragon breath which produces buildings and terraces' (this is the end of the quotation from the *Ling-wai tai-ta*). Others yet say: '[Ambergris] is the spittle ejected by the dragon on the sea when he comes out or plunges in. There are three sorts [of it]; the first is called «water-drift» (汎水 *fan-shui*); the second, «sand-leaked» (滲沙 *shên-sha*); the third, «fish-eaten» (魚食 *yü-shih*). The «water-drift» [ambergris] lightly floats on the surface of the water; good seamen watch the coming out and plunging in of the dragon, and following him they pick it up. The «sand-leaked» is [ambergris] that has been drifted by waves and billows to main lands and islands. It is that which has coagulated and accumulated for many years, wind and rain have soaked it, and all its scent has leaked into the sandy earth. As to the «fish-eaten» [ambergris], when the dragon emits his spittle (= ambergris), the fish vie with each other in eating it; when digested by them, it becomes dung which is scattered in the sandy desert (a somewhat curious location for fish excrements!); its smell is rank and dirty. Only the «water-drift» [ambergris] can enter into the [composition of] perfumes; the other two [sorts] are inferior. 鄧灝 Têng Hao of 曲江 Ch'ü-chiang (in Shao-chou-fu, Kuang-tung), after comparing the three opinions, said that the third one is nearest to the truth. The sayings of the various authors do not agree, and I do not know which one is correct. But according to my humble judgment, the first one must be fairly near the mark'.

Later works, with one exception which will be noted below, have little to add to the information collected by Chang Shih-nan. «Ambergris» entered Chinese pharmacopoeia only in the Ming dynasty, with 汪機 Wang Chi and after him with Li Shih-chên (cf. *Pên-ts'ao kang-mu*, 43, 7a; *Br*, I, 152). But Chang Shih-nan's text on the three sorts of «ambergris» finds a remarkable counterpart in Barbosa (ed. DAMES, II, 106-109), who also heard of three qualities of «ambergris», which agree, as to nature and as to order, with the Chinese description.

Wang Ta-yüan's *Tao-i chih-liao* of 1349-1350 mentions an «Ambergris Island», Lung-hsien-hsü [嶼], which also occurs on the Chinese map of the early 15th cent. and is made the subject of an independent paragraph in Fei Hsin's *Hsing-ch'a shêng-lan* of 1436. The island lay off the north-western coast of Sumatra. PHILLIPS (*JNCB*, XX, 221) identified it with Pulo Way; FUJITA (*Tao-i chih-liao*'s commentary, ed. *Hsüeh-t'ang ts'ung-shu*, 10-11) decided in favour of Pulo Bras; ROCKHILL, going back to an ancient opinion of GROENEVELDT, pronounced in favour of Pulo Rondo (*TP*, 1915, 158); I think that the identification with Pulo Bras is correct. The two notices on Lung-hsien-hsü have been translated by ROCKHILL (*TP*, 1915, 158-159), but the translation is sometimes incomplete and often inaccurate. The middle part of the *Tao-i chih-liao* paragraph in particular must be understood as follows: «The colour of the [dragon] spittle is sometimes blacker than 'black incense', and sometimes it resembles pumice-stone. If smelled, it has a somewhat rancid scent. But if it be used in combination with various perfumes, then their aroma becomes extremely pure and penetrating. Even such perfumes as calambac-wood, crystal camphor ('plum flower camphor'; cf. *HR*, 193), sandal, musk, gardenia flowers, gham-wood, and rose-water must have some of it to bring out [their full scent].» In other words, Wang Ta-yüan, like some other earlier authors, maintains that «ambergris» is not a perfume in itself, but an excipient

for other perfumes. The accounts concerning this «Ambergris Island» seem to be to a great extent legendary. At the end of the 15th or the beginning of the 16th cent., Mussulman sailors also knew of an «Ambergris Island», Jāzīrāt al-‘Anbar, the name of which was taken over in 1554 into the *Muḥīṭ* of the Turkish admiral Sīdī ‘Alī Čälābī (*Fe*, 537). Although its location is not easy to determine, it seems to lie far away from the «Ambergris Island» of the Chinese. In European nomenclature, if we leave out the Ambergris Island of the Gulf of Honduras, and also the «Cap d’Ambre» at the northern end of Madagascar, the etymology of which is doubtful (cf. GRANDIDIER, *Hist. de la géogr. de Madagascar*<sup>2</sup>, 69), there is a «capo de ambra» and a «riuo de ambra» in the Egerton ms. of c. 1508 [cf. KAMMERER, *La Mer Rouge*, II, App. I (7)]. Moreover, the name of «Ile d’Ambre» was applied in the 18th cent. to a small island close to Mauritius. In 1744 the *Saint-Géran* was wrecked in its neighbourhood, and that gave BERNARDIN DE SAINT-PIERRE part of the subject of *Paul et Virginie*, including the mention of «Ile d’Ambre».

I have still a few words to say on two points. The first is the use of the name of the «dragon» in Chinese. There is in Chinese a regular name of the «whale», 鯨 *ch’ing*, and the texts give a good description of the animal; we know also of several synonyms of *ch’ing*. The notion and the traditional representations of the dragon, *lung*, are totally different. Yet there is no doubt that the monstrous appearance of the huge Cetacea which were sometimes stranded on the coasts of China suggested to the popular mind an association with the proteiform and mysterious dragon. With some reserves as to the details of the case, I can only concur with what has been said on this point by SCHLEGEL (*TP*, 1895, 38-43), LAUFER (*TP*, 1913, 341-342) and ROCKHILL (*TP*, 1915, 158).

But, and this is the second point, even granting that we may to some extent substitute «whale» for «dragon» in the Chinese texts referring to «ambergris», it is somewhat surprising that the Chinese should have given the name of «spittle» to what is an intestinal product, while on the other hand Western belief attached the name of «whale sperm», *spermaceti*, to a product mostly found in the head of the animal (see «Capdoille»). The explanation of these apparent contradictions seems to me to lie in a confusion which has sometimes been made in China as well as in the West between «ambergris» and *spermaceti*. When a Chinese text of 1520, probably quoting from an earlier source, mentions the boats of the Maldive islands as «caulked with melted ambergris (‘dragon spittle’)», there is a great likelihood that *spermaceti* and oil are actually meant, not «ambergris» (cf. DEVIC, *Le pays des Zendjs*, 226, for such caulking). In English, «white amber» has been used in the sense of *spermaceti*, and in 1598 FLORIO gives the definition «*ambra*, amber, also amber greece, also the sperme of a whale called *Spermaceti*». This confusion is well illustrated by a passage of *Pantagruel* (RABELAIS, ed. LEFRANC, IV, 252) : When Panurge has received from a lady of Paris a letter which looks like a sheet of blank paper, Panurge resorts to various devices to reveal the writing, one being meant to ascertain whether the letter was written «avec sperme de baleine qu’on appelle ambre gris». The confusion must have originated in the East since the *Hsi shih chi* of 1263, the information in which was collected in Persia and is independent from Chinese traditional lore also speaks of «ambergris» as being the sperm of an animal, although the great sea-turtle takes in this case the place of the dragon, or whale, or mysterious *tāl* (?) fish of some Mussulman writers.

## 22. AMU

<i>amau, mau</i> G	<i>annui</i> V	<i>flum</i> P
<i>amu</i> F, Fr, t, L, LT, TA <sup>1</sup> , TA <sup>3</sup> ,	<i>auin</i> (? <i>aniu</i> ) FA	<i>ivi</i> VB
VA, Z; R	<i>camu</i> FB	<i>tamu</i> FB <sub>r</sub>
<i>ania</i> R		

The name is written « Amu » in most mss. (also « Amu » on Fra Mauro's map; cf. *Zu*, 40, HALLBERG, 29-30 [« Amui » is a misreading]), with other readings like « Anin », etc.; YULE has adopted « Anin », BENEDETTO « Aniu ». I have no doubt that all are corrupt and have retained « Amu » simply because I do not wish to choose a form which is not given by any ms.

Polo, describing the province of Qara Ĵang (Yün-nan) speaks of Zardandan and of Burma, and then devotes three paragraphs to countries that were « behind » and about which he had only vague information : « Bangala », « Caugigu » and « Amu ». All commentators agree that « Bangala » is Bengal, at least as far as the name is concerned; and « Caugigu », again as to the name at least, is Chiao-chih-kuo, the Annamite kingdom, then centred in Tonking. « Amu » alone remains, and all sorts of explanations have been proposed, which I feel it would be useless to discuss. Not lying on Polo's track, « Amu » must have been an important country, since the traveller heard of it, and he knew it to border on the Gulf of Tonking (as a matter of fact, he says the same of « Toloman », which is not true, but can be explained nevertheless; see « Toloman »). PAUTHIER (*Pa*, 428) already felt that Tonking was meant, and CORDIER followed him (*Y*, II, 131), but both were mistaken in thinking that « Amu », etc., could represent 南越 Nan-yüeh. Nan-yüeh, as a name of Annam, was not in use at that time, and the two alternative names we always meet with in the texts of the Mongol period are Chiao-chih-kuo and 安南 An-nan (then read An-nam). Chiao-chih-kuo is represented by « Caugigu », of which Polo certainly heard in Yün-nan. He may then also have heard the name of An-nan, and failed to realize that it was the same as « Caugigu »; but he certainly heard of « An-nan » at a later date, when he went on a mission to Champa or on his return journey to Europe. It was then he knew that it bordered on the sea, and the information contained in his paragraph on the province of « Ania » either dates only from these sea travels, or, if Polo had already heard of An-nan in Yün-nan, was supplemented by them. In other words, I take « Amu » to be only a corrupt representation of \*Annam (> \*Annā > \*Aman > \*Amau > \*Amu, etc.). This explains how Polo, having distinguished « Caugigu » and « Amu », could give for « Caugigu » characteristics which apply only to the Upper Red River, while in his description of « Amu », bordering on the Gulf of Tonking, we should understand the region of the delta. VON TSCHARNER, 77<sup>2</sup>, accepts « Annam » as a likely solution.

Of « Amu » (< \*Annam > Anian) we have an echo in early modern geographical lore. The Venetian cartographer GASTALDI, on his early maps and down at least to 1550, maintained that there was a land junction between Asia and America, although some of his contemporaries already held the opposite opinion. GASTALDI seems to have changed his views as a result of

contemporary Portuguese exploration, which considerably reduced the extent of Asia towards the East, leaving place for some sea passage, though still theoretical, between the two continents. In the meantime, GASTALDI had been closely associated with RAMUSIO, for whom he designed the map of the New World included in the third volume of the *Navigazioni et Viaggi*. In his map of the «Terza parte dell'Asia», published in Venice in 1561, GASTALDI locates in the extreme north-east of Asia an «Ania Pro.» («Province of Ania»), but the map ends at the margin without showing the extremity of the continent; «Ania» is the form adopted in RAMUSIO's edition for the corrupt «Amu» of most mss. The next year (1562), GASTALDI published a new map of the world, with a descriptive booklet; the map is lost, but the booklet has come down to us. There, for the first time, GASTALDI speaks of the «straits called Anian», which *si distende con una linea per il golfo Cheinan et passa nel mar Oceano de Mangi*; the whole of this nomenclature is taken from RAMUSIO's text of Polo. The «Straits of Anian» named in this booklet actually occurs in cartography for the first time on a map probably also due to GASTALDI, the date of which is to be placed between 1562 and 1566; in 1566, they are shown on ZALTIERI's map, and were retained by cartographers for almost two centuries. It originated from a wrong interpretation of that part of Polo's text which refers to the Gulf of Tonking, influenced by recent discoveries. But it is the product of a theoretical view, and by mere accident it happens to anticipate the actual discovery of the Behring Straits.

Much has been written on the «Straits of Anian» since RUGE, in 1878 and 1888, and then SANDLER called attention to the Polian origin of the name, and one may be surprised that CORDIER should have said nothing about it in 1903 or in 1920. For more recent papers, cf. Godfrey SYKES, in *Bull. Amer. Geogr. Soc.* vol. 47 [1915], 161-172; MARINELLI, in *Riv. geogr. ital.* vol. 24 [1917], 39-49; H. VIGNAUD, in *Journ. Soc. des Améric.* vol. 13 [1921], 1-5; also *Geogr. Journal*, vol. 45 [June 1915], 540-541; vol. 50 [October 1917], 317-318; vol. 58 [November 1921], 396-397. A. HERRMANN, in his *Historical and Commercial Atlas of China* (1935; map 53), has entered Polo's «Anian» and «Toloman» as designations of the Behring Straits and of Alaska respectively.

### 23. ANDANIQUE

*anannum, anidanici* G  
*andaine* FA, FB  
*andanicho* TA<sup>3</sup>, VA  
*andanicium* P

*andanico* TA<sup>1</sup>, VB, R  
*andanicum* LT  
*andanicus* (adj.) L

*andaniqui* (gen.) Z  
*andonicho* V  
*ondanique, undanique* F

This is a puzzling word. YULE retained the «ondanique» of F, because this form was in closer agreement with the etymology he proposed (Y, I, 93); and he has been followed by Pe, 172, Ch, I, 56-57, and RR, 428; but the *andanicum* of the Latin texts has been preferred by

BENEDETTO (*B*<sup>1</sup>, 451). As the form is «andaine» in the mss. in Court French, and as the word begins with *a-* in V, Z and R, the agreement of these independent versions leaves little doubt that «andanique» is correct; it is moreover confirmed by the «fer d'andaine» in Huon de Mery, which YULE did not know (cf. *Y*, I, 215).

This «andanique» is plainly the name of a kind of steel, but the origin of the word remains obscure. PAUTHIER's «antimoine?» (*Pa*, 73) is out of the question, and CORDIER's hypothetical derivation from the name of the Forest of Andaine in the Orne Department of France (*Y*, I, 215) can hardly be taken seriously. RAMUSIO, in his *Dichiaratione*, alluding to the steel mirrors and the «andanico» mentioned by Polo at «Cobinan», says that, through an interpreter, he often inquired from Persian merchants who had come to Venice as to the nature of «andanico», and they all agreed that this was a kind of iron or steel, of which most precious mirrors and swords were made. On this «excellent evidence», YULE came to the conclusion that the real word must be «*hundwānty*», «Indian steel» (with references to JOHNSON, VULLERS and others), which is the reason why he adopted «ondanique». YULE's elaborate note is, as usual, full of most valuable information and makes a clear case of the early renown of Indian steel (on which see COOMARASWAMY, *Hist. of Indian Art*, 34; VALIDI, in *ZDMG*, 1936, 27). I need only add the following remarks: (1) Polo's word is not «ondanique», but «andanique». (2) I find no support for a reading «*hundwānty*»; all the words derived from ھند Hind are pronounced with *h* in the first syllable, and I would read *hinduwānī* (cf. VULLERS and DESMAISONS; in the quotation from the *Šāh-nāmāh* which YULE cites from VULLERS, the latter reads «Hinduwān», not «Hundwān»). (3) We must not give too much weight to the information elicited by RAMUSIO from Persian merchants; the question may have been put in such a way as to suggest the answer, and moreover, these different Persians were in all probability questioned through the same interpreter. But, despite the phonetic difference between *andanicum* (or *andanico*) and *hinduwān*, it remains possible that what the Persian merchants had in mind in giving an explanation was the same word *hinduwān* which has been thought of by YULE. (4) As a matter of fact, Indian steel was so renowned and *al-Hind*, «the Indian», had become such a current designation of it in Arabic that it passed as a name of steel into ancient Spanish as «alhinde», «alfinde», «alinde» (cf. DOZY, *Glossaire*<sup>2</sup>, 142-143). In Fra Mauro's notice on «Cobinan», Polo's «andanique» has been misunderstood and appears as «endego», indigo (cf. *Zu*, 44; HALLBERG, 87); curiously enough, this misinterpretation also connects «andanique» with a word derived from «India».

Whatever may be the origin of the word, it has been supposed by BRETSCHNEIDER (*Br*, I, 146, and *China Review*, v, 21) that Polo's «andanique» was the same as the *pin-t'ieh*, or «pin iron» of the Chinese (not «pig iron» as in *Pe*, 35). This opinion repeated later by PARKER, has passed into *Y*, III, 19. I cannot here discuss in detail the question of 寶鐵 *pin-t'ieh* or 鑄鐵 *pin-t'ieh*. Polo mentions «steel and andanique» in the chapters on «Cherman», on «Cobinan», and on «Ghinghin talas» (*q. v.*). The first two places are in Persia; the third is north of the T'ien-shan, and, in my opinion, far to the north-west of the Uighur country of Turfan. In Chinese texts, *pin-t'ieh* occurs for the first time in the 6th cent., in reference to Sassanian Persia; later on, *pin-t'ieh* is always mentioned as a foreign product, found mainly in Persia, but sometimes

also in different parts of Central Asia. The regular Chinese word for «steel» is 鋼 *kang*, a secondary character derived from 剛 *kang*, «hard», so that *pin-t'ieh* must have been somewhat different. In *YS*, 85, 14 *b*, an officer is put in charge of the working of «*pin-t'ieh*, copper (*t'ung*), steel (*kang*)», etc.; this shows that, in Polo's time, a distinction was actually made between ordinary steel and *pin-t'ieh*. Round gerfalcon tablets in *pin-t'ieh* were manufactured by Imperial order in 1277 (*YS*, 9, 9*a*). In the first half of the 15th cent., mention is made of daggers of damascened *pin-t'ieh* in Java (*TP*, 1915, 239) and of scissors of *pin-t'ieh* in Bengal (*ibid.* 440). Arabic and Persian texts refer also to two principal kinds of steel, viz. ordinary steel (*šāburaqān*) and Eastern steel (Pers. *pūlād* > Arab. *fūlād*; cf. VALIDI in *ZDMG*, 1936, 26-27, 33; borrowed in Turk. *pūlād*, Mong. *bolot*, and also in Tibetan, Armenian, Georgian, Russian; exists also in Ossetian; cf. LAUFER, in *TP*, 1916, 82, 479, and *Sino-Iranica*, 575). Rubrouck speaks of Germans who were extracting gold and making arms at Bolat (*Wy*, 225, 289, 299; «Bolac» is a wrong form), the Pulad of Rašid and Hethum, the Po-lo (= Bolot) of Chinese texts. The name certainly means «steel», and I explain under «Ghinghin talas» why I suppose that these Germans were really making arms with the «steel and andanique» from the mountain north of «Ghinghin talas» mentioned by Polo. Since *pin-t'ieh* is different from ordinary steel, I conclude that BRETSCHNEIDER was right in identifying it with Polo's «andanique».

As to the term *pin-t'ieh*, it does not mean «hard iron» as stated in *HR*, 19. *Pin*, without the metal radical, means «guest», and, with this radical, is a secondary form occurring only in the term *pin-t'ieh*; *pin* is certainly a transcription. LAUFER (*Sino-Iranica*, 515-516) derives it from «Iranian \**spaina*, Pamir languages *spin*, Afghan *ōspina* or *ōspana*, Ossetic *āfsān*». His explanation is probably right in principle, but the Chinese must not be held responsible for the fall of the initial *s*. In a Sanskrit-Chinese Vocabulary of the T'ang period, *pin-t'ieh* is given as the Chinese equivalent of Skr. *piṇa*; this is a pseudo-Sanskrit form, a Prakrit word probably borrowed from the Iranian, but it gives a clue to the *pin* of the Chinese (cf. my note in BAGCHI, *Deux lexiques sanskrit-chinois*<sup>2</sup>, 280-281). Although many so-called Sanskrit words in the Sanskrit-Chinese Vocabulary are terms of colloquial use which may never have been employed in texts, the case may be different with *piṇa*. *Pin-t'ieh*, at least, occurs in the Chinese versions of two sūtras, one of which was translated in 707-709 (NANJIO, *Cat.*, No. 317; Tōkyō *Tripit.* of Meiji, 閏, II, 23 *b*). The other I cannot trace, though it must be earlier than 817, since those two mentions of *pin-t'ieh* are commented upon by Hui-lin (*ibid.* 爲, IX, 43 *b*, 51 *a*); it is just possible that the original texts of the two sūtras had *piṇa* or a word connected with *piṇa*.

I suppose that «andanique» and *pin-t'ieh* are the same as the Mong. *sorunča kūrāl*, of which a marvellous sword was made, as stated in SCHMIDT's *Die Thaten des... Bogda Gesser Khan*, 4<sup>14</sup>. The translation, p. 6, renders it «die härteste und feinste Erzmasse», which is not satisfactory. I think we should read *sorunča gürü*, equivalent to Kalm. *sorṃtsṃ bol°* or simply *sorāntši*, the «sucking steel» or the «sucker» (cf. RAMSTEDT, *Kalm. Wörterbuch*, 332), all of them Mongolian names of the magnet.

FR. RISCH, in his *Wilhelm von Rubruk*, Leipzig, 1934, p. 200, has translated «ab Amorrigo» of Rubrouck's mss. as if it were «ab andanico». «Ab Amorrigo» is unexplained, but must be the (corrupt ?) name of a place in Persia. RISCH's tacit correction is not acceptable.

## 24. ANGAMAN

<i>agaman</i> Fr, t, VB	<i>angamam</i> P, VA	<i>angwerram, angwertam</i> G
<i>agamanam, augamanam</i> FB	<i>agaman</i> F, L, LT, VA, Z; R	<i>nangama</i> VL
<i>aghama, ghama</i> TA <sup>3</sup>	<i>angamanain</i> FA	<i>ongaman</i> VA
<i>aghaman, ghaman</i> TA <sup>1</sup>	<i>angreman</i> V	

All mss. point to « Angaman » (hence « Angaman » and « Angama » on Behaim's globe). In any case, YULE's « Angamanain » and his idea of an Arabic dual (Y, II, 310) must be abandoned; the re-editors of *Hobson-Jobson*<sup>2</sup>, 29, add that « The Ar. dual form is said to be from *Agamitae*, the Malay name of the aborigines », but this is entirely wrong.

The name of « Andaman » has been well known since the Arab travellers of the 9th cent., who write اندامان *Andāmān*, and this name can be traced in Arabic geographical literature down to our days (cf. *Fe*<sup>1</sup>, 689-690). In Chinese, 晏陀蠻 *Yen-t'o-man* (= Andaman) is the form given in 1225 by Chao Ju-kua (*HR*, 147), and we have also 安得蠻 *An-tê-man* (Andaman) in the beginning of the 15th cent. (cf. *BEFEO*, IV, 355; *TP*, 1933, 404; add that 按篤蠻 *An-tu-man*, instead of *So-tu-man*, is now guaranteed by a new ms. of Ma Huan; cf. FÊNG Ch'êng-chün's edition, 34; *TP*, 1936, 220; GERINI's \**Sudhāman* > *So-tu-man* [*Researches*, 416] does not exist). In the West, we find « Andamania » (or « Andemania ») in N. Conti, « Andamā » (= « Andeman ») and « Andamani » in Fra Mauro, « Andeman » in Cesare de'Federici « Andemans » in A. Hamilton (cf. *Hobson-Jobson*<sup>2</sup>, 29; *Zu*, 50; HALLBERG, *L'Extrême-Orient*, 27-28). In view of Polo's remarkably accurate nomenclature, it seems to me almost impossible that he should have used any form but « Andaman », and I consider « Angaman » to be a copyist's error, but one which existed already in the archetype of all our mss.

In Y, III, 149, Sir R. TEMPLE, who is certainly mistaken in approving of YULE's Arabic dual « Angamanain », proposes to explain the modern name « Andaman » by the Malay « Handuman » < Skr. Hanuman, taken in the sense of « monkey », and used as a depreciatory name of the « savage aboriginal antagonist of the Aryans ». The same explanation had already been put forward in 1886 by Sir E. MAXWELL (*JStrBrRAS*, No. 17, 88). In spite of the fact that an *-u-* in the second syllable would seem to find some support in one of the Chinese transcriptions of the early 15th cent., and in a Siamese map quoted by GERINI, *Researches*, 384, which gives « Antoman », the vocalization is not in agreement with the other and more ancient Arabic and Chinese transcriptions, which, like Polo's, have all an *-a-* in the second syllable. I do not know how far back one can trace « Pulau Handuman » (« Islands of Hanuman »), given by MAXWELL as the Malay name of the Andaman islands.

The pseudo-« Agamitae » of the re-editors of *Hobson-Jobson* seems to be Ptolemy's Agannitai, which has sometimes been referred by modern authors to the Andaman and Nicobar groups. GERINI has even tried to derive both Ptolemy's « Agannitai » and Polo's « Angaman » from Prakr. forms *nagga, naṅga*, of Skr. *nagna*, « naked » (*Researches*, 383). Although I have no doubt that « Angaman » is a simple alteration from « \*Andaman », one might suppose that « \*Andaman » became « Angaman » under the influence of Ptolemy's Agannitai. But I find no trace of Ptolemy's nomenclature in Polo's names, and the coincidence is more probably accidental.



## 25. APUSCA

*apascha* VA  
*apusca* F, FA, FB, L, R; S  
*apuscha* VL

*apusta* P, P<sup>5</sup>  
*alpusva* LT  
*hapuoscha* V

*posiha* VB  
*pusciai* TA<sup>1</sup>, TA<sup>3</sup>

This is certainly the Turkish *Abušqa*. *Abušqa* (< *avčya*, *avčqa*) means «old man» (cf. «abusca» = *abušqa*, in KUUN, *Cod. Cum.*, 116), and has often been used as a proper name; it is also pronounced *abišya* and *abišqa*; «Apusca» may be a clerical error, as in «Patu». A Prince of Qubilai's reign, are often mentioned by Rašidu-'d-Din (*Bl*, II, 164, 381, 393, 404, 430). BLOCHET has wrongly restored «Apishégha» and «Abishégha», as if the name were from Skr. *abhiṣeka*. A *p'ing-chang* 阿必失哈 *A-pi-shih-ha*, *Abišqa*, appears in *YS*, 102, 6 *b*, under the year 1285. Polo's «Apusca» is very likely the *aqtači* *Abišqa* (see *Ha*<sup>1</sup>, II, 383 : «Abischka»), one of Ghazan's high officials. YULE (*Y*, I, 33) has already noted that Rašidu-'d-Din, according to ERDMANN's *Temudschin*, 205, mentions an «Apuschcka» who was sent on a mission from Persia to the Great Khan, and thinks he may be the same person. I quite agree, but Rašid does not write «Apušqa»; he gives ایشقا = ایشقا *Abišqa* > *Abušqa* (misread «Aqisqa» in *Ber.* I, 160; *ibid.* p. 106, the spelling ایشقا *Abušqa* adopted by BEREZIN for a namesake is not countenanced by any manuscript); *Abišqa* belonged to the Qongqotan branch of the Ornaut. For a prince «Abusta» (read «Abusca») of the Golden Horde, who is said to have been baptized, cf. GOLUBOVICH, *Bibl. bio-bibl.* II, 73. It seems as if the pronunciation *Abišqa* had been maintained officially, while *Abušqa* was more in popular use.

## 26. ARABIE (and ARABI)

*abrahi*, *cabya* LT  
*arabam* L  
*arabbia* TA<sup>1</sup>  
*arabe*, *araben*, *arabien*, *atabi* F  
*arabes* P, Z

*arabi* F, L, TA<sup>1</sup>, TA<sup>3</sup>, V, VA,  
 VB, VL, Z; R  
*arabia* L, TA<sup>3</sup>, Z; R  
*arabie* F, FA  
*arahi* R

*arrabe*, *arrabic*, *arrabie*, *arrabiz*  
 FB  
*arrabes*, *arabbia* Z  
*arabis* FA  
*tabian* V

Although the Arabs were known to the Chinese at least from the middle of the 7th cent., their name never occurs in Chinese transcription before modern times. The Chinese used to call the Arabs 大食 *Ta-shih* (\*D'ái-dž'jək or \*T'ái-zi), once 多氏 *To-shih* (\*Tā-žie), transcribing either the Pers. *Tāzi* (< Pahl. *Tāčik*, derived from the Arabian tribal name *Ṭai*) or the other Persian form *Tājik*; *Tāzi* is to *Ṭai* in the same relation as for instance Pers. *Rāzi* to *Rai*. Cf. HORN, *Grundriss der neupers. Etym.*, No. 367; HUBSCHMANN, *Pers. Studien*, No. 367; CHAVANNES, *Religieux éminents*, 25; CHAVANNES, *Documents sur les Tou-kiue*, 361; *HR*, 119 (with several inaccuracies). The transcription *To-shih* (\*Tā-žie), used by the pilgrim I-ching in the second

half of the 7th cent., is not ambiguous and makes it probable that the other and more usual transcription Ta-shih is to be restored as \*T'âi-zi and represents also the form Tâzi, not Tâjik.

The word has been supposed to occur in 732 in the Turkish inscription of Kül-tâgin (cf. BARTHOLD, in RADLOV, *Die alttürk. Inschr. der Mongolei*, Zweite Folge, 12); the passage is however illegible. But two epigraphic mentions have been found in other Turkish monuments of the 8th cent., in the form Tâjik. It seems that in both cases Tâjik is not taken in the sense of « Arab », but already in that of « Persian » which, owing to the conversion of Persia to Islam, it soon took and has since retained in Central Asia (cf. SAMOÏLOVIČ, in *Doklady Ak. Nauk*, B, 1927, 155-156; KOTWICZ and SAMOÏLOVIČ, in *Rocznik Orientalistyczny*, IV, 101-102). Kâşyari, writing in 1076, gives *Tâzik* as meaning « Persian » (BROCKELMANN, 250). Both forms « Tâzik » and « Tazi » occur in the *Qutaşyu bilig* of 1069 and SAMOÏLOVIČ may be right in supposing that they still retained at that time two different meanings, « Tâzik » meaning « Persian » and « Tazi » meaning « Arab ». As a matter of fact, « Tazi », with the specific meaning of « Arab », still occurs in Turkish literature as late as the beginning of the 14th cent. (RADLOV, III, 930). As *tazi* or *tazi*, it survives now in Turkish only as the name of the « greyhound », literally « the Arabian [dog] ». This Turkish use of Tazi, rather than Tâzik or Tâjik, in the sense of « Arab » seems to confirm the view that the Chinese transcriptions are really based only on « Tâzi ».

Until the destruction of the Abbasid caliphate in 1258, Bagdad remained for the Chinese the centre of the Mussulman world. Mecca appears for the first time as a separate country in the *Hsi shih chi* of 1259 in which it is called 天房 T'ien-fang, « Heavenly house » (cf. *Br*, I, 141). Also under the Yüan, the *Tao-i chih-liao* of 1349-1350 describes it under the name of 天堂 T'ien-t'ang, « Paradise », with a note saying that the ancient name was 筠冲 Yün-ch'ung. That « ancient name », which does not appear to be a transcription from a foreign language, has not so far been discovered in any earlier Chinese text; but the information was taken over about eighty years later by the *Hsing-ch'a shêng-lan*. On the other hand, the *Hsing-ch'a shêng-lan*, like all the other works of the Ming dynasty, does not call Mecca T'ien-t'ang, but 天方 T'ien-fang, which may mean « Heavenly country » or « Heavenly square » (cf. ROCKHILL, in *TP*, 1915, 618-620). BRETSCHNEIDER was probably right when he thought (*Br*, II, 303) that the two forms of T'ien-fang and the one of T'ien-t'ang all refer to the Great Mosque of Mecca, to the square Ka'bah, also called Bâitullah, « House of God ». But one would like to know more about the history of these different Chinese names (cf. also *TP*, 1933, 445).

## 27. ARAINES

### *araines* F

BENEDETTO and MOULE are of course right when they reject YULE's *harem*; on the other hand, I agree with MOULE when he thinks that BENEDETTO's emendation to *le serores* is palaeographically very improbable (cf. Vol. I, 452). I have since thought of another solution, perhaps

more satisfactory. The ancient French had a word *antain*, « aunt » (cf. GODEFROY, s. v. « antain »; TOBLER-LOMMATSCH, *Altfranz. Wörterbuch*, I, s. v. « ante »; W. VON WARTBURG, *Franz. Etym. Wörterbuch*, s. v. « amita »). The actual form *antaine* is not listed by GODEFROY or TOBLER-LOMMATSCH, but two examples of it are given in LACURNE DE SAINTE-PALAYE, as for instance the following taken from *Contin. de G. de Tyr*, in MARTÈNE, *Ampl. Coll.* 626 : « Henri ot à fame l'antaine le roi Guillaume de Secile, soror son pere ». It seems just possible that « araines » is corrupt for \**âtaines* = *antains*.

## 28. ARÇIÇI

*agrazi* VB  
*arciéri* VA  
*arciři* VL  
*arciçi* LT

*arzici* TA<sup>1</sup>, TA<sup>3</sup>  
*arzici* V  
*barzirim* P<sup>5</sup>  
*darçiçi* F, L

*darsisi* FA  
*darzirim* P  
*darziz* R  
*darzizi* FB

Arjîš, on the north-eastern side of the Lake of Van. RR, 412, and B<sup>1</sup>, 412, have adopted « Arzizi », as YULE had done before them; but I do not think there was any *z* in the original Polo-Rustichello spellings. Judging only from the Polian mss., I prefer « Arçiçi ». But in Polo š appears generally as *-sc-*, and a final mute *-e* occurs in « Abasce » and « Chescemir ». The confusion of *c* and *g* is very common; we find « Argis » on the Catalan Map (HALLBERG, 43-44). The original form may have been \*Argisce.

On Arjîš, see REINAUD, *Géogr. d'Aboulféda*, II, II, 148; EI, « Ardjîsh », notice by STRECK; LS, 183; Mi, 395.

Arjîš, Greek Ἀρσισσα, old Armen. Arčëč, was destroyed by the Georgians in 1209. Owing to the expansion of the lake, the ruins have been surrounded by water since the middle of the last century.

## 29. ARÇINGAN

*aranga, atechaze* V  
*archinia* VL, S  
*arçinga* F, L  
*arcinga* Z  
*arçingal* F

*arcingan* R  
*arçinoga* LT  
*areuicha* VL  
*armenia* S  
*arsenga, arsingua* FA

*arsinga* FB  
*artinga* VA  
*arzinga* P, P<sup>5</sup>, VB  
*arzingha* TA<sup>1</sup>, TA<sup>3</sup>

The final *-n* is only given by RAMUSIO, but F has « Arçinga » and « Arçingal », and Polo's original spelling must have been either « Arçingā » or « Arçingan ». The place is of course the

modern Erzingan or Erzinjan, written «Arsengan» by Simon de Saint-Quentin (in Vincent de Beauvais xxxi, 146), «Arsengen» by Rubrouck (*Wy*, 327), «Arzinga» by Pegolotti, «Arzegan» by Fra Mauro («Arzegā»; not «Arzengan» as in HALLBERG, 48-49), ارزنجان Arzinjan by Rašidu'd-Dīn (*Bl*, II, 549); cf. also GOLUBOVICH, *Bibl. bio-bibl.*, II, 544; and the articles in *EI*, «Erzindjān», by R. HARTMANN, and in *LS*, 118.

## 30. ARGIRON

<i>agiron</i> P <sup>5</sup>	<i>arciron</i> LT	<i>arsion</i> FA
<i>agyron</i> P	<i>argion</i> V	<i>arsus</i> FB
<i>architen</i> VL, S	<i>argiron</i> F, VB, V, VA, L, R	<i>arziron</i> TA <sup>1</sup> , TA <sup>3</sup>

The modern Erzerum, on which cf. *EI*, notice by R. HARTMANN, and *LS*, 147-148. The name is written «Arseron» by Simon de Saint-Quentin (in Vincent de Beauvais, xxxi, 147); «Aarserum» by Rubrouck (*Wy*, 321); «Arceron» or «Arziron» by Odoric (*Wy*, 415); «Arseron» by Hethum (*Hist. des Crois.*, Arm., II, 156, 291); «Arzerone» in Pegolotti; «Arzerum» or «Erzerum» on the Genoese Map (HALLBERG, 1-2). For other forms in the 14th cent., «Arzerono», «Alzaron», «Arzeroni» (read «Arzerom»?), «Orzaloni» (read «Orzalom»?), «Elzerom», «Larzeron», cf. GOLUBOVICH, *Bibl. bio-bibl.*, II, 544-545.

As to Polo's form, *-on* may be a wrong rendering of *-ō* = *-om*, as in the case of «Caracorom». On the other hand, the *-gi-* of F, *-si-* of FA, *-zi-* of TA, point to a possible original *-çi-*, as in the case of «Arçingan».

31. ARGON<sup>1</sup>

<i>amon</i> V	<i>argho, arghon</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>argon</i> F, Fr, t, FA, FB, L, LT,
<i>arago, aragon, ragon</i> VB	<i>argo</i> F, Ft	P, P <sup>5</sup> , V, VA, VB, VL, Z; R
<i>archon</i> TA <sup>1</sup>	<i>argom, ragona</i> VA	<i>argun</i> Z
<i>argan</i> Ft		

Mong. Aryun, written ارغون Aryun in Persian; the name is fairly common, and I think it is taken from the tribal (or social) designation of the Aryun people (see «Argon<sup>2</sup>»). It is always spelt with *-o-* («Argon») in Western documents (cf. CHABOT, *Hist. de Mar Jabalaha III*, 190, 200, 203, 214, 229, etc.) and in Hethum (*Hist. des Crois.*, Arm., II, 188). In Chinese, 阿魯 A-lu (*YS*, 107, 7b), 阿兒渾 A-êrh-hun (*YS*, 14, 5a), 阿魯渾 A-lu-hun (*YS*, 134, 3b), all = Aryun (the first one, incomplete, is certainly a mistake).

Aryun, son of Abaqa (see «Abaga»), succeeded his uncle the usurper Aḥmad (see «Acmat<sup>2</sup>») on August 11, 1284 (*Ha*<sup>1</sup>, I, 359), and died on March 10, 1291 (*Ha*<sup>1</sup>, I, 393, where the name of the Mussulman month is wrong).

As Polo says, it was Aryun who asked Qubilai for the bride whom the Polos escorted from China on their way back, but who arrived long after Aryun's death (see «Cocacin»).

Aryun was favourably disposed towards the Christians (BLOCHET, *Moufazzal*, 30, mentions a Nestorian hymn for Aryun, written in Syriac, which was discovered by NAU) and sent to the West several embassies, which visited the Pope in Rome, Philip the Fair in France and Edward I in Gascony and in England. The original letter in Mongolian sent by Aryun to Philip the Fair in 1289, now kept in the Archives Nationales in Paris, is a document of the greatest interest. First published by Abel RÉMUSAT, it has been reprinted in the original text and with a French translation in *Pa*, 775-777; but the translation could be improved considerably, and a detailed commentary is highly desirable.

### 32. ARGON<sup>2</sup>

*angaroni* VL  
*argom* VA

*argon* F, FA, FB, L, P, P<sup>5</sup>, VB,  
Z; R

The term is certainly Turk. Aryun. *B*<sup>1</sup>, 96, 451, does not make it a proper name, but takes it only as a common noun, which he renders by «bastardo», the «Guasmul» which Polo uses by way of comparison. In my opinion, that does not correspond to the facts, even if, in Italian, «bastardo» does not imply the same meaning, when speaking of men, as «bâtard» in French, «bastard» in English, and although, under BENEDETTO's influence, «Guasmul» has also been replaced by «bastard» in the English version of *RR*, 95. The «Argon» and «Guasmul» were not said by Polo to be «bastards», but merely «half-breeds».

First of all, the «Guasmul», with whom Polo compares the «Argon», are not half-breeds in general, but a particular type the name of which ought to be retained in the translation. To the texts already quoted in *Y*, I, 290, some more may be added, in particular the notice of the «Gasmulins» in *Directorium ad passagium faciendum*, probably of 1317, in *Hist. des Crois.*, *Arm.*, II, 490-491; and the quotations in O. TAFRALI, *Thessalonique au XIV<sup>e</sup> siècle*, Paris, 1913, 43-44. I suspect that the letter sent by the Pope to «Girardo Galinuri de Constantinopoli» in CHABOT, *Hist. de Mar Jabalaha III*, 210, for whom LANGLOIS's edition gives «Girardo Gasinuri» while older editions read «Girardo Casmuri», is really a «Gasmuri», a Guasmul, just as the «Giorgius Gasmurus» of Constantinople who was at Caffa in 1289 (cf. BRĂȚIANU, *Actes des notaires génois*, 217; *Rech. sur le commerce de Gênes*, 186). In Matteo Polo's will, we find the name of «Albertus Vasmulo» (cf. Vol. I, 532). The Guasmuls (more often Gasmuls) were children one of whose parents was Latin and the other Greek. Their name in Greek is Βασμούλοι or

Γασμουῖλοι, the form Vasmuli being older than Gasmuli. No satisfactory etymology has been suggested. In 1896, ARISTOV, in his *Remarks on the ethnical composition of Turkish tribes* (*Živaya Starina*, VI, 365-366), proposed to see, in Vasmul > Gasmul, the name of the Turkish tribe of the Basmil, already known in the Orkhon inscriptions. The same explanation is given as a fact by BARTHOLD in *12 Vorlesungen über die Geschichte der Türken Mittelasiens*, Berlin, 1935, p. 40. But in spite of the phonetic correspondence, which is perfect, the difference in time and in meaning makes one hesitate to accept that solution (H. H. SCHAEDEER doubts it too in his *Remarks on 12 Vorlesungen*, 277).

As to the «Argon», I must first state definitely what they are not. In spite of CHARIGNON (*Ch*, I, 244-245) and others, there is no relation between their name and that of *ärkä'ün* given to Christians under the Mongols, first because there is no phonetic resemblance between those two words belonging to different «classes», and secondly because the «Argon» were not Christians, but, according to Polo, children of parents of whom one was a Mussulman and the other an idolater. For the question of «class», I must insist on the fact that Mong. *ärkä'ün*, written *ärkägün* and transcribed in Persian as اركاگون *ärkäwün*, in Armenian as *arkhawun*, and in Chinese as 也里可温 *yeh-li-k'o-wên*, is of the palatalized class, while the name of the Aryan (Polo's «Argon»), in Chinese A-êrh-hun, belongs to the velar class. Polo probably knew the Mongol name *ärkä'ün* of the Christians, but he never mentioned it in his book, probably because he saw no reason to do so; if he had used it, we should expect the word to appear as \**Arcaun* or \**Arcaon*, not as «Argon». I may add that the origin of the word *ärkä'ün* is still unknown; the current explanation which makes it a transcription of the Greek ἀρχων is not convincing (cf. *Mo*, 218), no more than N. MARR's learned attempt to trace it back to Armenian (*Ark'aun, mongol-skoë nazvanie Khristian*, in *Vizantiiskii Vremennik*, XII [1906], 1-68; according to MARR, *ärkä'ün* represents Armen. *arkhaun*, itself transcribing Greek ἀρχων, but used in the sense of «royal», «kingly», and would be the equivalent of «Melchite», so that the term would have been carried to Central Asia, in pre-Mongol times, not by Nestorians, but by Armenian-Melchites). The word occurs in Western transcriptions of proper names as «Archaon» (cf. CHABOT, *Hist. de Mar Jabalaha III*, 208, 237). As to the religion of the «Argon», Z and R agree that the «Argon» were half-breeds of Mussulmans and idolaters; F is corrupt, but without any indication that the «Argon» were Christians, and implying rather the contrary; the text similar to F from which the redaction in Court French (FG) derives was probably corrupt also in more or less the same way as F, and I suppose that the new redaction, peculiar to FG, according to which the «Argon» were Christians, resulted somehow from the fact that the passage was corrupt. PAUTHIER, as usual, only took FG into account, hence his long and still too often quoted note on the Christian «Argon» (*Pa*, 212-217; I think YULE, *Y*, I, 290, is mistaken in supposing that the text in FG does not mean that the «Argon» were Christians); but I consider FB as having no authority in the present case. If Polo says that «Aryon» has the same meaning («vaut a dire») as «Guasmul» in French, it is only to illustrate the case of the «Argon» by another well-known example of half-breed, exactly I conjecture as he says elsewhere that «Mulecte vaut a dire \*Patarain», which does not mean that the Ismailians were Christian heretics (see «Mulecte»).

We can even say with a great deal of probability where Polo found these «Argon»; it was at

the extreme north-eastern part of the territory of the Öngüt princes of «Tenduc», and particularly at the Mussulman colony of Simali. Polo mentions the manufacture there of silk and gold textiles called *nāḥ* and *nasīj* (see «nac»). But we know that they were woven at Simali in particular, by the members of that colony of Mussulmans who had originally been brought there from the region of Samarkand (cf. *JA*, 1927, II, 261-279). And it will be shown further on that there is a connection between that colony of Mussulmans and the tribal name Aryun.

As to the name of the «Argons», which is certainly «Aryun», two etymologies are possible, and we have to see whether they can be reconciled.

Polo says that «Argon» means a half-breed. It has long been noticed that in the western part of Chinese Turkestan, the name of *aryun* was used to designate the half-breeds of Turkestan and Ladakh parents, etc. (cf. *Y*, I, 290); I have heard it in Kučā, and VON LE COQ has registered it as far as the region of Turfan (*Sprichwörter und Lieder*, 81). This Turkish word, under the form *ar-gon*, has passed into the Tibetan dialect of Ladakh; it is in JÄSCHKE's *Dictionary*, and, in Sarat Chandra Das (p. 1348). CUNNINGHAM wrote to YULE that *aryun* must be the Turkī word meaning «fair», «not white, but ruddy or pink, and therefore fair», the word being «both Turkī and Mogholī» and «applied to all fair children, both male and female». But *aryun*, with such a meaning, is not found in any Turkish dialect I know of, nor does it exist in Mongolian, unless CUNNINGHAM has confused it with Mong. *ariyun* (= Turk. *arıy*), «pure»; and it is in Persian that *aryun* means a kind of red or purple flower, and subsequently purple (BLOCHET's note, *Moufazzal*, 702, on *arqun* or *aryun* meaning «pure red» in Eastern Turkish is entirely wrong). By some oversight, *aryun*, in the sense it has in modern Turkī, is not registered by RADLOV.

The sense of half-breed must be an old one in Turkish. In the *Qutadγu bilig* of 1069, there is a word *aryun*, designation of a horse, which RADLOV has considered as an equivalent of the later name *aryımaq* or *aryamaq* given to western thorough-bred horses (I, 299, 302; cf. also RAMSTEDT, *Kalm. Wörterbuch*, 13<sup>2</sup>). I shall not discuss here the question of the *aryamaq*, also named in Chinese sources, but must point out that, in 1076, Kāšyarī gives *arqun* as meaning «a foal produced by a wild stallion and a domestic mare, running very fast» (BROCKELMANN, 12). If we take into consideration that in Kāšyarī himself we have an alternation of -q- and -γ- in *aryula-*, but *arqučī*, it seems to be certain that his *arqun* is identical with the *aryun* of the *Qutadγu bilig*, so that the meaning of «half-breed» for *aryun*, at least in a special application, existed already in Turkish in the 11th cent. So there can be no doubt that the meaning given to *aryun* by Polo did exist.

But at the same time, there was a tribal name Aryun. Kāšyarī gives the name of Aryu to a country between Talas and Balasayun, and mentions it more than twenty times; the name appears also in the Turfan documents (cf. BROCKELMANN, 10, 240); a tribe Aryun is known in Čay., and the Aryin (= Aryun) are one of the constituents of the Kirghiz Middle-Horde (cf. RADLOV, I, 300, 302; ARISTOV, *Zamétki*, Index of 1903, p. 5). Now this name of Aryun people appears very often in Chinese texts of the Mongol period. The tribe A-êrh-hun (Aryun) is named among the non-Mongolian tribes (*sé-mu*) in *Cho-kêng lu* (ch. I). They formed an «army» (*chün*) or a «guard» (*wei*), like the Qipčaq, the Alans, etc. They were Mussulmans, as appears

from their names, for instance in the funerary inscription of Ha-chih-ha-hsin (Hāji Qasim) in Hsü Yu-jên's *Chih-chêng chi*, 53, 1 ss. If we remember that the Kerait Ha-san-na, who was at the head of the Aryun army, is the same who established 3000 Mussulman families from Russian Turkestan in Simali, we may conclude that the Aryun tribe is probably the same as the Aryu tribe of Kāšyari, and that the «Argon» Polo saw in «Tenduc» were really the Mussulman Aryun tribe people settled around Simali (cf. *JA*, 1927, II, 265, 266). I do not know whether any etymological connection can be proposed between the name Aryun (< Aryu ?) of the tribe and the word *aryun* meaning «half-breed»; it does not seem to me probable, and anyhow Polo could not have been aware of it. I suppose Polo heard in the region of Simali, and possibly more or less to the south-west of it, the name of the Mussulman Aryun settlers; but he knew also the Turkish word *aryun* used in the sense of half-breed, and applied it wrongly.

I cannot accept BOODBERG's views when, in his *Sino-Altaiica*, III, 2, and IV, 4, he tries to trace Aryun, in the sense of «half-breed», to the old Wu-huan tribes of the Mongolo-Manchurian border.

### 33. ARMÉNIE

*armenia* L, LT, P, P<sup>5</sup>, VA, VB,  
VL, Z; G, R  
*armenie* F, Fr, t; G  
*armin* (adj.), *arminie*, *armonie*,  
*erminie*, *harmenie*, *harmi-*

*nie* F  
*arminia* L, VB  
*ermene*, *ermeyne*, *ermyne*,  
*ermynie* O  
*armenia* LT, TA<sup>1</sup>, TA<sup>3</sup>, V

*ermenie* FB  
*ermine* F, O  
*erminia*, *herminia* TA<sup>3</sup>  
*hermenia* LT, TA<sup>3</sup>  
*hermenie* FA

This form occurs in F, but it also gives «Erminie», which was no less current in the Middle Ages, and I am in doubt as to the form actually used by Polo. In Hethum, the name is always written «Ermenie» (*Hist. des Crois.*, Arm., II, 127 sq.). In Arabic it is Armīniyah.

Polo's distinction between a Lesser and a Greater Armenia is in agreement with mediaeval usage. Lesser Armenia is fundamentally Cilicia, with Sis as its capital. Greater Armenia is our Armenia, beginning in the west at Arzinjan, and bordering to the south-east with Mosul territory (cf. *Y*, I, 42-43, 46-49; *LS*, 182-184; *EI*, «Arménie», by STRECK; *Mi*, 484). The kingdom of Lesser Armenia, which acknowledged the supremacy of the Pope, was consecrated in 1198 and lasted until 1375; its kings generally managed to keep on good terms with the Mongols.

The name of Armenia seems to have been unknown to the Chinese.

For RAMUSIO's passage which mentions «Greater Turkey» instead of the «Greater Armenia» of other mss., see «Caraunas».



## 34. ASCIAR

*aciar* FA  
*arziar* FB

*aschar* V  
*asciar* F, TA<sup>1</sup>, Z

*astiar* VB, R  
*sciar* TA<sup>3</sup>

This must represent an original \*Ašar, which remains unidentified. In Polo, *-sci-* is simply *-š-*; this practically excludes *āšcar* < Skr. *āścariya* of Y, II, 374, and also Bernier's «*achar*», which CORDIER quotes (Y, II, 374) from a ms. note found amongst YULE's papers, but which, from the translation given, must be \**ačar* < Skr. *acala*, «motionless», «immovable».

## 35. ASIDIN SOLDAN

*asendi soldan* V  
*asidin soldan* F, FA, FB

*asidin soldano* R

*sedin soboan* VB

I can only agree with the general opinion that this must be Ghiyāthu-'d-Dīn (γiyāθu-'d-Dīn) Balaban of Delhi (1265-1286). I write on purpose «Balaban» instead of the usual «Balban», as I think Balaban in the correct form of that Turkish name; cf. «Balaban» in BRÄTIANU, *Actes des notaires*, 341. «Balaban» means «falcon»; cf. KUUN, *Codex Cumanicus*, 129; RADLOV, IV, 1494; BANG, *Türk. Briefe*, II, 247; we must also, I think, read Balaban, and not «Balbān» or «Belbān», when this same name occurs in the history of the Mamluk sultans of Egypt. But it is surprising that Polo, generally so accurate in his transcriptions, should not have found a more nearly exact spelling for Ghiyāthu-'d-Dīn, and his «Asidin» looks more like 'Izzu-'d-Dīn. The form «soldan» for *sultān* is common in the Middle Ages, and accounts for the title «soudan» then given in French to the sultans of Egypt; cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Sultan».

## 36. ASSASSIN

*asasin*, *asesinz* Fr  
*asasini* VA  
*ascilcinç*, *asciscinç* Ft  
*asciscin*, *asescin*, *asisim* F

*assasini* VA, P  
*assasini* L, LT, VL  
*assessini* TA<sup>1</sup>r

*harcaşsis* FB  
*hasisins* FA  
*sassinum* Z

I am only concerned here with the word itself; for particulars concerning the Assassins, see «Mulecte» and «Old Man of the Mountain».

Although F never has an *a* in the second syllable, the «*assasini*» of VA may be almost as correct a form as «*asciscin*»; the question is mainly one of etymology.

Since the publication of DE SACY's *Mémoire sur la dynastie des Assassins* (*Mém. Ac. des Inscr.* iv [1818], 1-84), there has been no doubt that our word «assassin» goes back to an original form meaning «consumer of حبشي *ḥašīš*», i. e. of Indian hemp (Father LAMMENS's *Remarques*, Beyrut, 1890, No. 28, in favour of «Ḥasan Sabāḥ», miss the mark). But there are in Arabic two words meaning «consumer of *ḥašīš*»: 1. حبشي *ḥašīšyy* (> vulg. *ḥašīšī*), pl. حبشيين *ḥašīšyyīn* (> vulg. *ḥašīšīn*) and more grammatically, حبشية *ḥašīšyāḥ*; 2. حشاش *ḥaššāš*, pl. حشاشين *ḥaššāšīn*. DE BIBERSTEIN KAZIMIRSKI (*Dict. arabe-français*, I, 430), LOKOTSCH (*Etym. Wörterbuch*, 1927, No. 839), *Der Grosse Brockhaus* of 1928 (s. v. «Assassinen»), O. BLOCH (*Dict. étym. de la langue franç.*), G. WIET (in G. HANOTAUX, *Hist. de la nation égyptienne*, iv, 434) agree that «assassin» comes from *ḥaššāšīn* (BLOCH gives in fact «ḥachchāchī», i. e. *ḥaššāšī*, which does not exist, any more than «ḥachāchī» [= *ḥašāšī*], in DEVIC's *Dict. étymol.* or in the Supplement to LITTRÉ). In *EI*, «assassin» is said to come from *ḥašīšiyun* (as if the unwritten final *-un* of the nominative in classical Arabic had given the final nasal of «assassin»); but that final was no longer pronounced in the current speech of the 12th-13th cent., and I do not know of any word then borrowed from Arabic where it can still come into account). D'OHSSON (*Oh*, III, 203) simply derives «assassin» from *ḥašīšīn*. MURRAY thinks that the old French forms are based on the Arabic singular, but that the form finally adopted in European languages is based on the plural; although he does not express any definite view, it seems that he derives the old French plural forms of the «hassisis» type from *ḥašīšī* (sing.) and «assassin» from *ḥaššāšīn* (pl.). This solution may look fairly reasonable; still I am not satisfied that it meets all the requirements of the case.

Benjamin of Tudela has in Hebrew «Kašīšīn» (DE SACY, *loc. cit.* 23); the Armenian form (with final *-h* of the plural) is «Hašīšīḥ» (cf. *Hist. des Crois.*, Arm., I, 382, 482, 486); *Χασίοι* of Anna Comnen and Nicetas must be wrong forms of *Χασίοιοι* (DE SACY, 22); Nicephorus Phocas gives *Κασύοιοι* (with *υ* pronounced *i*; cf. QUATREMÈRE, *Hist. des Mongols*, 122). Forms with the initial aspiration are met with in the West as «Heissessini» in Arnold of Lubeck (DE SACY, *loc. cit.* 21), «Heresim» (read «Hesesim» < \*Hesesin) in Jacques de Vitry (QUATREMÈRE, *loc. cit.* 122), «Haxasini» in Rubrouck (*Wy*, 210, 286, 287), «Hassisi» etc., in *Gestes des Chiprois*, 775-777; «Hassassins» and «Hasassins» in *Annales de la Terre Sainte* (*Arch. de l'Orient latin*, II, II, 454, 455); and even «Chazisii» (*Y*, I, 141).

There are innumerable examples without *h*-, including Lat. pl. «assassini» in William of Tyre (*Gesta Dei per Francos*, I, 994), Joinville's «assacis», Hethum's «assasins» or «assisens» (*Hist. des Crois.*, Arm., II, 168), «assissini» and «assisini» in *Liber bellorum Domini* (*Arch. de l'Orient latin*, I, 304, 312), etc.

The omission of an initial *h*- in transcriptions is of frequent occurrence and does not affect here the problem of etymology (see «Abasce», «Avarium»). But the two points in the above transcriptions which create difficulties are the alternation of the *-i*- and *-a*- in the second syllable, and the presence or absence of an *-n* in the last. Sometimes, the presence or the absence of the *-n* may be due to mere chance, as, for instance, in the *Gestes des Chiprois* (in French), pp. 775-777, where the following forms are met with: p. 775, «Hassissés» (3 times); p. 776, «Hassisins» (once), «Hassissins» (twice), «Hassisi» (5 times); p. 777, «Hassisi» (once). It seems evident that

the author used only one word, and I think the *-n-*, in the present case, crept in unduly. The author, who lived in the Holy Land, must have used a form strictly borrowed from the sing. *ḥašīšī*, just as with the Greek transcriptions.

I also admit as probable that the forms with the *-i-* in the second syllable and *-n* in the last, as Polo's «asciscin», represent the plural *ḥašīšīn* of *ḥašīšī*, although I do not entirely discard the possibility of a coincidence with a final *-n* of purely Western origin, such as occurs in «Tartarin» for «Tartar», or in Polo's «Taurisin» for \*Taurisi < Tabrizī.

The case is more difficult with forms like Joinville's «assacis», William of Tyre's «assassini», Rubrouck's «haxasini», and the question here is not only one of transcription, but it also affects the use of the terms in Arabic. QUATREMÈRE (*Hist. des Mongols*, 122-125) has added many examples to those formerly adduced by DE SACY; almost all of them, as might be expected in works of a literary character, use the plural *ḥašīšīyāh*, not the more vulgar *ḥašīšīn*, and only one text speaks of the *ḥaššāšīn* (pl.), and in the sense of *ḥašīš*-addict (in Arabic texts, the *ḥaššāš* is also the man who sells *ḥašīš*). But it may very well be that *ḥaššāš*, pl. *ḥaššāšīn*, was in current colloquial use, in a depreciatory sense not conveyed by *ḥašīšī*. There is no such form as \**ḥaššāšī*, however, and I do not know how to account for Joinville's «assacis», unless we see in it a hybrid form, in which Joinville has unconsciously combined the two forms, the one derived from *ḥašīšī*, the other from *ḥaššāšīn*, both of which were then in use among the Franks in the East.

The figurative use of the word in French began with «assasis» already in the 13th cent; the real modern sense of «assassin» dates, in England as in France, from the 16th (although in French «assassin» meant then generally «assassination»). It may be noted that the oldest English example of the modern sense quoted by MURRAY has still an *-i-* in the second syllable: *Hee is an Ascicinus* (1531).

As has been suggested by DE SACY (*loc. cit.* 83), the name of *ḥašīšī*, strictly speaking, must have first been applied only to the men who were drugged with *ḥašīš* before being sent on their murderous errands, and it was by an undue extension of meaning that it became the name of the whole sect in the East. But it is a mistake, in view of the numerous Arabic texts quoted by QUATREMÈRE, to say, as does Sir P. M. SYKES (*A History of Persia*, II, 107), that «Assassins» is a «European name».

*Ḥašīš*, the Anglo-Indian *bang*, was probably, with opium, one of the ingredients entering into the composition of «theriaca», which was very much in use in the East during the Middle Ages (though not as an antidote as in the Western sense of «theriaca»). *Tariyaq* is in the *Qutad̄yu bilig* (46<sup>19</sup>, p. 97); *tariyākī* has even been translated as «opium-eater»; this «theriaca» reached China in 667 as 底也伽 *ti-yeh-ch'ieh*, also written a little later 底野加 *ti-yeh-chia* (cf. HIRTH, *China and the Roman Orient*, 276-279). The YS, 27, 4 a, mentions that in 1320 the Mussulman physicians offered to the Emperor the drug called 打里牙 *ta-li-ya* (*tariyaq*), and in 1332 the *ilkhan* Abū-Sa'id sent, among other presents, 88 pounds of 塔里牙 *t'a-li-ya* (*tariyaq*; YS, 37, 2 a). This seems to leave no doubt that, among Marignolli's presents to Özbäg-khan, we should not include the unknown *cytiacam* (acc.) as is given in the Prague ms. of his chronicle and as has been adopted by MOULE (*JRAS*, 1917, 4; *Mo*, 255) and VAN DEN WYNGAERT (*W*, 527), but *tyriacam* (= *theriaca*) as in the Venice ms., this last reading being justly preferred by YULE (Y<sup>1</sup>,

III, 212) and GOLUBOVICH (*Bibl. bio-bibl.* IV, 272). The word travelled as far as the Tagal language of the Philippines (cf. *triacca* in BLUMENTRITT, *Vocabulaire*, 80).

## 37. ASYA MEDIA

*aise la grant* FA<sup>4</sup>  
*asia* R

*asya media* Z  
*maior asia* L

*media* R

This occurs in Z, and seems to represent some such form as «Asie mediane» or «Aise mediane» which is not in any of the French texts. RAMUSIO had it too in his original, but has taken «media» to be Media, the country of the Medes, and has inserted «Asia» a few words later; there is no good reason why Polo should bring in here the ancient Medes. Hethum uses archaistically «Medie» as a designation of Kurdistan (*Hist. des Crois.*, Arm., II, 127), and it is also in a list of ancient names that we find «Media» in Jourdain Cathala (ed. CORDIER, 122). But since «Asya Media» was in Z and in the mss. used by RAMUSIO, I think the name ought to have been included in B<sup>1</sup>, I. A similar confusion was current in the Middle Ages for the «Empire of Čayatai», which was called «Imporium Medium», but which was often altered to «Imperium Medie» or «Imperium Medorum» (cf. Y<sup>1</sup>, III, 85; Mo, 197; HALLBERG, 344-346; GOLUBOVICH, *Bibl. bio-bibl.*, II, 573; W<sup>y</sup>, 503; in Y<sup>1</sup> and HALLBERG, «Combaleh» or «Côbalek» on the map of Andrea Bianco are perhaps not simply alterations of «Armalek» as YULE, who is followed by HALLBERG, 18, maintains, but alterations of a legend analogous to that of the Catalan Map which makes the sovereign of the Empire of «Medeia» [for «de Medio»] called «Chabech», that is to say Kābāk († 1327), reign at «Emalech» [= Almaliq].

L and FA<sup>4</sup> seem to confirm that there was an epithet, not another word representing a name, after «Asya». «Aise» in FA<sup>4</sup> is the form used by Hethum (*Hist. des Crois.*, Arm., II, 125, 215-216). Moreover, Hethum distinguishes «Aise la Profunde», which is Central Asia and the Far East, from «Aise la Majour», roughly speaking the territory of the Mongol *ilkhans*. This «Aise la Majour» is evidently the same as «Asia maior» of L, «Aise la grant» of FA<sup>4</sup>. But it must also be the «Asya Media» of Z. In other words, there were three «Asia»'s if we include Asia Minor, which was the same as ours.

## 38. AVA

*ava* F, FA, FB, Z, L  
*deuine* V

*dyava* LT  
*jaua* (?) TA<sup>1</sup>

*zava* TA<sup>3</sup>

On Ava, 𑀅𑀲 Avah, cf. Y, I, 81; it must be the «Ove» of STIELER's *Handatlas*, south-east of Savah. There have been two places if not three of that name, but the one here meant is the Ava which was the head-city of a group of 40 villages in the district of Savah; unfortunately,

I cannot make much of the description given in BARBIER DE MEYNARD, *Dict. hist. de la Perse*, 56, n. 1 (cf. also *ibid.*, p. 2, s. v. « Âbah », and also p. 299). Cf. also *LS*, map V and p. 211; LE STRANGE, *Nuzhat-al-qulûb*, transl. p. 66. A place 阿哇 A-wa, Ava, is mentioned in *Ming shih*, 326, 7a, without any details.

## 39. AVARIUN

<i>anania</i> R	<i>auarian</i> F, FA, L	<i>auauon</i> V
<i>anariam</i> VA	[ <i>au</i> ]arion LT	<i>dauana</i> VL
<i>auairen</i> FB	<i>auarium</i> Z	<i>varia</i> TA <sup>3</sup>
<i>auana</i> VB	<i>auaruam</i> P	<i>varria</i> TA <sup>1</sup>

I suppose « Avarium » of Z to be a wrong rendering of « Avariū », under the influence of the Latin finals in *-um*. The word has long been recognized as Arabic حواريون *hawāriyūn* (*hawārijjāt*), a plural form particularly used as an epithet of the apostles and disciples of Christ. It may seem curious that Polo should have heard that Arabic name applied to S. Thomas in India, but Polo expressly says that it is a name used by the Mussulmans. For the fall of *h-* in the transcription, cf. « Abasce » and « Assassin ». By some strange coincidence, *Hawāriyūn* (perhaps through a corruption of the text) appears in Rašidu-'d-Dīn as the name of a city on the eastern coast of India (cf. ELLIOT, *History of India*, 1, 72).

Although omitted from RADLOV's great dictionary, the word has been used in Osmanli Turkish. It occurs twice, written *χαβαριγοῦν*, in the Turkish texts which Gennadios transcribed with Greek letters in 1455-1456 (cf. HALASI Kun Tibor, in *Körösi Csoma-Archivum*, 1936, 160, 165, 245).

## 40. AVENIR

*auenir* VB

The name, in fact the whole passage on the legend of Barlaam and Josaphat, occurs only in VB. « Avenir », for \*Auēnir = Avennir, is the phonetic transcription of the Ἀβεννίρ of the Greek text (for another example of a Greek name transcribed in the old Latin version of the book according to the mediaeval pronunciation of Greek, cf. PEETERS, in *Anal. Bolland.*, XLIX [1931], 281 : « Eufimium » = Euthymium). The name is sometimes altered to « Auemur », for instance in Vincent de Beauvais, ed. Mentellin, 1473, book 16, ch. 1 sq. (hence « Auemur »

in an early Latin edition and « Anemur » in a Spanish version; cf. KUHN, in *Abh. d. Ph.-Ph. Kl. d. k. Bayer. Ak. d. W.*, xx [1894], 55, 66), or to « Auennie » (KUHN, *ibid.*, 54). « Auen-nir » is correctly given on the engraving of the German edition of Barlaam's legend, printed c. 1477, and reproduced in *Y*, II, 327.

The form of the name of the king Abennêr seems to have been adopted by the author of the Greek text under the influence of the name « Abner » of the Bible (II Sam. 3 sq.). The Georgian version gives « Abenes », sometimes « Iabenes » (MARR, in *ZVOIRAO*, III, 259). The old Arabic version, in the Bombay edition, writes جنيسر Jnisr. KUHN (*loc. cit.* 16) reads « Janaisar » and moreover suggests a correction to جنس \*Jabanas to fit the Georgian form « Iabenes ». Yet one cannot but be struck by the fact that the Arabic form, as we have it, ends in -r like the Greek form, and that would lend colour to the theory, which PEETERS does not absolutely reject (*loc. cit.* 308), that both the Georgian text and the Greek version from which the novel was developed have been translated direct from the Arabic. In the present case, none of the readings brings us any nearer the true Indian name of the man whose part king Abennêr plays, and who is the well-known Śuddhodana, Śākyamuni's father. I do not find the name of the king in the old Persian version (partly published by OLDENBURG in *ZVOIRAO*, IV, 229-265). For further research, I only wish to call attention to the fact that Śuddhodana's name must have taken in Central-Asiatic languages, under Prakrit influences, a sonant initial, since we find it transcribed in Chinese as 閔頭檀 Yüeh-t'ou-t'an (\*I'wät-d'äu-d'ân), \*Yudhodan. As we know from alternations like *yaβyu*, *žaβyu*, *ɣabyu*, both the Georgian *i*- and the Arabic *j*- are possible at the beginning of a transcription otherwise too corrupt to be corrected for the present.

For the general problem of the legend of Barlaam and Josaphat, see « Iosafat ».

## 41. AVIGI

*avigi* L; R

Only in L and R. *B*, 17, gives « Arigi » as the reading in R; this other reading « Arigi » is also mentioned in *B*<sup>1</sup>, 451; but I find only « Auigi » in the text and in the Index of all the editions of RAMUSIO; BENEDETTO seems to have taken his *avigi* not directly from RAMUSIO, but from BALDELLI-BONI, 29, where it is a slip or a misprint.

According to Polo, this is the name of the famous goshawks of Georgia. YULE has surmised that it is derived from Arabic *'afšī*, « *Falco montanus* » (*Y*, I, 57), and this has passed into *RR*, 412. But there are hardly any Arabic words in Polo (except for instance « avariun » and « çerme », and these for special reasons), and neither the phonetic nor the semantic correspondence is adequate. I have no satisfactory explanation to offer. In *Codex Cumanicus*, 129, the name of the goshawk (*astor*) is *qarčiyai*, in Turkish as well as in Persian, a word well known even now under the forms *qarčiyai*, etc. The sparrow-hawk is called in Turkish

*qīryul* (miswritten «cheegey» for «chergey» in *Codex Cumanicus*, 129; cf. also BROCKELMANN, *Kāšyari*, 148). But both would suppose very corrupt forms to be represented by *avigi* (\**cariga*, or the like).

## 42. AZAMBALE, see CIAMBA

*accambale* R *azambale, azamballe* VB

The pseudo-king of Champa, whose name is due to a misreading; see «Ciamba» and Vol. I, 366.

## 43. AZURE

<i>açur</i> F	<i>azuro</i> V, VA, VB	<i>lapis laçuli</i> L
<i>açurum</i> Z, L	<i>azurro</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>lapis lazuli</i> P, VL
<i>arzurum</i> LT	<i>azurum</i> L	<i>lasur, lazur</i> G
<i>asur</i> FA	<i>azzurro</i> TA <sup>3</sup> , R	<i>oltra marin (azuro)</i> VB
<i>azur</i> FB, Gr	<i>lapis laçul</i> LT	<i>ultramarinum (azurum)</i> L
<i>azurium</i> P, Gr		

In French and Italian texts it is usually *lazur*, etc., but, with the exception of G, the *l* may be taken as the article *l'*.

Polo mentions «*açur*» (as a mineral) three times, in the chapters on «Badascian» (Vol. I, 137), on «Tenduc» (Vol. I, 182), and in the description of the palace of Qubilai (Vol. I, 211). He certainly takes «*açur*» in its original meaning of lapis-lazuli, and commentators have admitted that we are really concerned with lapis-lazuli in the three passages; this will be discussed later.

The genealogy of the word «*açur*», azure, is generally established as follows (cf. LOKOTSCH, *Etym. Wörterb.*, No. 1311; BENVENISTE, in *JA*, 1936, 228) :

Skr. *rājāvarta*, *rājavarta*, *lājavarta* (cf. FINOT, *Lapidaires indiens*, XVIII, 92, 201) > Pers. *lājvard*, *lažvard* > Arab. *lāzvard*; late Armen. *lažvard*, *lažurd*. From Arab. *lāzvard*, > low Greek *λαζούριον*; low Latin *lazurius*, *lazur*, *lazulum* (hence «lapis-lazuli»); French *azur*, It. *azurro*, *azzurro*, Span. *azul*, Port. *azul* (> Indian modern vernac. Konk. *āzūl*; Tet. in Timor, *ajul*; cf. DALGADO, *Influência do vocab. português*, 16), the initial *l* having been mistaken for the article. *Azur* appears in French already in the *Chanson de Roland* (c. A. D. 1110).

From Skr. *rājāvarta* > Buddh. Sogd. *r'čβrt* (? BENVENISTE) > Uigh. *ražwart* and *aržurt* (MÜLLER and von GABAIN, *Uig. IV*, in *SPAW*, 1931, 703) > Mong. *rašivar* (KOVALEVSKIĪ, 2659) > Manchu *rasivar* (ZAKHAROV, 1019, with a wrong Skr. etymology *rašitar*).

The same word has been supposed to appear in the Chinese account of Ch'ang Tê's mission of 1259, in the form 蘭赤 *lan-ch'ih* (*Br*, I, 151; LAUFER, *Notes on Turquois*, 45; *Sino-Iranica*, 520). But, as a matter of fact, the account has the name twice, and the first time (*Br*, I, 140) it writes it 蘭石 *lan-shih* («lan stone»). *Lan* would in the Mongol period have been a regular transcription of \**lal*, and I agree with WANG Kuo-wei that the «lan stone» must be the same as the 東| *la* of the *Cho-kêng lu*, that is to say Persian *la'al*, «ruby» (cf. *Br*, I, 173), although in the Mongol period *la'al* had become in Mongolian *nal* through dissimilation; *lan-ch'ih*, in the second case, may be a wrong reading for *lan-shih*. As for LAUFER's idea that 蘭 *lan*, «orchid», is an error for 藍 *lan*, «indigo», «blue», it has no basis in fact since the final *-ch'ih* shows the term to be a transcription; moreover, *lan*, «indigo», was still pronounced as *lam* in the 13th cent., and so is phonetically out of the question. LAUFER also brought into the argument the name of the city of 藍市 *Lan-shih*, which he translated by «Blue Market», «a designation which apparently refers to the blue color of lapis lazuli». But *Lan-shih* is only one among many forms given by the different texts for the old capital of the Great Yüeh-chih; there is no reason to try to explain the name by its trade in lapis-lazuli. Contrary to the common opinion, however, I agree with LAUFER (*Notes on Turquois*, 44) that lapis-lazuli, which is not indigenous to India, is not likely to have spread to the West under an Indian name; *rājavarta* and the like must be sanskritizations of the same native name, which remains unexplained, and which is also represented, and perhaps more accurately, by the Persian *lāvard*.

Lapis-lazuli, to be found in certain parts of Tibet, has a Tibetan name, *mu-men*, which has produced in Mongolian and Manchu, through dissimilation, the word *nomín*, although *mumin* is also a bookish designation of the stone in both these languages. As an equivalent of the Mong. *rašivar*, which is certainly < *rājavarta*, «lapis-lazuli», KOVALEVSKII, 2659, has found Tib. *stan-zil*; but to this word our Tibetan dictionaries, which are, however, very unsatisfactory, do not give the meaning of the stone.

The Chinese names for lapis-lazuli have been discussed at some length by LAUFER, and also by H. T. CHANG in his *Lapidarium Sinicum* (1921, 1-13) and in his annotated translation *Metals and stones as treated in Laufer's «Sino-Iranica»* (1925, 75-77); in BEFEO, xxiv, 277-283, DEMIÉVILLE has shown that most of the solutions proposed in that part of the *Lapidarium* cannot be accepted.

The main difficulty lies in the fact that Chinese names of minerals are often vague, and that their meaning has sometimes changed. The ordinary modern Chinese word for lapis-lazuli is 青金石 *ch'ing-chin-shih*, «blue golden stone»; there has also been, from the 10th cent. down to our days, a term 金星石 *chin-hsing-shih*, «stone with golden stars», a fitting designation of lapis-lazuli with its brilliant spots of sulphide of iron which were mistaken for gold. In his *Notes on Turquois* of 1913, 44, LAUFER has stated unhesitatingly that this was lapis-lazuli, though in *TP*, 1915, 194, he declared no less authoritatively that *chin-hsing-shih* was «golden mica».

But there is one Chinese name (not mentioned in TARANZANO's *Vocabulaire*) the meaning of which is not open to doubt, to wit 金精 *chin-ching*, «essence of gold». It is given in the *Hsin T'ang shu* as a product of Kurān, which is still the main centre for the extraction of lapis-



lazuli (cf. Y, I, 162; CHAVANNES, *Doc. sur les Tou-kiue*, 159; LAUFER, *Sino-Iranica*, 520). I may add that the whole paragraph in the *Hsin T'ang shu* is taken from Hsüan-tsang (JULIEN, *Mém.*, II, 200, where «or pur» is a mistranslation of *chin-ching*). In the same chapter, in the notice on Fu-lin, mention is made of a tribute including «green [lū] *chin-ching*» (cf. HIRTH, *China and the Roman Orient*, 54, 86, who does not know how to translate it; but his hypothesis of «dust» must be excluded; VISDELOU was perhaps right in thinking of emeralds). The information on the *chin-ching* produced in Kurān has also passed into *T'ang hui yao*, 100, 2a, and *T'ai-p'ing huan-yü chi*, 186, 11b.

With Hsüan-tsang, we go back to the first half of the 7th cent., but I can establish that *chin-ching*, with the same meaning, is at least two centuries older. In the 6th chapter of the *Ta-fang-t'êng ta-chi ching* (NANJIŌ, No. 61), translated in A. D. 397-418, there is a list of the 32 «principal signs» (*hsiang, lakṣaṇa*) of the Great Man (Tōkyō Trip. of Meiji, ㄆ, I, 33b-34a). The list is not in its usual order, and it enumerates 34 *lakṣaṇa* actually, instead of 32. The 32nd *lakṣaṇa* is to have «hair of the colour of *chin-ching*». Now, although there is no such *lakṣaṇa* in the traditional list, one of the last among the 80 «secondary signs» or *anuvyañjana* is variously called (cf. BURNOUF, *Lotus de la Bonne Loi*, II, 605; *Mahāvvyutpatti*, ed. Sakaki, 342) *asitakeśa*, «with black hair»; *bhramarasadrśakeśatā*, «the quality of having hair similar to the [black] bee»; *sunīlakesatā* (in Pali). BURNOUF says the last term means «the quality of having very black hair», but *sunīla* means «very blue» (cf. FINOT, *Lapidaires indiens*, 47), and *sunīlaka* is even listed as a name of the sapphire. Now, in the *Pentaglot Vocabulary*, compiled in the 18th cent., the corresponding *anuvyañjana* is explained in Chinese as «the colour of his hair is [like] 青球 *ch'ing-ch'iu*», which A. RÉMUSAT (*Mél. asiat.* I, 173) and DE HARLEZ (*TP*, 1896, 372) have rendered by «his hair is of the colour of lapis-lazuli». *Ch'ing-ch'iu*, «blue-green gem», is not attested elsewhere, and the Skr. of the *Pentaglot Vocabulary*, as BURNOUF (II, 605) has already remarked, is of no help, since, through the omission of a Chinese and of the following Skr. term, there is no real Skr. correspondence here for the Chinese; but RÉMUSAT and DE HARLEZ have been guided in their translation by the Mongol and Manchu equivalents *rašivar* and *rasivar*, which are indubitably lapis-lazuli. Although I have not tried to follow the tradition through all its stages, it is certain that, until a late period, it was admitted that the Great Man had hair of the colour of lapis-lazuli, and we are entitled to translate *chin-ching* by lapis-lazuli when we find it used in the same way in the beginning of the 5th cent.

The first time that Polo speaks of «ačur» is in reference to Badaḥšan, and there is no doubt that he meant then lapis-lazuli, and that he was right in doing so. The case is somewhat different in his chapter on «Tenduc» (north-west Shan-hsi). YULE (Y, I, 289) has already quoted MARTINI, *Atlas Sinensis*, 50, who, in his paragraph on the Ta-t'ung prefecture, says: «On trouve dans ses montagnes de très bonne pierre d'Azur», and DU HALDE who, speaking also of Ta-t'ung, writes: «On trouve dans ses montagnes d'excellentes pierres d'azur» (YULE's quotation, taken from ASTLEY, is not a literal rendering). I do not think that lapis-lazuli was really found in Ta-t'ung. The T'ang and Yüan geographies are silent on the subject, but the *Ta-Ch'ing i-t'ung chih* (III, *in fine*) has a paragraph on 石縑 *shih-lü*, which, according to the gazetteer of the prefecture, is to be found on a mountain called, for that reason, Shih-lü-shan. Now *shih-lü*, «stone-

green», is a name not of lapis-lazuli, but of green malachite (cf. TARANZANO, *Vocabulaire*, I<sup>3</sup>, 678; II<sup>1</sup>, 389), and I am inclined to believe that Polo and MARTINI, followed by DU HALDE, have mistaken malachite for lapis-lazuli; moreover, MARTINI knew Polo's narrative. There has been in Shan-hsi, in 代州 Tai-chou (now Tai-hsien), an important production of 青綠 *ch'ing-lu*, mentioned already in the *Hsin T'ang shu* and the *Sung shih*; but Tai-chou seems to be too far south to be included in Tenduc; moreover *ch'ing-lu* means green malachite, perhaps also blue malachite (azurite); both are carbonates of copper and have nothing to do with lapis-lazuli (cf. CHANG, *Lapidarium Sinicum*, 313).

As to the third mention of «açur» in Polo, see «Green Hill».

#### 44. BABILONIE

*babelonie* F  
*babillonia* L  
*babelon(em, is)* Z

*babelonia* TA<sup>3</sup>, LT, P<sup>5</sup>, Z, V,  
VA, S, R  
*babelonie* FA

*babilojne* FB  
*babylon* G  
*bambellonia* TA<sup>1</sup>

In agreement with mediaeval usage, Polo means by «Babilonie» Egypt, and the sultan of «Babilonie» is the sultan residing at Cairo (on the more ancient occurrence of the Babylon of Egypt, cf. Y, I, 24). That is why, in a passage found only in VB and R where the name of Cairo occurs (B, 212), it has been tacitly replaced by «Babylon» in RR, 353, and by «Babilonia» in B<sup>1</sup>, 369. YULE (Y, II, 439) had already surmised that there was here some «editing» due to RAMUSIO, because the name of Cairo did not occur elsewhere in Polo; but YULE then forgot VB, which RAMUSIO simply copies.

I am not convinced that the substitution of «Babilonie» for Cairo in that passage is justified. For Polo, «Babilonie» is rather the name of the country than of the city, and he may have repeated the name of Cairo for the town as he heard it; there are in that passage other technical names, such as «çerme» and «Calizene», which point to a very precise informant. Moreover, the fact that a name appears only once in Polo does not warrant its suppression; the name of «Egippte» also appears only once, and in conditions which are not absolutely satisfactory; nevertheless, it would not occur to anybody to change it.

#### 45. BACHU

*abacco* Z  
*abaccu* R

*abaco* L, Z

*bachu*

L, Z, R all have «Abacu» or the like, but my reading is supported by «Bachu» in V. BENEDETTO (B<sup>1</sup>, 437) has adopted «Abaco» and added that, as the Caspian was also called in the

Middle Ages «Sea of Bascon», from the «island of Abaskun», there had probably been a contamination between the two names

It is true that the name of «Sea of Abaskun [or Ābaskun]» has sometimes been given in Persian, in Arabic and in Turkish to the whole or to part of the Caspian (cf. BARBIER DE MEYNARD, *Dict. hist.* I; QAZWINI, *Juwaynī*, II, 115; *Ber*, III, 66, 183; *Mi*, 386; BROCKELMANN, 240 [with an abnormal form «Abysgun»]; HERRMANN, in *Imago Mundi*, 1935, 23, No. 13). But the «Sea of Bascon» is without authority. Odoric speaks of the «Sea of Bachuc»; CORDIER found in the French version of Odoric «Bascon» instead of «Bachuc» and supposed that «Bascon» stood for «Abascun» (*Y*, I, 59; *Y*<sup>1</sup>, II, 105); but Odoric's French version has no value independent of the original Latin text, and «Bascon» has been simply altered from «Bachuc». As to «Abacu» or «Abaco», I hold it to be one of those copyists' errors to which we owe such readings as «Abraaman», etc., for «Braaman», «Amien» for «Mien», «Assara» for «Sara», etc. In some cases, misinterpretations of the Italian *da* as *d'A*<sup>o</sup> may account for these errors. But I am convinced that Polo said «Bacu» like all his contemporaries, the only other form of the name being «Bacuc». «Bacu» and «Bachu» are the forms used by Jourdain Cathala, a Genoese document of 1374, the Catalan Map of 1375, Fra Mauro, Clavijo, J. Barbaro, etc.; «Bachuch», «Bacuc», «Bacuc» are given by Odoric, Pascal of Vittoria, and Marignolli. The final -c (-k, -ch) of this second form remains unexplained; it can hardly be a survival of the ancient form Bākūh with a final weak -h.

The oldest form of the name is Bākūh; but the modern spelling بک، Bākū occurs at the end of the 10th cent. in the *Hudūd al-Ālam* (*Mi*, 411); the naphtha wells were then already famous (cf. also *Y*, I, 49; add Jourdain Cathala, ed. CORDIER, 94; LE STRANGE, translation of the geographical part of the *Nuzhat al-Qulūb*, 198, 278; PETRUŠEVSKIĪ, in *Izvestiya Ak. Nauk*, 1937, 906).

Polo also calls the Caspian the «Sea of Gel or Chelan» (see «Gel or Chelan»), and the «Sea of Sarai» (see «Sarai»).

The «Mare Abacuc sive Mare de Sala» on Waldseemüller's map of 1516 combines wrong forms of «Bacu» and «Sarai»; the name of the Prophet Habacuc helped perhaps in creating «Abacuc».

Among mediaeval Western authors, Simon of Saint-Quentin and Rubrouck are the only ones to call the Caspian the «Sea of Shirvān», a name which, among Eastern writers, I have only met with in Ḥajjī-Ḥalfa's *Jihān-numā* (cf. DORN, *Caspia*, 100). The most common Eastern name is «Sea of the Ḥazar». The name of «Sea of Khvalis» in ancient Russian texts is a corruption for «Sea of Ḥwarēzm». Cf. DORN, *Caspia*, 44, 100-102; BARTHOLD, *Zur Erforsch. des Orients*, 100; *Rev. des ét. slaves*, IX [1929], 120-123; FERRAND, in *JA*, 1924, I, 206; 1925, II, 112, 269; *LS*, 180-181.

The Chinese name in the 13th cent. is 寬田吉思海 «The K'uan-t'ien-chi-ssü Sea» (once K'uan-ting[定]-chi-ssü); cf. WANG Hui-tsu<sup>2</sup>, 40, 11a. *T'ien-chi-ssü* is of course Turk. *tängiz*, «sea»; I do not think that *k'uan* means here «broad»; it must be a transcription, theoretically *kön* or *köl*; *köl* means «lake» in Turkish, and \*Köl-tängiz would be literally the «Lake-Sea». A Mongolian form \*Gün-dängis, «Deep Sea», good from a semantic point of view, is objectionable phonetically.

## 46. BACSI

*bacsi* F, Z, V, L*bachsi* R

*Baqši, baḥši*, first met in Uighur texts, passing afterwards into Mongolian, found its way, during the Mongol period, into Persian بخشى *baḥši* (for instance *Bl*, II, 405), when it also occurs occasionally in Chinese texts (*pa-ha*[<sup>1</sup>𐰽𐰺]-*shih*, translated by 師 *shih*, «master», «teacher»); it is even known in Tibetan (Sarat CHANDRA DAS, 177). Although originally an epithet mainly used in reference to Buddhist priests, it is applied in modern Turkish dialects of Central Asia to all sorts of masters, from musicians to quacks and sorcerers, and always with a necromantic implication (cf. SHAW, *Vocabulary*, 40; RADLOV, IV, 1445-1446). This amply justifies Polo's use of the word. It has long been accepted that *baqši* was derived from Skr. *bhikṣu*, «Buddhist monk» (cf. YULE, *Hobson-Jobson* 2, s. v. «Buxee»), which is hardly satisfactory from the point of view of phonetics. It is more probable that it was borrowed at an early date from Ch. 博士 *po-shih* (\**pâk-dz'î*), «man of great learning»; cf. LAUFER, in *TP*, 1916, 485-487 (the additional note of p. 552 must be suppressed; the term quoted there from the *Yu-yang tsa-tsu* has nothing to do with *baqši*), and my remarks in *JA*, 1925, I, 254; *TP*, 1930, 14-15.

## 47. BADASCIAN

<i>badaciam, balaciam, balaciem</i> FA	<i>balascam, balausciam, bau-</i> <i>dascia</i> TA <sup>1</sup>	<i>balassia, ballasia, ballassia</i> VL
<i>badasciam, badausiam, bala-</i> <i>sian</i> F	<i>balascha, bandasciam, ban-</i> <i>dascian</i> TA <sup>3</sup>	<i>balatian</i> FBr <i>balaxan, balaxiam</i> R
<i>badascian</i> F, Ft	<i>balascia</i> LT, P, V (cor.)	<i>balaxian</i> Z
<i>badasian</i> F, Fr, FB	<i>balasciam, bandascam, ban-</i> <i>dascham</i> LT	<i>baldasciam</i> F, TA <sup>3</sup>
<i>badassan, baddasan, balasan,</i> <i>balassa, baldasan balda-</i> <i>siam, ballassan</i> VB	<i>balascian</i> F, Ft, L, Z	<i>baldasian, balesian, balsian,</i> <i>belesian, belestian, sobasain</i> V
<i>balacia</i> P	<i>balasia, balastia, balaxia, bal-</i> <i>daxia, ballastia</i> VA	<i>baldasya</i> G
<i>balacian</i> FA, FAr, FB	<i>balasiam</i> Fr	<i>ballaschan</i> Lr <i>ballascian</i> L
<hr/>		
<i>balais</i> FA	<i>balasi</i> V, VA, Z	<i>ballascii</i> L
<i>balaiž</i> FB	<i>balassi</i> V, Z; R	<i>pallazi</i> VA
<i>balasci</i> F, LT, TA <sup>1</sup>	<i>balasti</i> G	

The best mss. give both «Badascian» and «Balascian» (with an occasional «Baldasciam» in F, and «Baldascia» and «Baudascia» in TA, reflected in the «Baldasia» of the Catalan Map; cf. HALLBERG, 61-62; add «Balasian» in Fra Mauro). Both forms are theoretically possible (cf. Y, I,

161), and *B*<sup>1</sup>, 438, while adopting «Balascian», thinks Polo may have used both. As a general rule, I am not readily inclined to believe that the same man uses at the same date two different forms (that is, fundamentally different, and not only in spelling) for one and the same name. In *F*, the ms. gives «Badascian» in the notice on the «Caraunas» and when speaking of the river of Badaḥšān, but «Balascian» in the notice on the country where Polo speaks also of the rubies «balasci». This may perhaps help us out of the difficulty.

The name of the country is بدخشان Badaḥšān (and بدخشان Badaḥšān), from which the name for the ruby mined in Badaḥšān is regularly derived, in the forms *badaḥšānī*, *badaḥšī* and even *badaḥš* (cf. VULLERS, I, 201). There is no form \*Balaḥšān for the country, nor \*balaḥš for the stone in Persian dictionaries, although \*Balaḥšān must have existed in some eastern dialects (on similar forms, cf. BENVENISTE, *JA*, 1936, 234), and the *Codex Cumanicus* of c. 1300 gives *balacs* as the Persian form. In Arabic, a popular form *al-balaḥš* for *badaḥšī* (*badaḥšī*) is given by Ibn Baṭṭūṭah. It is this popular form which passed to the West c. 1200, and has given our «balas» («balais» in French); on this word, cf. QUATREMÈRE, *Hist. des Sultans Mamlouks*, II, 71 (but read «balakhsch» instead of «balkhasch»); *Not. et Extr.*, XIV, 222-223; HEYD, *Hist. du Commerce*, II, 653-654; YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Balass» (where Polo's «balasci» has been forgotten); DALGADO, *Vocabulario Luso-Asiático*, s. v. «Balais, balax»; DAMES, *Barbosa*, II, 220; *Codex Cumanicus*, p. 108, Lat. *balaxius*, Pers. *balacs*; *Fe*<sup>1</sup>, 387. Polo called these rubies of course by the form of the name which was understood in the West. But if he had always spoken of «Balascian» nobody in the West could have changed it to the correct form «Badascian». Let us suppose, on the contrary, that Polo always used «Badascian» for the country, but «balasci» for the stone. A learned transcriber, who knew the form «Balascian» because he had found it elsewhere, could easily correct «Badascian» to «Balascian» in this chapter, so as to bring the name of the stone into agreement with that of the country; and «Badascian» would remain in the other chapters. I think this is what happened with *F*. The next stage would be to unify the name as «Balascian» everywhere; that is the case with *Z* and *R*. Now, there is a well-known work which could have provided learned copyists with the form «Balascian»; that is the work of Hethum, written in 1307, which gives «Balacxan» (cf. *Hist. des Crois.*, Arm., II, 125 and 265, taking the various readings into more account than the editors have done). I have suspected elsewhere (see «Ulau») that Hethum's work may have influenced early copyists of Polo, and we have perhaps another example of it in the present case. Provisionally, I suppose that Polo himself only used «Badascian» (or «Badascian»?) and «balasci». On the forms, cf. also MARQUART, *Ērānšahr*, 279; LENTZ, in *ZDMG*, 1932, 12-14; *Mi*, 349, 485. For general information on the subject, cf. BARTHOLD'S important notice in *EI*, s. v. «Badakḥshān».

The name of Badaḥšān appears in Chinese texts already in the 7th cent. (cf. CHAYANNES, *Doc. sur les Tou-kiue*, 69, 275; *Br*, II, 65-66), and the geographical documents of the 14th cent. write 巴達哈傷 Pa-ta-ha-shang (*YS*, 63, 15 b; for final Persian -n then indicated by -ng [= nasalized vowel], see «Caxan», «Ispaan», and cf. other cases in *Br*, II, VII-VIII). But it may be worth while to quote here the following text: «The 13th year *chih-yüan*, in the first moon, . . . on [the day] *wu-tzū* (February 8, 1276), the officials of the Chung-shu-shêng said that Wang Hsiao-chung, for some fault, had been ordered to go to 八答山 Pa-ta-shan (Badaḥšān) to mine precious stones

(*lit.* 'precious jade', *pao-yü*); that his road passed through Sha-chou (see 'Saciou') and that he encountered there the revolt of 火忽 Huo-hu (Hoqu); that [Wang] Hsiao-chung had escaped and had come for orders; and that [he ought] to be ordered to till the ground in Kua[-chou] and Sha-chou. The Emperor agreed» (YS, 9, 2 a). Hoqu (see also «Barac») was Güyük's third son, and took Qaidu's side when Qaidu rebelled against Qubilai; the present text proves that Hoqu's rebellion cannot be dated later than 1275. But the most interesting feature of the text is that it shows that, in 1275, Qubilai would and could send people to work for him in Badaḥšān, which was thus outside the territory of the *ilkhans* of Persia. It must be this text of 1276 which is alluded to in Y, III, 34, with PARKER's erroneous remark that it is the first mention of Badaḥšān in Chinese sources (the Pa-ta-shan of 1276 must not be confused with the Pa-ta[達]-shan mentioned in 1275, YS, 8, 10 a; the latter was north of the great bend of the Yellow River; cf. T'u Chi, 7, 31 a).

The text of 1276 is important for another reason. When Rabban Çauma and the future Mār Yahbalaha III undertook their pilgrimage to the Holy Land, they arrived at Khotan (see «Cotan», where the King «Oqo», having rebelled against Qubilai, had sought refuge (cf. CHABOT, *Hist. de Mar Jabalaha III*, 22-23). This king «Oqo», who has not hitherto been identified (cf. *Mo*, 100), is certainly the prince Hoqu, Güyük's son; Rašīd writes his name *موقر* Hoqu (*Bl*, II, 5), with the weak *h*- not marked in Uighur-Mongol spelling. If Hoqu's rebellion occurred, as I think, in 1275, that would give us the clue, so long desired, for fixing the chronology of the two monks' journey.

## 48. BAGHERLAC

*bagherlac* L  
*barchelac* VB  
*barcholach* V  
*bargelach* R

*bargeolach* LT  
*bargherlac* F  
*bargolac* VA

*bargolach* P  
*barguelac* FB  
*barguerlac* FA

This is the sand-grouse (*Syrhaptēs Pallasii*, a kind of *Pterocles*); the common Chinese name, 沙雞 *sha-chi* («sand-fowl»; not «*sha-ch'i*» as in Y, I, 273), is applied also to other birds. The name given by Polo is Turkish; we find already *bayırlaq* in Kāšyarī (BROCKELMANN, 18). ZENKER's *boyurilaq* must be wrong, but *bayırtlaq* is known in Osm. and Čay. (RADLOV, IV, 1452, 1455). For Turkī (in Chin. Turkestan), SHAW (*Vocab.*, 210) gives *بیتاق* *bayıtaq* (with the usual dropping of *-r* in the western Turkī pronunciation); I have myself heard the form *bayırtaq* used in Turfan and Qomul; LE COQ (*Sprichwörter. . . von Turfan*, 84) has noted another form *bäyriłtaq* in Turfan; cf. also E. D. ROSS, *A polyglot list of birds*, 129; RADLOV omits the Turkī forms by oversight.

With the sole exception of L, all the mss. have an *-r* at the end of the first syllable («*bargherlac*», «*bargelach*», etc.), but there should not be two *r*'s in the word. Even before

knowing L, I had decided in favour of «bagherlac»; the first *-r-* was perhaps introduced under the influence of «Bargu» which occurs a few lines earlier. The insertion, or the metathesis, must have taken place very early, and is supported by RAMUSIO; unfortunately the chapter does not appear in Z.

#### 49. BAIAN (brother of MINGAN, c. 93)

*bagiam* VL  
*baia* FA, FB  
*baiam* TA<sup>1</sup>, TA<sup>3</sup>

*baian* F, V  
*bajan* S  
*baiam* LT

*bayan* R  
*baym* P  
*chaiam* VA

In Mongolian Bayan, «rich». There were many Bayan in Mongol history; WANG Hui-tsu<sup>1</sup>, 32, 4-6, thinks he can trace nineteen in YS alone; but the three who have biographies in YS are out of the question here. Being a brother of Mingyan (see «Mingan»), Bayan must have been a Qangli. I have not yet found in Chinese texts a Bayan who was a Qangli, or who was brought into connection with the *kuei-ch'ih* troops. CHARIGNON (*Ch*, III, 91) is wrong when he says that one of Mingyan's grandsons was called Bayan; the very name he gives, 普顏忽里 P'u-yen-hu-li, represents the Turkish name Buyan-quli «the Slave of the religious merits», answering to a Sanskrit prototype Puṇyadāsa. We know of only one brother of Mingyan, and he was put at the head of the *kuei-ch'ih* troops, but that was some time after 1303, and Polo could not have known about it; moreover, his name was 脫迭出 T'o-tieh-ch'u (\*Tödäcü?). We might suppose that Mingyan had another brother, who was simply his assistant, and has left no trace in history, if a text of Rašidu-'d-Din did not raise fresh difficulties. Rašid (*Bl*, II, 501) mentions among Qubilai's high officials a man whom BLOCHET calls «تیکتادای پسر نایان کورکچی Tiktädäi son of the *kübügäci* Nayan». No office of *kübügäci* is known; I proposed in 1920 (*Y*, III, 70) to read «بايان کورکچی Bayan *güyükçi*», and to see here the «cuiucci» Bayan of Polo; I still think that the correction is almost certain. On the other hand, the would-be name «Tiktädäi» is certainly to be read ننگیادای Nangiadai (in written Mongol Nanggiyadai). The name of Nanggiyadai is very common (see «Mongatai»), but the best known Nanggiyadai in Qubilai's time was a Naiman, and his father was called Ma-ch'a (\*Mača, \*Mačaḡ?); cf. YS, 131, 2 a-3 a; no mention is made of the *kuei-ch'ih* in his biography. Another Nanggiyadai held a high rank in the Privy Council (he was *ch'u-mi fu-shih*), although we know very little else about him (YS, 126, 8 b); his father was really named Bayan, but this Bayan is the well-known general (see «Baian Cingsan»), who was a Bārin, not a Qangli. No mention is made of the *kuei-ch'ih* in Bayan's biography (YS, 127); moreover Rašidu-'d-Din calls him by the high title of «Bayan noyan» (*Bl*, II, 527), and it is extremely unlikely that he should have referred to him here and called him by the much lower title of *güyükçi*. To sum up, while Rašid's text confirms the existence of a «cuiucci» called Bayan, we are not in a position to say that he was really Mingyan's brother, and the case of his son Nanggiyadai, a high official at the Court of Qubilai, remains unsolved.

## 50. BAIAN CINGSAN

<i>baia, baiam enanza, baym</i> TA <sup>3</sup>	<i>baiam cogi, boiam</i> VA	<i>baian zinqueschan</i> V
<i>baia anasa</i> TA <sup>1</sup>	<i>baian</i> F, FA, FB, LT, P, V,	<i>bayam</i> LT, P; G
<i>baiaincuncan</i> VL	VB	<i>bayam chinsam</i> P
<i>baiam</i> FA, LT, TA <sup>1</sup> , TA <sup>3</sup> ,	<i>baian chinclan</i> FB	<i>bayan</i> Z; G
VA	<i>baian cinq san</i> F	<i>chinsambaian, chinsanbaian</i> R
<i>baiam cinasar</i> LT	<i>baian tincsan</i> FA	

This is «Bayan čingsan», «Pai-yen *ch'êng-hsiang*» in Chinese, the very form used in Chinese for instance in *TP*, 1915, 403. Rašidu-'d-Dīn generally calls him «Bayan» or «Bayan *noyan*», *noyan* being a high title of noblemen and commanders in Mongolian (*Ber*, I, 196; *Bl*, II, 447, 527). See also «Cingsan».

On Bayan, 伯顏 Pai-yen, cf. *Y*, II, 148-149; *YS*, 127; *T'u Chi*, 90. He was a Bārin; according to Rašid, his father, Kōkčü, had been executed for misbehaviour. Bayan, as a young man, was sent to Persia with Hülägi, but Qubilai having claimed him back, Bayan returned to his Court and was shown high favour (cf. *Ber*, I, 196; the name of Kōkčü, father of Bayan the Bārin, appears also in *Bl*, II, 447). According to the notices on the Bārin, this occurred in Hülägi's time, and that is why YULE placed Bayan's return to China about 1265. In the parallel text of *Bl*, II, 447 (cf. also *Oh*, II, 397), it is only in Abaya's time that Qubilai sent the embassy that ordered Bayan back to the Imperial Court, but this must be a mistake, as Bayan was already in China before Hülägi's death, and Rašid himself says a few lines later that Bayan was sent to China in 1265. The Chinese biography is somewhat different. While agreeing with Rašid as to the name Alaq of Bayan's grandfather, it gives to his father the name of Hsiao-kou-t'ai, \*Hä'ügütäi (written Mong. \*Ägügütäi), and says that this father followed Hülägi to Persia, so that Bayan was brought up in the West; and it is Hülägi who, in the beginning of the period *chih-yüan*, sent Bayan to report certain matters to Qubilai. Bayan must have arrived in China early in 1265 («1263» in *Ch*, III, 33, is wrong; Bayan's arrival early in 1265 seems to dispose of YULE's idea [*Y*, I, 10] that the elder Polos may have travelled with him); if his age at the time of his death as given by his biography be correct, he was then 29 years old. Qubilai was struck by the ability of the young man, married him to a younger sister of the Minister An-t'ung (see «Nomogan»), and, in August-September 1265, appointed him «Left *ch'êng-hsiang*», which was the highest ministerial post in the Grand Secretariat (but according to *YS*, 112, 4 b, five *ch'êng-hsiang* were appointed in 1265, and Bayan is the last on the list, which is headed by An-t'ung). In 1268, Bayan was called to the Privy Council; in 1273, he had the honour of presenting Chên-chin (see «Cinchim») with the jade tablet which made him heir to the throne. It is only in 1274 that Bayan was sent against the Sung, and he left the Court in the autumn of that year. On October 2, having assembled his armies at Hsiang-yang (see «Saianfu»), Bayan, as was usual in Mongol campaigns, divided them into three corps which advanced by different roads; Bayan himself was with the troops of the centre, which went down the Han river towards Han-k'ou. This was the beginning of the campaign which ended two years later by the capture of Hang-chou and the downfall of the



Sung dynasty. Bayan then went to the rescue of Nomoyan (see «Nomogan»). He maintained his high position through the whole of Qubilai's reign. Qubilai died on February 18, 1294, and the death of Bayan occurred on January 6, 1295, at the age of 58; he was thus born in 1236. Rašidu-'d-Din is not far from the truth when he says that Bayan survived his master only eight months (*Bl*, II, 527).

Bayan means «rich» (not «great» or «noble» as in *Y*, II, 148). In *F*, Polo seems to say that «cingsan» means «Hundred eyes», but in *R* this meaning can be understood as attributed to «Baian». Although Polo was no Chinese scholar, he could not fail to know the value of *ch'êng-hsiang*, which had passed into Persian and Mongolian circles with its correct meaning of «minister»; the text of *R* leads us to believe that the mistake is due to the redactor of *F*, who misunderstood Polo's original text, and although the case is not unique, it is not without interest for the general history of Polo's work. Unfortunately the whole chapter is omitted in *Z*. It is of course the name Bayan, in Chinese *Pai-yen*, which has been popularly understood as 百眼 *Pai-yen*, «Hundred Eyes». It has long been known that another prophecy which interpreted the name as 百雁 *Pai-yen*, «Hundred Wild Geese», had been preserved in Chinese works of the 14th cent. (*Y*, II, 150); cf. for instance, *Yüan-ch'ao ming-ch'ên shih-lïo*, 2, 4 *b*, and *Yü-t'ang chia-hua*, 4, 4 *a*. CHARIGNON (*Ch*, III, 33) holds that the very prophecy of the «Hundred Eyes» occurs in Chinese texts, and quotes ch. 159 of the *Hsin Yüan shih*; but this recent work here tacitly copies Polo himself. The two puns have got mixed up in *RR*, 412.

BLOCHET (*Bl*, II, 447) is wrong in saying that *YS*, 127, was silent about Bayan's father, since we have seen that Rašid calls that father twice *Kökçü* while the *YS* has \*Hä'ügütäi. It is *YS* which must be right, because the name of Bayan's father is given as  $\text{كۆچۈ}$  *Högütä* in *Waśśäf* (*Ha*<sup>2</sup>, 40).

Although I have quoted Bayan's biography in *YS*, more commonly accessible, I have checked its indications in one of its sources, Bayan's funerary tablet, the text of which, due to *Yüan Ming-shan*, has been preserved in ch. 24 of the *Yüan wên lei*. Some other extracts from contemporary documents are found in ch. 2 of the *Yüan-ch'ao ming-ch'ên shih-lïo*. Another tablet to commemorate Bayan's merits had been erected at Hang-chou in 1276; it was destroyed by fire, but its inscription re-engraved in 1294; the re-engraved text has been preserved and appears at the end of the *P'ing Sung lu* (ed. *Shou-shan-ko ts'ung-shu*<sup>2</sup>, 3, 4-6).

T'u Chi, 90, 1 *a*, says that Bayan was a Christian, and names «Marco Polo» as his authority. But Polo says nothing of the sort; is there here a confusion either with Nayan, or with Bayan's Christian Alan troops?

## 51. BAIDU

*baida* Lr  
*baidu* F, L

*barach*, *bardu*, *boldu* V

*baydu* Z

*Baidu*  $\text{بایدو}$  is a common Mongol name, but only in the West (cf. *Ha*<sup>1</sup>, II, 399); it does not occur in the *Secret History*, nor have I met it in Chinese sources. Its meaning is not clear;

perhaps it is an adjectival form in *-tu, -du*, of a Mong. dialectical *bai* for *bayan*, «rich» (the Turk. form is *baï*); or it may be another form of the more common *Baiju* (itself unexplained). Hethum (*Hist. des Crois.*, Arm., II, 189-190, 315) writes «Baydo».

Baidu, grandson of Hülägü and son of Taraqai («the Bald»), offended by Gāihatu (see «Quiacatu»), rebelled against him, and the *il Khan* was put to death on April 21, 1295. On May 6 (?) Baidu ascended the throne but, after five months, was overcome and on October 5, 1295 killed by Aryun's son Ghazan (see «Caçan»).

In YS, 107, 7 b, a king 亦憐眞八的 I-lien-chên-pa-ti is mentioned as the great-great-grandson of Hülägü, great-grandson of I-lien-chên-to-êrh-chih, Mong. Iränjin-dorji (< Tib. Rin-chen rdo-rje), grandson of T'o-t'o-mu-êrh (\*Toq-tömür = \*Toq-temür?), the name of I-lien-chên-pa-ti's father remaining unknown. There is of course something wrong in this filiation. Iränjin-dorji is another name of Gāihatu (see «Quiacatu»), who was Abaya's son and Aryun's younger brother; so he was a grandson, not a son, of Hülägü, and such is the case also of Baidu. Accordingly there is very little possibility of identifying I-lien-chên-pa-ti, the would-be great-grandson of Iränjin-dorji, with Baidu, as has been attempted by T'u Chi, 71, 5 a, and 148, 57 b, 59 b, since both are cousins of the same generation. Moreover, T'u Chi has laboured under the misconception that *pa-ti* of I-lien-chên-pa-ti could be a transcription of Baidu; wrong Western sources make him write «Kärlägä» for Baidu's father instead of Taraqai, and he believes that *i-lien-chên* is another transcription of *öljäi*. Although I do not believe in BLOCHER's «Ratnapati» (*Introduction*, 226), it is clear that the name is to be taken as a whole, perhaps a Mongol-Tibetanized form of Ratnapāla (cf. a Ratnapāla in YS, 106, 1 b) or Ratnabhadra. I do not know who he is.

Baidu is said by Bar Hebraeus and in the «Life» of Mār Yahbalaha III (cf. CHABOT, *Hist. de Mar Jabalaha III*, 106) to have been very well disposed towards the Christians, but we have no reason to believe that he was a Christian himself, as Polo says. Nevertheless his fall seems to have been due in great measure to the hatred of the Mussulmans, who resented his sympathy towards the adverse creed.

For Gāihätü's death (Baidu's accession to the throne took place a few days later), I have adopted the date of April 21, 1295, although D'OHSSON (*Oh*, IV, 113) and BROWNE (*Hist. of Pers. Lit.*, III, 39) who both, on the authority of Mussulman sources, say that it was a Thursday, give April 23, 1295; but April 23 was a Saturday. On the other hand, HAMMER (*Ha*<sup>1</sup>, I, 408), followed by Y, I, 38, and B<sup>1</sup>, 440, gives March 24, 1295, which is a Thursday, but HAMMER has confounded Jumāda I with Jumāda II. The correct equivalents are given by BARTHOLD, *EI*, s. v. «Baidū». The «Life» of Mār Yahbalaha (CHABOT, 104, 105) says that Baidu remained on the throne from April 24 to September 25, 1295; we would have lent more weight to this text if it did not include the qualifying words «more or less». But even with a possible error of a few days in the dates I have adopted, it is clear that MAQRIZI (QUATREMÈRE, *Hist. des Sultans Mamlouks*, II, II, 26) and Abū-'l-Ghāzī (*Ha*<sup>1</sup>, II, 30) are wrong when they attribute roughly eight months to Baidu's reign instead of five. Moreover, the term «seven months and ten days» given by HAMMER is a mistake even with his own dates.

## 52. BAILO (and see ACMAT, c. 85)

*bailo* R

This title appears only in conjunction with «Achmach» (read «Achmath»)’s name (see «Acmat»), in RAMUSIO’s account of Aḥmad’s murder. In *B*<sup>1</sup>, 128, the text, without any comment, is given as follows : Whenever Aḥmad heard of a beautiful girl, his ruffians would go to the girl’s father and say : «Tu hai questa tua figliuola; dalla per moglie al Bailo — poichè davano ad Acmat un nome che corrisponde al nostro *bailo* od al nostro *vicario* — e noi faremo che egli ti dia il tal governo . . . ». This is also the interpretation adopted in the English version made under BENEDETTO’s influence in *RR*, 125; later on, RAMUSIO speaks of the messenger which the conspirators sent «ad Bailo Acmat» (*B*<sup>1</sup>, 129), «to the *Bailo* Acmat» (*RR*, 127). But RAMUSIO’s original text is : «Dalla per moglie al Bailo, civè, ad Achmach, perche si diceua Bailo, come si diria Vicario . . . »; the second time, RAMUSIO’s text is that a messenger was sent «ad Achmach Bailo». It is clear that *RR* and *B*<sup>1</sup> do not translate, but correct RAMUSIO. The text, as it is, can only mean what it is made to mean in *Y*, I, 417 : « . . . Give her in marriage to the Bailo Achmath (for they called him ‘the Bailo’, or, as we should say, ‘the Viceregent’) . . . »

Of course, there is here a serious difficulty. «Bailo» was the name of the representative of the Venetian power in Constantinople and in Syria, the same who was called *podestà* during the Latin rule, and Polo uses this last name in the Prologue when speaking of the first voyage of his father and his uncle (cf. Vol. I, 74, and see «Ponte of Venese»). But, although that title was then used in Armenian as *bail*, in Syriac as *Pali* (read \**Pail*; cf. PATKANOV, *Istoriya Mongol. Magakii*, 79; BRUNS, *Chron. syriacum*, transl., 523), and, under the form باليرز *balioz*, has long survived in Osmanli as the designation of the Venetian and French representatives in Constantinople, no one can imagine, as YULE rightly says, that this European word was used at the Mongol Court of Peking. Consequently, YULE supposes that we may have here some confusion made by Polo with an Oriental title, and proposes the Arabic *wāli*, «prince», «governor», «chief magistrate». RICCI and ROSS, in their Index (*RR*, 412), have likewise said that *bailo* was «here probably a corruption of the Arabic *Wāli*», without seeing the contradiction between this note and the «amended» version adopted in their text. BENEDETTO, in his Index (*B*<sup>1</sup>, 451) prints *bailo* in italics like the words which are not Italian.

I think that YULE is partly right, and that some Oriental word underlies RAMUSIO’s «Bailo»; the way in which RAMUSIO speaks of «Achmach Bailo», not «Bailo Achmach», seems to confirm an Oriental title used in the Persian or Chinese manner. But *wāli* is out of the question; it has never been in use in the Far East, and it is unimaginable that Aḥmad’s illiterate servants, Mongols or Chinese, should have addressed Chinese parents in Arabic. Moreover, there is no necessity for charging the confusion to Polo’s account. Polo, when making the servants speak of their master, or when speaking of the letter addressed to Aḥmad by the conspirators, must have used the title as it was really known to him in the Far East. But RAMUSIO, or perhaps the men responsible for RAMUSIO’s posthumous edition, could not read the foreign title, and altered it to their native and well-known *bailo*; they did not suspect that there had never been a *bailo* in China.

As to the title from which *bailo* has been corrupted, we may perhaps make a guess. Although almost all-powerful, Aḥmad was not a *ch'êng-hsiang* (see «Cingsan») until shortly before his death, in 1282. From 1270, for a number of years, he was a 平章 *p'ing-chang* (YS, 112, 5 b-6 a), and there can be no doubt that such was the title under which he was commonly spoken of; it is the one which Rašidu-'d-Dīn (*Bl*, II, 508) gives to him, under the usual Persian form فنجان *finjān* (perhaps to be read *fūnjān*; see «Pianfu»). So, in Chinese as well as in Persian circles, *p'ing-chang* is certainly the title under which Polo knew Aḥmad in China, and it is the one we should expect to find in his narrative (for this use of the title when speaking of a *p'ing-chang*, cf. for instance *BEFEO*, IX, 677). I think Polo dictated \*Pinjan (or \*Punjan ?), which was transcribed something like \*Bicia or \*Bucia, and it is the form which, misread, and with the attraction of a familiar title, gave birth to «Bailo». Polo's original form probably began with *p*-, and we have other cases where a similar change between *p* and *b* occurs; apart from «Apusca» where we would expect «Abusca», cf. the double form «Batui» and «Patu» for Batu. In favour of \*Picia, with *i*, we may note the «Pinzanus» of a Papal letter of 1338, which is quite possible for *p'ing-chang* (cf. *Mo*, 264). For a probable Burmese alteration of *p'ing-chang*, cf. *BEFEO*, IX, 660. On *p'ing-chang*, cf. also *Y*<sup>1</sup>, III, 120; *TP*, 1916, 526-527.

Another explanation would be that Polo used *bailo* as a substitute for some Oriental title, and that «come si diria Vicario» was an explanation of *bailo* added by Rustichello or by RAMUSIO. This would have the advantage of retaining the text as it is, but the divergent translations suffice to show that the text, as it stands, is not easy to accept, and in such a case it would also be difficult to account for the second mention of «Achmach Bailo», instead of «Bailo Achmach». Moreover, *bailo* in Italian, *baile* in French, were well-known words, and I doubt that they should have required an explanation. I rather think that the explanatory words are due to Polo himself, as a sort of translation of the Oriental title he had used.

## 53. BALC

<i>balabach</i> V	<i>balach</i> VA, VL, R	<i>balch</i> Z
<i>balac</i> Fr, t, FA, FB, TA <sup>1</sup> , TA <sup>3</sup> , LT, L	<i>balc</i> F, VB, L	<i>baldach</i> P, S

This is Balkh, on which cf. «Balkh» in *EI*, by R. HARTMANN; *LS*, 420-423; *Mi*, 337. It is the only original Western mention of Balkh in the Middle Ages, if we except, a century later, «Vaeq» (corrupt for \*Valq) in CLAVIJO. «Balcia» of the Catalan Map, «Baleges» of the Medeci Map, «Balch» of Fra Mauro, and even «Waldach» of the Walsperger Map (cf. HALLBERG, 59-60) seem all to be based on Polo mss. In the translations made at Damietta in 1221 of an Arabic document bearing on the Mongol invasion of Iran, I believe that we must identify with Balkh not only the «Bolche» of the first «charta», as is said by ZARNCKE, but also the «Barach» of the second and the «Balch» of the third (cf. ZARNCKE, *Der Priester Johannes*, II, 40, 53, 57, 59). For mentions of Balkh in Chinese texts before the Mongol period, cf. CHAVANNES, *Doc. sur les Tou-kiue*, 319; also my remarks in *JA*, 1934, I, 42-43. For a Khotanese form Bāhūlaka, cf. *BSOS*, IX, 546.

On Balkh in the Mongol period, cf. *Pa*, 108-112 (to be used with caution); *Y*, I, 151-152; *Br*, II, 100-101. The Chinese transcriptions 班勒紇 Pan-lo-ho in *Shêng-wu ch'in-chêng lu*, 62*a*, and in *YS*, I, s.a. 1221, and 板勒紇 Pan-lo-ho in *YS*, 137, 1*a*, are learned transcriptions in which the second character, originally written smaller, is only meant to correct to -l the final -n of the first; the whole is a strict transcription of Balḥ, Balkh. In Chinghiz-khan's life-time, the name is transcribed 班 Pan (= Bal[kh]) by Yeh-lü Ch'u-ts'ai (*Br*, I, 23) and 班里 Pan-li in the account of Ch'ang-ch'un's journey (*Br*, I, 93). It is written 必里罕 Pi-li-han in the biography of Sübōtai (*YS*, 121, 1*b*), 巴里黑 Pa-li-hei on the Chinese map of c. 1330 and in the corresponding list of *YS*, 63, 16*b*. I doubt that the 阿刺黑 A-la-hei of the biography of Ho-ssü-mai-li (*YS*, 120, 7*a*), adduced by BRETSCHNEIDER (*Br*, II, 101), refers to Balkh. In Ming times, we find 八刺黑 Pa-la-hei in Ch'ên Ch'êng's diary of 1414 (*Hsi-yü hsing-ch'êng chi*, ed. Peiping Nat. Library, 18*a*), and the same form, but with an alternative form 八里 Pa-li, perhaps corrupt for 八里黑 Pa-li-hei, in his *Hsi-yü fan-kuo chih* (same ed., 13*a*). The transcription in the *Ming shih* (332, 12*b*) is 把力黑 Pa-li-hei.

The Nestorian priest Yazd-bōzēd, in Chinese 伊斯 I-ssü, the man at whose expense the famous Nestorian tablet of 781 was erected, was the son of a priest of Balkh, and had himself come to China from 王舍城 Wang-shê-ch'êng, the «City of the Royal Residence», which is not here Rājagrha as in Buddhist texts, but is another name of Balkh (cf. *Mo*, 43, 48). Under the Yüan dynasty, 察罕 Ch'a-han, Čayān, who translated various works from Chinese into Mongol and *vice-versa*, was born in China and had received a Mongol name, but was the son of a man from Balkh (*YS*, 137, 1-2).

It is generally said that Balkh was conquered by Chinghiz-khan in 1221, and almost destroyed in 1223. A. Waley (*Travels of an Alchemist*, 111) has adopted the dates 1220 and 1222; this is in apparent agreement with the statement of the diarist of Ch'ang-ch'un's travels, who says that at the end of September 1222, the inhabitants of Balkh had recently rebelled against the khan and had been removed. But, whatever may be the truth for the date of the «revolt» which led to Balkh's devastation, it seems impossible to doubt the accuracy of both Chinese and Mussulman sources fixing the taking of Balkh by Chinghiz-khan in the spring of 1221; cf. BARTHOLD, *Turkestan down to the Mongol invasion*, 438.

## 54. BALTASAR

*baldasar* LT, Z  
*baldisar* VB  
*baldissera* V

*balthasar* FB  
*balthazar* FA

*beltasar* F, L  
*boltasar* TA<sup>1</sup>

One of the Magi kings. On their names, cf. *Y*, I, 82-83; add H. KEHRER, *Die heiligen Drei Könige in Literatur und Kunst* (2 vols., 1908-1909), and V. SCHEIL, in *Florilegium Melchior de Vogüé*, Paris, 1909, 8vo, 552-553. The name of «Baltasar» is taken from the Septuagint, and represents the Assyrian Bêl šar ušur, «Bêl! Save the King».

## 55. BANGALA

<i>bagaliar</i> V	<i>bangalan</i> F	<i>bangilla</i> LT
<i>balgana, bulgana, malgana</i> G	<i>bangalla</i> V, VA, VB	<i>bangula</i> FAt
<i>banchale, banghala</i> TA <sup>1</sup>	<i>anganla</i> Z	<i>ghanghala</i> TA <sup>1</sup> , TA <sup>3</sup>
<i>bangala</i> F, Fr, t, FA, FB, L, LT, P, V, VA, Z; R	<i>banghalla, bughala, chan- ghala</i> TA <sup>3</sup>	<i>pangala</i> VA <i>sangala</i> FB

Polo's account of Bengal is what he had heard in Yün-nan. In it there are many errors and contradictions, for instance when he says that the king of «Mien» (=Burma) was also king of «Bangala» and that Qubilai took both kingdoms away from him, but later on he devotes a special paragraph to «Bangala», without any further mention of «Mien», and says that it had not yet been conquered by Qubilai, although Qubilai had already had troops assembled for that purpose. CHARIGNON's attempt (*Ch*, II, 260) to derive all mentions of «Bangala» in Polo from «Pagan» or from «Mangala» (on account of the Marigala-caitya at Pagan!) are valueless, and ought not to have been taken into account in *B*<sup>1</sup>, 438. YULE (*Y*, II, 99, 128) has thought that Polo mixed up the data relating to Pegu (Burm. Pagóh), which he had heard in Yün-nan, with the accounts of Bengal given by men who had gone there by sea. For Prof. BENEDETTO (*B*<sup>1</sup>, 438), the name «Bangala» is really that of Bengal, but Polo uses it only as a designation of an Indo-Chinese country which is probably Pegu.

I cannot agree with most of YULE's and BENEDETTO's views on «Bangala». The name of course is certainly that of Bengal. We could suppose that in the case of «Mien and Bangala», «Bangala» has been altered from «Baigo», or some such name, under the influence of the «province» of «Bangala», Bengal, which is described further on. We would thus escape the contradictions mentioned at the beginning of the present note. But all mss. agree, though there are no parallel passages in Z, so that provisionally at least, we can but keep the text as it stands. It looks as if the long account of the battle with the «king of Mien and Bangala» was a separate narrative somewhat clumsily inserted after the original dictation of the rest of these chapters.

As to the «province» of «Bangala», I have no doubt, despite BENEDETTO's objections, that Polo really means Bengal, and not Pegu. YULE has already noted that POLO's indications in that chapter, scanty as they are, apply to Bengal, and not to Pegu (cf. also DAMES, *Barbosa*, II, 147). Moreover, there has always been, from before the Christian era, a land route leading direct from Bengal to Yün-nan via Burma. Polo's contemporary, Rašidu-'d-Din, describes it (cf. *infra*), and it is only too natural that Polo should have heard of it in Yün-nan. But, hearing of «Bangala» in Yün-nan, and although knowing that it was towards India, he still attached it to Indo-China, the size and shape of which were to him a mystery, and so developed, beyond the southern borders of China, the line of his three «provinces» of «Bangala», «Caugigu» and «Amu», where the errors are not confined to Bengal. In truth, it is precisely the nature of

Polo's account which makes me conclude, notwithstanding YULE, that Polo got his information on Bengal not from seamen, but in Yün-nan from land-travellers.

The name of «Bengal» is a fairly late development of the old native name Vaṅga. Vaṅgāla (Vaṅgāla) occurs already in texts of the 11th and 12th cents., including the Tanjore inscription of 1030. But the two names Vaṅga and Vaṅgāla are not equivalent; the Vaṅgāla seem to have invaded and occupied part of Vaṅga or Bengal (cf. R. Ch. BANERJI, in *Indian Culture*, 1936, 755-760). It does not seem that we are justified in identifying Bengal with the 朋茄羅 P'êng-ch'ieh-lo of Chao Ju-kua writing in 1225; this latter is more likely to be the same as the 朋家羅 P'êng-chia-lo of the *Tao-i chih-liao* of 1349-1350, Bacanor (?) according to ROCKHILL, but perhaps Mangalor (cf. *HR*, 97, 102; *TP*, 1915, 466). So Polo is, up till now, the first non-Indian source to name «Bengal»; and from him, the name has passed into mediaeval cartography (cf. HALLBERG, 66-67). He is closely followed by Rašidu-'d-Dīn who describes an itinerary from the Coast of Coromandel to Yün-nan via بنگال Bangālā (cf. *Y*<sup>1</sup>, III, 131-132; ELLIOT, *History of India*, I, 72). Towards the middle of the 14th cent., Ibn Baṭṭūṭah speaks of بنجاله Banjāla (arabized form). In 1349-1350, the *Tao-i chih-liao* devotes a paragraph to 朋加刺 P'êng-chia-la, which is certainly Bengal (*TP*, 1915, 435-436). There are important mentions or notices of Bengal in Chinese texts of the first half of the 15th cent. (cf. *TP*, 1915, 436-444; 1933, 313-322, 422-430; DUYVENDAK, *Ma Huan re-examined*, 62-64). Barbosa has a chapter on Bengal (cf. DAMES, *Barbosa*, II, 135-148). For other mentions of Bengal, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 85; *Fe*, 693. From the time of the Moors in Spain, *al-bangala* was the name of a fine muslin, and that form has survived in Spanish; for *bengala* meaning a bamboo stick in Portuguese, and Port. *bengala* and Eng. *bengal* as names of textiles, cf. *Hobson-Jobson*<sup>2</sup>, 86, and DALCADO, *Glossário Luso-Asiático*, I, 116-117.

The Mongols did not conquer Bengal, and in any case certainly not from China, but they seem to have planned to do something in that way. In 1295, the Mongol governor of Yün-nan asked to have garrisons and postal relays established among the Chin-ch'ih (see «Çardandan») to keep them quiet, and also because there were two sorts of barbarians in 敢麻魯 Kan-ma-lu who had not yet submitted. Kan-ma-lu (then Kam-ma-lu) can only be a transcription of \*Kamarup = Kamarūpa, Assam (cf. BEFEO, IV, 177-182; *Fe*, Index, 709; S. LÉVI, in *JA*, 1923, II, 46-49), and it is quite possible that, in Qubilai's time, Polo should have heard of a scheme for the conquest of Bengal.

YULE (*Y*, II, 115) has already said that the would-be invasion of India in 1244 by the Mongols via China or Tibet, which is quoted from the *Āin-i Akbarī* by PAUTHIER (*Pa*, 81) to support part of Polo's account of «Bangala», is simply the result of a corrupt reading in the *Ṭabaqāt-i Nāsirī*, the indirect source of the *Āin-i Akbarī*. It may be worth while here to repeat YULE's perfectly correct statement, since the corrupt text given by the *Āin-i Akbarī* has been translated again without comment by JARRETT, II, 304 (cf. also RAVERTY, *The Ṭabaqāt-i Nāsirī*, I, 665-666).

In the (unpublished) section devoted to the history of China, Rašidu-'d-Dīn says that Śākya-muni's father came from the country of Kashmir and ?; the second name, miswritten in many forms, seems to represent «Bangal», or perhaps «Nīpal».

## 56. BARAC

balac LT

barac F, FA, FB, L,  
TA<sup>1</sup>, TA<sup>3</sup>, Zbarach L, P, P<sup>5</sup>, V,  
VA (?), VL, Z; Rbaracli V  
barat FA

Turkish Baraq, which means a long-haired dog of somewhat fabulous character; see BROCKELMANN, *Kāšyari*, 31; RADLOV, *Dictionary*, iv, 1477; and my note in *TP*, 1930, 339-340. Baraq is the prince who is generally called Bōrāq, Bōrrāq, or Burāq, in *Ha*<sup>1</sup>, II, 407; *Ha*<sup>2</sup>, 129; *Oh*, II, 359, etc.; Stanley LANE-POOLE, *The Mohammedan Dynasties*<sup>1</sup>, 342; *Bl*, II, 435; BARTHOLD, *12 Vorlesungen*, 251; GROUSSET, *Hist. de l'Extrême-Orient*, 594; *Pe*, 155; and lastly in *B*<sup>1</sup>, 438. BLOCHET, who writes «Borak» in *Bl*, II, 435, and in his *Introduction*, 223, 233, has nevertheless added in the Index of the *Introduction* «better Barakh»; I have already shown (*TP*, 1930, 339-340) that his explanations of that name «Barak» or «Barakh» are fantastic. The fact remains that باراق must be read Baraq, and that Bōrrāq is one of these later «arabizations» which have for instance given birth to a Šäibān and a Šäibānid dynasty, when the original Mongol form was Sibān > Šibān (see also «Berca»). The correct form given by Polo is corroborated by *YS*, 107, 5 a, who mentions a prince 八 朶 朶 Pa-la; this is not a transcription of Bala, as is said in *Bl*, II, 155, but of Baraq. Hethum the historian also gives correctly «Barach» (cf. *Hist. des Crois.*, Arm., II, 163, 296, 891). The Georgian chronicle writes «Barakha» (BROSSET, *Hist. de la Géorgie*, I, 1, 575, 580 sq.).

The *YS*, 107, 5 a, is wrong in making him a son of Qara-Hülägü, whereas he was his nephew; Baraq was the son of Yäsün-toa, himself a grandson of Čayatai (cf. *Bl*, III, 167-168; T'u Chi, 148, 39 b, is absolutely lost as to the identification of Baraq's father).

As Baraq is said in the text to have ruled in Bukhara when the elder Polos stayed there, it would be of some interest to ascertain the dates of his reign over the *ulus* of Čayatai. YULE (*Y*, I, 10) says 1264-1270; Stanley LANE-POOLE<sup>1</sup> (p. 242; followed in *B*<sup>1</sup>, 438), 1266-1270. But BARTHOLD, generally safer, says that Alyui (more correctly Aluyu; see under «Cibai») died at the end of 1265 or the beginning of 1266, and was succeeded by Baraq who reigned until 1271 (see BARTHOLD's note in MINAEV's *Marco Polo*, p. 6, and GRUM-GRŽIMALO, *Zapadnaya Mongoliya*, II, 481). Later on, in *EI*, s. v. «Burāq-khān», BARTHOLD brought forward fresh and sometimes contradictory information; the probabilities are that Baraq did not come into power before the spring or summer of 1266, and died about August 1271.

Baraq is named a second time when Polo relates the story of a war with Aryun, who very soon afterwards heard of the death of his father Abaya; this time, Polo says that Baraq was Qaidu's brother. Qaidu was a grandson of Ögödäi, the Baraq of 1266-1271 was a great-grandson of Ögödäi's brother Čayatai; thus these two were not brothers, but cousins, and not of the same generation. It may be supposed that Polo uses «brothers» in the loose Oriental way, in which it means cousins as well, the more so as he uses «flesh-brothers» when he wishes to indicate real



brothers. But there is another difficulty. Baraq died at the latest in 1271, and Abaya in 1282, so there must have been at least eleven years between the two events.

According to Rašidu-'d-Din (*Bl*, II, 9, 435), Baraq had been brought up under Qubilai's care, and Qubilai sent him to take over the *ulus* of Čayatai in order to fight against Qaidu; but after a battle which was won by Qaidu, they came to terms and joined together in opposing Qubilai and Abaya (cf. also T'u Chi, 74, 6 b-7 a).

WANG Hui-tsu (31, 2) finds nine Pa-la in YS, but it may be that not all of the nine names have to be read as Baraq. Anyhow, it is our Baraq who appears in YS, 5, 5 b, when, in the second moon of 1263, an edict prescribes the purchase of 129 horses for the dismounted soldiers of prince Pa-la (Baraq). Also, in the first moon of 1275 (YS, 7 a), the order is given to recover the 34 gold and silver tablets of the princes Qaidu and Pa-la (Baraq); Baraq had then been dead for four years. In the fifth moon of 1268 (YS, 6, 6 b), 60,000 pieces of silk were granted to the princes Hoqu (see «Badascian») and 八 刺 合 Pa-la-ha; in spite of T'u Chi, the situation which obtained at that time between Qubilai and Baraq leaves some doubt as to whether we should also restore Pa-la-ha as Baraq.

If it be true that the elder Polos stayed in Bukhara from 1262 to 1265, which is still for instance BARTHOLD's opinion (*12 Vorlesungen*, 193), and if Baraq ascended the throne only at the end of 1265 or the beginning of 1266, Polo's text is hardly accurate, and the inference has already been drawn that perhaps the elder Polos found Baraq at Bukhara only on their homeward journey.

## 57. BARGU

*bachu* V  
*baigu* F, FAt  
*banchu* TA<sup>1</sup>

*banchum* TA<sup>3</sup>  
*bangu* Fr, t, LT  
*barga* L, VA, VL

*bargu* FA, FB, L, L<sup>1</sup>, P; R  
*bergu* VB

B<sup>1</sup>, 438, says that the name and the identification are doubtful, and even alludes to the absurd hypothesis of *Ch*, I, 223-224, according to which the name of «Bargu» might be identical with that of Lake Baikal. But the name has been definitely ascertained, and the general localization too; YULE (*Y*, I, 270-271), availing himself of PALLADIUS's *Elucidations*, has already made a clear statement of the case, which can nevertheless be supplemented and corrected in some details.

Fra Mauro's would-be «Baregu» (HALLBERG, 68) is a mistake of ZURLA (*Zu*, 36); actually, the map gives the correct form «Bargu».

The name of «Bargu» may be the same as that of the 拔野古 Pa-yeh-ku of Chinese texts

of the T'ang period, the Bayırqu of Orkhon Turkish texts (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 352; RADLOV, *Die alttürk. Inschr.*, NF, 179; THOMSEN, in *ZDMG*, 1924, 171).

In the Mongol period, the *Secret History* of 1240 (§ 239) names the « Barqun » (= Baryun) among the tribes which submitted to Jöçi, after the Oirat and the Buriat, and before the Ursut (Urasut), the Qabqanas, the Qangqas and the Tubas. Elsewhere, there are several mentions of a region called Kōl-Baryujin-tögüm, or Baryujin-tögüm, or simply Baryujin (§§ 8, 109, 157, 177, 244); in the legendary account of Mongol origins, the name of a man Baryudai-märgän and of his daughter Baryujin-yoa, ruling at Kōl-Baryujin-tögüm (§ 8), are clearly ethnical names derived from Baryu or Baryun. A legendary predecessor of Chinghiz-khan had lived as a son-in-law in a family of the Pa-la-hu (Baryu or Baryut; *YS*, I, 1 b).

Baryujin-tögüm means « Depression of Baryujin ». The name often occurs in Rašidu-'d-Dīn (cf. *Ber*, III, 188; the « Bourkoudjin Toukroum » of QUATREMÈRE, *Hist. des Mongols*, 437, and « Barguchin Tugrum » of *Y*, I, 270, are misreadings). In the passage corresponding to § 157 of the *Secret History*, the parallel texts of *Shêng-wu ch'in-chêng lu* (ed. WANG Kuo-wei, 17 a) and of *YS*, I, 3 a, say that Toqto'a the Märkit fled to the low passage (隘 *yai*) of Pa-êrh-hu-chin (Baryujin). From the *Secret History*, § 109, we see that Baryujin was reached from Mongolia by following down the course of the Selenga; this leads east of the Baikal to the region where there is now the river « Bargugin », the name of which has also been given to three towns along its course and to the mountains between it and Lake Baikal. There can be no doubt that this is the Baryujin of Mongol times, and the Baryujin-tögüm may be either its southern entrance or the whole valley itself. *Tögüm*, which KOVALEVSKIĪ reads *dügüm*, is explained to-day in Mongolian as a stretch of fertile land between mountains.

Baryujin is an adjectival, and perhaps sometimes a feminine form regularly derived from Baryu or Baryun. It is well known that the final *-n* is very unstable in Mongolian, but a plural from Baryu would be regularly \*Baryus, while a plural from Baryun is Baryut, and it is Baryut (برغوت or بروت) which is listed as a tribal name in Rašidu-'d-Dīn's description of Mongol tribes (cf. *Bl*, Intr., 178; *Ber*, I, 85-87; III, 188, 193 [where « Baryujin » is a wrong reading, just as « Burghut » must be corrected to « Barghut » in *Mi*, 284]).

PALLADIUS has said (*Y*, I, 271) that the name of the Baryut disappears from Chinese texts after Chinghiz-khan's time, to reappear in the middle of the 16th cent.; at the end of the 17th, the Baryut are found on « the western slopes of the interior Hing'an, as well as between Lake Kulon and River Khalkha »; that is where we still find them nowadays.

But this is probably the result of a migration posterior not only to Chinghiz-khan's time, but to the end of the 13th cent., for we find, after Chinghiz-khan, mentions of the Baryu which have escaped Palladius and which tend to show that they continued then to live in a more northern locality and east of the Baikal.

I am not certain that the Pa-li-hun mentioned in *YS*, 15, 1 a, under the year 1288, represents Barqun, but there is another case which is not doubtful, and which is indirectly connected with what Polo says of the plain of Baryu. In *YS*, 10, 10 b, there is the following text : « The 16th year *chih-yüan*, . . . the 12th moon, . . . on the day *ting-yu* (January 27, 1280), the 八里灰 Pa-li-hui offered gerfalcons (*hai-ch'ing*). The Mussulmans and others, wherever they pass, do not

eat the mutton which is offered to them for their sustenance unless they kill the sheep themselves. The population suffers by it. The Emperor said 'These [Mussulmans and others] are our slaves; in their drink and food, how dare they not follow [the customs of] our dynasty?' An edict prohibited [the Mussulman mode of slaughtering], and the instructions of the Imperial edict were issued to the foreign rulers within and beyond the seas.» We seem to have to do here with two facts registered under the same date, but apparently unconnected. As a matter of fact, the complete text of the edict, translated in «spoken» Chinese from Mongolian, is preserved in *Yüan tien-chang* (57, 16 a-17 a), and shows that the gerfalcons were brought by Mussulmans from Pa-li-hui «straight to the North», and it is on account of the difficulties raised by these Mussulmans about their food that the rules already laid down by Chinghiz-khan and his successors against the Mussulman and Jewish custom of cutting the throat of animals were reinforced by Qubilai's edict (on previous cases of the same sort, cf. *Oh*, II, 94, 100; DEFRÉMERY, *Hist. des Khans mongols*, 53-54). The Pa-êrh-wa-na (= Parvana, Parwānah) whose death, at the hand of Abaya, is alluded to in the Chinese edict, is the one who was executed on July 23, 1278 (cf. *Ha*<sup>1</sup>, I, 299), and an account of whose death is given by Hethum (*Hist. des Crois.*, Arm., II, 179-180, 308-309; cf. also BROSSET, *Hist. de la Géorgie*, I, 587-588; *Y*, I, 312).

This edict of January 27, 1280, must have greatly moved the Mussulmans, since it came to the knowledge of Rašidu-'d-Dīn (*Bl*, II, 521, and App. 52; QUATREMÈRE, *Hist. des Sult. Mamlouks*, I, 93). According to the Persian historian, Mussulman traders had brought to the Emperor, from the land of the قُرَى Qurī, the بَرَقُ Barqu (= Baryu), and the Kirghiz, one or several gerfalcons (*sonqor*) with white feet and red beak and a white eagle (*'uqāb*). Qubilai, to show the Mussulmans favour, gave them portions from the Imperial table, but they refused to eat them; this was the cause of the edict, which was due also, according to Rašid, to the hostility towards the Mussulmans of 'Isa *kālāmāci*, 'Isa the Interpreter, that is to say the famous Christian 愛薛 Aihsieh of Chinese texts.

There can be no doubt that these Baryu, the Pa-li-hui of the Chinese edict, named along with the Qurī and the Kirghiz, did not live near the Khingān mountains, but just east of the Baikal, at Baryujin, in the region of the modern Barguzin river. We can thus safely conclude that Polo's «plain of Bargu» is also the Bargujin. It may even be that we have some sort of echo of the edict of 1280 or of the sensation it created among Polo's Persian friends when he adds that Qubilai, when he wants gerfalcons, sends to the «plain of Bargu». And perhaps, on account of the incidents of 1279-1280, the «plain of Bargu» took in Polo's mind an undue importance and extension.

As a designation of the gerfalcon, Rašid uses the name *sonqor* which, with minor changes (*šunqar*, etc.), is the common name of that noble bird in Turkish, Mongolian and Persian (cf. QUATREMÈRE's detailed note in *Hist. des Sult. Mamlouks*, II, 90-96). The Chinese name is 海青 *hai-ch'ing* or 海東青 *hai-tung-ch'ing*, «Grey-blue from the Sea» or «Grey-blue from East of the Sea» (cf. the «blue gerfalcon» in QUATREMÈRE, *Hist. des Sult. Mamlouks*, I, 94), and this is generally explained as meaning that they come from Liao-tung and Corea, «East of the Sea» (the explanation of *Bl*, II, 521, «un oiseau de proie qui se nourrit des poissons qu'il pêche», is wrong). This is a fact and we hear of gerfalcons from Nu-êrh-kan in Upper Manchuria (*YS*, 59,

3 b), and of the 24 postal stages established for the purpose of bringing gerfalcons from Liao-yang (YS, 18, 7 a; 22, 9 a). In the middle of the 12th cent., the 五總志 *Wu tsung chih* (ed. *Chih-pu-tsu-chai ts'ung-shu*, 21 b) says that in the woods on the sea-cliffs of Têng-chou (Shantung) there are hawks which come in one flight from Kao-li (Corea) over the sea, and are called « Grey-blues from East of the Sea ». But it may be that, at least in the Mongol period, *hai-ch'ing* and *hai-tung-ch'ing* were also understood to mean « Grey-blue from East of the [Baikal] Sea », that is to say from Baryu; in any case, the Mongols got them from far in the north, and Polo has only a very hazy notion of the « islands » of the northern « Ocean » where they were caught.

Polo states that the gerfalcons carried to Tartar dominions by Christians did not go to the Emperor, but only to Aryun and the other princes of the Levant. YULE (*Y*, I, 273) has only quoted a case of three white gerfalcons sent by James II of Aragon to the Sultan of Egypt. But there is a text to corroborate more expressly Polo's remark. In a letter written in 1276 from Viterbo to King Edward I by John and James Vassal, messengers of Abaya, the two messengers warn the king against some Catalans and a Nestorian who had been sent by Abaya with money and with a gold tablet ornamented with a gerfalcons (the *hai-ch'ing p'ai-tzū* of Chinese texts) to go and buy gerfalcons for Abaya in Norway (« Noroaigne »; see « Noroech »), and who were to go to England after they had tried in Sicily to pose as official envoys (cf. Ch. KOHLER and Ch. V. LANGLOIS, *Lettres inédites concernant les croisades*, in *Bibl. Ec. des Chartes*, LII [1891], 56-57). This is a striking illustration of the lengths to which Oriental princes would go at that time to procure gerfalcons.

## 58. BARIS

### *baris* I

The name occurs only in a passage of Jacopo d'Acqui's *Imago mundi*, the Polian origin of which has not been established beyond doubt (cf. *B*, cxcviii, 15<sup>e</sup>).

This « Mount Baris » or « Mount Olympus », as is clear from the text itself, cannot be Ararat, which Polo without naming it describes both before and after when he speaks of Noah's Ark. Moreover the name of the biblical Ararat was well known to mediaeval travellers : Simon de Saint-Quentin speaks of Mount « Arath » (< \*Arath = \*Ararath; in Vincent de Beauvais, xxxi, 97); it is also mentioned by Rubrouck (*Wy*, 319) and by Marignolli (*Wy*, 541); « Harach », in the French version of Odoric, must also stand for \*Hararath (*Y*<sup>1</sup>, II, 102; CORDIER, *Odoric de Pordenone*, 6; « monte Gordico » in the corresponding passage of RAMUSIO A may either be corrupt for the same name or mean \*« Curdic », from the name of the Kurds). In Hethum, we find not only the name of Mount Ararat, but also the information that on top of the snowy mountain there is a « grant chose noire » which is said to be Noah's Ark (*Hist. des Crois.*, Arm., II, 128, 268). Jacopo

d'Acqui's quotation also gives this detail, and if the whole is not a genuine Polo passage, Jacopo d'Acqui may well be indebted to Hethum for at least something of it.

The name of «Mount Olympus» seems to be used here merely figuratively and that of «Mount Baris» is the only one we need to examine. The best solution I can think of is to see in it a transcription of the name of the Elburz or Elbruz, <sup>أَلْبُرْز</sup> Alburz, in which *al-* has been dropped, perhaps because it was mistaken for the article (Arabic or Italian). At any rate, I think we must connect «Baris» with another name which occurs in documents of the early 14th cent., but has not been correctly read or explained.

By a bull of April 1, 1318, John XXII fixed the extent of the respective jurisdictions of the Franciscan archbishop of Khan-baliq (Peking) and of the Dominican archbishop of Sultanieh (Sultāniyah). The Dominican archbishopric wielded authority over the territory of the *ilkhans* of Persia and of the house of Čayatai or «Medium Imperium», the Franciscan archbishopric over Cathay and the Golden Horde, including the Crimea. In the bull (as published by EUBEL in 1897 and 1898), the line of separation between the two jurisdictions was drawn at «Mons Barrarius» (*a Monte Barrario*; and *a dicto Monte Barrario*). Jourdain Cathala says of Armenia that it extends in longitude from «Sebast» (= Sivas) to «Orogan» (read «Mogan»), representing over forty days' journey, and in latitude from Mount Barcarius (*a monte Barcario*) to Thaurisium (Tabriz), representing a good twenty-three days' journey (CORDIER, *Les merveilles de l'Asie*, 53, 110); Cordier however does not comment on what his translation calls «Mont Barcar». GOLUBOVICH (*Bibl. bio-bibl.* III, 200, 204; also 198, 199) gave the complete text of the bull of 1318, but wrote «Monteharrario», adding in a note «Not Monteharrario. Monsharrarius certainly is Mount Ararat in Armenia.» SORANZO (*Il papato*, 515-517) objected that the region of Mount Ararat was entirely within the jurisdiction of the archbishop of Sultanieh and that the limit between the two archbishoprics must be looked for more to the north, towards the Caucasus. I have no doubt that GOLUBOVICH was mistaken as to the name and as to the identification. Although he knew Jourdain's form from HALLBERG (cf. *Bibl. bio-bibl.*, II, 543), he took it to be another case of confusion between *b-* and *h-*. But there is not the slightest ground for such an assumption. SORANZO's objections to the Ararat theory are perfectly valid, and the name of the mountain, which begins with *b-* in both sources, can only be \*Barcar or \*Barrar. On the other hand, the question is merely one of a clerical error between these two forms, since Jourdain, writing a few years after the bull of 1318, certainly had the bull in mind when he gave the northern limit of Armenia.

Phonetically, the most tempting solution would be to adopt Jourdain's «Barcarius» and to see in «Mount \*Barcar» Mount Barkhar, from which the Kur takes its rise (the Παρυδάριος of ancient geography according to *Hist. des Crois.*, Arm., I, 136). But, if Mount Barkhar may be said to be the northern limit of Armenia towards Georgia, it was certainly not the northern limit of Armenia in general, nor the limit between the territories of the *ilkhans* and of the Golden Horde. That limit, which left the whole basin of the Kur and Araxes to the *ilkhans*, and consequently to the archbishopric of Sultanieh, was at Derbend, on the Caspian Sea (see «Gate of Iron»); this is expressly stated by Hethum (*Hist. des Crois.*, Arm., II, 128, 156, 268, 291) and is confirmed by the whole history of the rivalry between the Mongols of Persia and their cousins of the Golden Horde. Derbend lies north of the Caucasus, but in a very strong position, where a

detached ridge of the main chain plunges into the sea; as Hethum says, the Gate of Iron (*i. e.* Derbend) « touche la grant montaigne de Coquas ». Mount Elburz, properly so called, stands in the western half of the Caucasus; it is Hethum's « Albors », mentioned by the latter as the eastern (= north-eastern) limit of Georgia (*Hist. des Crois., Arm., II, 129, 268*). Hethum's mss. give other readings, « Albers » and « Alberz » in French, « Alboret », « Alboris » and « Albzor » in Latin (the last form accounts, I think, for Maundeville's « Abzor »). But the name of Elburz was often extended to the whole chain, right on to Derbend in the east (cf. QUATREMÈRE, *Hist. des Mongols, 390; Hist. des Crois., Arm., II, 129*). Speaking strictly from a historical point of view, it is the Caucasus, or the Elburz used in its broad sense, which we should expect to find in the bull of 1318 and in Jourdain Cathala. On the other hand, granting that *al-*, mistaken for the article, may have been dropped, Jacopo d'Acqui's « Baris » comes very near the reading « Alboris » in one ms. of Hethum. Yet I am convinced that there is some connection between this « Baris » and the forms « Barrarius » and « Barcarius », and I do not see how the article could also have been dropped in the bull of 1318, or how Albors or a form like Albors could have developed into a longer form in *-carius* or *-rarius*. The evidence at present available seems inadequate to reach a formal conclusion.

## 59. BARLAM

*barlam* VB (in *B*)

*barllam* VB<sup>a</sup>

This name, like those of « Avenir » and of « Josafat », occurs only in VB.

Barlam, or rather Barlaam, is the form of the name in the Greek text from which it has spread everywhere in the West, and it has never been doubted that the author of the Greek text adopted it under the influence of the Christian name βαρλαάμ, Syr. Barlāhā (KUHN, in *Abh. d. Ph.-Ph. Kl. d. k. Bayer. Ak. d. W.*, xx [1894], 19). The Georgian version gives Balavar (MARR, in *ZVOIRAO*, III, 259), and the Arabic and Persian texts write بلهر B.lüh.r (vowels not written or arbitrarily written). Moreover, the 10th cent. *Kitāb al-Fihrist* mentions in its bibliographical lists a *Book of B.lüh.r and Būdās.p* (see « Josafat »).

The only plausible explanation of B.lüh.r is to see in it a transcription of *purohita*, « royal chaplain » (cf. SACHAU, *Alberuni's India*, I, xxxiii; VON LE COQ, *Ein christl. und ein manich. Fragment*, in *SPAW*, 1909, 1205). B.lüh.r (to be read as \*Bulūhir > \*Balūhar and \*Bilūhar) is to *purohita* the same as \*Balar > Bilār is to Bharata (I am not convinced that the forms with *-r* like Syriac Bilār, Greek παλάριος, are due to a confusion between *-d-* and *-r-* in the Syriac writing, as is said in BICKELL, *Kalilag und Damnag* [1876], 41-411). The alternation *r ~ l* must have taken place in North-Western India or Eastern Iran, where it is of common occurrence; cf. Skr. Karāṭaka > Syr. Kalilag. The passage from final *-t* to *-r* is known even for purely Iranian

names; cf. for instance Avest. Speñtōdāta > Pahl. Spendiāt > Pers. Ispendiār, Arab. Isfendiār (JUSTI, *Iranisches Namenbuch*, 308).

For the general problem of Barlaam and Josaphat, see «Iosafat».

## 60. BARSAMO (< \*BARSOMA)

*barsamo* R

RAMUSIO is our only source here. No identification is proposed in *RR*, 34, or in *B*<sup>1</sup>, 438, although, as YULE has shown (*Y*, I, 77), the saint must be S. Barçauima. Polo may have dictated \*«Barsoma», and the name may have been «italianized» afterwards, perhaps by RAMUSIO. I must remark, nevertheless, that Vincent de Beauvais, as quoted by YULE, writes the name of the saint as «Brassamus» (Mentellin, in 1473, prints «Braisamus» [xxxI, 142]; I have not checked the name in the different mss. and other editions of Vincent).

I also agree with YULE when he believes that the said monastery is the famous monastery of S. Barçauima, south-east of Malatia, not far from Gargar, the present Borsün Qal'äsi. It was at times the residence of the Jacobite patriarch in the 12th and 13th cents. The name of the monastery was given in honour of the famous monophysite archimandrite, † 458. His cult was so profitable to the monastery in the 13th cent. that the Armenians then built another monastery of S. Barçauima (cf. *EI*, s. v. «Rüm Qal'a»; *Hist. des Crois.*, Arm., I, 163, 342-343; Bar Hebraeus, *Chron. Syr.*, transl. BRUNS, 343-344, 523; *Chron. ecclésiast.*, ed. LAMY, II, 691-798, *passim*).

While phonetic analogy and religious history seem to preclude any identification of the monastery of «Barsamo» other than with the monastery of S. Barçauima, south-east of Malatia, YULE does not refer to the difficulty raised by the geographical data of the text. Polo's account of Armenia is followed by that of Georgia, then by digressions on Mosul and Bagdad, then by a chapter on Tabriz. It is at the end of the chapter on Tabriz, and when we have already been told that we are about to enter Persia, that RAMUSIO inserts his chapter on the monastery of «Barsamo», «on the borders of Tauris». The monastery of «Barsamo» would thus appear to be to the south of Tabriz, or at least in Tabriz province. On the other hand, the monastery of Barçauima is to the west of the Euphrates, and about twice as far from Tabriz as from the gulf of Alexandretta. The only explanation I can suggest is that «on the borders of Tauris» is the result of RAMUSIO's editing, and that in RAMUSIO, our only source, the chapter is out of place. This displacement is not necessarily connected with the difference in the order of chapters which is discussed in vol. I, 104.

The order of the Carmelites had been founded in the Holy Land in the middle of the 12th cent. I am not sufficiently informed of their particular garb in the 13th cent. to draw from it any conclusion as to that of the monks of S. Barçauima.

As for the Brothers of the Holy Spirit, their order had been founded at Montpellier c. 1160, and they attended to the poor and the sick; their statutes were confirmed in 1198 by Innocent III.

## 61. BARSCOL

baiscol TA<sup>1</sup>  
barscol F; R  
barsoel P  
bascholulan V

bascol FB  
bascol VL  
bastol LT  
bayschol TA<sup>3</sup>

braschel VA  
brascol FA  
uarscon VB

B<sup>1</sup>, 438, has supposed that «Barscol» and «Bargu» might be one and the same name, diversely altered by copyists; but «Bargu» (*q. v.*) is certain and well known, and «Barscol», given in this form by F and R, is too strict a transcription of the Turkish Bars-köl, «Tiger Lake», or of the Mong. Bars-γol, «Tiger River», to be doubted. The identification, however, is not clear.

PALLADIUS (*Kommentaril*, 33) has said that a geographical name «Barhül», close to «Barscol», occurred in the Mongol text of the *Secret History*, but was unidentified, and that he did not know exactly where to locate it: there the modern Baryu (Baryut) are now, or towards the Barguzin. There is some mistake. I know the *Secret History* well, and there is no such name, geographical or other, in it. PALLADIUS's hesitation shows that he thought of the Baryu, and he must have misread the name «Barqun» (Baryun), which represents Baryu and not Bars-köl. The other solutions put forward by PALLADIUS, which have been quoted in *Y*, I, 345 («P'u-yü-lu» and «Pu-lo-ho»), are phonetically impossible, not to speak of other considerations (Polo, for instance, never transcribes the title *lu* of a district, etc.).

Parker (in *Y*, I, 345) has said that «Barscol may be Pa-la-ssü or Bars Koto [in Tsetsen]». I do not know if Pa-la-ssü comes from the Chinese translation of «Sanang Setsen»; but it is true that, according to the Mongol chronicler, the last Mongol Emperor, Toyon-tämür, when expelled from China in 1368, retired to the Kerulen, on the banks of which he founded a town Bars-ḥota (Bars-ḥoto), «Tiger City»; he died in 1370 (SCHMIDT, *Gesch. der Ost-Mongolen*, 139). SCHMIDT adds in a note (p. 403) that Bars-ḥoto still appears on the maps of the Jesuits and is without any doubt the 應昌府 Ying-ch'ang-fu of the Chinese.

Ying-ch'ang-fu is not mentioned in the geographical section of *YS*, where there is only the bare name, Ying-ch'ang-lu, in a list of districts about which the compilers say all information is lacking (*YS*, 58, 5 *a*). But one of the biographies (*YS*, 118, 3 *a*) says that the request to the Emperor for the foundation of Ying-ch'ang-fu was presented in 1270, and that the name was changed to Ying-ch'ang-lu in 1285 (cf. POPOV, *Mên-gu-yu-mu-czi*, 29, 257, 278 [where «1286» is wrong for «1285»]; POZDNEEV, *Mongoliya i Mongoly*, II, 334). According to later Chinese texts, Toyon-tämür actually settled in 1368 at Ying-ch'ang-lu, on the bank of the Pu-yü-êrh-hai (POPOV, *ibid.*, 278). An inscription written by Ch'êng Chü-fu in 1311, the text of which is preserved in his *Hsüeh-lou chi*, 5, 8-9, commemorates the completion of the temple Pao-ên-ssü begun at Ying-ch'ang-lu in 1309, and, in the course of the narrative, relates the past history of the city, founded in 1271 (it may be that the actual foundation took place in the year following the request). The site is well known. POZDNEEV, who describes it, has reproduced an inscription of 1325 found on the spot, on which the name of Ying-ch'ang-lu still appears. The tablet



lies at the south-western corner of the «Dalai-nür» or Pu-yü-êrh-hai, as POZDNEEV expressly says (*Mongoliya i Mongoly*, II, 330, 332). This is the 答兒海子 Ta-êrh hai-tzü («\*Dal Lake»; not «Toro-hai-tzü» as in POZDNEEV) of YS, 118, 3 a, the Dalai-nür situated about 120 kilometres north-north-east of Dolon-nür or Lama-miao, and not the Buir-nür or the Khulun which is also called Dalai-nür on our maps. Cf. also YANAI's map.

But this is not the Bars-ḥoto of the Jesuit maps. Bars-ḥoto is the place which is wrongly designated as «Para-Hotun» in D'ANVILLE's *Atlas, Tartarie Chinoise*, feuille VII, located on the northern bank of the Kerulen, at about 114° E. I have checked the name on the corresponding Chinese maps, and there it is Pa-êrh-ssü-ch'êng, «Bars Town», equivalent to Mongolian Bars-ḥoto or Bars-ḥoton; SCHMIDT, who did not read Chinese, perhaps found the correct name in a manuscript copy of the Jesuit maps sent to Russia. Some information on Bars-ḥoto is found in the *Hou ch'u sai chi*, an account written by one of the officials who participated in K'ang-hsi's campaigns against Galdan at the end of the 17th cent. The author describes there the ruins of a temple, with a much defaced inscription still showing, according to him, that it went back to the time of the Liao, and was consequently prior to 1125 (cf. POPOV, *Men-gu-yu-mu-zi*, 392-393). If so, the town could not have been founded by Toyon-tämür in 1368, notwithstanding what is said by «Sanang Setsen». And even if the author of the *Hou ch'u sai chi* was mistaken as to the date, the fact remains that Bars-ḥoto is not Ying-ch'ang. A solution would be to admit that there is an error in the *Hou ch'u sai chi* as to the date, and to suppose that the Chinese texts are wrong in assuming that Toyon-tämür settled in the district city of the Ying-ch'ang-lu; he may have fled to the territory of which Ying-ch'ang-lu was the administrative centre, but founded the city of Bars-ḥoto much farther to the north, on the northern bank of the Kerulen. I am not in a position to decide this point.

But this is not a sufficient clue to Polo's «Barscol», even if we admit that the «Tiger City» existed before 1368, since there is no necessary connection between a «Tiger City» and Bars-köl, a «Tiger Lake», or Bars-γol, a «Tiger River».

We do not know of a Bars-γol in any text, but «Sanang Setsen» mentions a Bars-köl (misread «Bars-kül» in SCHMIDT, *Gesch. der Ost-Mongolen*, 217). YULE noticed it, but added that «Sanang Setsen»'s Bars-köl seemed to be in Western Mongolia, and was perhaps Lake Barkul of our maps, whereas Polo's «Barscol» must have been «on the Manchu frontier». The historical geography of Mongolia is still so little known that I dare not be positive as to «Sanang Setsen»'s Bars-köl, but I must point out that it is a Turkish, not a Mongolian name. *Bars*, «tiger», is common to both languages, but *köl*, «lake», is only Turkish. There is consequently no likelihood that Bars-köl should be in Mongolia proper, and YULE's idea of Lake «Barkul» (more exactly Barköl), north of Ha-mi (Qomul), is the only one worthy of being retained. Although the explanation of Barköl by Bars-köl, «Tiger Lake» has been sometimes denied (but BARTHOLD accepts it in *12 Vorlesungen*. 215-216), I intend to show some day that there were in the Ming dynasty transcriptions really based on Bars-köl (for the name in the T'ang period, cf. *TP*, 1929, 251; for a derived form Barčūq from *bars*, with the same fall of -s as in Barköl, cf. *TP*, 1930, 55-56; *JA*, 1934, I, 60; a «Barbula[q]» in YS, 169, 4 a, may be \*Bars-bulaq, «Tiger Spring», also with the fall of -s).

The objection to locating «Sanang Setsen»'s Bars-köl in Mongolia proper, on account of its Turkish character, holds good also, and even more so, for a Bars-köl = «Barscol» which would be situated on the borders of Manchuria. Unfortunately no such name has been found in works of the Mongol period. The Pa-li-hun of *YS*, 15, 1 *a*, may perhaps be the Barkul, but it is not likely from the phonetic point of view, as it supposes \*Barqun or \*Baryul, not Barköl (Barkul), and we have here probably to do with a transcription Barqun = Baryu (see «Bargu»). A granary Barstu, «Place of Tigers», in the dependency of Shang-tu, is named in *YS*, 19, 6 *a*, and a «land of Bars» occurs under 1365 in *YS*, 46, 7 *b*. But all these names simply show the current use of *bars*, «tiger», in Turko-Mongolian nomenclature; they do not lead to an identification of «Barscol».

For the present, I see only two solutions : either to suppose that the place is on the borders of Manchuria, and then to restore it as a Mongolian Bars-yol, «Tiger River», otherwise unknown; or to take the name as a Turkish Bars-köl, in which case it must have been in a Turkish-speaking region. We know of a great many cases in which Mongol princes had appanages scattered in different regions of the empire. Nayan, though settled in Manchuria, may have also inherited rights in the Barkul region. Without laying too much stress on the point, I may add that the Bäkryn, many of whom were Christians, lived in the region of Lake Barkul in the Mongol period; if Nayan had power in Barkul, he may have drawn from the Bäkryn at least a part of his Christian contingents.

But, in favour of a location in Manchuria, some other arguments have been brought forward. In 1293, Qubilai is supposed to have said to Qara-baturu (*YS*, 169, 4 *b*) : «North from here, there is the ancient land of Nayan called 阿八剌忽 A-pa-la-hu, which produces fish; I am founding there a city... to which I give the name of 肇州 Chao-chou...». PALLADIUS (*Kommentarii*, 33; *Y*, 1, 345), in agreement with the former Chinese opinion which made Nayan a descendant of Bälgütai, identified «A-ba-la-hu» with the modern Butḥa on the Nomin River (south-west of Mergen and north of Tsitsihar). It is this «A-ba-la-hu» which PALLADIUS supposed to be phonetically connected with Polo's «Barscol», together with P'u-yü-lu or Pu-lo-ho. In fact, Bälgütai was appanaged more to the west (north of the Kerulen) and had also the region of Kuang-ning on the northern side of the Gulf of Liao-tung. We do not know who had the region of the present Butḥa, and moreover, there is, as far as I can see, no reason to place there the region of A-pa-la-hu, a name which occurs only once in *YS*. This name must be kept in mind as having a certain phonetic analogy with «Barscol»; but it may be fortuitous, and A-pa-la-hu, a fish-producing region, may be some nominal form derived from Mong. *abala*, «to hunt» and «to fish».

We ought to be able to locate A-pa-la-hu, since in 1293 Qubilai established there a certain city of Chao-chou, which is also mentioned in *YS*, 18, 7 *b*, under the year 1295; but the geographical section of *YS*, 59, 1 *b*, notes that the name is not registered in the official compilations of the dynasty and mentions it under Kuang-ning-fu simply because Kuang-ning-fu was Bälgütai's appanage and the compilers took Nayan (though wrongly) to be a descendant of Bälgütai. There is, however, a Chao-chou which already existed under the Chin (*Chin shih*, 24, 1 *b-2 a*) and seems to be correctly identified with 朱家城子 Chu-chia-ch'êng-tzü, a place

on the right bank of the Sungari, south-east of Bedune and south-west of Harbin (« Chu-chia » looks like a modern transcription of the ancient name \*Čuka, to judge from the other transcriptions of the « ancient » form ; cf. YANAI, 619). T'u Chi (22, 7 a) says that west of Hu-lan (Hu-lan is the Chinese city a little to the north of Harbin) there is a Bars-ḥoto, which means « Tiger City » and is the Yüan-chou of the Yüan dynasty ; YANAI, 620, while quoting T'u Chi, has added a passage from YS, 100, 7 a, where it is said that the myriarchy of the military colony of Chao-chou was, in 1295, founded near the administrative centre of Chao-chou with various elements, including 220 families of *bürgüči* (?) of Nayan. But here again our information is too fragmentary. I do not know where T'u Chi found his Bars-ḥoto west of Hu-lan, and why he identifies it with the Chao-chou of the Yüan and with A-pa-la-hu ; it may be due simply to the phonetic analogy between Bars-ḥoto and A-pa-la-hu, coupled perhaps with second-hand information about Polo's « Barscol ». On the other hand, the name of the district (*hsien*) established at Chao-chou under the Chin, 始興 Shih-hsing (not mentioned under the Yüan), has been quoted as being the first element of Nayan's fourth province, « Sichintingiu » (*q. v.*) ; cf. YANAI, 619. Although I am not positive as to this last phonetic equivalence, the fact remains that Nayan's northern and eastern domains having been already designated « Ciarcia » and « Cauli », the region south-west of Harbin seems bound to be either « Barscol » or « Sichin tingiu ». We must, however, remember that a Bars-ḥoto does not imply a « Barscol » and that the Chao-chou of the Mongols, created moreover after Polo had left China, is not necessarily to be identified with the Chao-chou of the Chin.

## 62. BASMAN

<i>basarom</i> , <i>besarom</i> , <i>bosaram</i> VA	<i>basmam</i> FA, P	<i>basmon</i> FA
<i>basma</i> F, TA <sup>1</sup> , TA <sup>3</sup> , VB, Z ; R	<i>basman</i> F, FA, FB, L, LT, P,	<i>bassina</i> VL
<i>basmai</i> V	TA <sup>1</sup>	<i>basym</i> G

RR, 413, and B<sup>1</sup>, 438, have adopted « Basman », which occurs once in F ; F has three times « Basma », which is also the form of R and Z, and consequently seems to have more authority. YULE opted for « Basma » (Y, II, 284, 288) ; nevertheless, as the final *-ma* or *-man* is not explained, both readings remain possible, and the reason for my adopting « Basman » will appear below. The « Hormar » of the Catalan Map is probably altered from « Basman ». Basma may have originated from \*Basmā = Basman.

From the position between « Ferlec » and « Sumatra », it has been considered as certain that « Basma » represented فاسى Pāsè, Pasè, Pasei (? < \*Pāsi), in the northern part of the island of Sumatra, as had already been proposed by VALENTIJN (the form « Passier », quoted by CORDIER in Y, II, 288, is without authority) ; Pasaman, on the south-western coast of Sumatra, suggested to CORDIER (Y, II, 289) by SCHLEGEL, seemed to be excluded by its position. « Pacem », the Portuguese form of Pasè, had led YULE to suppose that the Arabs pronounced it « Basam » or the

like», and this would account to some extent for Polo's «Basma». It has been objected (for instance by BLAGDEN in *Pe*, LIX) that the final *-m* of «Pacem» was only an example of the Portuguese tendency to nasalize final vowels, and this must be right; one is only somewhat surprised to find in 1727 a form «Pissang» in A. HAMILTON (YULE, *Hobson-Jobson*<sup>2</sup>, 683), but I am inclined to think that it represents Pasangan rather than Pasè.

In spite of «Pacem» and perhaps «Pissang», there can be no doubt that the correct form Pasè was in common use amongst foreigners. In 1511, Giovanni d'Empoli writes «Pazze» (*Hobson-Jobson*<sup>2</sup>, 682). It is also 巴西 Pa-hsi, Pasi, which is used twice in the *Ming shih* for 1521 and the following years (cf. *Y*, II, 296; *TP*, 1934, 84). For the Mongol period we should probably identify Pasè as the kingdom of 八昔 Pa-hsi to which three envoys were sent in 1309, while others went to Pu-lin-pa (Palembang?) and Champa (*YS*, 23, 4*b*; it seems to be this name which has been altered to 阿昔 A-hsi in *Yüan wên lei*, 41, 20*b*).

It may even be that the name occurs still earlier in Chinese texts. LAUFER has collected (*Sino-Iranica*, 468-487) a series of such texts in which, according to him, 波斯 Po-ssü is not, as usual, a transcription of the name of Persia, but represents a «Malayan Po-Se». FERRAND (*JA*, 1921, II, 279-293) has accepted the whole of LAUFER's material, but has come to the conclusion that it could not apply to one and the same country, even when Persia was to be left out of account. For FERRAND, that eastern kingdom of Persia is in some cases Bassein in Southern Burma (a name much more ancient than it is said to be in *Hobson-Jobson*<sup>2</sup>, 70-71), and in others Pasè in Sumatra; in particular, all mentions of «Po-ssü ships» in early Chinese texts would relate to Bassein sailors. I cannot enter here into a detailed examination of the case, but must remark that later (*JA*, 1924, I, 241-242), FERRAND, without any allusion to his notes of 1921, has spoken of the «Po-ssü ships» as being Persian; I have no doubt that this time he is right. As to LAUFER's material and its interpretation, they are vitiated by a number of doubtful forms and dates and by the extraordinary idea that Persia's intercourse with China was always carried on by land. My present view is this (cf. also *TP*, 1923, 196-197): all the texts mentioning Po-ssü before the Sung dynasty refer in all likelihood to Persia, and this is also the case probably for early mentions of Po-ssü in Japanese texts, in spite of what HAGUENAUER says in *JA*, 1935, I, 92. But, in the 11th and 12th cents., the name was sometimes misapplied to a Malay state; this is particularly the case for the Malay numerals of Po-ssü preserved in the *Kōdanshō* of c. 1100 (the author died in 1111; cf. *JA*, 1935, I, 92). Although we have no native mention of Pasè earlier than Polo's time, nor for some time after him, I have little doubt that the name must be more ancient. It may be the name Pasè (Pāsei or \*Pāsi), identical with the Chinese transcription Po-ssü (= \*Pāsi) of the name of Persia, which was then mistaken for Persia. And this also implies that the name of Pasè was at that time pronounced without a nasal final. But while holding that the views expressed above cover most of the facts relating to Po-ssü = Pasè, I must add that the name of Pasè does not appear among the twenty odd «islands» of Sumatra enumerated in 1365 by the *Nāgara-kṛētāgama* (cf. *Fe*, 652), an apparent omission of great moment. The reason is, I think, that the state of Pasè is already represented in that list under its alternative name of Sadra. As I explain under «Sumatra», the state of «Sumatra» [had its capital at Samudra] on the river of Pasè, and Dutch scholars now speak

currently of the princes of «Samudra-Pasè». The state may have had a native name Pasè and a Sanskrit name Samudra, just as the island of Singapore was called Simhapura, but also had the native name of Tumasik or Tēmasik. Almost all Chinese texts of the Mongol period speak of «Sumudra» (= Samudra, Sumatra); if I am right in supposing that the Pa-hsi of 1309 is Pasè, it is the only link hitherto available between the Malayan Po-ssü of the Sung and Pa-hsi, Pasem, Pазze of the 16th cent. The two Malay legends on the late foundation of Pasè, alluded to in Y, II, 288-294, have little historical value. Neither Pasè nor Samudra is named in 1225 among the dependencies of San-fo-ch'i (then Ĵambi), although the list includes Lamuri; but we can only conclude from this very incomplete list that Pasè-Samudra had not in 1225 the importance it attained later on.

But if Pasè is the same as Samudra-Sumatra, the conclusion must be drawn that Basman cannot be Pasè, since Polo has another notice on the kingdom of «Sumatra», and this unavoidable conclusion leads us to reconsider the case of Pasaman. Pasaman is a place of some importance; the king of Achin mentions it in 1615 among his possessions (*Fe*, 671). It is said, in the dictionary quoted by SCHLEGEL to CORDIER, that «Basman» is regularly the Arab pronunciation of the name Pasaman (it might have been \*Fasman), and thus it strictly corresponds with Polo's form. Of course, Pasaman is not on Polo's itinerary, and Polo attaches to his description of «Basman» details of the rhinoceros which can only be those of an eye-witness; as to his monkeys made up to look like pygmies nobody has yet offered an explanation. Nor does it seem that Polo went to «Fansur» (his mysterious «Dagroian» has also been looked for far to the south-east of Perlak, the point at which Polo's ship seems to have reached the island of Sumatra; but I think that view is wrong). Although I am not positive on the point, it may be that Polo gave his description of the rhinoceros when speaking of a kingdom in Sumatra of which he had only heard, and from which his monkey-pygmies were said to come. In such a case, Pasaman would have a fair chance of being «Basman», and that is the reason why I have adopted this spelling in preference to «Basma». In *Bijdragen*, LVII, 8, ROUFFAER, after giving the current identification of «Basman» with Pasè, adds «or Pasaman?». If «Basman» be Pasaman, we have, in Fansur and Pasaman, yet another example of Polo's digressions which have been altered so as to give to his account the appearance of a continuous itinerary.

### 63. BASORA

<i>balsara</i> R	<i>basra</i> F, FA, FB (?), TA <sup>1</sup> ,	<i>bastra</i> FB (?), P
<i>barsera</i> S	TA <sup>3</sup> , VB, Z	<i>bosera</i> VA
<i>bascha</i> V	<i>basora</i> L	<i>vassara</i> P
<i>baschra</i> VL	<i>lira sca</i> LT	

In Arabic, باصرة Bašra; the Basra of our maps. On it, cf. *LS*, 44-46. YULE (*Y*, I, 65) says that the mss. have «Basera», but that it is almost certainly an error for «Bastra», which he

adopts in the text. «Bastra» is also the form which has been accepted in *RR*, 25, and in *B*<sup>1</sup>, 438 (though with some doubt in favour of «Bascra»). But there is no reason to adopt «Bastra» instead of «Bascra» when YULE, RICCI and ROSS, and BENEDETTO retain «Nescradin» (*q. v.*); in both cases, the apparent -*sc-* would answer to the same letter *س* *ś*. As a matter of fact, I think we should read «Nesoradin» (confirmed by the Chinese transcriptions) and «Basora». «Bassora» occurs in *LT*, and all the forms in mediaeval maps show an *a-* (<*o-*) before -*ra*, so that the form of the name used by Polo must not have ended in -*scra* or -*stra* (cf. HALLBERG, 71; according to *Y*<sup>1</sup>, iv, 309, the form on the Catalan Map is not «Bassara», but «Bassora»). The Portuguese form of the name is «Baçora», and Italians even now use «Bassora». For various citations of the place and the name, cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Balsora, Bussora»; *Fe*, 693; DAMES, *Barbosa*, I, 88. The isolated form «Balsara» in RAMUSIO may not have been derived from a misreading of his mss., as a similar form «Balsera» was used before him by N. Conti and Fra Mauro. RAMUSIO perhaps «edited» the name according to a then current form among Italian travellers and geographers; but I doubt that this «Balsara», «Balsera», «Balsora», is due to a metathesis of Al-Baśra, as is supposed by DAMES.

Baśra is referred to in 1225 by Chao Ju-kua as 彌斯羅 Pi-ssü-lo (cf. *HR*, 117, 122, 137-138); the transcription is more regular than it is in the modern pronunciation.

## 64. BATU

*bacchia* TA<sup>3</sup>

*bachim* G

*bachui* LT, VA, V

*bacui* F, L

*bacuy* FA, FB

*barchia* (?) TA<sup>1</sup>

*bath* S

*bathyn* R

*batin* P, VL

*bote* VB

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*patu* F, TA<sup>1</sup>, Z, L

This is a correction. In one passage, the readings of the mss. would seem to point to «Batui» (the «Bacui» of F is a usual clerical error; R's «Bathyn» comes from a \*Bathin misread from \*Bathui; there is no Z reading here). In the second case, the «Patu» of F and Z is almost certainly altered from \*Batu (see «Apusca» for an analogous case, and cf. in F «Jacobit» for «Jacobit» [*B*, 18, 23]), but no ms. gives this last form. On the anomaly of the double form «Batui» and «\*Batu», see «Uiau». Plan Carpine, whose nomenclature shows Slavonic influences, writes «Bati» (*Wy*, 39 and *passim*), which corresponds to Russian «Baty», the Russian *y* (=Turk. *t*) having in the past a certain analogy with *u* and transcribing it in many cases. Rubrouck has always «Baatu» (*Wy*, 168, etc.), which shows a pronunciation Bātu, with long *ā*. Hethum gives «Batho» (var. «Bato», «Baccho», etc.; cf. *Hist. des Crois.*, Arm., II, 115, 157). Among Persian writers, the name is always باتو Batu. The *YS* writes 拔都 Pa-tu and 八都 Pa-tu, Batu, sometimes with the addition of 罕 *han*, which gives Batu-*han*, «Khan Batu» (cf. WANG Hui-tsu<sup>2</sup>, 41, 9). The name is fairly common, and there are in *YS* several Pa-tu

which are probably, at least in some cases, to be restored as Batu (cf. WANG Hui-tsu<sup>1</sup>, 29, 4). Batu is Mongol, and means «firm».

Batu was Jöči's second son, and was the real founder of the khanate of Qipčaq, or Golden Horde. He reigned from 1227 to his death, at the end of 1254, or more probably in the beginning of 1255. He was also known under the appellation of Sayin-ğan, «the Excellent Khan», and Polo is wrong in speaking of a Khan «Sain», as Batu's predecessor; see «Sain».

## 65. BAUDAC

<i>balach</i> L	<i>valdachi</i> P	<i>baudas</i> FA
<i>balcho</i> , <i>bandach</i> V (c. <i>mosul</i> )	<i>baldacho</i> , <i>baldaco</i> VA	<i>baudaz</i> FBr, <i>t</i>
<i>baldac</i> TA <sup>3</sup> , LT, VB, Z, G	<i>baudac</i> F, FB, TA <sup>1</sup>	<i>boldach</i> VB
<i>baldach</i> TA <sup>3</sup> , LT, VB, Z, L,	<i>baudach</i> Fr, <i>t</i> , Z, L	<i>baldato</i> LT
VL, S, R		

Baydād, Bagdad. Polo does not give here a transcription of his own, but uses the form «Baudac» or «Baldac» which was then current among the Franks (for instance «Baldac» in Plan Carpine, *W*y, 113). In his French text, the form was probably «Baudac» as in F, and I have kept it; but the Venetian version which seems to be the basis of Z and of RAMUSIO's main ms. had probably «Baldac». The «Baudas» adopted from PAUTHIER in Y, I, 63, 65, although it has passed into Froissart, is a secondary form of less value, which possibly originated from some clerical error, and which does not account for derived forms like Ital. *baldachino* or Engl. *baudekin*. In Pegolotti, the name is «Baldacca», but with a curious adjectival form «baccadeo», once altered to «gabbadeo» (ed. EVANS, 397, 398, 400); Spanish texts give «bagadel» (HEYD, *Hist. du commerce*, II, 697).

Bagdad appears in Chinese texts towards the end of the 8th cent. in the form 縛達 Fu-ta (\*B'wak-d'ât); the transcription is absolutely correct and HIRTH and ROCKHILL were mistaken (HR, 14) in supposing that it was corrupt or that is applied to «Fostat» (Fusât, Old Cairo). In 1178, Chou Ch'ü-fei writes 白達 Po-ta (\*B'ek-d'ât), which was copied in 1225 by Chao Ju-kua (HR, 117, 135; for possible wrong duplicates of the same name, see «Çiraç»). The account of Ch'ang Tê's mission of 1259 has 報達 Pao-ta, \*Baudad, and this has given rise to some strange misconceptions. While YULE (Y, I, 64) supposed that both Pao-ta and his wrong Polo form «Baudas» were «due to the Mongol habit of slurring gutturals», HIRTH and ROCKHILL (adopting also «Baudas») said that Polo «must have taken . . . Baudas from the Chinese» (HR, 135). But there was no «slurring» of real gutturals in Mongolian, and many parallel forms like Soldaia, Soydak and Sudaq, *sultan* > Fr. *soudan*, etc., are enough to show that the Mongolian or the Chinese have nothing to do with the case. Moreover, we also find 八哈塔 Pa-ha-t'a and 八吉打 Pa-chi-ta, correct transcriptions of Bagdad, in YS, 3, anno 1253, and 63, 16 b. The *Secret History* writes Baqtat (§§ 260, 261, 270, 274). Cf. also Br, II, 123-125. A Ming itinerary has 把魯旦 Pa-hei-tan (*China Review*, v, 239) = Baydan, a well-known mediaeval popular form of

the name; cf. M. Streck in *EI*, s. v. «Baghdād». In *Ming shih*, 332, 12 b, 把丹 Pa-tan is much more likely to be read as Baydan = Bagdad than as Badaḡšān, in spite of *Br*, II, 314. The latter work (326, 7 a) writes also 白葛達 Po-ko-ta, Bagdad, listing an embassy of 1426, but mentions moreover, for the same period, a 黑葛達 Hei-ko-ta which seems a fanciful creation derived semantically from Po-ko-ta (326, 7 a-b). T'u Chi's chapter on the history of Bagdad (ch. 147) and his geographical notice (160, 23 a) have nothing original.

Bagdad, founded in 762 (not 764 as in *Br*, II, 123, or 753 as in *HR*, 135), was taken by the Mongols on February 5, 1258.

In spite of CORDIER's reiterated argument against SYKES in *Y*, III, 4-5, it is plain that Polo never was in Bagdad; his outward route took him straight to Tabrīz (see «Tauris»), and on the return journey he landed at Ormuz without ever visiting the lower Euphrates or the Tigris. This whole section is therefore based on hearsay information.

## 66. BAUDOIN

*audouin* FA  
*baldionus* L  
*baldoin* V  
*baldoinus* LT

*baldouino* TA<sup>3</sup>, S  
*balduino* R, VA  
*baldwinus* P<sup>5</sup>  
*baudoin* F

*baudouyns* FB  
*bawdeuins* O  
*edaldwinus* P

Baldwin II de Courtenay, «Latin» Emperor of Constantinople, reigned nominally from 1228 to 1261, but in fact from December 1239 to 1261; in the latter year he was dethroned by Michael Palaeologos.

## 67. BELOR

*balor* VA  
*belor* F, FA, FB, LT, P, TA<sup>1</sup>, TA<sup>3</sup>, V, Z  
*beloro* VB; R

*bolor* L, L<sup>1</sup>, L<sup>4</sup>  
*bossor* VL

On the many controversies raised by this name, cf. *Y*, I, 178-179. The problem, long obscured by a forgery which seems to be due to KLAPROTH and by the confusions which modern Chinese scholars have made between ancient Chinese names and the modern Būrūt tribe, is now fairly clear, so that the name and its value can be correctly determined. Polo's Belor corresponds to the name which was written بُلُر in 982-983 by the author of the *Hudūd al-'Ālam* (ed. BARTHOLD, pp. 27 and 36; but بُلُر Bulūr, 26 b; cf. also *Mi*, 93, 121, 258, 369-370), in 1030 by Al-Bīrūnī (SACHAU, *Alberuni's India*, I, 117, 206, 207), in 1050-1052 by Gardīzī (BARTHOLD, *Očēt o komandirovké*, 117), and which editors and translators have transcribed «Bolor». In 518-522, Sung Yün mentions 鉢盧勒 Po-lu-lo, which supposes an Iranized \*Palūrāg; Hsüan-tsang, in the second quarter of the 7th cent., writes 鉢露羅 Po-lu-lo, and that, in his very scientific



system, can only be a Sanskritized form \*Palūra (cf. *BEFEO*, III, 406; not «Po-lo-lo», and not «Bolor», as in *LENTZ*, *ZDMG*, 1932, 27); as the first vowel is not marked in most Arabic transcriptions, we are justified in reading them as Balōr or Balūr instead of Bolor; and it was «Pálor» which was heard by Shaw in modern times (cf. *Y*, III, 42). The country of Balur or Baluristan is again described in his *Ta'riḥ-i-Rašīdī* by Mīrza Haidar, who visited it in 1528 (cf. trad. N. ELIAS and E. D. ROSS, 135, 385). By Baluristan, Mīrza Haidar understands the mountainous tract south of Badaḥšan, south-west of Yarkand, west of Balti, north and north-west of Kashmir, that is to say the valleys of Chitral, Yassin, and Gilgit; N. ELIAS's note on the subject has been quoted in *Y*, III, 42-43, and need not be repeated here; cf. also *LENTZ*, in *ZDMG*, 1932, 27-28. We must also see Balūr in the 勃律 Po-lü (\*Balūr? or Palūr?) of Chinese historical texts of the 8th cent.; there is a Po-lü which is Baltistan and a «Little Po-lü» which is the Gilgit valley (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 149-154). It is true that Mīrza Haidar seems to leave Baltistan out of Baluristan; but the localization may have been viewed differently by others at different dates, and CUNNINGHAM heard the name «Bolor» used as a designation of Balti by the Dards of Gilgit (*Y*, I, 178). It might also be supposed that the very name of Balti is connected with that of Balūr, but I do not think that such is the case, as the Tibetan texts distinguish sBal-ti from 'Bru-šal or Bru-ža, and it is Bru-ža which is likely to be the same name as Po-lü or Balūr (cf. LAUFER, in *TP*, 1908, 2-3). In such a case, it is the «Little Po-lü» which would be the true Balūr; and the extension of the name Po-lü to Baltistan might be due to the fact that the Chinese reached Baltistan by way of Gilgit.

Belor seems to be the form used by Polo, as it is given by all the mss.; we must then attribute to a simple coincidence that Fra Mauro should write «Bolor» (cf. HALLBERG, 74; the other name quoted from Fra Mauro, «Boler», seems to refer to quite a different place far to the north; see «Bolgara»). As to «Bolor», adopted in YULE's edition, it is devoid of authority. A last form looms up occasionally, that of «Billūr mountains», understood as «Crystal mountains» (for instance in ELLIOT's *History of India*, I, 46, 65, where Rašīdu'd-Dīn copies Al-Bīrūnī); this is simply a case of popular etymology, and it is also represented by the pseudo-Turkish name «Belur-tagh» still used in *B*<sup>1</sup>, 439. Cf. also STEIN, *Ancient Khotan*, I, 6; *Serindia*, 33, 61.

## 68. BERCA

abarca, barcalor FB  
 abarcha, abracha, VB  
 arbaca FA  
 bacara (? = bolgara) F, L  
 baraba, barba P

barbarba, burba P<sup>5</sup>  
 barca F, Fr, FA, FB, O, VL  
 barch F  
 barcha F, Fr, LT, TA<sup>3</sup>, VA,  
 VL; R

bargha, bergho TA<sup>1</sup>  
 bata, bercu Ft  
 berca F, Fr, t, Z  
 bercha F, L, Z  
 bracharchan, tharzara V

This is Bärkä, a Mongol name; bärkä means «difficult». Bärkä was Jöči's third son. The forms «Bérékeh» of *Bl*, II, 90, 433, 549, App. 54, «Baraka» of Stanley LANE-POOLE, *The Moham-medan Dynasties*<sup>1</sup>, 230, and «Bereke» of HOWORTH, II, 113, III, 193, and of *Wy*, LIII, 316, are due

to a wrong arabization; although Bärkä became a Mussulman, his name was never changed. Cf. also *Et. s. v.* «Berke».

Ĵuwainī (I, 144, 215, 221) calls him بركه Bärkä (with بركا Bärkä in one ms.), and so does Waśśāf (*Ha*<sup>2</sup>, 92) when he speaks of بركه اغول Bärkä-ogul, «prince Bärkä». We find also بركه Bärkä in Rašidu-'d-Dīn when Rašid gives the genealogy of Jöči's branch, but I am at a loss to explain why Rašid calls him بركاي Bärkäi in the rest of his book (*Bl*, II, 138, 139, 433, etc.), hence «Barcai» in *Oh*, II, 251, and in the genealogies at the end of *Oh*, III; here, Rašid's source cannot be Ĵuwainī. The name of the prince 別兒哥 Pie-êrh-ko (\*Bärgä, for Bärkä) occurs in *YS*, 3, *pên-chi* of the year 1251. Polo's form is in complete agreement with Plan Carpine's «Berca» (*Wy*, 66); Hethum the historian writes «Barcha», and we find «Bareque» in the *Gestes des Chiprois* (*Hist. des Crois.*, Arm., II, 891). Although Abū-'l-Ghāzī also writes Bärkä, his mss. often give بركا and بركه Bärkä (cf. DESMAISONS, *Hist. des Mogols*, II, 181); but it is simply a wrong form which ought not to have been adopted by the editors of *Hist. des Crois.*, Arm., II, 891 («Bourkai») and which does not deserve BLOCHET's attempt at an etymological interpretation (*Bl*, II, 114; moreover, *bürgä* means «flea» and not «partridge» in Turkish).

The *YS*, 3, in the *pên-chi* of 1253, says that 必蘭別兒哥 Pi-shê-pieh-êrh-ko was (or were) then sent to make a census in Russia (this census of the Russians and the Alans is also mentioned in *YS*, 63, 16a, but without names; it is the census which Kirakos places in 1254 [cf. BROSET, *Deux historiens arméniens*, I, 175; PATKANOV, *Istoriya Mongolov*, II, 78]). BRETSCHNEIDER (*Br*, II, 80) thought that *pi-shê* was a shortened form of 必蘭赤 *pi-shê-ch'ih* (cf. Uigh. *bitikçi*, etc., Mong. *bičigäči*), well attested as meaning «secretary». As to Pieh-êrh-ko (\*Bärgä, Bärkä), he proposed to see in him one of the commissioners of a census of 1259 whom the Russian chronicles call Berkäi. T'u Chi (6, 7b), on the contrary, considered that two men's names were referred to, restored Pi-shê as another transcription of Batu (which is impossible), and identified Pieh-êrh-ko with the prince Bärkä. I am afraid that both explanations may have to be discarded. From *YS*, 85, 2b, and *Yüan tien chang*, 21, 34a (cf. P. RATCHNEVSKY, *Un code des Yuan*, 123), we see that officers called 別里哥 *pieh-li-ko* were employed to levy taxes. Although the Mongol original of the title is still unknown, it is probable that we must read it as \**bärgä* (= \**bärkä*). In such a case, *pi-shê*[-*ch'ih*] and \**bärgä* would have been sent to the Russians and the Alans in 1253 and in speaking of a «Berkäi» in 1259, the Russian chronicles would have taken the title of an officer for the name of a man. But I cannot as yet give this as a final solution.

Batu died at the end of 1254, or more probably early in 1255 (see «Batu»), and was succeeded by his eldest son Sartaq (more or less a Christian, he was the man to whom Rubrouck carried a letter addressed by St Louis). Very soon afterwards, Sartaq died childless, and, after the short reign of Sartaq's brother Ulayči, the rule of the Golden Horde passed in 1256 or 1257 (1257 according to BARTHOLD, *12 Vorlesungen*, 171) to Jöči's third son, Batu's younger brother Bärkä, born c. 1206-1208 (cf. BLOCHET, *Moufazzal*, 118). Bärkä died in 1265 or 1266 (cf. *Ha*<sup>1</sup>, 254-255; *Bl*, II, 549; BARTHOLD, in MINAEV's *Marko Polo*, 5, and in *12 Vorlesungen*, 171, is in favour of 1266). Bärkä was the first Chinghiz-khanid to become a Mussulman. He was already a Mussulman when Rubrouck heard of him in 1253-1255 (*Wy*, 209).

Mufazzal quotes from a «Life» of Baibars the following description of Bärkä's appearance

when Egyptian envoys saw him at the age of 56 (BLOCHET, *Moufazzal*, 118-119; cf. also QUATREMÈRE, *Hist. des Sultans Mamlouks*, I, 1, 214-215) : « Bärkä had a scarce beard, a big face of yellow colour, and his hair was made up in braids close to his ears. He wore at each ear a golden ring set with a stone of very great value. He was dressed in a gown of Chinese silk and wore on the head a Mongol cap. He was girt with a golden belt set with precious stones, from which hung a purse in green Russian leather (*bulyārī*). He had put on boots of red velvet. No sword hung at his side; his belt was adorned with black curved horns set in gold.» Bärkä, having at his side his principal wife, was « sitting on a throne, his two feet softly resting on the dais on which a cushion had been placed, since he suffered from a fit of the gout ».

Bärkä's daughter married the Mamluk Sultan Baibars and bore him the son who succeeded him Sa'īd Khan Muḥammad, also named Nāsīru-'d-Dīn Bärkä after his grandfather (BARTHOLD, *12 Vorlesungen*, 175-176).

The coins called *asperi barichati*, which appeared towards the end of the 13th cent., seem to derive their name from Bärkä (cf. G. I. BRÄTIANU, *Actes des notaires génois*, 112, 126; *Recherches sur le commerce génois*, 238).

The main Polian problem connected with Bärkä is Bärkä's war with Hülägü, because that war was directly or indirectly the cause of the journey of the elder Polos to Bukhara and of the Polian journeys that followed. But deficient and contradictory sources make it difficult for us to arrive at the real dates and the true facts of the conflict. To the bibliography of Y, II, 495, we must add QUATREMÈRE, *Hist. des Sultans Mamlouks*, I, 1, 180, 187-188, 190, 211-218; BROSSET, *Hist. de la Géorgie*, I, 565-573; *Hist. de la Siounie*, 233; *Deux historiens arméniens*, I, 192-194; *Oh*, III, 377-394; PATKANOV, *Istoriya Mongolov*, II, 104-108; *Istoriya... Magakii*, 32, 92; *Bl.* II, 139, 549, and App. 54, 56; RAVERTY, *Ṭabaḳāt-i-Nāsīrī*, 1283-1292; BLOCHET, *Moufazzal*, 103 sq.; HOWORTH, II, 113-124; III, 193-201.

Despite minor uncertainties, we may take it for granted that the war between Bärkä and Hülägü broke out in the middle of 1262, that the main battles were fought at the end of 1262 and the beginning of 1263, and that Hülägü's armies never advanced to the north beyond the basin of the Terek; the statement in Kirakos (BROSSET, *Deux historiens arméniens*, I, 193; PATKANOV, *Istoriya Mongolov*, II, 106) that Hülägü's armies devastated Bärkä's territory in the region of the Volga is contradicted by all other sources and cannot be sustained. But then Polo's account is difficult to explain. The chapters descriptive of the battles are mere rhetoric and we need not attach too much importance to the fact that Polo says that Hülägü was the victor when in fact his troops were actually beaten. But Polo ought to have known the facts about his uncle and his father when he says that they went on from the Volga to Bukhara because the route was free in that direction, while they could not retrace their steps towards the Crimea and Constantinople for fear of being captured on account of the war with Hülägü. Now a war waged in the basin of the Terek and to the south of it would leave undisturbed the line of communications between the Volga and Constantinople. Moreover Bukhara itself was not so safe : there Hülägü took revenge for the death of some of his merchants in Bärkä's dominions by massacring the men of the five chiliarchies who were stationed in Bukhara and who belonged to the house of Bärkä (*Ha*<sup>2</sup>, 94). I wonder whether Polo's statement is as explicit and complete as it appears to be at first sight.

From Mussulman and Greek sources we learn that, at the time of the war between Bärkä and Hülägü, the Mussulman Bärkä asked for the help of Baibars, the Mamluk sultan of Egypt; the messengers from Bärkä and those from Baibars all passed through Constantinople coming and going both ways. The Egyptian sources relate how envoys from Baibars to Bärkä were detained by Michael Palaeologus, and how Bärkä's troops ravaged the suburbs of Constantinople in retaliation. Although Byzantine writers seem to give for these events dates later than 1262, the most probable year in which the elder Polos went on to Bukhara, it is very tempting to suppose a connection between the two. In other words, the extraordinary movement which made the elder Polos turn their backs to the place to which they wanted to return would be due, as Polo says, to the war between Bärkä and Hülägü, but inasmuch as the entanglements of that war had stopped all friendly intercourse between Bärkä's dominions and the Byzantine Empire.

## 70. BETTALA

*bachalar* TA<sup>3</sup>, LT  
*bartalar* VA  
*bathalar* TA<sup>1</sup>

*betala* R  
*betalat* V  
*betelar* FA, FB

*bettala* Z  
*bettalar* F, L  
*bitarssa* VB

The form «Bettalar» of F and cognate mss. has been preferred to the «Bettala» of Z (and R) by RR, 293, and B<sup>1</sup>, 439. But, in spite of B, 178, the *-a* final, which occurs in VB also, is confirmed by Ibn Baṭṭūṭah's «Baṭṭāla», already quoted by YULE (Y, I, 337), and the only other possible form would be \*«Bettalā» = \*Bettalam. The modern name is Patlam, on the coast of Ceylon; cf. YULE, *Hobson-Jobson*<sup>2</sup>, 746, s.v. «Putlam». I hold the *-r* to be of secondary origin, due to copyists; we have other examples of it (in «Succiu», «Caccia modun», etc.).

«Bettala» is perhaps the 第三港 *Ti-san-chiang*, or «Third Creek», of the *Tao-i chih-liao* of 1349-1350, but it may also be that the «Ocean of 大朗 *Ta-lang*» of the same work (the sentence has been misunderstood by ROCKHILL) is corrupt for «Ocean of [Pa]-*ta-lang*», i.e. of Patlam (cf. ROCKHILL in TP, 1915, 385-387; FUJITA, 89 b).

## 71. BEYAMINI

*beyamini* R

The word occurs only in RAMUSIO, as the name of the «wild ox» of Tibet. YULE's \**buemini*, «Bohemian» (Y, II, 52), is as much out of the question as RR's Arabic *bahā'im*, «cattle» (RR, 183). YULE took this «wild ox» to be some sort of *gayāl* (*Bos gavaeus*), perhaps the *gaur* (*Bos*

*gaurus*, etc.). Prompted by this identification, LAUFER proposed in 1900 (*Ein Sühngedicht der Bon-po*, 20, 52) to see in «*beyamini*» a transcription of the Tibetan name of the *gayāl*, *ba-men* (or *ba-man*); he has repeated it in *Chinese Pottery of the Han Dynasty* (1909; p. 260), and through a letter of his, it finally found its way in 1920 into *Y*, III, 83. Sven HEDIN (*Southern Tibet*, I, 139) approved of it, and even saw there a faint indication that one of the four languages acquired by Polo may have been Tibetan, which is most unlikely. In 1924, VLADIMIROV (*Doklady Ross. Ak. Nauk*, 1924, B, 118, «*Beyamini*» *Marco Polo*), unaware of LAUFER's explanation, proposed it independently.

But I doubt very much that Polo, who never went to Tibet, but who must have seen Tibetans south-west of Ch'êng-tu towards Ya-chou, speaks here of *gayāl*. There are four other mentions of the «wild ox» in his book. The first, in the chapter on «Ergiuul» (cf. Vol. I, 179), gives a minute description of the yak, and of the cross-breed of yak and common cow. The second, in the account of the great Imperial hunts (cf. Vol. I, 227), must refer to some kind of bovine animal which cannot be the yak. The third is at the end of the chapter on Burma (cf. Vol. I, 294), which is based only on hearsay information; no particulars are given, and it may or may not be the *gaur*. But later, in his description of Ma'bar (Coast of Coromandel; cf. Vol. I, 401), Polo says that the people attach to the head of their horses, or fix on their shields, or wear as plumes, «hair of the wild oxen of which I have spoken before». YULE (*Y*, II, 359) has not failed to see that Polo refers here to the use of yak-tails, so widely spread over the whole of Asia. But since Polo's «wild ox» is here the yak and since he has spoken of it before, the natural conclusion is that the «wild ox» of Tibet is not a *gayāl*, but the yak (long before Polo, Cosmas also describes the yak under the name of «wild ox»; cf. *Y*<sup>1</sup>, I, 223). And that is what one might expect. The yak is the most conspicuous animal of Tibet. On the very page preceding his note on *beyamini*, YULE reproduces a drawing of a «Village of Eastern Tibet on Sze-ch'wan Frontier»; there are three yaks depicted in the courtyard.

This would not be fatal to LAUFER's and VLADIMIROV's *ba-men*, since VLADIMIROV says that the Mongols have borrowed Tib. *ba-men* under the form *bamin* or *bamên*, and apply it sometimes either to the yak or to the calf of the half-breed *hainuh* buffalo. If *beyamini* was really a word used by Polo, *ba-men* is the best attempt so far made to explain it, although it is not quite satisfactory from the phonetic point of view, and although there is no support for LAUFER's surmise of dialectical forms like *beu-men* or *beu-min*. Still I feel somewhat sceptical about *ba-men*. Polo would not have come into touch with the Tibetans themselves or spoken to them in their own language; the words he uses are those which were accepted in Persian-speaking circles. Now the yak is widely known in Asia under the Turkish name *qutuz* (given in Kāšyārī as a «wild cow»; BROCKELMANN, 167; > Russian name Kutuzov) or *qutaz*, which has passed into Persian under the form *qutās* (cf. VULLERS, 728-729; QUATREMÈRE, *Hist. des Sult. Mamlouks*, I, 228) and applies to the yak itself and to the ornament made from a yak-tail (it has also been explained as a «sea-ox», and as such has been borrowed, I think, in the *hutū* of the Arabs and the *ku-tu-hsi* of the Chinese; cf. LAUFER in *TP*, 1913, 315-370; 1916, 348-389; *Sino-Iranica*, 565-567). Failing that word, Polo might have used the Mongolian name of the yak, *šarluḥ* (> Russ. *šarlık*). But *beyamini* is disconcerting. On the whole, I am not sure that it is not the result of some misunderstanding.

The name is only given by RAMUSIO; but in that passage, Z and R are strictly in agreement, except that Z has *et multi sunt ibi* instead of RAMUSIO's *che si chiamano beyamini*. Perhaps RAMUSIO «edited» a corrupt sentence which originally gave no name and was simply a counterpart of what we read in Z (? \**uocati beiamini < multi sunt ibi*). There are other *idola libri* in RAMUSIO, as for instance the king of Champa «nominato Accambale» (see «Ciamba»).

## 72. BLASIUS

*biagio* R  
*biaxio* VA

*blasius* P; G

*blaxius* LT

RAMUSIO, after «Sevasto» (=Sivas), adds *dove il glorioso messer San Biagio pati il martirio*. This has been omitted in *B, RR, B<sup>1</sup>* (although the same sentence, prior to RAMUSIO, occurs in many versions), because BENEDETTO (*B, CXIII*) considers it an early interpolation. That may be true, but there are many genuine passages not to be found in *F* and *I* see no sufficient reason to reject the present one.

On the martyrdom, in 316, of S. Blasius, bishop of Sebaste (Sivas), and on his tomb at Sivas, cf. *Y, I, 45*. The church of S. Blasius at «Sebaste» is mentioned by Rubrouck, who could not visit it, *quia erat sursum in castro* (*Wy, 327-328*). Vincent de Beauvais (ed. MENTELIN, 1473, XXXI, CXLII) speaks of the martyrdom of «Sanctus Basilius» at «Sauastia sine Sebasté». Among the Slavs, a god Volos or Veles is supposed to go back originally to Saint Blasius (cf. W. KOPPERS, *Die Indogermanen- und Germanen-Frage* [1936], 671).

## 73. BOGA

*bacha, dabaga* (?) V  
*baga* F, V (?)

*bagha* TA<sup>1</sup>, TA<sup>3</sup>

*boga* F, Z, L

«Boga» stands for Mong. Buqa, Buya; the word means «bull»; the pronunciation with *-u-* is used elsewhere by Polo (see «Tolobuga»). Buqa, a Jalair, was the most powerful official in Persia at the end of Abaya's reign and during that of Aryun. Cf. *Ha<sup>1</sup>, II, 408*; *Ha<sup>1</sup>* gives occasionally «Bugha». In such names, the usual Chinese transcription, 不華 Pu-hua, can represent Buqa as well as Buya; Rašidu-'d-Dīn generally writes بۇ, Buqa (cf. *Ber, I, 40*). In the beginning of 1286, Buqa received from the Mongol Emperor of China the title of *ch'êng-hsiang*, «minister» (see «Cingsan»), and he appears as «Buga-činksan» in Armenian texts (cf. *Ha<sup>1</sup>, I, 374*; PATKANOV, *Istoriya Mongolov, I, 52, 89-90*).

## 74. BOLGANA

*balchana* VA  
*balgana* F, L  
*balgaria, balgaris* P  
*balgonia* VL

*bolcara* FA  
*bolgana* R  
*bolgara* F, FA, FB, LT, VB  
*bolgaria* LT

*bolghara* TA<sup>1</sup>, TA<sup>2</sup>  
*bulgara* VB  
*bulgera, burgara* V

The final *a* is in all mss.; but most mss. confound the name with that of «Bolgara» (*q. v.*), and the possibility should not be excluded that Polo really used «\*Bolgan», which was soon contaminated by «Bolgara». Anyhow, the name is certainly Mong. Buluyan, «sable» (not «ermine» as in BLOCHET, *Moufazzal*, 600), but pronounced with the second vowel unaccented and slurred. The Persian transcriptions بلغان, sometimes بلغان (cf. BROWNE, *Hist. of Pers. lit.*, III, 25-26), can be read Bulyan (Bulqan) as well as Buluyan; but Waśśāf (*Ha*<sup>2</sup>, 249) writes بولغان; Buluyan, and so occasionally does Rašīdu-'d-Dīn; cf. also QUATREMÈRE, *Hist. des Mongols*, 95; VULLERS, I, 259. The Chinese transcription of the name (for princesses other than the one here mentioned) is 卜魯罕 Pu-lu-han, «Buluyan» (for instance for the Bayaut, Buluyan, Empress of Tāmūr Ūljāitū, *YS*, 106, 2*b*). The Turkish word for «sable», *Az*, is listed as a woman's name by Kāšyārī in 1076 (BROCKELMANN, 241).

Buluyan was the daughter of Kōkā (?), Secretary of State to Hülāgū, and belonged to the tribe of the Bayaut (cf. *Ber*, I, 177; *Ha*<sup>1</sup> constantly mixed her up with another Buluyan, and wrongly says that she was a Qonyrat; HAMMER's error is repeated in HOWORTH, III, *passim*; cf. also BLOCHET, *Sultans Mamlouks*, 599). After being Abaya's wife, Buluyan was taken over by Aryun, and died on April 7, 1286 (*Ha*<sup>1</sup>, I, 374). See «Cocacin».

HAMMER's indications on the three Buluyan who married *ilkhans* are so contradictory that it may be of some use to state the facts here briefly: 1. The first Buluyan, Abaya's and Aryun's wife, was a Bayaut. 2. The second Buluyan, taken successively by Aryun, Gāihātu and Ghazan, was a Qonyrat. 3. The third Buluyan, the one who was called Ḥorasānī, and married to Ghazan, was a Tatar. On the three Buluyan, cf. also BLOCHET, *Moufazzal*, 599-600, but the restorations of the names of the parents are open to serious doubt.

## 75. BOLGARA

*balgara* VA  
*bolgara* F, FA, FB, L; R  
*bolghara* TA<sup>3</sup>

*bulgara* O  
*bulgaria* VL

*buorgara, burgara* V  
*burgala* LT

The Oriental form is Bulyār; cf. *EI*, *s. v.* «Bulghār», by BARTHOLD; add BROCKELMANN, *Kāšyārī*, 242; J. NÉMETH, *Magna Hungaria*; *Mi*, 319, 438-439, and *Index*, p. 488; FERRAND, in *JA*, 1925, II, 117, 269-270; *Y*, I, 6-8.

All Polo mss. have a final *-a*, the origin of which is not clear; the Catalan Map writes «Borgar» without *-a* (HALLBERG, 80-81; but we should expect either «Bolgar» or the metathetic «Borgal»). Besides Bulyār, which is the ancient form, a secondary form «Bular» or «Bolar» occurs in Mongolian in the *Secret History* (§§ 262, 270); it is that form which is represented by Plan Carpine's «Bileri» (plural form; cf. *Wy*, 73, 138); in the first half of the 14th cent., Abū'l-Fidā writes «Bolar» or «Bular» (REINAUD, *Géogr. d'Aboulféda*, II, 81, 284, 323-325); Schiltberger also mentions «Bolar». I explain in the same way Fra Mauro's «Boler», misunderstood as «Bolor», *q. v.*, in HALLBERG, 74. On the Chinese map of *c.* 1330 and in the corresponding list of *YS*, 63, 16*a*, we find the somewhat abnormal spelling 不里阿耳 *Pu-li-a-êrh* (cf. *Br*, II, 81, 84), which seems to represent a \*Bul'ar, intermediary between Bulyār and Bular or Bolar. The 波臘 *Po-la* quoted from the *Hsin T'ang shu* (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 140, 353) by T'u Chi, 160, 19*b*, can have nothing to do with the name «Bulyār»; it is an ancient \*Puâ-lâp, hitherto unidentified.

The place mentioned by Polo is the city of Bulyār, the capital of the Volga Bulgars of the Middle Ages; its ruins, at Uspenskoe or Bolgarskoe, lie four miles east of the Volga and some 90 miles south of Kazan.

From the name of the Volga Bulgars, or rather of their capital, the people of the Middle Ages derived the name of what we call Russian leather, to wit بُلْيَارِي *bulyārī*. It is the word which is written «bourgal» or «borgal» in FA, FB. But the metathesis is not of Western origin, since Ibn Battūtah writes *boryāli* (or *buryāli*). Cf. *Y*, I, 395-396; YULE, *Hobson-Jobson*<sup>2</sup>, 125; VULLERS, I, 258; RADLOV, IV, 1850, «bulyar» and «bulyarī»; see also here under «Camut». An identical metathesis occurs in Osm. Turkish for the name of the «semolina» which is both *buryul* and *bulyur*.

## 76. BONDOCDAIRE

(nomine) andonch bondoc dairec	bandorquedar FA	bondoc daire F
L, L <sup>1</sup>	bendocquedar FB	bonduch daire LT
bandoc daire TA <sup>3</sup>	benhochdare R	

Zāhir Ruknu-'d-Dīn Bāibars al-Bunduqdārī, the fourth of the Mamluk Sultans, reigned from 1260 to 1277 (not 1259-1276 as in *Y*, I, 24, and *B*<sup>1</sup>, 439). On this remarkable man, originally a Turk brought from Qīpčaq as a slave, cf. *Y*, I, 23-24; *EI*, *s.v.* «Baibars I», by SOBERNHEIM (the Armenians have a different tradition about him; cf. BROSSET, *Hist. de la Géorgie*, I, Add., 459-460).

There is no doubt that Bunduqdārī, al-Bunduqdārī, is an epithet of appurtenance, due to the fact that Baibar's first master was a *bunduqdār* (cf. similar names in QUATREMÈRE, *Hist. des Sultans Mamlouks*, I, I, 107; I, II, 44; BLOCHET, *Moufazzal*, 142, 150, 315, 414, 685, 703), and



it is the form with a final *-ī* which is regularly used in Arabic texts written in Egypt. But *Bunduqdār*, without the final *-ī*, often occurs in Persian texts for the name of Baibars (cf. for instance *Waśśāf* in *Ha*<sup>2</sup>, 165, or *Rašidu-'d-Din* in QUATREMÈRE, *Hist. des Mongols*, 346, 387, and *Bl*, II, 551), and this is the form represented by Polo's «Bondocdaire». Abu'l Faraġ (*Hist. dynast.*, POCOCKE's ed., text 538, transl. 351) speaks of Baibar's former master as «the great *Bunduqdār*», and of Baibars himself as «the little *Bunduqdār*»; later, he invariably calls Baibars simply «*Bunduqdār*». The Armenians call Baibars «*Phenduġtar*»; the Georgians, «*Phunduqadar*» [? corr. «*Phunduqdar*»] (cf. PATKANOV, *Istoriya Mongolov Magakii*, 41; BROSSET, *Hist. de la Géorgie*, I, 586). Hethum uses «*Bendocdar*» (*Hist. des Crois.*, Arm., II, 240, 353), which is also the form used in the *Gestes des Chiprois* (*ibid.* 754-756) and in the *Annales de Terre Sainte* (*Arch. de l'Orient latin*, II, II, 449, 451-453, 455, 457). Pegolotti mentions «*Benducchodara*» (EVANS's ed., 133, where a reference is also given to an Italian chronicle which speaks of «*Bethogar*» or «*Betgodar*»). So the name used by Polo for Baibars is really the only one under which Baibars was known outside of Egypt.

YULE says *bunduqdār* means «arblasteer»; HAMMER (*Ha*<sup>1</sup>, I, 203) has translated the word by «bow-holder» («*der Bogenhalter*»); «arblasteer» appears again in GROUSSET, *Hist. des Croisades*, III, 607. But D'OHSOON (*Oh*, III, 348) has a note according to which *bunduqdār* was the title of an official whose charge consisted in handing the ball to the sultan when the latter played polo; PATKANOV, *Istoriya Mongolov Magakii*, 102-103, after quoting D'OHSOON, adds that HAMMER's «*Bogenhalter*» is «of course» wrong. The *EI*, under «*Bunduqdār*», refers the reader to the notice on Baibars, but there the epithet of *Bunduqdāri* is not even mentioned.

*Bunduqdār* is a hybrid word, composed of Arabic *بندق* *bunduq*, and Pers. *دار* *dār*, «bearer». *Bunduq* itself is an arabized form corresponding to Pers. *فندق* *funduq*, in Turk. *funduq* and *fındıq* («fenduc» in KUUN, *Codex Cumanicus*, 125), the original meaning being «filbert», hence the pellets thrown by a cross-bow (> Span. *bodoque*), then the «cross-bow» itself, and finally, in modern use, «musket» (> Hindustani *bandūq*, «musket»; cf. YULE, *Hobson-Jobson*<sup>2</sup>, s.v. «bandook», where the editors of 1903 have added that the *bunduqdār* was a «master of artillery»). But there is no corresponding Persian title such as \**funduq-dār*, and analogy shows that *bunduqdār* is certainly a Court title of the Egyptian Mamluk dynasty, like *bašmaqđār*, *dāwātdār*, *ġogandār*, etc., all applying to people whose office was to hand something to the sultan. The *bunduqdār*, consequently, could not be simply an «arblasteer», and there is no mention of him in QUATREMÈRE's long notes on balistae, cross-bows, etc., *Hist. des Mongols*, 285 sq.

But this does not imply that what the *bunduqdār* handed to the sultan was the polo ball. In QUATREMÈRE's quotations on Polo (*Hist. des Sultans Mamlouks*, I, I, 121-132), the ball is never called *funduq* or *bunduq*. The *Gestes des Chiprois*, which give the name of Baibars as «*Bendocdar*» (*Hist. des Crois.*, Arm., II, 754-756), say that he was the bearer of the «arc de mot» of the sultan, and that the «arc de mot» was called in Arabic «*caus bondoc*» (this would seem to imply that «*Bendocdar*» is a wrong reading for «*Bondocdar*», but «*Bendocdar*» is also the spelling of the name in Hethum and in the *Annales de Terre Sainte*, as has been said above). As a matter of fact, *قوس البندق* *qaūs al-bunduq* means «crossbow» in Arabic, and one would like to have had some authority for the note in the Glossary of *Hist. des Crois.*, Arm., II, 1000,

according to which the «arc de mot» (lit. «word-bow») was «a bow entrusted by the sultan, as a symbol of delegation of his power, to some one who was to carry out some order». I am almost inclined to believe that «mot» is wrong somehow, and that the *bunduqdār* was simply the bearer of the sultan's crossbow (cf. the *qorči*, «quiver-bearers», of Mongol history). As is shown by the above quotation, the *Gestes des Chiprois* mistake Baibars himself for a *bunduqdār*, while he was only al-Bunduqdāri, the former servant of a *bunduqdār*.

One more word on *bunduq*, *funduq*. YULE says (*Hobson-Jobson*<sup>2</sup>, 127) : «*Bunduq*, pl. *banādīk*, was a name applied by the Arabs to filberts (as some allege) because they came from Venice (*Banadīk*, comp. German *Venedig*)». This derivation is impossible. The filbert, or hazel-nut, was known under that name in the Orient long before Venice had any trade there. Pers. *funduq*, Arab. *bunduq* are borrowed from the classical name (κάρπov) ποvτικόν, (*nux*) *pontica*, which had passed into Aramaean (cf. L. LECLERC, *Ibn el-Beithar, Traité des Simples*, in *Not. et Extr.*, 23 [1877], 1, 273; S. FRÄNKEL, *Aramäische Fremdwörter im Arabischen*, 139; *EI*, s.v. *funduk*), and is even known in Pahlvi (cf. WEST, *Pahlavi Texts*, 1, 103). On the other hand, the *bunduqī*, or «sequin», is said in *EI* to have been so called from «Bunduqiya» the name of Venice among the Arabs, with a reference to Abū-'l-Fidā; but REINAUD, *Géogr. d'Aboulféda*, II, 1, 309, transcribes البندقية as «Benedekyé», and the vocalization with two *u*'s does not seem to have a real basis. If the name of the *bunduqī*, «sequin», is really derived from that of Venice, it must at least have been contaminated by the *bunduq*, «filbert», of identical spelling. But one may entertain some doubts when the Osmanli Turks speak at the same time of their gold coins as *funduqlī* or *findiqlī*, which is explained in *EI* as probably arising from the outer circle of «pearls» of the coins being compared to filberts. I find it hard to dissociate *bunduqī* from *funduqlī*. Moreover, in Roumanian *funduk* is used alone as the name of an ancient coin (cf. LOKOTSCH, *Etymol. Wörterbuch*, Nos. 355, 617).

## 77. BONUS

*alboro* V  
*bonus* F  
*bonusso* LT

*ebano* TA<sup>1</sup>, TA<sup>3</sup>, VA, VL;  
 G, R  
*ebanus* L, LT, P, Z

*ebon* (?) VB  
*ybenus* FA, FB

Although we have retained the «bonus» of F, I am not certain that it is the best form, nor that YULE's remarks on that reading (*Y*, II, 272) are really pertinent. The word «ebony» goes back to Lat. *ebenus*, from Greek ἔβεovos, which in its turn is borrowed from the Egyptian *heben*, probably through a Semitic channel. The Arabo-Persian ابنوس *abnūs*, which YULE mentions, is a retranscription from the Greek. The word was fairly well known in the Middle Ages, in Italian as *ebano*, in French as *ebaine*, though the Latin form in *-us* is also met with. YULE quotes a French inventory where the word is written *ibenus*. In POLO's text, FA and FB read «ybenus». I have little doubt that the «bonus» of F is a wrong apheretic form, and that we ought to adopt

\**ibonus* or \**ibenus*. But this form is purely Latin, and there is no reason to suppose that it was used by Polo as an Oriental word, or sounded as such to Rustichello. For Oriental references to ebony, cf. *Fe*, 235-237 and 701.

In F, ebony is only mentioned as growing in Champa, but VB speaks of it for Siam («Lochac») and Z for Sumatra («Lesser Java»). All are ebony-producing countries. The ancient classical world received ebony from the Upper Nile and, at the beginning of the Christian era, from India; Indo-Chinese ebony was not known to it. On the other hand, from the first centuries of our era, China was acquainted with that Indo-Chinese ebony. But I cannot agree with LAUFER (*Sino-Iranica*, 485) when he wants to refer to a Malayan Po-ssü a text supposed to be of the 4th cent. which speaks of the transport of 烏文木 *wu-wên-mu* («*wu-wên* wood») on board Po-ssü ships. This text (for which, by the way, *T'ai-p'ing yu-lan*, 961, is a much older source than the *Pên-ts'ao kang-mu* quoted by LAUFER) is of doubtful date and authority, but Po-ssü ships, until about A. D. 1000, can only mean «Persian ships»; even African ebony could well come to China on board Persian ships in the 6th-10th cents.

*Wu-wên*, later 烏構 *wu-mên* and *wu-mên-tzũ* [子], were supposed by HIRTH and ROCKHILL (*HR*, 216) to render the same original as Persian *abnūs*. LAUFER must have been right when he maintained on the contrary that *wu-wên*, «[wood with] black streaks», and its later substitute *wu-mên* or *wu-mên-tzũ*, «black *mên*» were purely Chinese terms; a more common name of ebony is 烏木 *wu-mu*, «black wood» (*Sino-Iranica*, 485-486). Another old Chinese name of ebony, as a produce of Annam-Tonking, is 鑿木 *i-mu*, also written 翳木 *i-mu*; this is the name erroneously read *wo-i-mu* in *HR*, 216, and *i muk-i* in *Sino-Iranica*, 486.

## 79. BRAAMAN

*abraaman*, *braaman* Z

*abraiamain* F, Ft, L

*abraiaman* Fr

*abraiamanin*, *abraiemanin*,

*brahaman*, *habraiamain* L

*abraianim*, *abraiani* VA

*abraiemant*, *abriuamam*, *abrue-*  
*main* F

*abramains*, *abriuaman* FA

*abramains* FA, FA<sup>4</sup>, FB

*abrassanyni* G

*abrayani*, *abriayani* P

*abremani*, *breamani* V

*abriamani*, *briuiama* VB

*abriuamain* FB

*abriuamains* FA, FB

*blagmani* LT

*bragmani* VL

*bragmanos* S

*bramini* R

*bregghomanni* TA<sup>3</sup>

*breghomanni* TA<sup>1</sup>

*grecomanvi* TA<sup>1</sup>

YULE (Y, II, 367), under the influence of «Abraiamain» in F (and of a final *-min* which, as a matter of fact, occurs only in RAMUSIO's «Bramini»), had thought of «an incorrect Arabic plural such as *Abráhamín* . . .», and this, repeated in *Hobson-Jobson*<sup>2</sup>, 111, has been adopted in *Pe*, 249. But I have no doubt that the initial *a-* is of the same type of clerical alteration as in «Abacu» and «Amien». RAMUSIO's «Bramini» looks like a learned correction, but there are other readings without *a-*, and the form in Z is regularly «Braaman», which is approved of in B,

CLXVII. I have adopted it because I do not wish to invent a spelling of my own; but, taking into account the various readings, my own feeling would be in favour of an original spelling like «Bracmain». On *brāhmaṇa*, «brahman», and its various forms in Western languages, cf. *Hobson-Jobson*<sup>2</sup>, 111.

## 80. BRAZIL

*beçi* F  
*berci* TA<sup>1</sup>, TA<sup>3</sup>, Z; R  
*berçi* F, L, Z  
*berer* (*de mastica*) LT

*berty* G  
*berzi* VA  
*birci*, *lurci* P  
*oro dimesticho* TA<sup>1</sup>

*orzi* V  
*verçi* L  
*verzi* L, V, VB  
*verzin*, *verzino* R

Brazil, or brazil-wood, *Caesalpinia sappan*, was much used as a dye-wood in the Middle Ages; the oldest known mention is of 1140 (HEYD, *Hist. du commerce*, II, 587-590). The oft-told story that Brazil in America owed its name to the dye-woods discovered there is discounted by the earlier legend of an Isle of Brazil which was supposed to exist in the Atlantic (cf. Y, II, 380-381<sup>14</sup>; *Hobson-Jobson*<sup>2</sup>, s. v. «Brazil-wood»; DALGADO, *Glossário Luso-Asiático*, I, 149-150). LOKOTSCH (*Etym. Wörterbuch*, No. 190) says that the word «brazil» is still unexplained, and at the same time, No. 2157, agrees with MEYER-LÜBKE in deriving the Italian name of the brazil-wood, *verzino*, from Arab. *wars*, *Memecylon tinctorium*, a well-known saffron-like plant yielding a yellow dye (cf. LAUFER, *Sino-Iranica*, 315-316). But brazil-wood and *Memecylon tinctorium* have nothing in common; moreover, French *brésil*, *brasil*, and Ital. *berçi* (in F) or *verzino* cannot be distinguished. The old explanation of *brésil* from Fr. *braise*, «glowing coal», on account of the red colour of the dye, still doubted by MURRAY, is adopted in BLOCH's *Dict. étymol.*, s. v. «braise», and finds strong support in the dialectical forms of «braise» collected by VON WARTBURG, *Franz. Etym. Wörterbuch*, I, 506.

The ordinary Arabic name of brazil-wood is *baqam*, *baqqam* (< Pers. *bakam*, VULLERS, II, 254), also used in Persian and in Turkish (cf. *Fe*, 246; KUUN, *Codex Cumanicus*, 92, where Lat. «braçile» is rendered in Persian «bachā» [= «bacham»] and in Turkish «bachan» [for «bacham»]); forms borrowed from *baqqam* exist in Armenian (*Hist. des Crois.*, Arm., I, 750 «bayyam»), in Roumanian and in Russian (LOKOTSCH, No. 190). In Polo's inventory (cf. Vol. I, 557), mention is made of «chamocha bachami clapo. I. grando», «a big bundle of chamocha bachami». The «chamocha» is a damask (see «Camocas»), and «bachami» must be an epithet, in the same way as the «chamocha blava» mentioned in the text is «blue camocas». Now, among the names of colours of the *Codex Cumanicus* (KUUN, 108), there is in Latin a word «bachami», rendered identically in Persian and in Turkish; it is evidently a Persian adjectival form \**baqamī*, which means «of the colour of *baqam*», i. e. of reddish colour. I have no doubt that the «Latin» form of the *Codex Cumanicus* is a form which was actually used in Venetian Italian, and that we have

there the explanation of the term of the inventory; «chamocho bachami» is «brazil-dyed damask».

The modern trade-name of the Asiatic brazil-wood is «sappan-wood», hence the botanical name *Coesalpinia sappan*. After CALDWELL's unfortunate attempt to trace *sappan* back to «Japan» (cf. LAUFER, in *Y*, III, 119), the generally accepted theory is that *sappan* or *sapan* is probably based on Malay سڤان *sāpañ*, but that *sāpañ* itself goes back to Tamil *šappu* or Malayäl. *shappañnam* (*Y*, II, 380; *Hobson-Jobson*<sup>2</sup>, s. v. «sappan-wood»; DALGADO, *Glossário Luso-Asiático*, II, 290; MURRAY, *NED*, s. v. «sapan»; LOKOTSCH, No. 190). Yet I have no doubt that LAUFER was right when he claimed the word as being originally Mon-Khmêr and Malayan (in *Y*, III, 119). The modern Chinese name 蘇木 *su-mu*, «*su* wood», would not have been very enlightening, had not a more complete form 蘇枋 *su-fang* (\**suo-b'ɿ'ang*) occurred at an early date. The oldest work to mention and describe the *su-fang*, the *Nan-fang ts'ao-mu chuang* attributed to Hsi Han, is not free from interpolations, so that the date of c. 300 adopted by LAUFER is far from being certain in the present case; but the passage cannot be later than the 6th cent. Although the Malay word is *sāpañ*, and not *supang* as said in *HR*, 217, we can safely assume that *su-fang* is a transcription of a form very near Khmêr *sbañ*, Malay *sāpañ*, etc. The early notice on *su-fang* gives it as a tree of northern Annam, and Chau Ju-kua says that the «*su* wood» comes from Cambodia. If there is a connection between the Indo-Chinese and Malayan name and some Indian forms, the word must then have travelled from Indo-China to India, but not *vice-versa*. KERN (*Itinerario . . . van Linschoten*, I, 83), commenting on LINSCHOTEN's «sapon», says that Mal. *sēpañ*, Jav. *sēpañ*, are probably derived from old Jav. *sapañ*, «red».

Polo mentions brazil-wood as a product of «Lochac», of «Lambri», of Ceylon and of «Coilum»; in the chapter on «Lochac», he says it was cultivated (*domesce*; see «Lochac»). Pegolotti also distinguishes the «*verzino dimestico*» and the «*verzino salvatico*» (EVANS, 295, 296), in the same way as he mentions «cultivated» and «wild» cardamoms, «cultivated» and «wild» cubebs (EVANS, 294). Moreover, Pegolotti speaks of brazil-wood «*ameri*», «*colonmi*» (or «*colombino*») and «*sieni*» (or «*seni*»). «*Ameri*» is probably for «*Lameri*» = «*Lambri*», as said by YULE and HEYD (cf. EVANS, 433), and «*verzino colombino*» is of course «brazil-wood from Coilum». For «*Sieni*» or «*Seni*», both YULE (*Y*, II, 380) and HEYD (*Hist. du commerce*, II, 589) agree in supposing that the term refers to brazil-wood brought to India by Chinese traders; «*Sieni*» or «*Seni*» would be the same as Arab. *Sinī*, «Chinese». Of course, there are other products which have been called «Chinese», although they were not indigenous to China (cf. LAUFER, *Sino-Iranica*, 543-544). Of the four sources of brazil-wood mentioned by Polo, two remain available for the «*Sieni*» sort: Ceylon and Lochac (= Siam). I do not think that «*Sieni*» or «*Seni*» can represent a corrupt form of «\**Seilani*», Ceylonese. On the other hand, in Pegolotti's time (c. 1340) the conquest of the lower Menam had not yet been achieved by the Siamese, whose name was still unknown in the West; consequently, «*Sieni*» cannot be an approximate rendering of «\**Siamī*». Without much conviction, however, I adhere to YULE's and HEYD's hypothesis, but at the same time suppose that, by «*Sieni*» or «*Seni*», the traders of the early 14th cent. understood the sappan-wood produced in Siam, Cambodia and Annam, that is in regions which, being coterminous with China or within the area of Chinese influence, could be loosely designated Chinese.

## 81. BREGANEGA

*bregane* VL*breganega* V*breganege* VL, It

This word unattested otherwise, but occurring on three occasions in different Polian mss., certainly refers to the bamboo (the word «bamboo» was not known before the middle of the 16th cent.; the equivalence *wambasium* = bamboo in *Pa*, 327, although repeated in HALLBERG, 63, is of course wrong; see «Cotton»). The etymology is not known. I agree with PENZER (*Pe*, 208) in rejecting the Persian form *\*bargānaya*, «leafy», proposed by Sten KONOW, or the Hindustanī *bargā*, «poutre». PENZER himself, who knew only the «berganegas» occurring in SANTAELLA's Spanish version and in the English translation made from the Spanish by FRAMPTON, thought of a corruption of *\*bengalega*, a form possibly derived from Span. *caña de Bengala*, «cane of bamboo». Such a sense of *caña de Bengala* in Spanish, *cana de Bengala* and also *bengala* alone in Portuguese is well attested (cf. the examples in DALGADO, *Glossário Luso-Asiático*, 1, 116-117; the word is not in LOKOTSCH, *Etym. Wörterbuch*), but *breganega* recurs in too many instances to be regarded as being altered from a hypothetical *\*bengalega*. Moreover, «caña de Bengala» and «bengala» are purely Spanish and Portuguese terms, not older than the 16th cent., which can have no bearing on the etymology of an Italian mediaeval word. PENZER's mention of DIEZ, *Etymol. Wörterbuch der roman. Sprachen*, without indication of catch word or of page, seems to refer only to *cana de Bengala*. Venetian *brega*, «net», leads us nowhere. One might think of German *\*brekan*, «to break», from which several Provençal forms beginning with *brega* are derived (cf. VON WARTBURG, *Franz. Etym. Wörterbuch*, 1, 511); the name may be attributable to the «crackling» of the bamboo under the action of fire. But I am afraid this is only a wild guess.

## 82. BRIUS

*briuis* VA*brunis* TA<sup>1</sup>, TA<sup>3</sup>*ligays* LT*brius* F, FA, FB, P, L, VB,*brus* Z

VL, R, S

Here practically all the mss. agree (except that a reading «Brins», the only one given in *Pa*, 386, is sometimes possible according to B<sup>1</sup>, 439, and Z has «Brus»); and the name can only apply to the Upper Yang-tzü, before its confluence with the Min River at Hsü-chou (the Sui-fu of our maps).

Writing about 1730, Orazio DELLA PENNA gives to the Upper Yang-tzü the name of «Bic'iu»,

which he owes to a lost letter of VAN DE PUTTE. According to ROCKHILL, *Land of the Lamas*, 196, both «Brius» and «Bic'iu» represent Tibetan «Dré ch'u»; but ROCKHILL denies the earlier assumption that this name means «Cow-yak River», as cow-yak «is *dri-mo*, never pronounced *dré*, and unintelligible without the suffix *mo*. *Dré* may mean either mule, dirty, or rice, but as I have never seen the word written, I cannot decide on any of these terms, all of which have exactly the same pronunciation.»

This note of ROCKHILL, reproduced in *Y*, II, 67, is not correct. It is true that Sarat CHANDRAS DAS, 930, registers only 'bri-mo, «cow-yak», but C. A. BELL's *English Tibetan Colloquial Dictionary*<sup>2</sup>, 558, gives merely 'bri, pronounced *dri*, without *mo*. On the other hand, our dictionaries do not know a Dre-čhu, «Mule River», but only a 'Bri-čhu, «Cow-yak River», pronounced Dri-č'u. The current explanation, which goes back to KLAPROTH (*JA*, September 1834, 191) and is based on Ch'ien-lung's polyglot dictionary of proper names, the *Hsi-yü t'ung-wên chih* of 1772 (where, however, I have not succeeded in finding it), holds good; Orazio DELLA PENNA's «Bic'iu» is really 'Bri-čhu, and based on a pronunciation less altered than in the present Lhasa dialect. The only difficulty is that the quotation made by Sarat CHANDRA DAS under 'Bri-čhu refers not to the Upper Yang-tzū, but to the Mekong, as can be seen by the course indicated and by the Chinese name Lan-ts'ang-chiang (I do not know what to make of its «Indian» name «Kaswokha»); there must be here some confusion due to Sarat CHANDRA DAS or to his source (on similar confusions for the name of rivers coming down from Tibet, cf. BEFEO, IV, 170). As to «Dre-čhu», which is also the pronunciation given by TEICHMAN, as for instance in *Geogr. Journal*, January 1922, map, it may be the local pronunciation now, but that does not affect the older form of the name (the «Dütschu» in GANZENMÜLLER, *Tibet*, 44, is certainly wrong). It would take too long to deal here with the ancient Chinese names of the Upper Yang-tzū, but it seems to be the 犛牛江 Li-niu-chiang or «Yak River» of the T'ang period (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 169, 344), and li-niu, «yak», is likely to be the first part of the name «Li-niu-shih» (noted in 1630) in *New China Review*, III, 337 (I intentionally read 犛 *li*, and not *mao*, as I think that the second pronunciation is due to the contamination, already in the T'ang period, of the popular name 毛牛 *mao-niu*, «hairy ox», of the yak, which then gave birth to the character 牦 *mao*; it even seems quite possible to me that the *li* of *li-niu*, yak, should be phonetically connected with, and perhaps borrowed from, an old form of Tib. 'bri).

HUC, who calls the river by its Mongolian name, which will be discussed below, adds that the Tibetans call it «Polei Tchou», or «River of the Lord». HUC may have actually heard this, but it is also possible that he took it from KLAPROTH's edition of Orazio DELLA PENNA, as KLAPROTH adds in a note that very name of Pho-lha'i-ch'u, «River of the Male God», found by him (KLAPROTH) in the *Hsi-yü t'ung-wên chih* (21, 2 b, and 21, 11 a, but without any information beyond the mere name).

It was also KLAPROTH who revealed the Mongolian name of the river, Murus-usu, or Murui-usu (again from the *Hsi-yü t'ung-wên chih*, but I do not find it there). In his note, ROCKHILL adds : «The Mongols call it *Murus osu*, and in books this is sometimes changed to *Murui osu*, 'Tortuous river'. It is true that Murui-usu means «Tortuous river» in Mongolian, but the form

cannot be purely bookish, since it was the one actually heard by HUC («Mourouf-Oussou»). As to «Murus», it conveys no meaning, and I take it as an unexplained proper name.

We can now come back to Polo's «Brius». As in the case of «Beyamini», I doubt very much whether Polo, in South-western Ssü-ch'uan, would have picked up any really Tibetan name; his nomenclature must be, as elsewhere, Persian, Mongolo-Persian or Sino-Persian. Now «Brius» cannot well be reduced to 'Bri-čhu, unless we replace Tib. *čhu*, «water», by the corresponding Mongolian *usu*, of which Polo would then have used an apocopated form. This hybrid is very improbable. There being a Mongolian name Murus for the Upper Yang-tzū, and taking into account the frequent changes between *m*- and *b*-, I incline to the supposition that it is Murus which is disguised under «Brius», and perhaps Z's «Brus» (< \*Burus ?) is nearer to the form Polo actually used.

There is nothing to learn from the description of Tibet which we owe to the Minjul-χutuqtu and which has been translated by VASIL'EV, as it is content with mentioning the Upper Yang-tzū only once (VASIL'EV, *Geografiya Tibeta*, 3), and under the name kLuñ-gser-gyi pnye-ma-čan, which is a simple Tibetan equivalent of the Chinese Chin-sha-chiang, «Golden Sand River».

After he had published and annotated in 1824 the notice on Tibet left by Orazio DELLA PENNA, KLAPROTH published in 1826 in his *Magasin asiatique*, I, 302-329, a paper on the Brahmaputra which he says he had written in March 1825, and to which he has added a map, the names on which, according to him, are spelt in agreement with the *Hsi-yü t'ung-wên chih* of 1772. On that map the Upper Yang-tzū appears as «Bourei tchou ou Ba tchou ou Kin cha kiang». «Ba tchou» (written «Be tchou») and «Kin cha kiang» were already on D'ANVILLE's map, but not «Bourei tchou». This last name does not seem to represent Pho-la'i-čh'u, but would rather be a pronunciation with an initial *b*- of Murui(-usu); unfortunately I do not know where the name has been found by KLAPROTH, who is not always reliable. If its origin could be traced elsewhere, it would support my explanation of Brius by Murus.

The second edition of «Marco Polo», which was the last to be revised by YULE himself, was published in 1874, but in 1880, YULE contributed an important preface to Captain GILL's *River of Golden Sand*, and has there a paragraph on «Brius» which CORDIER has not made use of, but which is reproduced in Sven HEDIN, *Southern Tibet*, VII, 8. After quoting the Mongol name «Murui-ussu, or Murus-ussu», and the Tibetan name «Di-chu, or Bhri-chu», YULE adds «from one or the other of which Marco Polo seems to have taken the name Brius». Once more, YULE had had some intuition of what I believe to be the true explanation of Polo's form.

I suspect that the 畢力朮江 Pi-li-shu-chiang, «Pi-li-shu river», of *Ming shih*, 330, 6 a, 7 a, and 331, 8 a, is the 'Bri-čhu, and think that the restoration «Bilidju» in *Br*, II, 335, should be rejected. *Chiang* is only used for a large river, and especially for the Yang-tzū (see «Quian»). On the other hand, the character 朮, although pronounced *shu* in Northern Mandarin now, was a transcription of the *ju* and *čü* of foreign words in the Mongol period. Our texts bear on the first half of the 15th cent., when the same phonetic value is probably still to be adopted for this word. If I am not mistaken, we must conclude that, at that date, either the pronunciation of 'Bri-čhu had not yet passed to Dri-č'u, or the change of pronunciation was not yet general.



## 83. BUCARA

*bacarch* VB  
*bacchara* TA<sup>3</sup>  
*bocara* FA, FB; R  
*boccara* F

*bochara* LT, P, VA  
*bochora* VL  
*bothara* P<sup>5</sup>  
*bucara* F, Fr, t

*buccara* L  
*buchara* V  
*bucharra* Z

This is of course Bokhara. On بۇخارا Buḫārā, cf. the excellent notice by BARTHOLD in *El*, s. v. «Bukhārā»; also *LS*, 460-463; *Mi*, 352 (and Index, 487). The modern Türkmen form is Buhāra.

The name has been supposed to come from a form \*Buḫār, which would be derived from Skr. *viḥāra*, « [Buddhist] monastery », and which occurs as *buqar* in Uighur and Mongolian, though the Sogdian form, which is the one one should expect to be used at Bokhara, was *varḥār* (the derivation of *varḥār* from *viḥāra*, which had been abandoned, is again maintained by HENNING in *BSOS*, IX, 570). *Viḥāra* > *buqār* is of the same type as *viṣaṇa* > *buṣan*, Śrīvijaya > Śrībuḷai, etc. I must add, however, that early Uighur texts only know *vrḥār* and *viḥār*; *buqar*, in a Uighur vocabulary of the Ming period (not « Puyar » as in RADLOV, IV, 1362), may be simply borrowed from the Mongolian. The word *viḥāra* occurs mainly in Iranian countries in the term نوبهار Naubihār or Nōbihār (< \*Navaviḥāra, « New Monastery »), which was later read Nōbahār and interpreted as meaning « New Spring » or simply « Spring ». The most famous Naubihār was the one at Balkh which Hsüan-tsang, c. 630, calls Navasaṅghārama, « New Monastery »; but there were a number of other Naubihār in Sind, at Samarkand, at Bokhara, and even at Ray (east of Teheran). I must confess that I do not see why we have only « New » monasteries in Iran; on them, cf. Stan. JULIEN, *Vie de Hsüan-tsang*, 65; *Mém.*, I, 30; BARBIER DE MEYNARD, *Dict. géogr.*, 112, 569; YULE, *Hobson-Jobson*<sup>2</sup>, 630, 967; ELLIOT, *Hist. of India*, I, 149, 195; MARQUART, *Ērānšahr*, 69, 91, 138; BARTHOLD, *Turkestan*<sup>2</sup>, 77, 85, 86, 102; *Mi*, 108, 337. On the whole, it has not been proved that Bokhara had anything to do with *viḥāra*.

Bokhara occurs first in Chinese texts in the 5th cent. as 柁密 Niu-mi (\*Nj̄u-mj̄ēt), a regular transcription of its ancient name Nūmīj, Nūmīj-kāθ (*Wei shu*, 102, 5 b). When the name of the town of Bokhara makes its appearance, it has from the start the same form Buḫārā both in Arabic and Persian texts. In the 8th cent., the Turkish runic inscriptions of the Orkhon write Buqaraq (with an ancient Iranian final *-k* or *-g*), but, curiously enough, Hsüan-tsang and the other Chinese sources of the 7th-10th cents. give transcriptions which are based only on \*Buḫār, and this is also the form supposed by other transcriptions in the Mongol and even in the Ming periods. So we have 播喝 Pu-ho (\*B'uo-χât) in Hsüan-tsang (JULIEN, *Vie*, 61; *Mém.*, I, 21); 布豁 Pu-huo (\*Puo-χuat, \*Puḥwar?; cf. *BSOS*, IX, 549) and 捕喝 Pu-ho (the latter form is clearly taken from Hsüan-tsang) in *Hsin T'ang shu*, 221 B, 1 b (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 136, 355; the « Pu-hwat-lu [= Buxār?] » of MARQUART, *Ērānšahr*, 309, does not exist);

蒲花羅 P'u-hua-lo in Chau Ju-kua (*HR*, 117; in a documented list which he reproduces and which came by sea, not through Central Asia); 蒲華 P'u-hua in Yeh-lü Ch'u-ts'ai's account of 1229 (cf. *Br*, I, 22); «Buqar» in the *Secret History* in Mongolian (§§ 257, 259, 263); «Buḥar» in Kalmuk epic legends (cf. VLADIMIROV in *Doklady Ak. Nauk*, 1929, 171); 卜哈兒 Pu-ha-êrh (=Buqar) in *YS*, I, 8 b, s. a. 1221; 李哈里 Po-ha-li in *YS*, 120, 1 a; 不花刺 Pu-hua-la (=Buḥārā) in the map of c. 1330 (of Mussulman origin) and in the corresponding list of *YS*, 63, 15 b; 卜花兒 Pu-hua-êrh in Ch'ên Ch'êng's *Hsi-yü fan-kouo chih* (Peiping National Library ed., 20 b), and in *Ming shih*, 332, 4 b, with special reference to the embassies of Ch'ên Ch'êng in 1414 and of Li Ta in 1432 (cf. *Br*, II, 147); 卜哈刺 Pu-ha-la in *Ming shih*, 332, 12 b (cf. *Br*, II, 271). T'u Chi (160, 4 a) adds the 八瓦兒 Pa-wa-êrh of *YS*, 123, 3 a, but this transcription seems to refer to some other unidentified place, perhaps in Chinese Turkestan. The Chinese forms supposing «Buqar» have Western counterparts in «Bocar» of the Catalan Map (here independent of Polo), «Bochar» of the Medici Map and «Boyar» (read «Bojar», with Spanish *j* = *h*) of Clavijo (*HALLBERG*, 80, 82; *SREZNEVSKIĪ*, *Clavijo*, 422).

The form «Buqar» of the *Secret History*, of Yüan and Ming transcriptions, and «Bocar», «Bochar», «Boyar» of some Western sources leave no doubt that, in the Middle Ages, the Uighur-Mongolian form of the name was «Buqar», not Buḥārā. The transcriptions in the *Hsin T'ang shu*, and still more the one used by such an accurate phonetician as Hsüan-tsang, tend to show that «Buqar» was already a current form of the name early in the 7th cent.; the «Buqaraq» of the Orkhon inscriptions stands to «Buqar» in the same relation as their Soydaq does to Soyda. It is true that Kāšyārī, in 1076, wrote Buḥārā (*BROCKELMANN*, 242); but it is only natural that a Turkish author who was a Mussulman, when writing in Arabic, should use the Arabic spelling; the very use of *-ḥ-* is in itself an indication that he is not giving an actual Turkish form. Moreover, the title of the Buḥār-ḥudāt («Lords of Buḥārā») who ruled at Bokhara prior to the arrival of the Arabs, also shows a form of the name without a final vowel. And, right or wrong, the explanation of the name by *buqar, viḥāra*, which goes back at least to the 13th cent. (in Jūwainī), supposes the form «Buqar» or \*«Buḥār». This form survives to-day in بوخار Boḥār, the name of Bokhara in the Turkish dialect of Kazan (*RADLOV*, IV, 1707).

In the West, the form with a final *-a* occurs in Fra Mauro's «Bochara», which may be dependent on Polo, and in Hethum's «Bocara» (*Hist. des Crois.*, Arm., II, 126, 266). In what appears to be the earliest Western text to mention Bokhara, *i. e.* the document of 1221 studied by ZARNCKE, the name occurs in the first version as «Bachara», with an occasional reading «Bochar», as «Bocara» in the second version, and «Bocharā» in the third (*ZARNCKE, Der Priester Johannes*, 49, 50, 57, 58). As the document is a translation from the Arabic, the true form was certainly «Bocara» (or «Bochara») everywhere. These Western transcriptions, as well as those without a final *-a*, show an *-o-* in the first syllable. I have decided for *-u-* in Polo's text, because it is given by Z and V, and because it also occurs in F; but both letters interchange so often in manuscripts that I am not positive as to the form which Polo actually used. That Polo should use «Bucara» and not a Turkish or Mongolian form without a final *-a* is in agreement with the general Persian character of his nomenclature.

When the elder Polos reached Bokhara, the city belonged to Hülägü (see «Barac»).

## 84. BUCEFALO

*bucefalo* R*bucifalch* Z

The name occurs only in R and Z, and I have adopted the form given in R, although it ought not to appear among our « French » forms of Western names; we should expect « Bucefale ». Z's « Bucifalch » must have come from a misreading \*Bucifalc for a French \*« Bucifale » or for an Italian \*« Bucifalo ».

On the Kataghan breed of Badaḥšān, cf. *Y*, I, 162.

An Alexandrian city of Bucephala existed on the western bank of the Jelam (*Y*, I, 104-105). On the other hand, the belief still obtains in the British garrisons of the Panjab that the Buddhist *stūpa* of Manikyala is nothing else than the tomb of Bucephalus. For Bucephala, cf. HALLBERG, 84. In Fra Mauro's maps there are two notices of the city of « Bucefala », in different scripts, one mentioning Alexander's « chaval », and the other his « caualo ».

## 85. BUCKRAM

<i>bocaran, bocarans, bocorain,</i>	<i>boccorame, bocharame, bocha-</i>	<i>bucarane, bucaranum, burca-</i>
<i>bocoranç</i> F	<i>ramine, bochorame, boco-</i>	<i>rani</i> G
<i>bocarani, bocarini, bocharani</i>	<i>rame</i> LT	<i>bucaranus, buccaramus, buccu-</i>
L, VB	<i>bocharano</i> VA	<i>ranus</i> P
<i>bocassini (e bocariri)</i> VB	<i>bochasin</i> V	<i>bucerani</i> TA <sup>1</sup>
<i>boccarano, bocharaui, bocharini</i>	<i>bochasini</i> V, VA	<i>bucherame</i> TA <sup>1</sup> , TA <sup>3</sup>
L	<i>boguerans</i> FB	<i>bucherani, bucheranum</i> Z
<i>bocassini, bochassini</i> R	<i>bouguerans</i> FA, FA <sup>4</sup> , FB	

Polo mentions « buckram » at Arzingan, at Mosul, in Tibet, at « Mutifili » (= Telingana), in Malabar, at Tana (near Bombay), at Cambay, and in Abyssinia. Two questions must be examined, the nature of the stuff and the origin of the name.

Although we must retain the word used by Polo, YULE already pointed out (*Y*, I, 47-48) that the mediaeval « buckram » was a valuable material, not as now « a coarse open texture of cotton or hemp, loaded with gum, and used to stiffen certain articles of dress ». While hesitating between cotton and linen, HEYD (*Hist. du commerce*, II, 703) added that the last scholar to have examined the question, Victor GAY, had come to the conclusion that the ancient « buckram » was « a fine linen-cloth »; and the quotation from FRANCISQUE-MICHEL (*Recherches sur le commerce, la fabrication et l'usage des étoffes de soie, d'or et d'argent...*, II, 1854, pp. 29-34) reproduced in *Y*, III, 80-81, would seem to imply that, according to FRANCISQUE-MICHEL, this « buckram » was made of linen. But this incomplete quotation does not do justice to FRANCISQUE-MICHEL, who, while he said that « buckram » was made of linen in the 14th cent., insisted that in the 13th it was a cotton-cloth. From the association of cotton and « buckram » in several passages of Polo,

I have no doubt that for him «buckram» was actually a cotton-cloth. YULE may be right when he says that in many cases «buckram» was quilted cotton. This would be hardly reconcilable with the passage in which YULE makes Polo say that the Mutifili «buckrams» are so delicate that «they look like tissue of spider's web» (*Y*, II, 361). But this translation, still repeated by EVANS (*Pegolotti*, 415), must be abandoned; Polo speaks in fact of «linen-cloth of Rheims» (*telle de lino de Rens*; see «Rens»). I see no reason to favour MARSH's suggestion, quoted by YULE, that «probably two words have coalesced» in «buckram».

The mediaeval forms which are at the basis of modern French «bougran», It. «bucherame», Engl. «buckram», are many. To judge from the finals, it would seem, as is said in MURRAY's *NED*, that the word was not adopted in English from the French, but from the Italian. The Spanish «bucarán» agrees with the French.

There is still some hesitation about the etymology of «buckram». REISKE's *abū-qirām*, rendered «*pannus cum intextis figuris*», is an artificial compound, without any textual support. D'AVEZAC derived «bougran» from «Bokhara» (*Rec. de voyages*, IV, 524); this was accepted by HEYD (II, 703) and is adopted without reserve by LOKOTSCH (*Etym. Wörterbuch*, No. 342), and by BLOCH, *Dict. étymol.*, I, 92. YULE esteemed that «if the name be local, like so many names of stuffs are, the French form rather suggests *Bulgaria*» (*Y*, I, 48). According to MURRAY, neither «Bokhara» nor «Bulgaria» agrees with the early French forms. I do not see that any serious objection can be raised against «Bokhara». The old French forms are «bouquerant» (not «bouqueraut» as in LOKOTSCH), «bouguerant», «boquerant», «boguerant» (to the forms already quoted by FRANCISQUE-MICHEL, add those in GODEFROY and TOBLER-LOMMATSCH), but the final *-t* does not seem to be etymological; the low Latin forms are «boquerannus», «bucaranum», «buchiranum» (DU CANGE), «bucaranum» (*Wg*, 259), «bucheranus» in Z, «bocharanus» (in BRÄTIANU, *Actes des notaires génois*, 189), and even in French, Joinville writes «bouqueran», and F gives «bocaran». YULE seems to have been influenced by the form «bougran», and it is true that French *bougre* (attested in 1178) comes from the name of the Bulgars, but, in the 13th cent., *bolyari* (>*boryali*) gave in French *borgal* (see «Bolgara»). The oldest French forms have *-qu-* rather than *-gu-*; on the other hand, the alternation of *-o-* and *-ou-* (pronounced as Latin *-u-*) is of no account, and we know both forms for the very name of Bokhara (see «Bucara»). Phonetically, the etymology with «Bokhara» seems to be unimpeachable, and I have no hesitation in adopting it. The name of another textile is supposed to be derived from «Bokhara». It is that of the woollen material known in Osm. Turk. as *buhur çoğa* (*çoğa* > *çoğa*. «cloth»), adjectival form *buhurlu* (RADLOV, IV, 1808), *buhuraki*, «striped shawl» (BARBIER DE MEYnard, I, 289), hence Rouman. *buhur*, «cashmere» (LOKOTSCH, No. 342).

YULE remarked (*Y*, I, 62) that RAMUSIO always wrote «bocassini» instead of «bucherami» and added that «bochayrani» and «bochasini» were coupled in a Genoese fiscal statute of 1339. Both words are used as synonyms in FRAMPTON's version from SANTAELLA («Bochachims or Buckrams»; *Pe*, 26). This shows that RAMUSIO, like FRAMPTON, had no longer a true idea of the ancient value of «buckram», and knew it only as the coarse material which was also designated *bochasino*; the equivalence of the two words is given by MURRAY (*NED*, s. v. «buckram»).

The word *bochasino* is not confined to Italian, but is also represented by Span. *bocacín*, Fr. *boucassin*, Germ. *buxkin* («fustian for linings»), Engl. *bocasin*. I find no support in MURRAY for LOKOTSCH's assumption that Engl. *buckskin*, in one of its uses, should be altered from the same word through popular etymology (LOKOTSCH, No. 324). The accepted etymology is Osm. Turk. بوغاصر *boyasī* (RADLOV, IV, 1649), written in other dictionaries بوغاسی *bohāsi* or بوغاسی *boyasī* (cf. MURRAY, s. v. «bocasin»), بوغاسی *boyasī* (BARBIER DE MEYNARD, I, 336), «cloth for linings». The origin of the Osmanli word is not clear, and its history is unknown. But, if *bochasino* is actually derived from it, it cannot be very ancient. MURRAY's earliest example in English is *bokesye* in 1485, and I cannot trace any in French before 1388 (in the Supplement to GODEFROY). There is no likelihood that it occurred in any early Polian text. This is one of the cases where we find RAMUSIO indulging in an arbitrary «editing» of his text. The «bougarassin» used once c. 1400 in the sense of «boucassin» (GODEFROY, I, 697<sup>2</sup>) seems to be the result of an accidental and isolated contamination.

While giving «Bokhara» as the etymology of Fr. «bougran» and It. «bucherame» (No. 342), LOKOTSCH derives Engl. «buckram» from the Arab. *barrakān* (No. 250). This is plainly unacceptable; «bucherame» and «buckram» cannot be separated. A similar confusion is made when BRÄTIANU (*Actes des notaires génois*, 7) gives *barachame* as another form of «bocharame». «Bocharame» is «buckram», but *barachame* is the same as Pegolotti's *baraccami*, the *barraccani* of a Florentine tariff of 1384 (EVANS, 414), which is derived from the Arab. باركمان *barrakān*, a word with a numerous progeny. *Barrakān* ordinarily designates a coarse woollen stuff or a cloak made of it, and such is the meaning of the late mediaeval French *bouracan*, as well as of Span. *barracan* and *barragan* (cf. DOZY, *Dict. des noms des vêtements*, 68-71). DOZY adds that, in more recent times, the name of *barrakān* was also applied to cloaks made of finer and more valuable material, though in the fashion of the ancient *barrakān*. I feel considerable hesitation in dissenting, even to a limited extent, from DOZY. I must state, however, that as a rule the names of textiles have deteriorated rather than grown in value. Moreover, FRANCISQUE-MICHEL (II, 34-37) has shown that in the 13th and 14th cents. the *barragan* or *barracan* was a precious material, and EVANS came independently to the same conclusion (*Pegolotti*, 414). The word, borrowed as *Barchent* in German, there means «fustian», but passed from German to Russian as *barhat* (not «*barham*» as in LOKOTSCH, No. 250), with the meaning of «velvet».

## 86. BULARGUCI

*barlarguci* VA  
*bubarguci* TA<sup>3</sup>  
*bulangazi* R  
*bulangugi* LT

*bularguci* F  
*bulargufi* FA  
*bulargufy* FB

*bulargugi* TA<sup>1</sup>  
*burgrami* P  
*burlaguzi* V

YULE (Y, I, 407-408) has already shown that this is the same as the *bularyučt* of the Mongol court of Persia, whose office was however not restricted to lost goods and animals, but who also took care of fugitive slaves and other people who had gone astray (cf. *Ha*<sup>3</sup>, 244-245, 476-477, 649;

HAMMER, rather inconsequently, writes and registers *bulayurčī* as well as *bularyučī*, without remark). Polo's definition is good; the title is formed with the suffix *-čī* of a *nomen agentis*, and *bularyu* certainly meant «lost property», «property gone astray»; YULE has quoted a charter of 1320 from the *ilkhan* Abū-Sa'īd, known only in Italian, where mention is made of «bolargo» horses.

In Rašīdu-'d-Dīn's description of the Mongol Court at Peking, we find a word بلارغو and بلارغو which, since KLAPROTH (*JA*, 1833, 356), has been read «belargoui» or «*bālarghui*». PAUTHIER (*Pa*, 331) has explained it by the Mong. *balargai*, given in SCHMIDT's Dictionary as meaning «écrit, mémoire peu net avec des ratures ou phrases retranchées», and this explanation has been adopted in *Y*<sup>1</sup>, III, 122, and *Bl*, II, 479. I do not know SCHMIDT's authority for *balargai*, which does not appear in the later and fuller dictionaries of KOVALEVSKIĪ and GOLSTUNSKIĪ; but the word is really known, with the required meaning, in Western Mongolian (cf. RAMSTEDT, *Kalmück. Wörterbuch*, 31). Nevertheless, the context is not in favour of PAUTHIER's explanation; Rašīd says that officers «capture» the *b.laryu*, and send them along with a «report of the circumstances»; I have no doubt that, in Rašīd's text, we should read *būlaryučī* and *būlaryu*, «men (or animals) gone astray».

I have transcribed *bularyučī*, in agreement with Polo's vocalization, but the charter of 1320 writes «bolargo»; the Mongol and Arabic writings can give no clue as to the proper pronunciation. The word is unknown now. RADLOV (IV, 1670) registers *bōlaryučī* in Čay., I do not know from what source; but his reading *bō-* is due to the fact that he derives the word from *bōla-*, «to report», and this is certainly wrong: *bōla-* is a late denominative verb, borrowed from Chin. 報 *pao*, «to report». The word *bularyu* remains of obscure formation.

The terms *bularyu* and *bularyučī* do not occur in Chinese texts, but we have a number of mentions of, and detailed regulations about the 孛蘭奚 *po-lan-hsi*, also called *pu-lan-hsi* (with 不 *pu-* and 卜 *pu-*). PALLADIUS has seen in them «Farangi» falconers, Frank falconers (*Vostočnit Sbornik*, I [1877], 47), and the notion of the «Bo-lan-ghi» falconers has thus passed to *Br*, I, 188, and to *Y*, I, 408. But there is no doubt that *po-lan-hsi* means «men (and things) gone astray»; its Chinese synonym in the texts of the period is 闕遺 *lan-i*.

CHARIGNON (*Ch*, II, 97-98) was right in saying that the *pu-lan-hsi* of Chinese texts corresponds to the *bularyu*, but mistaken in seeking for *pu-lan-hsi* a Chinese etymology. It is certainly a transcription, which, according to the rules of transcription in use during the Mongol and early Ming dynasties, cannot represent *\*bularki*, but only *\*buralki* or *\*buralgi*. *Pu-lan-hsi* is also a common proper name (about twenty *Pu-lan-hsi* are listed from *YS* in WANG Hui-tsu<sup>1</sup>, 26, 8 b-10 a), and I have no doubt that it is the same name which appears fairly often in Rašīdu-'d-Dīn under the forms بوالقي *Buralqi* and بوالقي *Buralyi* (cf. for instance *Bl*, II, 121, 124, 185; *Ha*<sup>1</sup>, II, 409, «*Buralghi*» and, wrongly, «*Buralighi*»). Perhaps it then meant «foundling»; the Mongols, out of superstition, often gave to their children ill-sounding names.

*\*Buralki*, *\*buralgi*, as a common noun (that is not as a proper name), has not been met with as yet in any Mongol document, and its probable linguistic connection with *bularyu* is not clear; there seems to have been one of those metatheses so frequent in Central Asia, but we cannot say which way the transposition was effected until we are more clear about the etymology

of *bularyu*. For similar cases of metathesis, cf., in the Mongol period, *käräl*, «king (of Hungary, etc.)» > *kälär*, and the then current form *Kälürän* as the name of the river Kerulen. Under unknown circumstances, *Buralqi*, in modern times, has come to designate Kashgarians (cf. VON LE COQ in *Festschrift Kuhn*, 155).

The existence of the *bularyučī* (without the mention of their name) was known to Plan Carpine (*Wy*, 45).

In the beginning of the 14th<sup>th</sup> cent., two *Bilaryu* or *Bularyu* are known, one with the *ilkhans* of Persia, the other in Egypt (cf. *Ha*<sup>1</sup>, II, 405; *Hist. des Crois.*, Arm., II, 16-17, 867); I explain their name by *bularyu*, without the metathesis met with in *Buralqi*. The «*Burlughi*» of QUATREMÈRE, *Hist. des Sultans Mamlouks*, II, II, 211 and 213, must represent the same name, with wrong vocalization; and the would-be «*Burlugou*», *ibid.*, II, II, 279, is no other than the Mongol *Bilaryu* or *Bularyu* who was in the service of the *ilkhans*.

## 87. CAAGIU

*caagiu* F, L  
*caagu* LT, TA<sup>1</sup>  
*calacuy* FA

*calcuy* FC<sup>1</sup>  
*calicuy* FB  
*chaagu* TA<sup>3</sup>

*chaangu* VA  
*thaigin* R

Polo says that Chinghiz-khan, six years after he had defeated Prester John (= Ong-khan, see «*Uncan*»), died from the shot of an arrow received at the siege of a «castle» named «*Caagiu*». As a matter of fact, Chinghiz-khan survived Ong-khan by some twenty-four years, and there is no likelihood that he died of an arrow-wound (see «*Cinghis*»). Moreover, the name of «*Caagiu*» cannot well be accounted for.

YULE (*Y*, I, 245), following OPPERT's suggestion, supposed that a confusion had occurred here between Chinghiz and Mongka, since Mongka was said by some to have died of an arrow-wound, and in any case died in the administrative district of 合州 Ho-chou (Ssü-ch'uan), of which «*Caagiu*» would be the transcription; this solution is fully accepted in *RR*, 414. Although it makes perhaps the best of a difficult passage, it is open to certain objections. First of all, it is rather strange that Polo should have mistaken Mongka for Chinghiz not only with regard to the cause, but also as to the place, of his death. Moreover, Ho-chou ought to be transcribed \**Cagiu*, while all mss. agree in having a longer name, of which «*Caagiu*» is only one reading.

Without stressing the point, there is another possibility. We may leave out RAMUSIO's «*Thaigin*» (? < \**Caigiu*), where a contamination has perhaps occurred with «*Cangiu*» (*q. v.*), also read «*Thaigin*» in RAMUSIO. But FA has «*Calacuy*», FB «*Calicuy*», FC<sup>1</sup> «*Calcuy*». With the usual confusion between *c* and *t*, a form «*Calatuy*» is possible, and it is even the one which PAUTHIER believed to be the reading of his two mss. and which he adopted in his edition (*Pa*, 183), whence it has passed into *Ch*, I, 186. Starting from it, CHARIGNON (*Ch*, I, 188) tries an

impossible Sino-Mongolian combination where «Calatuy», identical for him with «Calachan», would represent Mong. *qara*, «black», and Ch. *ch'êng*, «city»; but the two names must be kept apart (for the second one, see «Calacian»). Now the *Yüan shih* (YS, I, 9 b) says that Chinghiz-khan died at the «temporary palace» (*hsing-kung* 行宮 = *ordo*) of 哈老徒 Ha-lao-t'u, which can be restored to \*Qala'utu, perhaps \*γala'utu, both not very far from «Calatuy» (it is Ha-lao-t'u which, through Ch'ien-lung's «reformed» spelling and a Russian intermediary, became the «camp of *Caratouski*» in *Pa*, 183, whence it has passed into CHAVANNES's note in *TP*, 1905, 2-3). In spite of a wrong restoration, PAUTHIER had thus thought already of connecting «Calatuy» with Ha-lao-t'u, and I would have little hesitation in doing the same, if it were not for the doubts which have been raised as to the real location of Ha-lao-t'u, and, generally speaking, about the death of Chinghiz-khan either in Kan-su or in Upper Mongolia. This is a very intricate question, about which I have given some information in *TP*, 1934, 164-166. One point may be considered as certain: in spite of the tradition of the Ordos, the tomb of Chinghiz-Khan is somewhere in Upper Mongolia, and not in the great bend of the Huang-ho (cf. also, with caution, *Ch*, I, 197).

## 89. CACANFU

*cacanfu* F, FA, FB, L<sup>1</sup>  
*caçanfu*, *capçanfu* Z  
*cacianfu* F, Fr, t  
*caianfu*, *cainfu* L  
*cancafu* S  
*cancassu* VL  
*candanfu* FBt

*cangiafu*, *changiafu* VB  
*cantasii* G  
*catanfu* FB  
*catenfu* FAt  
*caucasu* P  
*chaciafu* TA<sup>3</sup>

*chacafu*, *chancafu* TA<sup>1</sup>  
*chanchasu* VA  
*chanzanfu* V  
*pazanfu* (twice) R  
*tacanfam*, *tancafam* LT  
*tacanfu* FBr

This correct reading is actually found once in F, but the other form of F, «Cacianfu» (> Z «Caçanfu»), may be an early alteration of a still better «Cacainfu», and «Cacanfu» is perhaps «Cacainfu» > «Cacamfu» > «Cacanfu»; for the name certainly covers 河間府 Ho-chien-fu (pronounced then Hō-kiän-fu), and *chien* (*kiän*) is rendered *gain* or *quen* by Polo (see «Gaindu», «Quenlinfu»). I have some recollection of reading the name in Rašidu-'d-Dīn, although I cannot trace it now. The transcription of Ho-chien-fu's *ho* by *ca-* is identical with that used in Mong. Qasi > Qaši = 河 西 Ho-hsi (Hō-si), a name for the «Tangut» country in Chinghiz-khan's time (see «Tangut»); see also «Cacu». Moreover, the name of «Cacanfu» must have often been used in Mongol- and Persian-speaking circles.

The identification has been accepted by all except CHARIGNON, and is almost self-evident. Polo's starting point was Cho-chou (see «Giogiu»), south-west of Peking, where the two main roads followed by envoys branched off, one reaching Chêng-ting, where it again divided, to Hu-nan and Chiang-hsi, or to Shan-hsi, Shàn-hsi, etc., the other leading to Hang-chou and Fu-chien. We know that Polo passed through Chi-ning (on the Grand Canal, south-west of Yen-chou; see «Singiu matu»). From Cho-chou to Chi-ning, the direct road passes through Ho-chien-fu



(«Cacanfu»), Tê-chou (on the Grand Canal), Kao-t'ang, and Tung-p'ing-fu [(see «Tundinfu»)]. I show elsewhere that Polo passed through the modern Tê-chou (see «Ciangli»). Thus «Cacanfu» is certainly Ho-chien-fu. The *Yung-lo ta-tien*, 19423, 18 *b*, states definitely that the postal road from Peking to Hang-chou and Fu-chien went through Ho-chien-fu.

The name of Ho-chien goes back to the Han; it was a *chün* under the Wei (T'o-pa) and the T'ang, and became a *fu*, Ho-chien-fu, under the Northern Sung; the Yüan made a *lu* of it in 1265 (YS, 58, 9 *b*). Surprise has sometimes been expressed at Polo calling *fu* what was a *lu* in the Mongol period; but if Polo never uses *lu*, neither do the Persians. As a matter of fact, the *lu* (already existing in Sung and Chin times) were the seats of *tsung-kuan-fu*, or «general administrations», and the name *lu* has never been much in use; the Ho-chien [lu *tsung-kuan-fu*] went on being called Ho-chien-fu, as it had been under the Sung, and was to be under the Ming and the Ch'ing; the case is the same for all *lu* of the Mongol period. The itinerary of 1276 translated by MOULE (TP, 1915, 401) uses «Ho-chien-fu» just as Polo does.

The «great river» mentioned by Polo may be the 滸沱河 Hu-t'o-ho, a river which has undergone many changes of course, but which, even in the Mongol period, must have passed far to the south-east of Ho-chien-fu; in such a case, Polo would speak of a river which flowed through the prefectural district of Ho-chien-fu, but not through Ho-chien-fu itself. Nevertheless, it is more probable that Polo refers to the river which passes through Ho-chien-fu itself and which seems to have been a much more important water-course in the 13th cent. than it is now; river conditions in that part of Northern China have altered considerably in modern times. In YS, 64, 8 *a*, the «River of Ho-chien» is one of 18 watercourses to each of which a special paragraph is devoted.

The *Yung-lo ta-tien* (19426, 4 *b*) has preserved a list of the Yüan postal stations from Cho-chou to Ling-chou, which it may be worth-while giving here: from Cho-chou (see «Giogiu»), due south, 70 *li* to Hsin-ch'êng; south slightly east, 70 *li* to Hsiung-chou; 60 *li* to Jên-ch'iu; 60 *li* to Ho-chien (= «Cacanfu»); 60 *li* to Hsien-chou; 70 *li* to Fou-chêng; 60 *li* to Ching-chou; 90 *li* to Ling-chou (see «Ciangli» = Tê-hsien). This is still the road to-day.

## 90. CACCIA MODUN

*cacciar modun* F, FA, I  
*cacciar mondum* FB  
*caciamordin* P  
*caciamordoi* VA

*caczarmodin* R  
*caziamondun* S  
*chaccia triodum* LT  
*chatai* V

*chazumondun* VL  
*tarcarmondu* TA<sup>1</sup>  
*tarchar mundo* TA<sup>3</sup>

Neither the name, nor its location has as yet been identified. In 1920, I had said to CORDIER that «Cacciar modun» (as most mss. give it) was probably the 哈察木敦 Ha-ch'a-mu-

tun of YS, 100, 2 a. CHARIGNON (*Ch*, II, 100) opposed this interpretation on two grounds, first because Ha-ch'a-mu-tun is in a list headed by Qara-mürän, which means «north of the Yellow River at the great bend of the Ordos», and secondly because the name in YS may as well be the name of a man as that of a place. I take the second argument first. The list of YS is altered in many cases; for instance, it begins with T'a[塔]-la-mu-lien, and I agree with CHARIGNON that the name really intended is Ha[哈]-la-mu-lien, Qara-mürän. Every entry in the list consists of the name of a place followed by the name or names of the men in charge; after every such entry, an interval is left, but the rule is not observed for the two places which precede Ha-ch'a-mu-tun. In the case of Ha-ch'a-mu-tun, the name is given alone, between two intervals. It could, of course, be taken as the name of an individual, to be added to the name of the man in charge of the place named just before. I have taken it for the name of a place, the mention of the man in charge being omitted, because, while I know of no analogous name of an individual in Mongol history, it can be explained as a place-name. The second element of the Chinese form is certainly *mudun*, which is the Mongol *modun* (now *modon*), «tree», with the same pronunciation in *u* as in *mürän* instead of *mörän*; needless to say, that *modun* is also the second element of Polo's «Caccia modun». If the two places are identical, we could suppose, because of the reading «Cacciar modun», that a final *-r* has not been transcribed in Chinese. But that is not satisfactory, as both transcriptions would not fit with a \**yaĵar-modun*, «Earth-wood», which, moreover, is not very good in itself, and would require \**Qaĉar-modun*, «Cheek-tree», which seems impossible. My opinion is that Ha-ch'a-mu-tun represents *yaqĉa-mudun* (= *yaqĉa-modun*), «the Lone Tree», the *-r* in the reading «Cacciar modun» of Polo's mss. being the same erroneous final *-r* as in «Succiur», «Bettalar», etc.; in any case, the double *cc* seems to indicate that the first element really contains *-qĉ* (for the value of *-cc* and for the wrong addition of a final *r*, see «Succiur»). Such a name as *yaqĉa-modun* is so natural that it occurs elsewhere. PAUTHIER has adduced, as possibly being Polo's «Lone tree» (see «Dry [Lone] tree»), a very old tree which he says existed in «ancient Bactria» in the 18th century, and was then mentioned in the *Ta-Ch'ing i-t'ung chih* as 獨樹 Tu-shu, «Lone tree» (*Pa*, 96). The said tree did not stand in ancient Bactria, but at the north-western border of the Left Qazaq Horde, that is to say north-west of the Talas River and of the Syr-Daria (*Ta-Ching i-t'ung chih*, 420, 1, 1 b). But the main point for us here is that PAUTHIER, who was quoting KLAPROTH, *Mag. asiat.*, I, 102, omitted the following sentence correctly translated by the latter: «The Qazaq call it 'Oru(?)-yin yaqĉa-modo'; Oru (?) is the name of a river; 'yaqĉa-modo' means 'lone tree'». I am not certain of the identification of the river at the source of which the tree stood. Of course the Qazaq spoke Turkish as they do now, while the whole name is Mongolian; but the preface to Ch'ien-lung's poem on the Lone-tree (*Hsi-yü t'u-chih*, 44, 4 b) informs us that the name originated with the Toryöt, who are Mongols. Ch'ien-lung's poem is of 1757, and consequently prior to the migration of the Toryöt back to China.

So I take it as being very likely that the two names Ha-ch'a-mu-lun and «Caccia modun» are identical, even if we had to do with two different places.

But I do not think that the places are different. The commentators have strangely erred about Qubilai's yearly hunting expeditions and have taken him far into Manchuria (MARSDEN; and PAUTHIER, *Pa*, 304), or north of the eastern end of the Great Wall (YULE, *Y*, I, 408; and

CHARIGNON, *Ch*, II, 99). It is remarkable that PAUTHIER, YULE and CHARIGNON, while accepting Polo's text which makes Qubilai start from «Cambaluc» (Peking) southwards, should have admitted that such a direction leads towards Shan-hai-kuan and Manchuria. CHARIGNON has said that the spring hunts, from 1281 to 1285, took place in 柳林 Liu-lin, or «Willow Forest», which is, according to him, beyond the Great Wall; and that identification has duly embarrassed BENEDETTO (*B*<sup>1</sup>, 439, where he also attributes it to me quite gratuitously), who comes to doubt the southern direction given in F and most mss., the more so as RAMUSIO has *greco*, «north-east». He might have thought that if CHARIGNON was right, there was no reason for Qubilai to come back from such a place every year to «Cambaluc» and leave immediately for Shang-tu; it would have been simpler for him to have gone to Shang-tu direct. But the mss. are right. Qubilai travels in a southern direction (or rather south-eastern) for two days (this interpretation, supported by the Court French text in *Pa*, 304 and 307, is adopted in *Y*, I, 402, 403, though not in *RR*, 142, nor in *B*<sup>1</sup>, 144), and that would not lead him very far. We know from *YS* (II, 5 a; 12, 1 a; 13, 5 b) that in 1281, 1282 and 1285, Qubilai went to Liu-lin, but this was much nearer than the commentators have supposed. Liu-lin, the Willow Forest, was in the district of 鄆州 Kuo-chou, a district abolished under the Manchu dynasty, the seat of which was 45 *li* to the south of T'ung-chou (the well-known place of that name east of Peking). After the hunt of 1281, Qubilai ordered a *hsing-kung* or temporary residence to be built there (cf. the *Ti-ming ta-tz'ü-tien*, ed. 1931, 633). This is the residence of «Caccia modun» described by POLO. PALLADIUS's identification with Ho-hsi-wu, which CORDIER rejected (*Y*, I, 408), is wrong phonetically, but very near the mark geographically, and moreover supported by *YS*, 14, 5 b.

I think that CHARIGNON is wrong again when he says that Ha-ch'a-mu-tun is out of the question here because it was, according to him, somewhere north of the Ordos. Most of the names of the list in which Ha-ch'a-mu-tun appears are still unidentified, and, amongst them, the Čayan-nör might be supposed to be the Čayan-nör of the Ordos just as well as the Čayan-nör which lay west of Shang-tu (see «Ciagannor»). But the Gün-nör, or Deep Lake, is almost certainly the place where Mongka sometimes resided, and which is named in *YS* (3, 2 b; 3, 3 b) under 1253 and 1257 (cf. also *TP*, 1904, 380; WALEY, *Travels of an Alchemist*, 31; *Tōyō gaku hō*, XII, 103; YANAI, 388-389, 676); it was situated in Upper Mongolia. There have also been several Qara-mörän (see «Caramoran»). So we cannot say that all the names of the list refer to the Ordos region. As to Ha-tch'a-mu-tun, it comes immediately after 希徹禿 Hsi-ch'ê-t'u; I have not met this name elsewhere, but it is clearly Hičätü, which means «the Place of Willows» (from *hičäsün*, written Mong. *ičäsün*; on *hičäsün*, cf. *JA*, 1925, I, 217). It is very likely that we have here the Mongol name of Liu-lin or Willow Forest, and «Caccia modun» being in the Willow Forest, it is practically certain that Ha-ch'a-mu-tun, named along with Hičätü, is really identical with Polo's «Caccia modun».

I cannot collect and discuss here all the mentions of Liu-lin in *YS*. Apart from the details of *YS*, 11, 5 a, there is a text of importance under 1282 in *YS*, 14, 5 b. At random, I have noted that the Emperor went to Liu-lin in 1299 (20, 1 b), 1323 (28, 5 a; in that year, a gale destroyed the temporary residence; *ibid.* 28, 6 a), 1328 (32, 1 a), 1335 (38, 5 a). In 1358, Liu-lin was devastated by a rebel (*YS*, 45, 4 a).

## 91. CACIONFU

*cacianf* F, FA, FA<sub>t</sub>*cacianfu* F, Z, L, R*caciansu* LT*caciauf* FB*cacionphu*, *cacionfur*, *chation-*  
*phu* VL*cacionphu*, *caciunphu* S*camanfu* P*cancianfu* F*casianf* FB<sub>t</sub>*casiauf* FA*catianf* FA<sub>r</sub>*catiauf* FB<sub>r</sub>*chaccianfu* TA<sup>3</sup>*chacianfa* TA<sup>1</sup>*chanzianfo* V*chazianfu* VA

Although the mss. then available to us authorized only «Cacianfu», I had originally corrected it to «Cacionfu», since I could see no solution except 河中府 Ho-chung-fu, the name under the Mongols of the modern P'u-chou-fu; «Cacionfu» is now confirmed by S and VL. It is true that Ho-chung-fu is east of the Yellow River, and not west as Polo's text would imply. But there is no other *fu* which can fit in the itinerary, and the transcription «Cacionfu» is quite regular. The identification, which was first proposed by KLAPROTH, has been adopted by all recent editors. WADELL's identification with T'ung-chou-fu (*JRAS*, 1910, 1260-1261) has no value.

The name of Ho-chung-fu was given under the T'ang, and it was only in the Ming dynasty that it was changed to P'u-chou-fu. In the new Republican nomenclature, it is Yung-chi-hsien. CORDIER (*L'Extrême-Orient dans l'Atlas Catalan*, 21) thinks that this name is written «Caysam» on the Catalan Map; but the two names north and south of it on the map are doubtful, and the phonetic correspondence is too remote to be convincing.

## 92. CAÇAN

*achasan*, *chasian*, *chaxian*,  
*chazen*, *chonsanson* V*caçan*, F, L*casan* F, FA, Z, R*caxan* FB*chaçan*, *scaçan* L

This transcription represents a pronunciation قازان Qazan which is quite admissible, although غازان «Ghāzān» (*yazan*) has gained early and almost general recognition. The name is not Mongol, but seems to be the Turk. *qazan* (<*qazyān*), «kettle». There must be something wrong in the story *Ha*<sup>1</sup>, II, 7, gives, according to which the name would be Mongol and mean «tooth»; I do not know of a Mongol word for «tooth» having any similarity with *qazan* or *yazan*; moreover, there is no *z* in Mongolian. Josafat Barbaro explains by «kettle» the name of the town of Kazan on the Volga (RAMUSIO, ed. 1559, II, 98A). I must add that, although the Turk. *qazan* is old in many dialects, and already attested c. 1300 in *Codex Cumanicus*, it is the form *qazyān* which is at the basis of the Persian borrowed forms *qazqan*, *qazgan* and (corrupt?) *ħazyan*, «kettle» (cf. VÜLLERS, II, 705). During the Mongol period, the name was borne in Persia by different persons. For this particular man, the texts hesitate between Qazan and *yazan* (Ghazan); cf. *Ha*<sup>1</sup>, II, 439, 464. The Armenians write «Gazan» (PATKANOV, *Istoriya Mongolov*, I, 57).

For Polo's «Caçan», KOVALEVSKIĪ (p. 765) gives a Mongol form «Qasan» without saying from what text it is borrowed (it was probably taken from contemporary coins); the Syriac form is Qāzān (see CHABOT and BUDGE). Hethum writes «Casan» (var. «Chasan», «Chassan»; *Hist. des Crois.*, Arm., II, 191, etc.); *Les Gestes des Ciprois*, generally «Cazan» (*ibid.* 844, 847-848; once «Caçan»); as does Florio Bustron, in *Doc. inéd.*, Mém. hist., v, 129. Papal letters of 1291 give «Cassianus» (CHABOT, 243, 247). I do not know how to account for the use of  $\gamma$ - for  $q$ - in the usual Persian form of the name; perhaps it is due to a more or less spirant pronunciation, among the Mongols of Persia, of Mong. (and Turk.)  $q$ -; it is well known that this spirant pronunciation is now almost general in Mongol dialects, and it may be that a spirant pronunciation has something to do also with the alteration of the name which has come down to us as Gāihatu (see «Quiacatu»).

Ghazan, Aryun's eldest son, was born on December 4, 1271 (so in BROWNE, *Hist. of Pers. Lit.*, III, 40; the dates generally given are November 4, 1271, from *Ha*<sup>1</sup>, II, 4, or November 30, 1271, from *Oh*, IV, 153; but it must have been a Friday, which is not the case with either date. We should probably read «29 rabī' II» instead of «29 rabī' I» of *Ha* and «25 rabī' II» of *Oh*).

Having converted himself to Islam on June 19, 1295, Ghazan killed Baidu (October 5, 1295), and ascended the throne on November 3, 1295 (*Oh*, IV, 152-153). He died on May 17, 1304 (cf. BROWNE, III, 40, 43; also BLOCHET, *Moufazzal*, 600-601).

The *YS*, 107, 7*b*, mentions Prince 合贊 Ha-tsan (Qazan), son of Aryun and great-grandson of Hülägü; it is of course our Ghazan. But the *YS* is wrong when it calls him «prince of Ching-yüan», and when it says (108, 5*a*; cf. *Bl*, Introd., 225) that Ghazan was created «prince of Ching-yüan» in 1290. There was no reason to give a Chinese princely title to Ghazan, particularly when his father Aryun was still alive. In the *pên-chi* (*YS*, 16, 1*a*), it is stated that, in the 1st moon of 1290, a silver gilt seal was granted to «合帶 Ha-tai, prince of Ching-yüan», and this «哈 歹 Ha-tai, prince of Ching-yüan» is listed in *YS*, 107, 6*a*, as a great-grandson of Kōčü, the third son of Ögödäi; his name is Qadai, and he is mentioned in the *Mu'izz al-ansāb* (cf. *Bl*, II, 6, n. *h*). There is no doubt that Ghazan never received a Chinese title, in 1290 or later, and that there is a confusion in *YS*, 107, 7*b*, and 108, 5*a*, between him and Qadai. Ghazan had nevertheless inherited rights over certain people and industries in China. In 1304, a special office was created for the charge of the hunters, falconers and artisans who belonged to the house of the great prince Ghazan in the region of Peking (Ta-tu) and other places; this seems to have been only a return to a state of things which had existed from 1261 to 1275 (see «Abaga»). In 1311, the autonomy of that office was suppressed, as there was no longer anyone in charge, «Ha-êrh-pan-ta (Harbanda) governing a corner far away» (*YS*, 85, 12*a* and *b*; Harbanda Öljäitü was Ghazan's brother and successor). But the rights of Hülägü's house had not been forfeited. We happen to know that, out of 25,056 families, domination over which had been given in 1257 to Hülägü in the district (*lu*) of Chang-tê (Honan), there were still 2,929 in 1319 which belonged to his descendants (*YS*, 95, 4*b*); and the (? re-established) office of these hunters, falconers and artisans still existed in the time of the *ilkhān* Abū Sa'īd (1317-1335); cf. *YS*, 101, 8*a*.

In 1298, Ghazan sent a mission, with rich presents, to the Great Khan Tämür Öljäitü; the mission was also entrusted with collecting the long outstanding dues owed to Ghazan's

house by certain manufactures in China, which are evidently those alluded to in the paragraph above. At the head of the embassy were the *mālik* Faḥru-'d-Dīn and Noyai (wrongly read «Bocai» in *Oh*, iv, 320, «Boqai» in *Ha*<sup>1</sup>, ii, 149, but «Nokai» in *Ha*<sup>1</sup>, ii, 197; the reading Noyai is attested by Chinese texts). It was only on July 30, 1304, that the envoys arrived and were received in audience in Shang-tu (not in Ta-tu as has been said by Waśśāf) by the Great Khan Tāmūr (*YS*, 21, 6 b); it is certainly in consequence of this visit that an office in charge of Ghazan's interests was created in 1304. A Chinese envoy, Yang Ch'u, accompanied the envoys on their way back to Persia, but Faḥru-'d-Dīn died in India, and it was not until 1307 that Noyai and Yang Ch'u reached Ormuz; the envoys had been away nine years, a good illustration of the toils of such a journey in Polo's day. On this remarkable embassy, see *Oh*, iv, 320-321; *Ha*<sup>1</sup>, ii, 149, 197; Waśśāf in ELLIOT's *Hist. of India*, iii, 45-47; *Y*, i, 38; and the Chinese sources I have quoted in *TP*, 1933, 431, to which add *T'u-shu chi-ch'êng*, Shih-huo-tien, 334, 1 b (Waśśāf is wrong in saying that the journey took only seven years, and goes against the Chinese texts when he states that the Chinese envoy died on the way).

The *YS*, 26, 6 b, registers, under May 5, 1319, the death of Prince (*chu-wang*) Ha-tsan (Qazan); as the *ilkhān* Ghazan died in 1304, it would seem that we have here to do with an undetermined homonym. But although there are in *YS* some mentions of Ghazan's brother and successor Ḥarbanda Öljäitü, if we remember that Ḥarbanda Öljäitü's son Abū Sa'īd ascended the throne in 1317, and that a text mentions in 1319 Abū Sa'īd's hereditary possessions in Honan, it would appear more probable that envoys announcing Abū Sa'īd's accession reached Peking in May 1319, that they spoke of the deaths of Ghazan in 1304 and of Ḥarbanda in 1316, and that Ghazan's death, which had never been entered in the registers, was by some oversight placed at the date of an audience granted in 1319 to Abū Sa'īd's envoys. In *Athār-é Irān* of 1936 (i, 37-44), I have published an edict of Abū Sa'īd, dated September 1320, on which appears the same Chinese seal as was used on Ḥarbanda Öljäitü's letter to Philip the Fair in 1305. This would tend to show that Abū Sa'īd's envoys were not yet back from China at that time. But if there had been no official notification of Ghazan's death to the Imperial Court, it may be that this Chinese seal originally belonged to Ghazan, and was inherited by Ḥarbanda Öljäitü who used it throughout his reign.

It was Ghazan who finally married, as Polo says, the Lady Kōkāčīn sent from China to his father Aryun (see «Cocacin»).

### 93. CAGUY (< \*CACU)

*caguy* FA  
*caigiu* F, L  
*caigui* P  
*caiguy* FB

*caycuy* S  
*caygui* P, VL  
*chaigui* VA

*guagui* V  
*quaçu* Z  
*quanzu* R

This is the name, altered to «Quaçu», «Caguy», «Caigiu», etc., of the small town which was situated on the north bank of the Huang-ho, opposite to the much more important Huai-an which was on its southern bank. Commentators have been at a loss to trace it, even going with

CHARIGNON as far as to identify it with Hai-chou, some two or three days' journey north of the crossing (*Ch*, III, 27; cf. also *TP*, 1915, 415). But the case seems fairly simple.

The itinerary of the *Yung-lo ta-tien* which I have translated under «Lingiu» shows, going south, as the last station before Huai-an, a place called 大清口 Ta-Ch'ing-k'ou. The commissioners of 1276, coming from the south, left Huai-an and crossed the river at 清河口 Ch'ing-ho-k'ou, after which they soon reached 小清河 Hsiao-Ch'ing-ho-k'ou (*TP*, 1915, 397). These names have many counterparts in different parts of China, and I am not going to try and trace their history here. But the main element in them is the common Ho-k'ou, «Mouth of the River», so frequent in Chinese toponymy. It seems clear that the Ta-Ch'ing-k'ou or Ch'ing-ho-k'ou was locally called simply Ho-k'ou, «Mouth of the River», and it is this name which Polo heard. F has «Caigiu», FA «Caguy», Z «Quaçu». We have in the various readings an example of the mischief done by the oft-recurring *-giu = chou*. Polo must have written or dictated «Cacu»; *-cu* would be a regular transcription of *k'ou*, and *ca-* is the normal transcription in Polo of *ho*, «river», as for instance in the name of «Cacanfu» = Ho-chien-fu.

#### 94. CAICIU (cc. 107-110)

<i>cacianfu</i> F	<i>cantuy, chantuy</i> TA <sup>3</sup>	<i>caytui</i> LT
<i>caiciu</i> F, L	<i>catay</i> FA, FA <sup>r</sup>	<i>chaichui</i> VA
<i>caicui</i> FA	<i>cayafu</i> Fr	<i>chaychui</i> VL
<i>caicuy</i> FA <sup>t</sup>	<i>cayanfu</i> Ft	<i>chugiun</i> V
<i>caituy, chaituy</i> TA <sup>1</sup>	<i>caycui</i> FB <sup>t</sup>	<i>ciaciu</i> Lr
<i>cangiasu, zafu</i> (?) VB	<i>caycuy</i> FB, FBr, P	<i>thaigin</i> R

In Polo's account, «Caiciu» lies between «Pianfu» and «Cacionfu», which are certainly P'ing-yang-fu and Ho-chung-fu (the modern P'u-chou-fu) respectively. The readings of the name are at great variance (unfortunately there is no corresponding passage in Z) and the narrative is not countenanced by real historical data. So we can only rely upon the general trend of the itinerary to determine what place is meant. According to Polo, «Caiciu» lay two days west of P'ing-yang-fu; the Huang-ho was reached after a further twenty miles to the west, and having crossed the Huang-ho another two days' journey to the west brought the traveller to P'u-chou-fu.

For «Caiciu», MARSDEN thought of 解州 Chieh-chou in Shan-hsi, north-east of P'u-chou-fu. We should expect Polo to pronounce this name «Caigiu», which could easily produce the various readings\* of the mss. But Polo says that «Caiciu» lay west of P'ing-yang-fu, while Chieh-chou is south-south-west of it; moreover for Chieh-chou the distance from the Huang-ho, as given by Polo, is too short. YULE (*Y*, II, 25-27) then suggested 吉州 Chi-chou, which is due west of P'ing-yang-fu and only a few miles east of the Huang-ho; having reached the Huang-ho, Polo would either travel down by boat to P'u-chou-fu, or follow the west bank of the river to a point opposite P'u-chou-fu.

After MARSDEN, but prior to YULE, PAUTHIER proposed quite a different solution. Although generally paying RAMUSIO scant attention, PAUTHIER (*Pa*, 354-355) accepted in the present case the latter's isolated reading «Thaigin» (which he miscopied «Taighin») as the only correct one and substituted in his edition «Taïcin» in place of the readings of his French mss. «Taïcin», according to PAUTHIER, was a transcription of 太慶 T'ai-ch'ing, the name of a fortified pass (關 *kuan*) on the western bank of the Huang-ho, and somewhat south-west of P'u-chou-fu.

RICHTHOFEN, who did not accept YULE's explanation of Chi-chou, himself expressed the opinion that Polo may have used two different routes, passing through «Kieh-chou» («Caiciu» of F) when going to Yün-nan and through «Taiching-kwan» («Thaigin» of R) on his return, or *vice versa* (*Y*, II, 26-27). PENZER (*Pe*, XLVII-XLVIII), although attracted by YULE's Chi-chou, repeated RICHTHOFEN's argument and traced on his map the itineraries resulting from the different solutions proposed.

I am surprised that PAUTHIER's T'ai-ch'ing should have been taken seriously. The true name is 大慶關 Ta-ch'ing-kuan in all Chinese sources, but this is of small account since *ta* was still pronounced with a final *-i* in the Mongol period. It is also of no great consequence that Ta-ch'ing sounded Dai-k'ing or Tai-k'ing in Polo's time, and not with the modern northern pronunciation which we transcribe Ta-ch'ing and which PAUTHIER and RICHTHOFEN render as «Taiching» and «*Taigin-kwan*» respectively. The result is that Polo's transcription of such a name should be \*«Taichin», not «Thaigin» as in RAMUSIO, or «Taïcin» as the name was restored by PAUTHIER in his edition. More important is the fact that Ta-ch'ing-kuan, although known as one of the crossings of the Huang-ho, was never the residence of the military governor of Shan-hsi province, contrary to PAUTHIER's statement, and in 1328 Āsān-tāmūr simply passed through it to enter Shan-hsi. But what is fatal to PAUTHIER's theory is the position of Ta-ch'ing-kuan. We are looking for a place between P'ing-yang-fu in the north-east and P'u-chou-fu in the south-west. Now Ta-ch'ing-kuan is south-west of P'u-chou-fu, and moreover on the western bank of the Huang-ho (although BRETSCHEIDER and PENZER's maps put it on the eastern bank); it was a sort of advanced post of P'u-chou-fu, a bridge-head still belonging to Shan-hsi, although situated on the other side of the river. It is evident that the place does not fulfil any of the conditions required for its identification with «Caiciu».

There is another reason which ought to have at once disposed of RICHTHOFEN's opinion. While admitting that «Caiciu» is Chieh-chou, RICHTHOFEN looked for «Thaigin» at Ta-ch'ing-kuan because, Polo having, possibly, travelled by two different routes when going and returning, «the traveller himself might have put into Ramusio's text the name of *Thaigin* instead of *Cai-chu*». But such an opinion betrays a fundamental misconception of the way in which Polo's book has come down to us. It is quite clear that Polo used but one name, and that discrepancies such as «Caiciu», «Thaigin», etc. are the result of copyists' errors.

As for the name which Polo used, I must frankly admit that, whatever solution we adopt, we shall have to make some allowances for certain inaccuracies in the text, the first one, quite evident, being that Polo locates «Cacionfu», the modern P'u-chou-fu, west of the Huang-ho, while it is east of it and reached from P'ing-yang-fu before crossing the river.

YULE's identification of «Caichu» with Chi-chou is not probable. Chi-chou is practically



due west of P'ing-yang-fu, so that PENZER's remark on the subject (*Pe*, XLVIII) is inaccurate; on the other hand, the distance from Chi-chou to the Huang-ho, while it is certainly less than Polo's «twenty miles», is long enough to be acceptable in a part of the itinerary where the bearings and distances are very far from accurate. But, and this is the main point, Chi-chou is not at all on the route which Polo was following, and this leads YULE to make Polo travel down the river by boat or to advance along the western bank. But both hypotheses are in utter disagreement with the postal relays of the time; therefore I think that Chi-chou must be left out of account.

MARSDEN's Chieh-chou was a brighter inspiration. Chieh-chou, although too much to the south, could be said to be more or less between P'ing-yang-fu and P'u-chou-fu. But again Chieh-chou is not on the main track. On this route, between P'ing-yang-fu and P'u-chou-fu, there is only one important place, the name of which moreover can easily account for most readings of the mss.; it is 絳州 Chiang-chou, in Polo's transcription, theoretically, «\*Cangiu». I feel confident that «Caiciu» is Chiang-chou. The distances and the bearings given in the text do not agree with this solution, but they do not agree with any other, and here we have at least possible forms of the very name we ought to expect.

The name of Chiang-chou dates from the middle of the 6th cent. In December 1218-January 1219, the Chin promoted the place to the rank of a *fu* and called it 晉安府 Chin-an-fu. But the Yüan soon gave it again the name of Chiang-chou under which it continued to be known till the present day (cf. *Chin shih*, 26, 3 b; YS, 58, 17 a; *Ta-Ch'ing i-t'ung chih*, 118, 1 a).

For the contents of the two chapters devoted to «Caiciu», see «Roi d'Or».

## 95. CAIDU

<i>caidu</i> F, Fr, FA, L, VB, Z; R	LT, P, VL, Z	<i>chaydu</i> TA <sup>1</sup> , TA <sup>3</sup>
<i>candu, chadu</i> VB	<i>chaidu</i> TA <sup>1</sup> , TA <sup>3</sup> , V, VA	<i>ghaydu</i> TA <sup>3</sup>
<i>caidu</i> Ft, FA, FA <sub>t</sub> , FB, L,	<i>chardo, chardu, gaidin, gaidu</i> V	

Mong. Qaidu, known only as a proper name; Ch. 海都 Hai-tu; Pers. قایدو Qaidu (also قایدو Qäidu). One of Chinghiz-khan's legendary ancestors was called Qaidu, Qaidu-khan (cf. *Secret History*, §§ 46, 47; YS, I, 1 a [with details not found elsewhere as yet]; 107, 1 b; *Ber*, III, 238); a third Hai-tu in YS, 134, 9 b, is probably also a Qaidu; so are also, in all likelihood, the five 懷都 Huai-tu of WANG Hui-tsu<sup>1</sup>, 16, 3-4. The name was still in use under the Timurids (cf. «Mirza-Qaidu» in *Not. et Extr.* XIV, 127, 149, 162). The word *qaidu* (> *haidu*) is no longer known in Mongolian, but it has survived in geographical nomenclature: Haidu-yol is the name of the lower part of the Yulduz river in Chinese Turkistan. BLOCHET (*Bl*, II, 434) says that Qaidu is «evidently» connected with Mong. *yaitu*, «violent», «insolent», but *yaitu* means merely «miserable», «unfortunate»; moreover, I would almost prefer, for once, the explanation of Ch'ien-

lung's commissioners and suppose a dialectical form \**qaidu* of Mong. *qidaq*, «alone», «single», from which Manchu *kaidu*, of identical meaning, would be borrowed (cf. also BLOCHET, *Moufazzal*, 608).

According to Polo, Qaidu was a grandson of Čayatai, and twice (cf. Vol. I, 144, 447) Polo makes Čayatai a brother of Qubilai; at the same time, he calls Qaidu a «neveu» of Qubilai, and the word may be taken to mean either «nephew» or «grandson». Polo is here mistaken. HAMMER (*Ha*<sup>1</sup>, I, 142) speaks of a Qaidu, sixth son of Ögödäi, and of a Qaidu, seventh son of Čayatai, but both are due to misunderstandings or misreadings. The Qaidu, son of Čayatai, mentioned by HOWORTH for the campaign of 1240-1241 in Hungary (I, 137, 142), is due to another error: WOLFF (*Gesch. der Mongolen*, 154, 159) had absurdly changed the Qadan, son of Ögödäi, given by his sources, into a would-be Qaidu, son of Čayatai; this has led GROUSSET, *Hist. de l'Extrême-Orient*, 435, to include the true Qaidu among the princes who took part in the campaign of 1240-1241, while Qaidu was still at that period a young boy, as will be seen farther on. There is in fact only one prince Qaidu in the 13th cent., and he is a grandson not of Čayatai, but of Ögödäi; to Qubilai, he was the son of a first cousin, what we call in French a «neveu à la mode de Bretagne».

The date of the birth of Qaidu is not given by any text, but can be determined approximately. All the texts agree that he was the son of Qaši or Qašin, a son of Ögödäi (YS, 107, 6a; BL, II, 7, 434); Rašidu-'d-Dīn adds that Qaši was the name then used by the Mongols for Tangut (= Hsi-Hsia), and that Qaši owed his name to the fact that he was born when Chinghiz had just led a victorious campaign against the Hsi-Hsia. It is true that Qaši or, with a paragogical -n, Qašin, Ch. 合 [= 哈] 失 Ha-shih, represents a Mongol alteration of 河西 Ho-hsi (mediaeval pronunciation Hō-si), then the common Chinese name for the Hsi-Hsia country, the modern Kan-su (see «Tangut»). The passage in Mongolian of s- to š- before i was not yet general, and the name of Qaši, with the adjectival or ethnical suffix -tai, -dai, is still transcribed 河西解 Ho-hsi-tai (\*Qasidai) in the *Hei-Ta shih-liao* of 1232 (cf. TP, 1928-1929, 167; ed. WANG Kuo-wei, 1b) and 合昔歹 Ha-si-tai (\*Qasidai) in the *Cho-kêng lu* of 1366 (I, 3a). So there is no doubt about the meaning of Qaši, and the way in which the name was given is also in agreement with Mongol habits.

But from 1205 to 1227, Chinghiz marched five times against Tangut; HUNG CHÜN, 15, 1a, refrained from any formal opinion about the time which is meant here. T'U CHI (37, 1) says that Qaši was born in 1205 (he gives in fact *i-hai*, 1215, but the whole reasoning in the preceding paragraph shows that this is a slip for *i-ch'ou*, 1205); this I cannot accept. Ögödäi was born in 1186; he had seven sons, the eldest of whom, GÜYÜK (see «Cui»), was born in 1206; Chinese sources and Rašidu-'d-Dīn agree in making Qaši the fifth son, and Rašid says that the first five sons were born of the same mother (YS, 2, 4a; 107, 5b-6b; BL, II, 4-7; T'U CHI's attempt to show that Qaši was the eldest son and born of another mother is a failure). The result is that Qaši could not have been born before the third campaign of Chinghiz against the Hsi-Hsia, stated to be in 1209 by the YS, I, 6b, but more correctly in 1210 by Rašid (*Ber*, III, 16) and the *Shêng-wu ch'in chêng lu* (ed. WANG Kuo-wei, 49a). We cannot make it any later, knowing what we do of Qaši's son Qaidu. So Qaši must have been born in 1210.

Qaidu, whose father was born in 1210, cannot himself have been born much before 1230; on the other hand, he could not have been born after that date, since he died «very old» in 1301, according to Rašid (*Bl*, II, 7), although he clearly could not have then been «almost a centenarian», as is said by HAMMER (*Ha*<sup>1</sup>, II, 143).

Qaidu's mother was a Bekrin (a partly Christian tribe) called سبکینه Säkkinä (?; cf. ERDMANN, *Vollständige Uebersicht*, 69; *Ber*, I, 130; *Bl*, II, 7). The boy was always called simply Qaidu. HAMMER (*Ha*<sup>1</sup>, II, 144) makes Dua call him once «Qaidu Andaman», but I have no doubt that we must understand «Qaidu, my *anda*»; Dua and Qaidu were «sworn brothers», *anda*.

Qaši was in great favour with Ögödäi, and Hsü T'ing, the commentator of the *Hei-Ta shih-lio*, who visited the Mongols in 1235-1236, heard the rumour that Ögödäi intended Qaši to succeed him. But Qaši died of drunkenness, while still very young, in 1236 or shortly afterwards; at his death, a taboo was placed on the name Qaši, and from that moment the Mongols called the Hsi-Hsia country by its Turkish name of Tangut, the very one used by Polo (*Ber*, I, 120; *Bl*, II, 7; see «Tangut»).

When, after the death of Güyük (1248), the power passed from the branch of Ögödäi to that of Tului with the election (1250) and the enthronement (1251) of Mongka, Qaidu shared the fate of the surviving members of his lineage whom Mongka, in the spring of 1252, after they had taken part in a diet (*quriltai*), dispersed to their various appanages; Qaidu was sent back to 海押立 Hai-ya-li, Qayaliq, the «Cailac» (*var.* Caalac, Ceialac; read «Caialac») of Rubrouck (cf. *YS*, 3, 2 a; *Br*, II, 40; *Wy*, 190, 191). Although the position of Qayaliq has not been ascertained, it must have been in the region of Kopal, east of the Balqaš lake, and north of the Ili river (the location on the Ili in BLOCHET, *Moufazzal*, 683, is impossible). Founded in the beginning of the 12th cent. by the Qarluq, Qayaliq has been supposed to be included in Jöci's appanage (BARTHOLD, *12 Vorlesungen*, 190); on the other hand, the currently admitted, but wrong equivalence of Rubrouck's «Organum» with the name of the queen Oryana (?; the true form of this name is doubtful; but «Ergene» in BARTHOLD, *ibid.*, 184, 185, 197, is not correct) would place Qayaliq in the appanage of the house of Čayatai (so in HOWORTH, I, 173). But the text of *YS* relating to 1252 can leave no doubt that Qayaliq belonged to the appanage of the line of Ögödäi, and it is Qaidu who must have ruled in Qayaliq when Rubrouck reached that city in 1253. For Qaidu's later history, we must remember (what is often forgotten) that Qaidu always lived in the region of the Ili and the Chu, where he had his proper dominion, and that he only occasionally took possession of Qara-qorum or made incursions towards Bešbalıq.

Already in Mongka's time, Qaidu seems to have fretted at the eviction of his branch. In 1256, Mongka dispatched to him 石天麟 Shih T'ien-lin, a Chinese who had become enough of a Mongol to receive the new name of Mêng-wu-t'ai (? Monyoltai [or \*Monyutai]). Qaidu detained Shih T'ien-lin, and released him only with the prince Nomoyan in 1283 (see «Nomogan»); Shih T'ien-lin had thus remained with Qaidu, in some sort of captivity, for twenty-seven years (*YS*, 153, 5 a; he must not be confounded with the Shih T'ien-lin of *YS*, 127, 3 b).

But Qaidu's resentment reached its climax when Mongka's brother Qubilai ascended the throne in 1260, and not through election by a general diet convened in Mongolia, but proclaimed mainly by his own men on the borders of China. Qubilai's younger brother Arıq-bögä was elected

Great-khan in Mongolia, and Qaidu sided of course with Arıq-bögä, only too glad of an internecine quarrel which was likely to bring to ruin the house of Tului (*Bl*, II, 7). But Arıq-bögä was defeated and had to surrender to his brother (1264). Somewhat later, Qubilai tried to ensure his free intercourse with his brother Hülägü in Persia by installing as head of the house of Čayatai a man on whom he thought he could rely, the prince Baraq (see «Barac»). This was too much for Qaidu, who soon opened hostilities against Baraq; defeated at first, he later took a decisive revenge, and for more than thirty years the branches of Čayatai and Ögödäi worked hand in hand under the supremacy of Qaidu. In 1269, Qaidu convened a diet on the bank of the Talas river, where the princes pledged themselves to keep to nomad life and Mongol habits (*BARTHOLD, 12 Vorlesungen, 185-186*). Qubilai had long cherished the hope of bringing the dissensions with the princes in Mongolia to a peaceful solution. Still in 1265, when attributing to four great princely houses the revenues of the cities of the province of Nan-ching (= K'ai-fêng; see «Namchin»), he gave those of 蔡州 Ts'ai-chou to Qaidu (*YS, 6, 1 b*); and more than once he ordered Qaidu to appear at the Court. But Qaidu had greater ambitions than mere revenues and a position of attendance on a Great-khan whose title he always challenged. War was therefore inevitable. Already in 1266, Qubilai's son Nomoyan had received the ominous title of Pei-p'ing-wang, «Prince of the Pacification of the North», and finally, in 1271, he was sent with an army to Almalıq in the Ili region (north-west of Qulja, our «Kulja») to ward off Qaidu and his allies of the Čayatai branch (see «Nomogan»). This implies that Almalıq, near which Čayatai lived his latter years (see «Ciagatai») and which is given by *YS, 63, 15 b*, as Qaidu's appanage, had been captured for the Great-khan (the future Mâr Yahbalaha III and Rabban Čauma, at a date which has not yet been established satisfactorily, found Qaidu at Talas; cf. *CHABOT, Hist. de Mar Jabalaha III, 25*). Nevertheless, Qaidu's attitude was, for some years to come, more one of sullen opposition than of open rebellion. Nothing supports the note of *YS, 63, 2 a*, according to which Qaidu revolted in 1268, unless we see there an allusion to Qaidu's fight against Qubilai's liege man Baraq (cf. *T'u chi, 74, 7 a-b*). In the beginning of 1275, Qubilai claimed back the thirty-four gold and silver tablets formerly granted to Qaidu and Baraq (see «Barac»); this is a token of at least very strained relations. All references to military difficulties towards 1274 also point to Chinese Turkistan as being then the main seat of trouble (cf. *T'u Chi, 74, 7 a*), and we may safely conclude that the Čayatai princes, not Qaidu, were most concerned in the case. It is generally said, on the faith of *GAUBIL (Hist. de Gentschiscan, 168-169)*, that open war with Qaidu began in 1275, when he and Dua, leading more than 100,000 men, besieged the Uigur *ıdıqt* in his capital Qara-ħočo (*Oh, II, 451-452; Pa, 718; HOWORTH, I, 176; Y, II, 462*; for this attack on Qara-ħočo, see the references to Rašidu-'d-Din under «Cibai and Caban»). *GAUBIL's* account is based, directly or indirectly, on the biography of the *ıdıqt* in *YS, 122, 1 b*, where it is said that «in the 12th year *chih-yüan* (1275), Dua and Pu-ssü-pa (the Busma of other texts, Buzma in Rašid, Baraq's fifth son; cf. *Bl, II, 168*), with 120,000 men, besieged [Qara]-ħočo». But *T'u Chi (36, 9 a)* has made it probable that «12» in that text is a mistake for «22» (= 1285), which is given in Bayan's biography (*YS, 127, 7 a*) for what seems to be the same event. I cannot find any direct confirmation in Chinese texts for *GAUBIL's* other statement that in 1275 Qubilai wanted Bayan to abandon the campaign against the Sung and fight in the north; *T'u Chi, 90, 6 a*, alludes to the

fact only from Western sources. Whatever the case may be, and notwithstanding the fact that Qaidu's hostile policy and even military moves for many years can scarcely be doubted, as late as 1276-1277, when the prince Nomoyan and others were made prisoners by some of their treacherous companions, Qaidu was no party to the plot; he was dragged afterwards into a quarrel which was not his own (*Y*, II, 462, is wrong). But from that moment, and although he would not or could not rush to the aid of far-off Nayan in 1287 (see «Naiian»), he never laid down his arms. After Polo had left China, and after Qubilai had died (1294), Qaidu continued to fight against Qubilai's grandson and successor, Tämür Öljäitü; Polo's prince Georges (see «Giorge») then fell a victim in the struggle (1298). In 1299, Tämür Öljäitü replaced the feckless prince Kökčü, a son of Qubilai (see «Nomogan»), as commander of his armies, by a grandson of Qubilai called Qaišan, who, under the name of Qaišan Külük, was himself to ascend the Imperial throne in 1307. Qaišan's troops reached the Altai in January 1301; the final battle was fought in September of the same year at Ha-la-ha-t'a; Qaidu died very soon afterwards (*YS*, 22, 1; cf. also 119, 10 a; 132, 3 a). Ha-la-ha-t'a (\*Qara-qata?; perhaps Mong. \*Qara-qada, «Black Rock») has not been identified, but must be the place in the region of Qayalıq which is mentioned by Waśśāf as the seat of Qaidu's last battle (*Oh*, II, 516; I do not find that the battle is stated by *YS* to have taken place between Qara-qorum and the river Tamir, as is said in *Br*, II, 37; d'OHSSON [*Oh*, II, 516] quotes only GAUBIL on that point, and the mention of the Altai and the Naiman in *YS*, 22, 1 a, seems to establish that \*Qara-qata was far to the west-south-west of Qara-qorum; nor do I agree with the identification with the Khangai mountains put forward by T'u Chi, 74, 13 b). According to Rašid and the Chinese, Qaidu was defeated; Waśśāf says he won the day. Even the Chinese account, partial of course to the Imperial family, shows that Qaišan's army was more than once in great danger (cf. T'u Chi, 74, 13 b), and the encounters may have resulted in a drawn battle, which put an end to the war only on account of the fatal wound (*Bl*, II, 9) received by Qaidu. The fierce soldier was buried on a mountain, between the Chu and the Ili (BARTHOLD, *12 Vorlesungen*, 186, 197); although said to be «very old» (*Bl*, II, 7), he was in fact scarcely over seventy («over sixty» according to T'u Chi, 74, 13 b, but he gives very poor reasons). His faithful ally Dua (< Du'a, Duwa; also Tuwa), the son of Baraq, paid him a hearty tribute and procured the election of Qaidu's eldest son Čapar (or Čabar) as his successor (*Ha*<sup>1</sup>, II, 144). But everyone was tired of the long struggle. Dua himself persuaded Čapar to make his peace with Tämür (1303; cf. *YS*, 21, 4 a). Envoys from Tämür, together with others from Čapar and Dua, gave the happy news in September 1304 to the *ilkhān* Öljäitü, then at Marāya, and this explains why Öljäitü speaks at length of the peace restored among the Mongols in his Mongolian letter to Philip the Fair of France, written in 1305 (BARTHOLD, *12 Vorlesungen*, 199-200). A short-lived peace it was. Dua and Čapar seemed so close to each other that Hethum, in 1307, says they were brothers (*Hist. des Crois.*, Arm., II, 214, 235). But war had broken out between them in 1305-1306; in 1309, one only of Qaidu's twenty-four sons still held his own (BARTHOLD, *12 Vorlesungen*, 201-202). In the course of these infernal struggles, the Mongols split into many factions, which finally brought their power to an end.

The name of Qaidu was known in the West before Polo's book. When Nicholas IV entrusted John of Montecorvino with the mission which finally brought him to Peking, but via

the Indian Ocean, he gave him, among other letters, one dated July 13, 1289, and addressed «Caydono principi Tartarorum» (cf. GOLUBOVICH, *Bibl. bio-bibl.*, II, 442). Paolino da Venezia (c. 1316-1334), in a chapter which seems otherwise to have been lifted from Hethum, has a sentence on an invasion of «Caydo» into Ghazan's dominions of which I can find no trace in Hethum (GOLUBOVICH, *Bibl. bio-bibl.* II, 97). Between 1314 and 1328, Guillaume Adam writes that there were four empires of the Tartars, the fourth being the «medium imperium», «middle empire», called empire of «Doa or Caydo» (*Hist. des Crois.*, Arm., II, 530); a bull of 1318 mentions «regna Doha seu Chaydo regum» (GOLUBOVICH, *Bibl. bio-bibl.* II, 572). The names had thus survived in the West after the deaths of both Qaidu (1301) and Dua (1306); an identical belated use of Qaidu's name occurs in Mufazzal (BLOCHET, *Moufazzal*, 607, 630, 682). A sort of legend must have been woven around Qaidu already in his lifetime; the tales circulated about his daughter Qutulun seem to form the basis of the story of the queen Urduja in Ibn-Baṭṭūṭah (see «Aigiaruc»).

## 96. CAIGIU (c. 148)

<i>caichui, chaichui, pungino</i> V	<i>caygiu</i> Ft, L	<i>changgu, chayngui</i> TA <sup>3</sup>
<i>caicui, caigiu</i> F	<i>caygui</i> LT, P	<i>ciangui</i> VL, S
<i>caigui</i> Fr	<i>cayngui</i> G	<i>cucuy</i> FAT, FB
<i>cangiau, cangiu</i> VB	<i>cayngui</i> P <sup>6</sup> , R	<i>mangui</i> VA
<i>cayçu</i> Z	<i>chaigui, chaygui</i> TA <sup>1</sup>	<i>tucuy</i> FB
<i>caygin</i> P <sup>5</sup> , Lr		

Although the reading of F, supported by Z, is «Caigiu», I have little doubt that the real form used by Polo was either \*Cagiu or \*Cuagiu, and the name, as has been admitted by all commentators, is 瓜洲 Kua-chou, «Gourd Island», the place where the Yang-tzŭ was crossed to reach Chên-chiang on the southern bank. Although it was sometimes written 瓜州 Kua-chou (already in T'ang times), it has never been a *chou* in the administrative sense of the word (both forms occur on the same page of YS, 8, 9 b). In 1276, the Imperial Commissioners left Chên-chiang, crossed the Yang-tzŭ, reached Kua-chou and started for Yang-chou, exactly in the same manner as Polo did in the opposite direction. The region of Ts'ai-shih and Kua-chou had been the scene of fierce fighting between the Chin and the Sung in 1161; cf. 采石瓜洲記 *Ts'ai-shih Kua-chou chi* in *Han hai*, and the parallel work, *Ts'ai-shih chan-shêng lu*, described in *Ssŭ-k'u* . . . , 52, 15. For the strategic importance of Kua-chou at the time of the campaign against the Sung, cf. YS, 8, 9 b, 10 b; 129, 4 b.

Kua-chou, formerly an island which gradually became an advanced point of the river bank, was a *chên* in the 11th cent., and was walled for the first time in 1168. It is now in ruins and

owing to the encroachments of the river, is disappearing (cf. *Ta-Ch'ing i-t'ung chih*, 67, 3 a; *Ch*, III, 63). The *Yung-lo ta-tien*, 19426, 7 a, 9 b, names our Kua-chou, but, in the list of relays which I have translated elsewhere (see «Cingiu»), writes 瓜步 Kua-pu. There have been two *chên*, one of Kua-chou, the other of Kua-pu, but I am not prepared to discuss the more or less conflicting data which have been collected about them in *Tz'ü yüan* (cf. *Ch*, III, 63) and in *Ti-ming ta-tz'ü-tien*, 237, 238.

Stan. GUYARD (in REINAUD, *Géogr. d'Aboulféda*, II, II, 125) identified Abū-'l-Fidā's خاجو Ḥājū with Polo's «Caigiū» = Kua-chou. This is certainly wrong, but Abū-'l-Fidā's data are contradictory. The name may be borrowed from the Ḥājū of the *Ḥudūd al-'Ālam*, and this actually is a Kua-chou or «Gourd District», but the well-known one of western Kan-su (*Mi*, 85, 233).

## 97. CAIL

*cael* R

*cail* F, Fr, *t*, FA, FB, VB, Z,  
V, L

*cauer* TA<sup>3</sup>

*cayl* FA<sup>t</sup>  
*chail* V

*chauer* TA<sup>1</sup>, TA<sup>3</sup>

*ciail* LT

Now the poor village of old Kāyal, Palayakāyal, on the Tāmraparnī river, about a mile and a half from its mouth; cf. *Y*, II, 372-373; *Y*<sup>1</sup>, III, 231; *Hobson-Jobson*<sup>2</sup>, 140; ELLIOT, *Hist. of India*, I, 72-73; DAMES, *Barbosa*, I, 190; II, 122-124; *Fe*, 530. The name is said to represent the Tamil *kāyal*, «lagoon», «backwater».

Polo's form is in agreement with Conti's «Cahila» (Latin form, hence the Genoese Map's «Caila»; HALLBERG, 93), V. da Gama's «Cael», Giovanni da Empoli's «Gael» (read «Cael»?), Varthema's «Chayl», Barbosa's «Cael», Canerio's «Cailo» (1502). Rašidu-'d-Dīn, 'Abdu-'r-Razzāq, Sulayman al-Mahrī, Sīdī 'Alī Čālābī write قایل Qāil (cf. *Fe*, 530), which there is no reason to read Qāyal as is done by FERRAND in *JA*, 1922, II, 93 («Kāyāl», given there twice, is wrong in any case). And the spelling «Koil» of the Ordnance Map of India, denounced by CALDWELL, is perhaps not so wrong. There must have been a pronunciation *kāil* of *kāyal*, or the modern form Kāyal may be due to popular etymology.

The pronunciation Kāil, established by Mussulman and Western sources, is moreover confirmed by Chinese transcriptions. It is said in *YS*, 210, 7 b, that in 1281, the Sultan (*suant'an* = \**soltan*) of Ma'abar and his four brothers, intending to invade Quilon, «assembled on the territory of Chia-i» (Kāil; 皆聚加一之地; the name has been misunderstood in *Pa*, 604, and *TP*, 1914, 431). The kingdom of 加異勒 Chia-i-lo (Kāil) was visited by Chêng Ho in 1409-1411, and its king, in 1411, is called Ko-pu-chê-ma (cf. *TP*, 1933, 289; 1934, 294). The names of Ko-ku-lo and Ku-lo, mentioned in *Y*<sup>1</sup>, III, 118, have nothing to do with Kāil.

## 98. CAISERIE

*caserie* FA, FB  
*casserie* F, LT, L  
*cassorie* P, P<sup>5</sup>

*cayssaria* R  
*cesare* VA  
*chasana, chaseta* V

*chayseria* Z  
*isirie* S

The «Kaisariye» of our maps, the ancient Caesareia of Cappadocia. In view of Z and R, I think that «Caiserie» is more likely to be Polo's form than the «Casaria» adopted in *B*<sup>1</sup>, 440. *Les Gestes des Ciprois* speak of «Sezaire la grant, que l'on apele Caiserie» (*Hist. des Crois.*, Arm., II, 843). Simon de Saint-Quentin (in Vincent de Beauvais, XXXI, 147) writes «Gasaria». Pegolotti's «Casena» is probably a corruption of «Caseria», as proposed by EVANS, 389.

## 99. CALA ATAPERISTAN

*cala ataperiscam* FA, Z  
*cala-ataperistam* FB  
*cala ataperistan* F, L

*chala atepetischan* V  
*chaliasata* TA<sup>3</sup>  
*chaliasata periston* TA<sup>3</sup>

*cholasata per istranee* VA  
*ghaliasacha* TA<sup>1</sup>  
*talasata* LT

Pers. Qal'ah-i Ātāšparastān, «Castle of the Fire-worshippers». It is difficult to admit that Polo, who at least knew Persian, dropped the -š- of *ataš*; perhaps he had simply written «\*Cala Atasperistan», and the group -sp- has been altered in the archetype of all our mss., as it was altered in most of them when «Ispaan» became «Istanit».

The «village» of «Cala Ataperistan» was, according to Polo, three days distant from Savah. YULE, supposing that the information was acquired on the homeward journey, sought for the place «between Sāvah and Abher» (*Y*, I, 82); but that was because YULE believed that, on the outward journey, Polo had gone to Ormuz via Bagdad. I agree with the view that, on the contrary, Polo never visited Bagdad (see «Baudac»), and probably passed through Savah on both journeys. In such a case, the normal trend of the narrative is that Polo reached «Cala Ataperistan» on the outward journey three days after leaving Savah. This points in the direction of Kāšān, where W. Jackson has proposed to place «Cala Ataperistan» (cf. *Y*, III, 18). But this also seems most improbable, now that we have a Kāšān in Polo himself, in such circumstances that make it unlikely that Polo should have used both names for one and the same place (see «Caxan»). «Cala Ataperistan» was probably a fortified village in the vicinity of Kāšān, but distinct from that city.

The legend which Polo (or his father Nicolò according to Z) heard at «Cala Ataperistan» has almost a Manichean flavour, with Jesus who is at the same time God, king and physician (cf. *JA*, 1911, II, 586; and W. BANG, *Manich. Hymnen*, in *Museon*, XXXVIII, 48); but the same symbolism has been attached to gold, incense and myrrh in Christian tradition. As to the stone thrown into the well, an Uighur Nestorian version of the same tradition has been published in F. W. K. MÜLLER,



*Uigurica*, 5-10, and is much closer to Polo's account than those already collected by YULE; a much improved edition of that Uighur version has been given in 1926 by W. BANG, *Türk. Bruchstücke einer nestor. Georgspassion*, in *Museon*, xxxix, 41-75.

Fire-temples (*ātāšgāh*, *ātišgāh*) were still frequently mentioned in the region Yāzd-Sava-Ispahan in the beginning of the 15th cent. (cf. *Not. et Extr.* xiv, 175, 192, 254).

## 100. CALACIAN

*calacia* P; R  
*calacian* F, FA, FB, L, Z  
*calatia* LT

*chalacia* VL  
*chalatia* TA<sup>1</sup>, TA<sup>3</sup>  
*chalazian* V

*galaci* VA  
*galagian* VB

The «Calajan» of the Catalan Map is identified with Qara-Ĵang (see «Caragian») by CORDIER (*L'Extrême-Orient*, 22), but tentatively connected with «Calacian» by HALLBERG, 97. While the position on the map corresponds only to Qara-Ĵang in Yün-nan, the spelling with *-l-*, which no ms. gives for «Caragian», tends to show that the name on the Catalan map has been contaminated by «Calacian».

According to Polo, «Calacian» is the name of the capital of a province called «Egrigaia»; but «Egrigaia» (*q. v.*) is simply the Mongol form of the Hsi-Hsia name of Ning-hsia, the capital of the Hsi-Hsia kingdom. PALLADIUS (*Y*, I, 282-283) supposes that «Calacian» is the «temporary residence» of the Hsi-Hsia sovereigns, 60 *li* west of Ning-hsia, at the foot of the Alashan mountains, and thinks that Polo's form represents «Halachar» or «Halachar», a name mentioned in the 西夏書事 *Hsi-Hsia shu shih*. I am not in a position to suggest a final solution to all aspects of the problem, as I do not have at my disposal the 寧夏府志 *Ning-hsia fu chih* (which PALLADIUS consulted), nor have I been able to trace the origin of often contradictory statements of modern Chinese compilers. But a certain number of points can be determined.

First of all, I do not think we can doubt the equivalence of «Calacian» with the town of Tangut (= Hsi-Hsia) named by Rašidu-'d-Dīn (*Bl*, II, 598) under the form خالجان Ḥalājan (or Ḥaljan); this equivalence, already noticed by KLAPROTH (*Y*, I, 282), has escaped BLOCHET. And it disposes at once of the otherwise wild speculations in *Ch*, I, 188, 238-239, based on a first element *qara*, «black».

A. HERRMANN, *Atlas of China*, map 42-43, has located at the site of the 定遠營 Ting-yüan-ying or 駙馬府 Fu-ma-fu of the Manchu dynasty, west of the Alashan mountains, a place which he calls «Kalachar» or «Hsing-ch'ing», with the indication that the tombs of the Hsi-Hsia princes were there. Hsing-ch'ing is, I think, a mistake, as I take this name to apply to Ning-hsia itself (see «Egrigaia»). But I am doubtful as to the name and the situation of the would-be «Halachar».

I do not know where PALLADIUS has found the alternative form «Halachar» or «Halachar». Only one form of the name is given in *Hsi-Hsia shu shih*, 42, 12 *a*, where it is written 合刺合

察兒 Ha-la-ha-ch'a-êrh, and as the name of a region, not of a city. Wu Kuang-ch'êng printed his *Hsi-Hsia shu shih* in 1825 (prefaces were added in 1826), and he certainly took his Ha-la-ha-ch'a-êrh from the biography of A-shu-lu (? \*Ajul; on him, cf. WANG Hui-tsu<sup>1</sup>, 17, 10 b; on the name, see «Agiul») in *YS*, 123, 2 b. The name appears to be Mongolian, perhaps (in spite of some phonetical difficulties) it is \*Qara-γaġar, «Black earth» (more likely than \*Qara-qajār, «Black bridle», adopted in *Ch'in-ting Yüan-shih yü-chieh*, 7, 7 a), and its form cannot be reconciled with those used by Polo and Rašid, which end in -n. If the two names are to be connected, we ought at least to think of a Mongol adaptation of the original Hsi-Hsia name. But the case remains doubtful.

I am still less convinced that the region (地 *ti*) of Ha-la-ha-ch'a-êrh can be «Calacian» if «Calacian» was the «temporary» residence of the Hsi-Hsia kings 60 *li* west of Ning-hsia. In the winter of 1226-1227, the Mongol armies took Ling-chou, on the eastern side of the Huang-ho, and proceeded further south-east into the valley of 鹽州川 Yen-chou-ch'uan, now 花馬池 Hua-mach'ih; they then turned back to the west to attack Chung-hsing-fu, *i.e.* Ning-hsia, and it was to stop them that the Hsi-Hsia sovereign is said to have quartered troops in «the region of Ha-la-ha-ch'a-êrh». 張鑑 CHANG Chien (1768-1846), in his 西夏紀事本末 *Hsi-Hsia chi-shih pên-mo* (ed. 1884, 36, 8 a), also writes Ha-la-ha-ch'a-êrh, and brings the battle that took place there down to July 1227. Although the details of the campaign have not been studied critically, the natural surmise, if the *Hsi-Hsia shu shih* be correct, would be that the «region of Ha-la-ha-ch'a-êrh» lay in the neighbourhood of Ning-hsia south-east of this place, and not due west and at the foot of the Alashan mountains, as must have been the case with the «temporary residence» spoken of by PALLADIUS (and still less west of the Alashan mountains as in HERRMANN). But I am far from positive on so slender a basis as the single passage taken by Wu Kuang-ch'êng from a biography in the *YS*, which is, moreover, difficult to reconcile with the other accounts of the campaign.

Leaving aside the question of Ha-la-ha-ch'a-êrh, I agree with PALLADIUS on the probable identification of «Calacian» with the «temporary residence», or 離宮 *li-kung*, built by Li Yüan-hao in 1047 on the Ho-lan-shan (on this last name, see below); cf. *Hsi-Hsia shu shih*, 18, 11 b. The *Ta-Ch'ing i-t'ung chih* (204, 2 a) still quotes a passage from an older compilation saying that on the Ho-lan-shan, 60 *li* west of Ning-hsia, there are remains of over a hundred temples, and also of Li Yüan-hao's ancient palace.

I think PALLADIUS is also right when he sees in «Calacian» the Alašai, or Alašai-nuntuq, «Camp of Alašai», which appears four times in the *Secret History* of 1240, § 265, with the Chinese translation 賀蘭山 Ho-lan-shan («Ho-lan mountains»). This last is the Chinese name of the mountain now called by the Mongols 阿拉善 A-la-shan mountains, our «Alashan» (Alašan). The form Alašai of the *Secret History* is confirmed by Rašidu-'d-Din, who speaks of the mountain running along the Hsi-Hsia country, and gives it a name which has been read السای Alasai by ERDMANN, انگشان Ingšan by BEREZIN (on account of the same wrong idea which made him read the name of the Yin-shan mountains where Rašid speaks of the city of «Eçina», *q. v.*, and cf. *JA*, 1920, 1, 182), but which is certainly الشای Alašai (cf. ERDMANN, *Vollständ. Uebersicht*, 62; *Ber*, 1, 119).

The geographical equivalence of Alašai and Ho-lan raises another problem.

Ho-lan appears in Chinese at an early date. The Ho-lan mountains are mentioned for about A.D. 600 in the *Sui shu* (29, 3 b) and for 630 in the *Hsin T'ang shu* (cf. *Br*, I, 239). As a «double» surname, Ho-lan goes back even farther, to about A.D. 400, under the T'o-pa Wei, and it is generally assumed that the surname was taken from the name of the mountain (cf. *Wei shu*, 113, 22 b; *Chou shu*, 20, biogr. of Ho-lan Hsiang; 張澍 Chang Shu, 姓氏尋源 *Hsing-shih hsün-yüan*, 35, 8 b-9 a; 陳毅 Ch'ên I, 魏書官氏志疏證 *Wei-shu kuan-shih chih shu-chêng* of 1894 [unpublished?], ff. 72-73 of my ms. copy). A title of «khan of Ho-lan» (? 賀蘭汗 Ho-lan han) occurs unexpectedly in an inscription of 502 (cf. CHAVANNES, *Mission archéol.* II, 482). In 813-814, the *Yüan-ho chün-hsien t'u-chih* (4, 4 a and 9 a) says that the Ho-lan mountains, because of their trees and grass, have a motley appearance and look from afar like a piebald horse (駟馬 *po-ma*); and as the Northerners (*pei-jên*) say *ho-lan* for «piebald», the mountains have been called Ho-lan-shan, «Ho-lan mountains». The name of Ho-lan-shan continued in regular use in Chinese texts during the Sung and Yüan dynasties (cf. for instance, *Hsi-Hsia shu-shih*, 4, 15 a; 7, 10 b; 10, 8 b-9 b; 18, 11 b; for 989, 1002, 1020, 1047; on Ho-lan-shan in general, cf. *T'u-shu chi-ch'êng*, Shan-ch'uan-tien, end of ch. 79).

The *Yüan-ho chün-hsien t'u-chih*, while mentioning once the Ho-lan mountains 30 *li* north-east of the *hsien* of 寧朔 Ning-shuo, places them 93 *li* west of the *hsien* of 保靜 Pao-ching in the passage where the name is explained; both leave us in the region of Ning-hsia. But the same text, with the same location 93 *li* west of the *hsien* and the same explanation of the name, is quoted in *T'ai-p'ing yü-lan*, ch. 44, as an extract from the 經陽圖經 *Ching-yang t'u-ching*, an unknown work which may go back to the Sui or is perhaps only contemporary with the *Yüan-ho chün-hsien t'u-chih*. The name of Ching-yang is certainly corrupt, and stands probably for 涇陽 Ching-yang, as Ch'ên I corrects it tacitly; but then the Ho-lan mountains would be 93 *li* west of Ching-yang, far to the south-south-east of Ning-hsia. The passage cannot be merely a misquotation from the *Yüan-ho chün-hsien t'u-chih* (unless there is now a lacuna [of a whole line?] in the last work, as is often the case), since the *Ching-yang t'u-ching* alone adds: «The Hsien-pei and other tribes often divide clans according to mountains and valleys; at present, those who have Ho-lan as a surname are named after that mountain.» Moreover, more than one mountain may have been called Ho-lan, «the Motley», under the Wei. There was also during the T'ang a *chou* of indirect administration (*chi-mi chou*) called *chou* of Ho-lan, which is supposed to have been on the territory of Liang-chou and which may or may not owe its name to the well-known Ho-lan mountains (cf. *Ti-ming ta-tz'ü-tien*<sup>1</sup>, 946). But the fact remains that both texts give *ho-lan* as an Altaic word for a «piebald horse».

The foreign names of places in the *Yüan-ho chün-hsien t'u-chih* are generally accurate, and their interpretation is not only old, but based on good information (cf. *TP*, 1929, 251). Ho-lan, in 500-800, was pronounced approximately \* $\gamma\hat{a}$ -lân. We know a number of cases, beginning with the very name Hui-hu of the Uighurs, where a Chinese  $\gamma$ - of that period simply represents à kind of *alif* (') at the beginning of Altaic words, so that we are tempted to see, in the explanation offered by the *Yüan-ho chün-hsien t'u-chih*, an allusion to Turk. *ala*, Mong. *alaq*, «motley». Moreover, it may very well be that the mountain was not really named from its appearance, but from a tribe or a clan called the tribe or clan «of the Piebald [horses]», which would account also for the

surname of Wei times (there are, under the Wei, more than half a dozen double surnames beginning with *ho* [\*γā]; if I am right, all must represent Altaic words beginning with *a*-). The T'o-pa Wei, in spite of the current opinion, were not Tungus, but Turco-Mongols, more probably real Turks, and a Turkish name for the present Alashan may imply that a Turkish-speaking clan had settled there at the beginning of the T'o-pa Wei domination.

Such a name is no surprise, as we meet it elsewhere. In the T'ang period, Chinese texts speak of a Turkish tribe or «kingdom» which they call 駃騠馬 Po-ma, «Piebald Horses», in north-western Mongolia towards Siberia (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 28, 29, 56, 307). J. NÉMETH (*Ungar. Jahrbücher*, x, 32) has already noticed that this Chinese name is equivalent with the Turkish name of an Oyuz tribe, the Ala-yondlu, lit. «Those with piebald horses», and that down to the 16th cent. there was in Siberia, near Narym (on the Ob), a Pégaya Orda, «Piebald Horde» (cf. the Alaḡčïn of Rašïdu-'d-Dïn, quoted by VALIDI in *ZDMG*, 1936, 47, and, in Mongolian, the Alaḡčit [plural form] of «Sanang Setsen», in SCHMIDT, *Gesch. der Ost-Mongolen*, 179). I may add that in Kāšyari (BROCKELMANN, 251) «Ula jonduluγ» is a misreading for Ala-yondluγ and gives the proper Uighur form of the Oyuz tribal name. In the Tibetan translation I found at Tun-huang of an Uighur geographical report of about the 9th cent., mention is made of the «Dru-gu Ha-la yun-log», i.e. the Turks (Oyuz) Ala-yondluγ. As a matter of fact, the native name of the Po-ma tribe has also been preserved in Chinese texts, but sometimes in a corrupt or incomplete form; it is to be read 遏剌 O-la (\*Āt-lât) and 遏羅支 O-lo-chih (\*Āt-lâ-t'šie); cf. CHAVANNES, *Doc. sur les Tou-kiue*, 29; *T'ai-p'ing huan-yü chi*, 200, 2 a, where it is said that *o-la* is the native word for «piebald». There can be no doubt that these transcriptions represent a word connected with Turk. *ala*, «motley», but which does not seem to be *ala*; they suggest \**alač*, unattested as such, although it probably survives in Leb. *alaš*, Sag. Koib. *alas*, «spotted woodpecker» (RADLOV, I, 364, 365, with a wrong etymology by *ala* + *quš*), and possibly in the Kirghiz clan-name *Alaš*; the developed form *alača* exists in many dialectical forms, with which I propose to deal now.

Coman, Kaz., Ĵay. *alača*, Kir. *alaša* (< *alača*), Bar. *alažaq* (< *alačaq*) all mean «motley», «variegated». The same etymology probably holds good for Uiy. *alaču* (BROCKELMANN, *Kāšyari*, 6), Ĵay. *alačuq*, Kkir., Kir., Kač., Kaz., Krm. *alačiq*, Tel. *alančiq*, Küär. *alanžty*, all meaning a kind of tent, hence Russian *lačuga*; the meaning «tent of motley material» survives in Türkm. *alafa*; moreover, Kumd. *alančiq*, a kind of «woodpecker», is fundamentally the same as the above-mentioned *alaš*, *alas*, of other dialects.

The problem is not so clear when we come to the dialectical forms applied to horses. Turk. *alaša* means «gelding» in Kazan and the Crimea, but «any animal trained to carry a saddle or pack-saddle» in Osmanli, and one is tempted to connect with this last meaning Ĵay. *alaša*, «the back». From *alaša* (or from the Čuvaš form *laža* of the word) is derived the common Russian word for «horse», *lošad'*, appearing in a Russian chronicle s.a. 1103 (for fall of *a*- in Russian, cf. *alačuq* > *lačuga* [although *lājuq* < *alačuq* occurs also in Arabic; cf. QUATREMÈRE, *Hist. des Sultans mamlouks*, I, I, 192]; \**alafa* > *lafa*); other connected forms are Russ. *lošak*, Pol. *losz*, *loszak*; cf. M. MELIORANSKIĪ, id *Izv. Otd. R. yaz. i slov. I. Ak. N. 1905 g.*, x, 4, 122-124; E. BERNEKER, *Slav. etym. Wörterbuch*, s. v. *losz*, etc.; the apparent resemblance between Russ.

*lošak*, «mule», and Mong. *lausa*, *lōsa*, id. (? < Ch. *lo-tzū*), seems to be purely accidental. In Siberian dialects (Bar., Tur., Tob.), *alaša* means only «small», «small-sized», and such is the case also with Kir. *alasa* (< *alaša*). MARR (*Izv. Ak. Nauk*, 1925, 971) would derive the word from the Abkh. word for «horse», which he divides into *a-laša*; speaking without authority, I should rather think that Abkh. *alaša* is borrowed from the Turkish.

This *alaša*, although unknown to-day in Mongolian, existed in that language in the past, since we find in the *Secret History* (§ 273) the plural form *alašas* of a word *alaša*, with a Chinese translation 淮馬 *huai-ma*, «*huai*-horse». *Huai* is only the name of a well-known river, and I cannot trace the origin of the term *huai-ma*. But it just happens to be used, also in the 13th cent., in a poem by Yüan Chüeh (1266-1327), as a designation of polo-ponies; and this connects Mong. *alaša* both with the «horse» and the «small-sized» meanings of the word in various Turkish dialects. The word has survived in Manchu, written *alašan*, with the meaning 驚馬 *nu-ma*, «broken-down horse», «jade» (*Ssü-t'i ho-pi wên-chien*, 31, 69 b).

Since *-a*, *-ai*, *-an* endings are commonly interchangeable in Mongolian, it seems a natural conclusion to identify with *alaša* the Alašai or Alašai[-nuntuq] of the *Secret History* and the modern «Alashan» (Alašan). An 阿喇沙阿爾山 *A-la-sha-a-lan-shan* occurs in YS, 100, 2 a (also *Hsin Yüan shih*, 100, 4 a), but I do not know what to make of it. Our «Alashan» is written Alašan in Manchu and Mongolian, but 阿爾善山 *A-lan-shan-shan* («\*Alašan mountains») in Chinese, in *Ssü-t'i ho-pi wên-chien*, 3, 62 a. In any case, it is clear that «Alashan» is to be transcribed in one word, and not «A-la Shan» or «Ala Shan» as in almost all our books and maps, including *Br*, I, 239, and STEIN, *Serindia*, 724 (not to mention «Ara-shan» in both editions of GILES's *Chin.-Engl. Dict.*, or the «Alade-shan» of *Y*, I, 282, which I corrected in *Y*, III, 62).

But we are faced now with two difficulties: the initial *c*- of Calacian if we are to connect it with Alašai, «Alashan», and the choice which is to be made between Ho-lan < \**ala*, «motley», «piebald», and *alašai* = *alaša*, «small-sized horse».

The initial *c*- of «Calacian» cannot be explained as a notation of mediaeval Mongolian *h*-, as is the case for instance with «Cogacin» (*q. v.*) = \*Hügäčün, since *ala* is not among the Mongol words formerly beginning with *h*-, and since moreover Rašidu-'d-Dīn would then write the name with *h*-, while he uses here *ḥ*-. Provisionally, I suppose that Polo's «Calacian» and Rašid's Ḥalājan or Ḥaljan represent the Hsi-Hsia form of the name, which had hardened the initial *alif* rendered by *γ*- in the ancient transcription Ho-lan (\**γā-lân*), and by *h*- in the Tibetan «Ha-la yun-log». Unfortunately, this conjectural Hsi-Hsia form does not seem to have any modern representative. A word 喀爾占 *k'a-êrh-chan* enters into a place-name of the Alashan region (*Ta-Ch'ing i-t'ung chih*, 412 B, 1 a), but it occurs also in many different place-names all over Mongolia; although POPOV has read it sometimes as *ḥarjan*, sometimes as *ḥaljan* (*Mên-gu-yu-mu-czi*, 62, 66, 83, 110, 111, 487), it seems to be always Mong. *ḥaljan* (< *qaljan*), «bald», and I do not think it can have anything to do with «Calacian».

As to the second point, I feel that we should separate *ala*, \**alač*, *alača*, «motley», and *alaša*, «small», «gelding», «small-sized horse». It is possible that, in the course of time, the original name meaning «piebald horse» was replaced by the other one, of somewhat similar sound, meaning «small-sized horse», but I can offer no proof beyond the bare fact of the double name;

it took longer for the Chinese to abandon «Ho-lan mountains» in favour of «Alashan», and even now both forms occur on Chinese maps. The *Mêng-ku yu-mu chi*, transl. POPOV, 413-415 (cf. also MOSTAERT, *Textes oraux ordos*, 329), distinguish the Ho-lan mountains, in Mong. Alašan, and to the south-west, the 龍首 Lung-shou mountains («Dragon-head mountains»), in Mong. Alaq-ōla, as if by a sort of revival of the old nomenclature; Alaq-ōla means «Motley mountains». Perhaps both names are also to be recognized in the above-mentioned A-la-sha-a-lan-shan (? Alaša and Alan mountains) of YS; \*alan would be a dialectical Turk. form of *ala*, while *alaq* is the purely Mong. form. An Alaq-ōla (but not the «Alašan») is mentioned in «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 259).

It is interesting to note that Polo found three Nestorian churches in «Calacian». I am not aware that any relics of ancient Western creeds have as yet been discovered in those parts, but we know that foreigners had settled there early, in the 5th cent., particularly near the present *hsien* of Ling-wu (= Ling-chou of the Manchu dynasty), east of the Yellow river, and south-east of Ning-hsia. When Su-tsung ascended the Imperial throne at Ling-wu itself in 756, with the support of Western troops, we know from the Nestorian tablet of 781 that he re-established the Nestorian churches «in the five *chün* Ling-wu and others». Either «Calacian» or «Egrigaya» itself (= Ning-hsia) must be the «city of Tangut» where, about Polo's time, Rabban Çauma and the future Mar Yahbalaha III met with a very warm reception at the hands of the local Christians (cf. *Mo*, 100). At a later period, there was a Jewish community at Ning-hsia; members of the Jewish Chin family of Ning-hsia are mentioned several times for the years 1489 and 1512 in the Jewish inscriptions of K'ai-fêng (cf. TOBAR, *Les inscriptions juives*, 49-50, 54, 64).

## 101. CALAMANZ (INK-HORN)

*calamanç* F  
*calamaria* LT

*chalamai* TA<sup>3</sup>  
*chalamari* TA<sup>1</sup>

*charamalli* V  
*pugillares* Z

This is the word translated «ink-horns» in Vol. I, 368. YULE, who wrote *calamanz* in agreement with the 1824 edition of F, thought that the form was «more suggestive of the Persian *Ḳalamdān* than of the Italian *Calamajo*» (*Y*, I, 84; II, 272). The old Latin translation (1824 ed., 441) gives *calamaria*. «Calamans» is surprising, and probably corrupt for «calamars». At any rate, the Pers. *qalamdān* is out of the question. Just in Polo's time, the *Codex Cumanicus* (KUUN, 90) correctly gives *calamarium*, with Pers. «duet» and Turk. «duat» as equivalents (< Arab. *dāwāt*, id.). Consequently, and with the exception of «Mulecte», not one remains of the additional Oriental words which YULE thought he had discovered in Polo and which he listed in *Y*, I, 84.

Engl. *calamar* or *calamary*, the name of a cephalopod, is the same word as *calamarium*, with a new application. The case of the ancient French *calamar*, *calemer*, etc., «inkstand», which has given French *calmar*, the name of a cephalopod, is of course parallel.

## 102. CALATU

<i>calaiati</i> (city) R	<i>calatu</i> F, Fr, FA, FB, TA <sup>1</sup> ,	<i>chalatu</i> TA <sup>1</sup> , V
<i>calata</i> Ft	LT, VB, Z, V, L,	<i>chatalu</i> TA <sup>3</sup>
<i>calati</i> VB	R (gulf)	

This represents قلات Qalhāt on the coast of Omān, now in ruins. Cf. on it Y, II, 451; Fe, 336; HALLBERG, 97-98; DAMES, *Barbosa*, II, 69. The Portuguese form of the early 16th cent. is «Calayate», and this may be responsible for RAMUSIO's «Calaiati», since Z has also «Calatu». Nevertheless, Conti's «Calacatia» and Fra Mauro's «Calahat» suggest that the pronunciation may have been «Qalahāt».

In 1225, Chao Ju-kua names a «country» of 伽力吉 Ch'ieh-li-chi (equivalent to \*Kalikit or \*Kalkit) which may be Qalhāt, as supposed by the translators (HR, 117, 122); but the phonetic correspondence is not very satisfactory.

## 103. CALIF

<i>alchalif</i> Z	<i>califfus</i> G	<i>calis</i> F
<i>calif</i> F, L, Z	<i>califica</i> (adj.) L	<i>chalefo</i> VA
<i>califa</i> VB; R	<i>califo</i> VB, VL	<i>chalif</i> TA <sup>3</sup>
<i>califfa</i> , <i>callifa</i> , <i>calliffo</i> , <i>chalifa</i>	<i>califus</i> LT, Z	<i>chaliffo</i> TA <sup>1</sup> , TA <sup>3</sup> , VB
VB	<i>caliphe</i> FA, FB	<i>chalifo</i> TA <sup>1</sup> , V, VA, VB, VL
<i>califfo</i> TA <sup>1</sup>	<i>caliphus</i> P	

«Calif» is the Arab. *ḥalīfa*, lit. «lieutenant», «successor», «vice-gerent»; cf. LOKOTSCH, *Etym. Wörterbuch*, No. 798; YULE, *Hobson-Jobson*<sup>2</sup>, 146-147. Z gives «califus», but also «alchalif». The latter form is worth noting, as it seems to agree with one of the forms of the word in ancient French, the «algalife» of the *Chanson de Roland* (a form which came from Spain and refers to the calif of Cordova). The form without the Arabic article *al-* came from the Crusaders and always referred to the calif of Bagdad. In the case of Z, I hesitate, however, to be too positive, as *al-* of «alchalif» might be a misunderstood Italian article, in case Z was retranslated into Latin from Italian.

The name of the calif is mentioned in Mongolian in the *Secret History* as Qalibai-soltan,

«Sultan Calif» (§§ 260, 270). It is written 哈里發 Ha-li-fa in Chinese in the *YS*, s. a. 1253 and 1258, and also in 郭侃 Kuo K'an's biography (*YS*, 149, 6b) and in the *Hsi shih chi* (cf. *Br*, I, 122, 138-139).

## 104. CALIZENE

*calizene* VB, R

[*chalicene* VB<sup>1</sup> B. p. 213]

An adjectival form derived from Ar. خليج *Halīj*, which is applied particularly to canals fed from the waters of the overflowing Nile; YULE (*Y*, II, 439) has already quoted a parallel form «Caligine» from Dati's *Sfera*. The identical text in VB shows that, contrary to YULE's hypothesis, there is in this passage no editorial change due to RAMUSIO. For other references, cf. HEYD, *Hist. du Commerce*, II, 60; BLOCHET, *Moufazzal*, 124.

The spelling adopted in *B*<sup>1</sup>, 369, is «Caligene», and I think it is more probable that this rather than «Calizene» is Rustichello's original spelling; but we have retained the form occurring in VB and R, which is also adopted in *RR*, 415.

## 105. CAMADI

*camadi* FA, Z

*camady* FB

*camandi* F, L

*camandu* LT, R

*camath* VL, S

*camadi* (?) FA

*chamadi* TA<sup>1</sup>

*chamand*, *chamandi* TA<sup>3</sup>

*chamandin* V

*chomandi* VA

*comandi* Fr

*comandu*, *comanou* P

Qamādin («Camadi» < \*Camadi = \*Camadin?), a suburb of Jiruft (Jīrāft); cf. *Y*, I, 106, 113; the identification seems certain, although from CORDIER's note it is not clear whether HOUTUM-SCHINDLER's «Shehr-i-Daqīánús» is the same as SYKES's «Sher-i-Jiruft». *Pe*, map facing p. xxxvi, places «Camadi» at «Sargāz» («Sardjas» of STIELER's *Hand-Atlas*). Cf. also MINAEV, *Marko Polo*, xxxi; *LS*, 315; *Mi*, 374. BENEDETTO had preferred «Camandi» in *B*, 28, but later decided rightly in favour of «Camadi» in *B*<sup>1</sup>, 41, 440.

## 106. CAMALOTI

*camaloti* VB

This seems to be a corruption of «Ciandu»; see «Naian».



## 107. CAMBAET

<i>cambaech</i> P	<i>cambaich</i> G	<i>chambaet</i> TA <sup>1</sup>
<i>cambaet</i> FA, FB, TA <sup>1</sup> , L	<i>canbach, chanbach</i> V	<i>chanbaet</i> TA <sup>3</sup>
<i>cambaeth</i> Z	<i>canbaet</i> F, VB	<i>chanbrach</i> VA
<i>cambaia</i> R	<i>canbaot</i> Fr, t	<i>tambaet</i> (?) FB

Mohammedan authorities write كنباية Kanbāyāh or كنبایت Kanbāyat. On this port of Guzerat, the Cambay of our maps, cf. *Y*, II, 398; *Y*<sup>1</sup>, IV, 285; *Hobson-Jobson*<sup>2</sup>, 150; *Fe*, 203 and 709; DAMES, *Barbosa*, I, 117, 138-140; HALLBERG, 101-102. To the early mentions quoted by YULE and DAMES, add perhaps that of Ya'qūbī c. 875-880 (*Fe*, 48), and certainly that of the *Ḥudūd al-'Ālam* of 982-983 (cf. *Mi*, 245). Almost in Polo's time, the city was also referred to as «Combaeth» in 1307 by Hethum, as «Cambeth» in 1320 by Marino Sanudo, and as «Cambaeyt» in 1314-1328 by Guillaume Adam (*Hist. des Crois.*, Arm., II, 126, 266, 552). Pegolotti's «Iacca conbaiti» or «ganbainti» is probably «Cambay lac» (ed. EVANS, 420).

Cambay is mentioned in 1225 by Chao Ju-Kua in the form 甘瑟逸 Kan-p'a-i (then pronounced \*Kām-b'a-·jët); cf. *HR*, 88, 90. It is written 坎八葉 K'an-pa-yeh in the early 15th cent. map published by PHILLIPS (and once, by a clerical error, K'êng [坑]-pa-yeh, which PHILLIPS has wrongly adopted in his list, *JNCB*, xx, 224). The 乾伯 Ch'ien-po (then read K'ien-pai) of *YS*, 12, 4b, s. a. 1282, has been identified with Cambay by KUWABARA (*Mem. of Toyo bunko*, 7, 87). But, because of the names associated with it, it must be Kämpe in Sumatra, on which cf. *BEFEO*, IV, 344; *Fe*, 652.

## 108. CAMBALUC

<i>cabale, cambalii</i> G	<i>cambalu</i> F, FA, L, LT,	<i>chamblai, chanblai, cianblai,</i>
<i>cabaluc, ganbalu</i> F	P, VL, Z; R	<i>cianblau</i> TA <sup>3</sup>
<i>cablau, camblau, chablau,</i>	<i>cambaluc</i> FA, Z	<i>chamblay, chanblau</i> TA <sup>1</sup> , TA <sup>3</sup>
<i>chamblau, choblau</i> TA <sup>1</sup>	<i>cambalut</i> FB	<i>chanbaluch, chanbalun, chan-</i>
<i>caibalut</i> FA	<i>canbalu</i> F, Fr, t, L, VB	<i>belun, charain chabuer, gan-</i>
<i>camalau, camallau, chamalau,</i>	<i>canbaluc</i> F, Z	<i>belun</i> V
<i>chamaut</i> VB	<i>chabaluc</i> L	<i>chanbellu, chanbelu</i> VA
<i>camballu</i> LT	<i>chambalu</i> Lr, VLr	<i>tambalut</i> FBr
(c. 85)		
<i>cambalu</i> LT; R	<i>ganbalu</i> L	<i>geribalu</i> TA <sup>3</sup>
<i>chanbaluche</i> V	<i>garibalu</i> F, FA, FB	<i>gharibalu</i> TA <sup>1</sup>

Represents خان‌باليق Han-baliq, «Royal City». This name of Peking is purely Turkish, and YULE (*Y*, II, 14), CORDIER (*L'Extrême-Orient dans l'Atlas Catalan*, 8), BLOCHET (*Bl*, II, 455, who gives an imaginary form in Mongol writing), and CHARIGNON (*Ch*, II, 55, 163, 187) are

wrong in thinking it to be Mongol; it has not even been borrowed by the Mongols, and no text in the Mongol language gives it; in Turkish, *balıq* is already known in the runic inscriptions, and is listed for «town» in Kāšyari (BROCKELMANN, 29); it has survived to the present day. Rašidu-'d-Dīn's mss. hesitate between خان بالیق Ḥan-balıq and خان بالیغ Ḥan-balıy (*Bl*, II, 456); Waśśāf (*Ha*<sup>2</sup>, 45) writes خان بالیغ Ḥan-balıy. We find «Cambaliech» in Montecorvino (*Wy*, 347) as well as in the much-debated letter attributed to *Peregrinus de Castello* (*Wy*, 367). Odoric's form is «Cambalec» (*Wy*, 471, 475, 478), which also appears in the Emperor's and the Alans' letters of 1336 (cf. *Y*<sup>1</sup>, III, 181, 183), and in Marignolli (*Wy*, 526, 529); hence «Chambalech» on the Catalan Map (*Y*<sup>1</sup>, I, 301). Andrea da Perugia writes in Latin «Cambaliensis civitas» (*Wy*, 374). Pegolotti's very bad transcriptions provide us with a «Gamalec» (*Y*<sup>1</sup>, III, 149). The name has long survived among Turkish- and Persian-speaking peoples. In 1419-1421, Šāh-Rūḥ's envoys always speak of خان بالیق Ḥan-balıq, but the *Zafār-namāh* has «Ḥan-balıy» (cf. *Not. et Extr.* XIV, I, 395, 401, 500). The ms. Sino-Uighur Vocabulary of the Ming dynasty in the School of Oriental Studies («Kö Kwö i yü») writes 罕 巴 里 Han-pa-li = Ḥan-balıq, with its Chinese equivalent, Pei-ching, Peking. Cf. also the «Kambaluk» and «Chaan balug» in WITSEN (1785), I, 277, 495. For other Mussulman writers who, from the middle of the 13th cent. down to the end of the 17th, have named Ḥan-balıq or Ḥan-balıy, see *Fe*, 711. In the first years of the 17th cent., Matteo RICCI was able to satisfy himself that the Mussulmans from the West still called Peking by the very name Polo had used (cf. TACCHI-VENTURI, *Opere stor. del P. M. Ricci*, I, 296-297, 377; II, 352). The name has now been forgotten for more than a century; the itinerary copied in 1812 by Mir 'Izzet Ullah has already بچین Bājīn, the only form known to-day in Chinese Turkestan (*JRAS*, No. XIV, 308, where the name is miswritten and misread بچین «Peḥin»; LE STRANGE [*Nuzhat al-Qulūb*, transl., 235, 250] is mistaken when he thinks that the name of Peking could already have existed in the first half of the 14th cent.). In the transcriptions of Ḥan-balıq, the variations between <sup>o</sup>baluc and <sup>o</sup>balec are due to the peculiar nature of Turk. *i* (see «Achbaluch»).

It would most probably be wrong to suppose that the name of Ḥan-balıq, as applied to Peking, originated when Qubilai moved the capital there in 1260. The Syriac work on Mār Yahbalaha III speaks of Bar Čauma's father who lived «in the town called Ḥan-balıq, that is to say the Royal town of the land of the East»; and the time when that father could have lived in Ḥan-balıq is 1240-1250 at the latest. Of course, it might be suspected that the author, writing at the beginning of the 14th cent., used the name current in his own age; but there are no traces in the work of a modernized nomenclature (see CHABOT, *Hist. Mar Jabalaha III*, 9, and *Mo*, 94). The name of Ḥan-balıq appears also in the *Tables* of Našīru-'d-Dīn Aṭ-Ṭūsī (cf. *Fe*, 358), who is said by FERRAND to have died in 1261; but FERRAND is wrong; the death of Našīru-'d-Dīn occurred in June 1274 (cf. BROWNE, *Lit. Hist. of Persia*, II, 485). In fact, I think that the name of Ḥan-balıq was given in Central Asia to Peking while it was still the capital of the Chin, that is prior to 1215, and had perhaps then been in use already for a whole century. It is met with even earlier, but as an epithet for another capital (= Ch. *ti-ch'êng*, «Imperial City»): in the Uighur translation of Hsüan-tsang's biography, Ḥan-balıqī is the designation of the then capital Ch'ang-an (Hsianfu); cf. A. VON GABAIN, *Die uig. Uebersetzung*

in *SBPrAkdW*, 1935, 156, 168. Moreover, it is to the ancient Chin capital that the name, strictly speaking, is applied by Polo and Odoric, to distinguish it from Qubilai's new capital of Ta-tu (see «Taidu»).

Before becoming the capital of the Chin, Peking had been that of the Liao. The Liao had their Southern Capital (Nan-ching) there first in 938 (*Liao shih*, 4, 1a; not 936, as in BRETSCHNEIDER, *Rech. archéol.*, 18, and in *Y*, 1, 375, nor 986, as in *Ch*, 11, 55); in 1012, resuscitating an old Chinese name, they called it 燕京 Yen-ching. The Chin at first retained that name, but, in 1153 (*Chin shih*, 5, 4b; 24, 9a; not 1151, as in *Y*, 1, 375, *Ch*, 11, 55, and *RR*, 429), changed it to Chung-tu, «Middle Capital», the prefectural name of the city being 大興府 Ta-hsing-fu. The plans for the Imperial Palace had been submitted to the Throne in 1151 (*Chin shih*, 5, 3b; 24, 9a), and, in the same year, orders had been given to extend the walls of the city. These walls and the Palace suffered great damage when the Chin capital was conquered by the Mongols in 1215. Ögödäi is reported to have created there a census administration in 1235 (*YS*, 58, 1b). Already in 1215, the Mongols are said to have established there a «prefectorial office (*fu*) of Ta-hsing in general charge of the district (*lu*) of Yen-ching» (Yen-ching-lu tsung-kuan Ta-hsing-fu); but, besides the names of Yen-ching and Ta-hsing, one still meets, occasionally, the name of Chung-tu (as for instance, in 1261, 7th moon; 1262, 10th and 11th moon; *YS*, 4, 5a, 8b, 9a). This must be a survival in Mongolian and in texts translated from Mongol originals of the name the Mongols used at the time of the conquest of 1215; for we have in Mongolian Jungdu (= Peking) in the *Secret History* (§§ 247, 248, 251, 252, 273).

In 1263, the name of K'ai-p'ing-fu had been changed to Shang-tu («Upper Capital»). The next year, the officials of the Imperial Secretariat reported as follows: «The site of the Palace at K'ai-p'ing-fu has received the higher name of Shang-tu. We beg that the Grand Secretariat separately established at Yen-ching should also be named appropriately». The name of Yen-ching was changed to Chung-tu on September 5, 1264 (that is to say the Capital again took the name given to it by the Chin in 1153); but the prefectural name of Ta-hsing-fu remained the same as before (*YS*, 5, 9a; 58, 1b-2a). On November 7, 1261, the rebuilding of the ancient walls of Yen-ching had come under deliberation (*YS*, 4, 8b), but it seems that no decision was reached. At any rate, nothing was done until 1267, when it was decided, instead of restoring Chung-tu, to build to the north-east of it a new walled town which was called Ta-tu and to which the offices of the Capital were transferred in 1272 (see «Taidu»). The name of Chung-tu was known to Rašidu-'d-Dīn, who writes it چونگدو Jungdu (*Bl*, 11, 19, 386, 455), and expressly says that it is the Chinese name of Ḥan-baliq, while he does not say the same of Ta-tu. Elsewhere, he speaks of Chinghiz-khan, who stopped in the neighbourhood of «Jungdu, which the Mongols formerly called Ḥan-baliq» (*Ber*, 111, 22). It is Jungdu which has been mistaken for Shang-tu by LE STRANGE, *Nuzhat al-Qulüb*, transl., 250-251.

The historian Hethum speaks of a city built by Qubilai, where Qubilai always lived, which was called «Jong»; this name seems to be meant for Chung-tu, which was mistaken for Ta-tu (cf. *Hist. des Crois.*, Arm., 11, 988). The «Jung» or «Jong» of Paolino da Venezia is simply taken from Hethum (cf. GOLUBOVICH, *Bibl. bio-bibl.*, 11, 93, 97, 603, where the identification with Yang-chou is wrong).

The metropolitan see of «Cambaluc» («Cambaliensis»), created in 1307 for Giovanni de Montecorvino, lasted a fairly long time, theoretically at least; a Dominican friar Jacobus was still appointed to it in 1426 (cf. C. EUBEL, *Hierarchia Catholica*, 1898, p. 165).

«Garibalu» of FA, FB, and «Gharibalu» of TA («Gazibalu» is a misreading of PAUTHIER of his ms. C = our FB) are alterations of Cambaluc. As for the sources of the Catalan Map of 1375, it may be worth noticing that it used a Polo ms. giving that wrong reading («Guaribalu» and «Chambalech» are the forms of the map); cf. HALLBERG, 235; CORDIER, *L'Extrême-Orient...*, 14, 38.

Nothing can be made out of Mufazzal's fantastic description of the Chinese town of Qārā, which may or may not have been altered from Ḥan-baliq (cf. BLOCHET, *Moufazzal*, 691).

### 109. CAMLET

*cambelloti, cambeloti* P  
*cameloz* FA, FB  
*chamelles* FB  
*ciambellotti* TA<sup>1</sup>, TA<sup>3</sup>  
*cianbellotti* TA<sup>3</sup>  
*çambelloti* L

*çambeloti* L, Z  
*çamelloit, giambellot, giam-*  
*belot* F  
*giambellotti, giambeloti* LT  
*gianbellotti* TA<sup>1</sup>

*zambellotti* R  
*zambeloti* VL  
*zambelotis* G  
*zanbelloti* V, VA, VB  
*zaneloti* V, VA

YULE (*Y*, I, 283) and HEYD (*Hist. du commerce*, II, 703), following AMARI (*Storia dei Musulmani di Sicilia*, III, 890), are in agreement in deriving «camlet», «camelot» from the Arabic *ḥaml*, «pile or plush», or from *ḥamlah*, which JOHNSON'S dictionary explains as «Camelot, silk and camel's hair; also all silk or velvet, especially pily or plushy». DAMES (*Barbosa*, I, 120) did not believe that camel's hair had even been used in making «camlet», but that clearly shows that he was unaware of Polo's text. MURRAY'S *NED* gives the same explanation as YULE and HEYD, but adds that the word had been contaminated at an early date by «camel». LOKOTSCH (*Etym. Wörterbuch*, No. 653) shows hesitation. I feel inclined to go further than both and to revive the old opinion that «camel» provides the true etymology of «camlet». It is also the solution adopted in BLOCH'S *Dict. étymologique*. Although «camelot» is not given under *camelus* in VON WARTBURG'S *Franz. etymol. Wörterbuch*, I think he agrees with BLOCH since he revised all the notices in the book.

JOHNSON'S rendering of *ḥamlah* as «camelot» has a fair chance of being the result of his personal etymology of «camelot» or «camlet» rather than the exact translation of a definition given in native sources; and what follows «camelot» is simply a definition of the latter word as it is to be found in European dictionaries. If «camlet» was a mediaeval borrowing from Arabic *ḥamlah*, I find it hard to account for the largely prevalent forms with a palatalized initial, from JOINVILLE'S «chamelot» to all the readings of the Polian mss. «Camelot» is a Provençal, Picard and Normand dialectical form, which has persisted in classical French and in English. But, if

the word came into being in the East among the crusaders, it was quite natural that the French «chameau» should give «chamelot», and that the latter palatalized form should be adopted in Ital. *giambellotti*, *zambellotti*, etc.

Moreover, *ḥamlah*, not very satisfactory from a phonetic point of view, is semantically possible, but no more. *Ḥamlah* means «a carpet with long nap», or «fringed»; from the same root comes *miḥmal*, *maḥmal*, «velvet» (cf. *JA*, 1920, II, 185; *TP*, 1933, 429). Whatever the material used for «camlet» may have been, the technique of its weaving was and is quite different from that of a carpet or of a velvet.

As soon as *ḥamlah* is rejected, the only remaining requisite to enable us to derive «camlet» from «camel» is that it should have been made of camel's hair. Now it is quite true that mohair was much used in Western Asia to make camlets, but no positive evidence of this has been adduced for a date prior to the 15th cent. Polo's text, which describes camlets made at Ninghsia with camel's hair, is almost two centuries earlier. Chou Ch'ü-fei, writing in 1178, in a passage which Chao Ju-kua reproduced in 1225, speaks of «camel-hair satin of all colours» (五色駝毛段 *wu-sé t'o-mao tuan*; 段 *tuan* = 緞 *tuan*) which was made at 吉慈尼 *Chi-tz'ü-ni*, probably Ghazni (*HR*, 138). Both authors also speak of «hair satin» (*mao tuan*) as made in Rüm (= Asia Minor; *HR*, 138), and this may or may not be a textile made of mohair. Chao Ju-kua alone mentions a third time «hair satin» as a product of Maghreb; HIRTH and ROCKHILL have translated «wool (or camel's hair)» (*HR*, 154). Even granting that the last two examples are ambiguous, the first one leaves no room for doubt, and I believe that Chou Ch'ü-fei's «camel-hair satin» is «camlet». *C.* 1340, Pegolotti often speaks of «ciambellotti» and three times he mentions «lana da ciambellotti» or «lana da fare ciambellotti». EVANS (*Pegolotti*, 416, 421) has explained «lana da ciambellotti» as «mohair», because Pegolotti's words indicate «a woolly substance». He may be right, but camel's hair could also be termed «lana». Pegolotti's contemporary Marignolli says of camel's hair in general (not of camel's hair in China as might be deduced from HEYD, *loc. cit.*, II, 704) that it is «the finest wool (*lana*) in the world after silk» (*Wy*, 540; *Y*<sup>1</sup>, III, 241). But even if Pegolotti's «lana da ciambellotti» was mohair, the fact remains that the oldest text which speaks definitely of the material of which «camlet» is made says it is camel's hair. This evidence, I think, enables us to conclude that «camlet» is derived from «camel».

I leave out the question of «camelin», which LITTRÉ treats as the same word as «camelot»; BLOCH's *Dict. étymologique* (1932) still says, *s. v.* «camelot», that the latter word was also used in the forms «chamelin», «camelin». Early English texts mention the «cameline», which MURRAY derives from «camel». BRĂTIANU (*Rech. sur le commerce génois* [1929], 110) speaks of the «camelin» as having been made in the East. But YULE had already said, in his note on Polo's «camlets», that «*camelin* was a different and inferior material». HEYD (II, 704) insisted that the «camelin» was probably a European fabric (but there is some contradiction in the latter part of his note 8). In the *Codex Cumanicus* (KUUN, 108), «*gamelinus*» only occurs as the name of a colour, while «*boxac*» and «*boxag*» are given as its Persian and Turkish equivalents, respectively; although the final of the Oriental word (*\*bozaq*) remains unexplained, the editor and RADLOV were probably right in seeing in *\*bozaq* the Turk. *boz*, «grey».

Marignolli, when discussing the first garments worn by Adam and Eve, expresses the opinion that they must have been made of vegetable fibres, like the rain-cloaks of the Indian «camalli, portantes scilicet onera», who were also litter-bearers, and adds that he brought back to Rome (*portavi*; not «wore» as in YULE) one of these cloaks of the *camalli*, similar to S. John the Baptist's garment (*Wy*, 539-540, Y<sup>1</sup>, III, 241). Of course the «portantes onera» are *ḥammāl*, an Arabic word still used in India, especially to designate palankin-bearers (cf. YULE, *Hobson-Jobson*<sup>2</sup>, s.v. «hummaul»; DALGADO, *Glossário Luso-Asiático*, s.v. «amal»; LOKOTSCH, *Etym. Wörterbuch*, No. 806 [correct for Prov. *camálo*, doubtful for French «camelot» in the sense of «petty haberdasher»; cf. O. BLOCH, *Dict. étymol.*, s.v. «Camelot II»]). But at the same time, Marignolli seems to have confused the *ḥammāl* themselves with their wool- or hair-coat, frequently mentioned after the Portuguese conquest in forms varying from *camlees* or *kummul* in English to a more fixed *cambolim* in Portuguese; it is sometimes referred to as «a cloak for rainy weather» (cf. *Hobson-Jobson*<sup>2</sup>, s. v. «cumbly, cumly, cummul»; DALGADO, *Glossário Luso-Asiático*, s. v. «cambolim»; add «cambals» in FILCH's relation of c. 1591, in J. C. LOCKE, *The first Englishmen in India* [1930], 117, 199). Barbosa speaks of *cambolim* at Socotra and DAMES (*Barbosa*, I, 63; II, 233) said that the word was «evidently identical with the old French and English *cameline*». This is a mistake, although it also occurs in MURRAY's *NED*. *Cambolim* and the other forms posterior to A. D. 1500 represent an adjectival form based on Skr. *kambala*, «wool», such as Hindī *kamli* or Konkan. *kāmbḷém*. «Kembeli» has passed into Malay as the name of a coarse hair cloth (FAVRE, *Dict. malais-français*, I, 352). A connection would only be possible if «camelin» and «cameline» could go back to the same original as *cambolim*, an assumption which the early date and the conditions of the appearance of these forms make, I think, impossible.

## 109 A. CAMOCAS

This word does not occur in Polo's text, and it is by mistake that it has been introduced as «camucca» by BENEDETTO into his translation (*B*<sup>1</sup>, 140, 452), instead of the correct «camut» (*q. v.*). But we know from Polo's inventory that the traveller left several pieces of «chamocha» (cf. Vol. I, 556, 557), so that a note on «Camocas» may not be out of place here.

The word occurs hundreds of times in mediaeval inventories, but not before the beginning of the 14th cent. Its forms are many: Fr. «camocas», «camocaz», «camocatz», «camoukaз», «kamoukas», «kamokaus» (pl.), «kamoquau», «quamoquau», «camoquois»; It. «camucca», «cammocca» (in Pegolotti), «chamucha», «camucha», «camocato»; Engl. «camaca» (still given in MURRAY's *NED*), «camacaa», «cammaka»; Med. Lat. «camoca», «camocatus» (DU CANGE; KUUN, *Codex Cumanicus*, 108), «camboca»; Span. «camocan» and «camucan» (in Clavijo, but with some inconsistencies; cf. also Dozy, *Glossaire*<sup>2</sup>, 246); Med. Greek, *καμουχᾶς*, rarely *χαμουχᾶς*; Bulg. «kamuha», Hung. «kamuka». The difference between certain French forms in *-au* and Clavijo's forms in *-an* is perhaps due to misreadings. The bulk of the quotations have

been collected in FRANCISQUE-MICHEL, *Recherches sur le commerce, la fabrication et l'usage des étoffes de soie, d'or et d'argent*, II [1854], 171-174. «Camocas» was still made in Cyprus at the beginning of the 16th cent.

Although the components and the technique of the manufacture of «camocas» may have changed in course of time, there is no doubt that the word at first designated a costly Oriental textile of silk damask, woven in all colours, sometimes with stripes of gold and silver, the designs being mainly birds. The «camocas» may be said to have been a brocade, but only if we do not take brocade in its most ancient meaning of a textile exclusively woven with gold and silver threads.

DU CANGE and FRANCISQUE-MICHEL had no satisfactory etymology for «camocas». DOZY (*Glossaire*<sup>2</sup>, 246) was, I think, the first who proposed, in 1869, to derive «camocas» from Pers. *kāmḥā*, «damasked silk»; this theory met at once with general acceptance. For *kāmḥā* itself, DOZY added a Chinese etymology which will be discussed below.

The history of Pers. *kāmḥā*, in so far as I am in a position to trace it, is somewhat puzzling. The first occurrence of the word is in Arabic, when Ibn Ḥordāsbāh, himself of Persian origin, mentions in 844-848 *kīmḥāw* among the products which were imported from China (*Fe*, 31), and in relating the legend of the submission of the sovereign of China to Alexander the Great, speaks of the 500,000 pieces of *kīmḥāw* which formed part of the presents made by this sovereign to Alexander (cf. DE GOEJE, *Ibn Khordādhbeh*, Leyden, 1889, 51, 206, 215). The word is again used in the same form in 902 by Ibn al-Faḥīh, a man from Hamadan, also writing in Arabic (DE GOEJE, *ibid.* 215). A third instance is provided, again in Arabic, by Tha'ālibī († 1037), whose mss. give *kīmḥār*, corrupt for *kīmḥāw* (cf. DOZY, *Glossaire*<sup>2</sup>, 246; KARABAČEK, in *Mitt. d. K. K. Oester. Mus. f. Kunst u. Ind.*, VII [1879], 302). The first mention I can trace in a Persian text occurs in Wāssāf, that is at the beginning of the 14th cent., with the spelling *kmḥb*, which can be read both *kīmḥāb* or *kāmḥāb* (cf. VON KREMER, in *WZKM*, III [1889], 111-112). At the same time, Ibn Baṭṭūṭah, writing in Arabic, mentions twice the *kmḥā* manufactured at Zāitūn (transl. DEFRÉMERY and SANGUINETTI, IV, I, 269; the rendering «velvet» is wrong). A Persian spelling *kīmḥā* is used twice by 'Abdu-'r-Razzāq in his account of Šāh-Rūḥ's embassy to China in 1420-1422 (cf. QUATREMÈRE, in *Not. et Extr.*, XIV, I, 214, 315; here again, the word is wrongly translated «velvet»).

Consequently, the *kīmḥāw* of the 9th cent. evolved in two different ways, giving both a form in *-ā* as well as a form in *-b*; when the first vowel is noted, it is *-ī-*, but a short *-a-* is also possible with both forms. It seems evident that «camocas» and all the mediaeval Western forms are based on *kāmḥā*, and this is confirmed by a Greek letter of c. 1300, quoted by YULE (*Hobson-Jobson*<sup>2</sup>, s. v. «kincob»), which speaks of the textile which «the Persian tongue calls *καυχᾶν* (acc.)». But an epenthetic labial vowel after *m-* must have been developed already in the East to account for all the European forms which have an *-o-* or an *-u-* in the middle of the word.

Our Persian dictionaries, following the indications of the *Burhān-i qāṭi'*, list different forms, *kāmḥā*, *kīmḥā*, *kīmḥā*, *kāmḥāb*, *kīmḥāb*, *kīmḥāb*, *kīmḥāb*, and say that the forms with *-ā-* in the first syllable refer to a damasked silk of one colour, and those with *-ī-* to a polychrome variety (cf. VULLERS, II, 885, 938, 939). I do not

believe in such a distinction. What remains to be considered is that the forms in *-ā* and in *-b* spread to various directions.

So many influences have been at work in Malayan that no conclusion can be drawn from the coexistence there, as foreign words, of both *kimhā* and *kimhāb* (cf. FAVRE, *Dict. franç.-malais*, I, 353). But in India, the only current forms are Hindī *kamkhāb*, *kamkhwāb*, «vulgarily» *kimkhwāb*, Bengali *kimkhwāb*, with a popular etymology Pers. *kām* + *hwāb*, «little sleep»; the word became familiar in Anglo-Indian as «kincob», «keemcob», etc., in the 18th cent. (cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «kincob», but YULE's definition «gold brocade», though reproduced by LAUFER, *TP*, 1916, 477, and *Sino-Iranica*, 539, is discountenanced by the very examples he quotes). The tibetanized form *rkyen-k'ab* probably came from India (cf. LAUFER, in *TP*, 1916, 478). I am in doubt whether we should trace to India or direct to Persia the form *kingāb* which I have heard in Kāšyar and Turfan.

*Kāmhā* has been more prolific in its progeny than *kimhāb*. Not only is it the basis of the mediaeval forms of the «camocas» type, but it has spread to the Turkish and Slavonic languages. Here again, the term is of fairly late appearance. No such word is known from Uighur texts, nor is it listed in 1076 by Kāšyarī, although Kāšyarī mentions other Chinese textiles. In modern Turkish dialects, we find Alt. Tel. Kir. Khir. *qamqa*, Sag. *qamyī* (RADLOV, II, 490), Crim. *kimha* (*ibid.* II, 1405). From Turk. *qamqa* come Serb. *kamka*, Pol. *kamcha* (*ch* = *h*), Russ. *kamka* (BERNEKER, II, 477; already in THEVET, 16th cent.; cf. BOYER, in *Rec. de mél. orient.*, 1905, 468), all meaning «damask». Čay. *qumqa* of RADLOV, II, 1049, and BLOCHET, *Moufazzal*, 119 (cf. also *Bl*, I, 245), is probably to be read *gomqa* < *qamqa*. HERZFELD's hypothesis (*Iranische Felsreliefs*, 175), which would connect *kimhā* with *καυνάχης*, is certainly a failure. It is probable that the word is of Chinese origin.

Any attempt at a Chinese etymology must start from the *kimhāw* of the 9th cent., that is to say must have *kim* as its first element, and a second one ending with some labial sound. DOZY (*Glossaire*<sup>2</sup>, 246), quoting HOFFMANN, had proposed Ch. «*kincha* ou *kimcha*», which YULE (*Hobson-Jobson*<sup>2</sup>, s. v. «kincob»), mistaking DOZY's French spelling with *ch* = *š* for *kh*, hence «*kin-kha*», tentatively read 金花 *chin-hua*, «golden flowers», *chin* being pronounced *kim* in Fu-chien (this is, however, not quite true). KARABAČEK (in *Mitt. d. K. K. Oester. Mus. f. Kunst u. Ind.*, VII [1879], 302) again adduced HOFFMANN's Chinese «*kincha*» or «*kimcha*» which has been adopted by HEYD (*Hist. du commerce*, II, 697), CORDIER (in *Y*<sup>1</sup>, III, 155) and LOKOTSCH (*Etym. Wörterbuch*, No. 1043). HOFFMANN probably meant 金紗 *chin-sha*, «golden gauze», a possible, though unusual combination. HIRTH (*JNCB*, XXII [1888], III) thought he could find corroboration of YULE's *chin-hua* in Chinese dictionaries. PHILLIPS (*JNCB*, XXIII [1889], 28-30, and *TP*, 1890, 237) preferred 錦綺 *chin-ch'i*, «damasked silks». ROCKHILL, certainly unaware of the history of «kincob», wrote it «kincob» or «chincob», took it to be Tibetan and proposed to derive it from what he read 金段 *chin-chia* and translated by «gold brocade edging» (*JRAS*, 1891, 125); in the same year (*The Land of the Lamas*, 282), he spoke of the «gold brocades» called «chincob», the latter name being derived «from the Chinese *chin* (or *kin*), 'gold' and *cha* (or *ka*), 'to twist, to weave in', gold threads being woven in among the silk ones». LAUFER, who reproduced ROCKHILL's explanation (*TP*, 1916, 477), did not notice that ROCKHILL's



金段 *chin-chia* was a misreading of 金段 *chin-tuan*, commonly used for 金緞 *chin-tuan*, «gold brocaded satin», and phonetically, could not give the etymology of *kīmḥāb*. LAUFER himself (TP, 1916, 477; *Sino-Iranica*, 539) proposed 錦花 *chin-hua*, «as, for instance, used by Čao Žu-kua with reference to the brocades of Ta Ts'in».

Some of the above hypotheses can be disposed of at once as phonetically impossible : such are HOFFMANN's *chin-sha* (\**kjəm-ša*) and ROCKHILL's *chin-chia* once correctly read *chin-tuan* (\**kjəm-d'uān*). LAUFER's *chin-hua* is a slip and does not in fact exist; in the above-mentioned passage, Chao Ju-kua speaks of *hua-chin* «flowered damask» (HR, 103), a term which he uses more than once (for instance in the sections corresponding to HR, 115 and 155).

The *chin-ch'i* (\**kjəm-k'jiē*) adduced by PHILLIPS is, from Han times downwards, a well-attested term, which is used for instance by both Chou Ch'ü-fei in 1178 (*Ling-wai tai-ta*, 3, 2 b) and Chao Ju-kua in 1225 (HR, 141<sup>21-22</sup>) in their accounts of foreign countries. One of the components of *chin-ch'i* is 錦 *chin* (\**kjəm*), the very word we usually translate «brocade». The word *chin* is formed with a right half *po* meaning «silk», which ought to be the «radical» if it had not been left out of the modern system of 214 radicals, and a left half *chin* which originally meant «metal», and later «gold», but which is to be taken here as a phonetic. Of course the composite character might indicate that there was in the spoken language a «silk» fabric called *chin* because it was partly made of «metal» or «gold» (*chin*) threads, so that, in the written character meaning «brocade», the *chin* half would play a double part, semantic and phonetic, a case of frequent occurrence in Chinese script. We must not forget, however, that such a view would run counter to the oldest definitions of *chin* in native works : all agree in making *chin* a «silk textile of different colours», without any allusion to the use of gold or silver in its manufacture. *Chin* is in principle «silk damask»; the usual, but misleading translation «brocade» (not to speak of the mistaken «gold brocade») can be retained only for the modern loose use of the latter word, not for its original meaning of a textile made of gold and silver threads. True *chin* had a «ground» (地 *ti*) on which the portions in other colours were woven; when no «ground» was used, the textile was called 織成 *chih-ch'êng* «weaving-made» (a term which misled European translators, and in particular HIRTH in *China and the Roman Orient*). As to *ch'i* (\**k'jiē*), it differed from *chin* by being a damask silk of one colour only. The purpose of this apparent digression on the history of *chin* is to show that Chinese *chin*, not being a «gold» brocade, exactly fits in with the descriptions we have of mediaeval «camocas». On the other hand, the *kīm-* of *kīmḥāw* is exactly what we should expect, in T'ang times, as a transcription of *chin* (\**kjəm*), be it *chin* «metal», «gold», or *chin* «silk damask», «brocade» (the Cantonese pronunciation «*kam*» adduced by HIRTH, in fact *kəm*, has not to be taken into consideration since we must start from *kīmḥāw*, not from *kamḥā*). There are, however, certain difficulties in PHILLIPS's hypothesis. One is that *chin* and *ch'i* are properly the names of two different textiles and that the composite term *chin-ch'i* is a class designation, somewhat bookish, and not very likely to have passed abroad. But the main objection is of a phonetical order. While *chin* (\**kjəm*) accounts for *kīm-* of *kīmḥāw*, *ch'i* (\**k'jiē*) cannot well be transcribed as *-ḥāw*. In the 9th-10th cents., we know a few cases when Ch. *k* is rendered by Arabs or Persians as *ḥ*-, for instance in Ḥanfū < Kuang-[chou]-fu (see «Quinsai» and «Çaiton»), or Ḥamju (Ḥamčū) < Kan-chou (see «Campçio»); but in neither case

is Ch. *k-* followed by a palatal vowel; and *k-* before a palatal vowel remains *k-* in *kīm*. We may perhaps get over the difficulty by arguing that the difference of treatment is due to the fact that in the case of *ch'i* (\**k'jiē*) the initial was aspirate. But the difficulty is still greater with the final : there is nothing in the pronunciation of *ch'i* at any time or in any dialect which could explain the final *-w* of *kīmḥāw*. On the other hand, I do not think that we should discard the oldest form *kīmḥāw* in favour of the latter *kīmḥā*; the form with *-w* cannot be a textual error since it occurs in three different authors and is moreover supported by the later *kīmḥāb*. I have no solution to suggest on this point.

YULE'S 金花 *chin-hua* (\**kjəm-χ<sup>w</sup>a*), «gold flower», won the support of HIRTH, who said he had found the term listed in the Chinese dictionary *P'ien-tzū lei-pien* in the sense of «sil embroidery». Even this is not quite correct, apart from the fact that *kīmḥāw* or «camocas» was not embroidered. «Gold flowers» were often used as ornaments in the form of metal flowers stuck into the hair, woven ones in textiles, embroidered ones on shoes, pasted ones on paper, etc. The term was even employed figuratively for decorations which did not always represent flowers; but such mentions are not tantamount to a specific use which could provide the original of *kīmḥāw*. Moreover, *chin-hua* implies gold, and we have seen that the «camocas» was not a «gold brocade». Yet I am far from rejecting *chin-hua* entirely. It has a certain phonetic advantage, inasmuch as the *-h-* of *kīmḥāw* would regularly render the initial of *hua* (\**χ<sup>w</sup>a*); as to the final *-āw*, it might be explained as a metathesis of Ch. *-<sup>w</sup>a*. The «kincob», if not the «camocas», was perhaps sometimes partly woven with gold thread. Even in China, we find in the Mongol period (YS, 78, 4b, 8a; cf. *Br*, II, 125) a technical term 金錦 *chin-chin*, «gold damask silk» (consequently real «gold brocade», and in fact with two different values; earlier examples of *chin-chin* are of an inconclusive literary character). *Chin-hua* were applied on red silk damask (*hung chin*) in a type of official car of the Sui dynasty (*Sui shu*, 10, 2b); also during the Chin (Jučen) dynasty, it was the privilege of the highest officials to use *chin-hua* for the «mud-avoiding» panels of their saddles (*Chin shih*, 43, 3a); unfortunately, we are left in the dark as to the real meaning of *chin-hua* in this last case. In the Middle Ages, the «damask silks» (*chin*) which were mostly sent abroad were those from Chien-ning-fu in Fu-chien (see «Quenlinfu»); they are referred to either as «Chien-ning *chin*», or as «Chien-yang *chin*», Chien-yang being the name of a district to the north-north-west and within the territory of Chien-ning-fu (cf. *Yü-ti chi-shêng*, 129, 7a, mentioning the red [*hung*] *chin* and green [*lü*] *chin* of Chien-yang; the «Chien-yang *chin*» exported to Borneo according to Chao Ju-kua, *HR*, 156; the «Chien-ning *chin*» exported to Cambodia according to the *Tao-i chih-liao*, *TP*, 1925, 107). Now it just happens that the *Yüan-ho chün-hsien t'u-chih* of the early 9th cent., in its list of five products which Chien-chou (an earlier name of Chien-ning-fu) had to send as tribute to the Court in the *k'ai-yüan* period (713-741), includes 金花練 *chin-hua lien*, «gold flower *lien*», a term which I have not found used anywhere in the whole book for the tribute of any other place. The value of *lien* is not very clear. *Lien* was the designation of a fairly strong silk fabric (絲兼 *chien*) after it had been boiled. It may well be that the shortened term *chin-hua* «gold-flower», had become the popular name of the damask silk of Chien-ning among the tradesmen of southern China, and as such was heard by Arabs and Persians who transcribed it *kīmḥāw*. For want of a Chinese term of suitable meaning which would begin with *chin* (\**kjəm*) and end

with a word having an ancient final *-p*, I hold *chin-hua*, «gold flower», to be the nearest approach to a satisfactory solution which we can hope for at present.

There is no support for DU CANGE's hypothesis that Fr. *moquette*, formerly *mocade* (cf. Engl. *mockado*), may be derived from «camocas». As to DEVIC's idea that *canque*, the name of a Chinese cotton fabric in French accounts of the 17th and 18th cents., should be «the same word» as «camocas», it looks most unlikely and ought not to have been accepted by LOKOTSCH, No. 1043.

It goes without saying that, although the word «camocas» ultimately goes back to a Chinese term, most of the «camocas» mentioned in mediaeval inventories had been manufactured in the Near East. There is not much to add to what HEYD has said on this point already (*Hist. du commerce*, II, 697).

## 110. CAMPÇIO

<i>campçio</i> Z	<i>campcio</i> V (cor.), Z	<i>capyon</i> G
<i>campicion</i> FA, L, LT, P	<i>cancipu, canpitiu, capicion</i> F	<i>chanpiciom, chapitum</i> TA <sup>3</sup>
<i>campion</i> VL; R	<i>canpicion</i> F, Fr, t	<i>chanpion, champion</i> VA
<i>campision, campituy</i> FB	<i>canpitui, champion, chapituy</i>	<i>chapetin, chapion</i> V
<i>campition</i> LT	TA <sup>1</sup>	<i>tampision</i> FB <sup>r</sup>
<i>campitui</i> FA	<i>capition, capitoi, capiton</i> VB	

I have adopted this Z reading, as being the least remote from what I suppose to have been the original spelling, \*Campciou, which will be discussed hereafter. The Catalan Map of 1375 gives «Cansio» (cf. Y<sup>1</sup>, I, 302) or rather «Chancjo» [? for «Chamcio»] (cf. CORDIER, *L'Extr.-Or. dans l'Atlas Catalan*, 19). The form on Fra Mauro's map is «Capiton» (not «Campit» as is said in Zu, 35, and HALLBERG, 107).

It is evident and accepted by all that 甘州 Kan-chou is meant; the name was still pronounced Kam-chou in the Mongol period. The *Hudūd al-'Ālam* of 982-983 writes خامچو Ḥamčū (*Mi*, 495) and so does Gardēzi in the 11th cent. (BARTHOLD, *Otčēt o komandirovké*, 117). Rašidu-'d-Dīn writes قامجو Qamju and قامجير Qamjiu (*Bl*, II, 496, 598); Waśśāf gives قاجر Qabju (*Ha*<sup>2</sup>, 24; *Bl*, II, 497); Abū-'l-Fīda, قامجو Qāmju (cf. REINAUD, *Géogr. d'Aboulféda*, Arab. text, 367; transl. II, II, 125; Y<sup>1</sup>, I, 258). Qamju occurs again in the account of Šāh-Rūḥ's embassy in 1419-1421 (*Not. et Extr.*, XIV, I, 396, 424), and in an inscription of the middle of the 16th cent. (*TP*, 1905, 319). Ḥamju (< Qamju) still occurs in a Mongol document of 1725 (cf. ŽAMCARANO, in *S. F. Ol'denburygy... sbornik* [1934], 194). The «Campion» which RAMUSIO attributes to his Turkish informant «Chaggi Memet» is certainly influenced by the «Campion» which RAMUSIO adopted in his edition of Polo. Our poor texts of Pegolotti write «Camexu» (Y<sup>1</sup>, III, 148, 156).

YULE (Y, I, 220-221) thought that the name was pronounced in three syllables, on account of ERDMANN's «Ckamidschu» in his translation of Rašidu-'d-Dīn's text and of Pegolotti's «Camexu», but at the same time believed that the *p* was superfluous as in the readings *dampnum*, *hympnus*, *tirampnus* of ancient mss. He also thought that RAMUSIO's posthumous edition was perhaps revised, and that RAMUSIO himself perhaps read «Campjou», «more correct»; nevertheless, YULE himself adopted «Campichu». There is here a double contradiction in YULE's reasoning, generally

so sober and sound. As to the facts, ERDMANN's reading is arbitrary and certainly wrong; and the text of Pegolotti is too corrupt to give any solid clue. There can be little doubt as to the name having been read in two syllables, and the *p* is really superfluous; perhaps this *p* (in spite of isolated cases like «tiramprus», or Marignolli's «Campsay» [see «Quinsai», and «Campsay» in one ms. of Odoric]) helped mainly to prevent a reading in *n* of a contracted form in *m* («Căciou» could be read «Canciou» or «Camciou», but «Căpciou», even if developed into «Canpciou», would sound «Camciou»); the cases of «ianb» for *iam* (*yam*) and, above all, of «Campigiu», are analogous (see «ianb» and «Campigiu»). I explain in the same way the «Nemptai» (= Nemtai or rather \*Namtai) of Nicolo Conti (see «Namghin»), and the transcription of Pers. لمس *lāms* as «lambs» in KUUN, *Codex Cumanicus*, 58. One might be tempted to revive the idea of a pronunciation in three syllables when finding the name written Kammicū in a Khotanese document of the 10th cent. (cf. H. W. BAILEY, in *BSOS*, VIII, 884, and IX, 522, who quotes from other Khotanese mss. the forms Kamacū and Kammacū). But a glance at the lists published by F. W. THOMAS, in *ZDMG*, vol. 91, 33-48, is enough to show that these countless -*i* endings of syllables in Brahmi script are a purely orthographical Khotanese device, and do not affect the pronunciation.

In Polo's text, *chou* is generally rendered by forms like °*giu*, °*ciu* (F), °*gui* [read °*giu*] or °*zu* (R), °*çu* (Z); but, just as we have a final °*ion* [read °*iou*] in F and R, and °*io* in Z, for «Campciou», the third *chou* Polo passed through on entering Western China, we find for the first one, Sha-chou, a final °*ion* [read °*iou*] in F and R, and °*io* in Z. The parallelism of the two cases is striking (Z has °*io* also for «Succiu»), although I am at a loss to say why Polo or Rustichello transcribes *chou* differently for Western Kan-su and for the rest of China. For other examples of *u* written *ou*, see «Oucaca», «Oulatai».

The recension in Court French says that Maffeo and Marco Polo spent one year at Kan-chou on an official errand («en légation»), and that is the version followed by PAUTHIER, YULE and CHARIGNON. But F and R agree that the three Polos were there together, for some business of their own. I do not think that any commentator has seriously discussed this point. It is practically certain that we have to accept the version of F and R. But when were the three Polos likely to have been together a whole year in Kan-chou? YULE (*Y*, I, Introd. 22) seems to take it for granted that it was at some undetermined moment after they had entered Qubilai's service; but that may be because he accepted «en légation» in his text. It seems to me much more natural to suppose that the three Polos, who are said to have spent three years and a half on their journey from Acre to Shang-tu, had stopped a whole year in Kan-chou. There is little chance of their having been there together at a later date. I do not even exclude the possibility that it was from Kan-chou that their arrival was reported to Qubilai; the messengers Qubilai sent on a forty day's journey to meet the three Latins must have travelled post-haste, and may have covered in that period of time the distance from Khan-baliq or from Shang-tu to Kan-chou.

The region of Kan-chou, after forming part of the territory of the Yüeh-chih, passed to the Hsiung-nu, and it was conquered by the Han who created there the *chün* of 張掖 Chang-yeh in 111 B. C. The name of Kan-chou was first adopted, for a short time, in 553, and revived in 619. The place acquired a greater importance when a Vice-Commissioner (*fu chieh-tu-shih*) of Ho-hsi (for this term see «Caidu» and «Tangut») was installed there in 710. In 766, Kan-chou fell into

the hands of the Tibetans, renewed nominally its allegiance to China in 851, but became soon afterwards the seat of an independent Uighur principedom. The documents found at Tun-huang have revealed the existence at Kan-chou, in the 9th and 10th cents., of Uighur and Tibetan Buddhist monasteries which were active in translating Buddhist texts into Chinese and from Chinese. We know also of Manichaeans Uighurs at Kan-chou at the same time (cf. *JA*, 1913, I, 303-306). The *Hudūd al-‘Ālam* of 982/3 perhaps repeats some belated information when it represents the place as owned half by the Chinese and half by the Tibetans, and, in spite of continuous warfare, as being in the dependency of the Tibetan sovereign (cf. *Mi*, 85, 227). Curiously enough, the text is silent about the presence of Manichaeans at Kan-chou, while it says that the inhabitants of Sha-chou (see « Saciou ») and of Hāču (= Kua-chou in western Kan-su) follow the religion of Māni. In 1028 Kan-chou was conquered by the Hsi-Hsia (« 1208 », copied from PALLADIUS in *Y*, I, 220, is a misprint; cf. *Hsi-Hsia shu shih*, 11, 1 a). The Chinese name of Kan-chou, which had certainly remained in popular use, was officially adopted again by the Mongols. A « general administration of the *lu* of Kan and Su » (*Kan-Su lu tsung-kuan-fu*) was created at Kan-chou in 1264 and became the *lu* of Kan-chou in 1271; Kan-chou became the seat of a province (*hsing chung-shu-shêng*; cf. « Scieng ») of Kan[-chou] and Su[-chou] (> Kan-su) in 1281. The Ming made of it a « march » (*wei*) in 1372; it became in 1725 Kan-chou-fu, and that official name lasted until the fall of the Manchu dynasty. It is now the *hsien* of Chang-yeh (cf. *Ta-Ch’ing i-t’ung chih*, 205, 1 a; *YS*, 60, 12 a). I find nothing to corroborate CHARIGNON’s statement (*Ch*, I, 158) that the present city is 20 *li*’s distance from that which was known to Polo. On the contrary, the *Ta-Ch’ing i-t’ung chih* (205, 1 b) expressly says that the walls of the modern town were rebuilt in 1427 on the site of the ancient ones; and such is also STEIN’s opinion (*Serindia*, 1132).

Polo is not the only traveller to mention in Kan-chou the huge reclining figure of Buddha entering *nirvāṇa*; YULE (*Y*, I, 221; III, 53; *Y*<sup>1</sup>, I, 277, 294) has already reproduced the description which is given of it by Šāh-Rūḥ’s envoys. Following PALLADIUS, YULE and CHARIGNON (*Ch*, I, 158) say that the temple was founded in 1103; but the texts at my disposal agree for 1099 (first year *yung-an*; *Ta-Ch’ing i-t’ung chih*, 205, 4 a; 辛卯侍行記 *Hsin-mao shih-hsing chi*, 4, 42 a; the date of 1103 comes from the *Hsi-Hsia shu-shih*, 31, 16-17, which seems to repeat here a late legendary account). The ancient name is 弘仁寺 *Hung-jên-ssü*, changed [in 1411?] to 寶覺寺 *Pao-chio-ssü*; but the temple has long been popularly known as 臥佛寺 *Wo-fo-ssü*, « Temple of the reclining Buddha », or 睡佛寺 *Shui-fo-ssü*, « Temple of the sleeping Buddha » (or even 大佛寺 *Ta-fo-ssü*, « Temple of the great Buddha », in STEIN, *Serindia*, 1132). In the notes on colossal images of Buddha in China, ROCKHILL’s and YULE’s « *Yung-kán* » (*Y*, I, 222) and CHARIGNON’s « *Yong-kan* » (*Ch*, I, 158) are wrong forms for the well-known 雲岡 *Yün-kang* caves near Ta-t’ung. To YULE’s indications, CHARIGNON adds that of a less known seated Buddha, at least 15 metres high, cut in the cliff one kilometre outside the southern gate of Hsü-chou in Chiang-su.

According to Polo, there were three Christian churches at Kan-chou. They are no longer known now, but a contemporary text certainly refers to one of them. The Kerait Christian princess who was the mother of Mongka, Qubilai and Hülägü, the « Seroctan » (? « Soroctan », *Soryaqtani bägi* [or *bäki*]) of Plan Carpine (cf. *TP*, 1932, 43-54), was no longer alive when Rubrouck visited Mongolia in 1253-1254 (*Wy*, 287; misunderstood by DEVERIA in *JA*, 1896, II,

420); as a matter of fact, she had died in the first month of 1252 (February 12-March 11), certainly in Upper Mongolia (YS, 3, s. a. 1252). A text occurring in the *pên-chi* of 1335 (YS, 38, 5 a) has been accepted as proving that « Seroctan » had been buried in Kan-chou (PALLADIÏ [PALLADIUS], in *Vostočnyi sbornik*, I [1877], 21; DEVÉRIA, in *JA*, 1896, II, 419-420; E. GEIL, followed by CHARIGNON in *Ch*, I, 159); this is certainly an error. The princess was buried in Mongolia, near Chinghiz-khan and near her husband, Tului, according to the Persian historians (cf. *Oh*, II, 267; *Bl*, II, 561). But after her descendants had become emperors in China, her images and tablets were honoured in several places, particularly at the capital and at Chên-ting (see « Achbaluch »). In 1335 an image of the Christian ancestress of the dynasty was installed in a Christian church of Kan-chou, and a report was made to the Emperor to fix the appropriate ceremonial. Unfortunately two characters seem to be corrupt in the relevant text, which I would translate as follows (with my correction between square brackets) : « The first year *chih-yüan*, . . . in the third month . . . , on (the day) *ping-shên* (April 7, 1335), the officials of the Chung-shu-shêng said that the Temple of the Cross of Kan-chou in (the province of) Kan and Su had respectfully put up (安奉 *an-fêng*) [the Imperial image] of the mother of Shih-tsu (Qubilai), the Bâgi Empress Dowager, and they asked (the Emperor) to fix the rites for the sacrifices (there); it was granted. » I have translated the text as if it read 御容 *yü-jung* or something similar, instead of 於內 *yü-nei*. Without alluding to the textual difficulty, PALLADIUS, who was the first to call attention to this passage of the *pên-chi*, said in his original Russian paper printed in 1872 (though only published in 1877) that « Seroctan » had been buried at Kan-chou. But in the English version published under his supervision in 1875 (that is two years before the appearance of the original Russian text), he already spoke only of an image of « Seroctan » in the church of Kan-chou (cf. *JA*, 1896, II, 420). MOULE (*Mo*, 225) is non-committal on the point. The whole of the surrounding historical circumstances and the funerary habits of the Mongols preclude the burial in Kan-su, and within a city. On the other hand, a number of texts, Chinese and even Persian, refer to the images of Mongol deceased emperors and empresses, including those of « Seroctan ». Some of the latter ones have been indicated by MOULE (*Mo*, 224-225); there are others, but this is not the place to enumerate them. Let us only remember that the Temple of the Cross celebrating a cult before the image of « Seroctan » in 1335 must be one of the three Christian churches of Kan-chou mentioned by Polo.

### 111. CAMUL

*camul* F, Fr, t, FA, FB, L, LT, *camuy* FBt  
P, VB, Z; G *canuil*, *chanuil* V

*chamul* TA<sup>1</sup>, TA<sup>3</sup>, VA, VB, VL;  
R  
*famul* G

The name of this place is now generally pronounced « Qomul » (not « Qumul » as in HERRMANN, *Atlas of China*, Index, 97); the usual Chinese form since the late Middle Ages is 哈密 Ha-mi; both are probably evolved from an earlier Qamîl, but Qomul < Qamul is the Uighur Turkish

form, while the Mongols pronounce *Ḥamil*. In a Sogdian document of the first centuries of our era (end of 2nd cent.?), *Km'yδ*, which can be read as \**Kamēl*, seems to furnish the first mention of the native name (cf. *TP*, 1931, 460). There is a huge gap between that first occurrence and the second example, found in 1050-1052 in the Persian account of *Gardēzi*; this time the name is written *قامل* *Qāmūl* (cf. BARTHOLD, *Otčēt o komandirovké*, 92, 117; *Mi*, 229). The same spelling occurs in 1340 in the *Nuzhat al-Qulūb* (transl. LE STRANGE, 249). In 1420, Šāh-Rūḥ's envoys passed through *قامل* *Qāmūl* (or *Qāmīl*?; cf. QUATREMÈRE, in *Not. et Extr.*, XIV, I, 310, 389; *Y<sup>1</sup>*, I, 273); they found there side by side a mosque and a Buddhist temple. Marignolli made a number of converts at «*Kamul*» (*Wy*, 550). The Catalan Map writes «*Camull*»; Fra Mauro, «*Chamul*» (HALLBERG, 108). RAMUSIO's informant «*Chajji Memet*» mentioned *Camul* (RAMUSIO, II, *Dichiaratione*, 160; *Y<sup>1</sup>*, I, 293). Benedict Goës stayed one month at *Camul* in 1606 (*Y<sup>1</sup>*, IV, 239). «*Hamil*» appears on RENAT's Map I (cf. BADDELEY, *Russia, Mongolia, China* [1919], I, ccii). The existence of a Nestorian bishopric of *Qomul* («*Camula*»), whose occupant was present at the inauguration of the *Catholicos Denha* in 1266, is mentioned as probable by YULE (I, 211), and given as established in SAEKI, *The Nestorian Documents and Relics in China*, 1937, chart facing p. 348, but omitted from HERRMANN, *Atlas of China*, map 45; and would require substantiating. The source is a list in ASSEMANI, *Bibliotheca Orientalis*, II, 455-456, and the name may be corrupt or refer to another place.

*Qomul* occurs on the Chinese map of c. 1330, in the form *合木里* *Ha-mu-li* (*Br*, II, 20), but as it is not included in the dominions of the house of Čayatai, it is not listed at the end of *YS*, 63, nor is it mentioned by T'U Chi, 160.

The name is repeatedly mentioned in the *YS*, though written in different ways. BRETSCHEIDER said (*Br*, II, 20, 21) that the modern form *Ha-mi* occurred in ch. 122, in the biography of the *tdiqtu* *Barčuq-art-tegin*, and CORDIER repeated it in *Y*, I, 211. But this is a mistake. The form in *YS*, 122, 2 a, is *哈密力* *Ha-mi-li*, *Qamīl* (with the wrong information that *Ha-mi-li* lay south of *Qarā-ḥōjo*). I have not so far been able to find the form *Ha-mi* before the Ming dynasty; but it is the only one used in the account of the mission of *Ch'ên Ch'êng* in 1414 (*Hsi-yü hsing-ch'êng chi*, ed. of the Peiping National Library, 4 a) and in the companion work, equally the work of *Ch'ên Ch'êng*, entitled *Hsi-yü fan-kuo chih* (same edition, 20 a-b).

The most usual transcription of the name in *YS* in *合迷里* *Ha-mi-li*, *Qamīl* (*合 ho* always stands for *哈 ha* in the proper names of the Mongol period), occurring first in 1286 (*YS*, 14, 4 a and b, perhaps also 8 b), then in 1294 (*YS*, 18, 2 a : «*Tegin-tāngrim of Ha-mi-li*»), in 1303 (*YS*, 21, 1 b) and in 1304 (*YS*, 21, 6 a). Other transcriptions are *合迷裏* *Ha-mi-li* in 1288 (*YS*, 15, 5 b), *渴密里* *K'o-mi-li* (*YS*, 133, 3 a), and *罕勉力* *Han-mien-li* which I had noted, but cannot now trace. All these are based on *Qamīl*. But *合木里* *Ha-mu-li* in 1289 (*YS*, 15, 7 a), *合木裏* *Ha-mu-li* also in 1289 (*YS*, 50, 11 a), *甘木里* *Kan-mu-li* in 1290 (*YS*, 16, 1 a) and *感木魯* *Kan-mu-lu*, the motherland of a Uighur monk (*YS*, 202, 2 a), are transcriptions of *Qamul*. A *合馬里* *Ha-ma-li* of *YS*, 8, 4 b, may represent a different name. The *含伊羅* *Han-i-lo* (\**Ham-i-lo*) mentioned in a preface of 1270 and a colophon of 1277 (*Tōkyō Tripit.* of Meiji, 寒 VI, 114 b, 124 a) renders *Qamīl*, our «*Camul*», but with a curious syllabic division. It is also *Qamīl* which is miswritten *阿模里* *A-mu-li* (for *K'o[柯]-mu-li*?) in the Chinese map of c. 1330 (cf. *Br*, II,

20; TING Ch'ien, *Yüan Ching-shih ta-tien t'u ti-li k'ao-chêng*, 3, 11 b). The kingdom of 哈梅里 Ha-mei-li, mentioned in the *Ming-shih*, 330, 12-13, for an event of 1380, has not been identified by BRETSCHNEIDER (*Br*, II, 219-220); it is certainly Qomul.

But Qamīl (Qamul, Qomul) is not the only name under which the place has been known. At the beginning of our era, the region of Qomul was inhabited by tribes leading a nomad life for the greater part. In A. D. 73, the Chinese created a military colony in the region, with a walled city, which was called the «walled city (城 *ch'êng*) of 伊吾廬 I-wu-lu (\*I-ngo-lo)» This is of course the transcription of a native name, Hsiung-nu in all likelihood, and I think it probable that it is connected with \*Kamēl and Qamul; but the latter name is based on the form which the native name had taken in another language or another dialect (cf., under «Cotan», the forms «Hotan», «Odon», etc., of one and the same name). This form I-wu-lu occurs in *Hou Han shu*, 2, 8 a; 53, 6 a (*TP*, 1907, 156; cf. also *TP*, 1906, 257), but was almost immediately abridged to I-wu (there is not the slightest foundation for the two different I-wu imagined by HERRMANN, *Low-lan*, map facing p. 31, and *Atlas of China*, map 24, the older I-wu being located between the Lop-nōr and Qarašahr). The military colony of I-wu-lu or I-wu did not thrive like that of Kao-chang (see «Carachoço»), and was abandoned with the whole of the region in 77. A new occupation in 90 was still less durable. The third effort, in 131, was more successful, but only for a time, and Qomul had already passed out of Chinese reach at the end of the Han dynasty. The district (*hsien*) of I-wu established in the 3rd cent. was no longer at Qomul, but in the northern part of Tun-huang territory. The current views on the early history of Qomul, as they are represented by the notes of CHAVANNES's translation of the *Wei lio* (*TP*, 1905, 529-535) or in STEIN's *Serindia* (705-710) and *Innermost Asia* (539-540), are biassed by an erroneous interpretation of the text relating to the «new road» laid in A. D. 1-5 to bring into more direct and less dangerous intercourse the region of Beš-baliq (north of the T'ien-shan) and western Kan-su. This road, as was already noticed in 1910 by HERRMANN (*Die alten Seidenstrassen*, 108, 123, and the accompanying map) went direct from Qara-ḥōjo to Tun-huang. It had nothing to do with Qomul, except to keep clear of it.

This road had been long neglected when a Chinese general, in or about 607, led an army across the desert, and founded, to the east of the ancient Han walled town, a new city which he called 新伊吾 Hsin-I-wu, «New I-wu». This new city corresponds to the mediaeval and modern Qomul. As to the I-wu-lu of the Han, which the commentary of 676 on the two passages of the *Hou Han shu* calls the «ancient small town of I-wu», it was located about 30 miles west of Qomul, in the district of Na-chih (Lapčūq; see «Lop»; cf. also *Yüan-ho chün-hsien t'u-chih*, 40, 11a). It was this new I-wu of 607 which, after one more term of abandonment, became in 630 the «I prefecture» or 伊州 I-chou, I being the first element of the former I-wu; I-wu was retained as the name of the *hsien* established at I-chou. In 710, an «army (軍 *chün*) of I-wu» was created and had its seat in a valley of the eastern T'ien-shan, 300 *li* north-east of I-chou (cf. *TP*, 1928, 290). It is I-chou which occurs as «Ičū» in the Khotanese document of the 9th cent. (?) published by THOMAS and KONOW (cf. *TP*, 1930, 230; 1931, 139). The above sketch of the past fortunes of Qomul is based on an unpublished monograph which I had prepared some years ago; a detailed discussion of the sources would be out of place in the present notes.



According to PALLADIUS (cf. *Y*, I, 212), Hung Hao, one century before Polo, attributed to the Uighurs, «nearly in the same words», the excessive hospitality which Polo says the people of Qomul used to extend to foreigners. The passage alluded to occurs in Hung Hao's *Sung-mo chi-wên*, written in 1243 on the author's return after a captivity of fourteen years among the Chin (*Hsio-chin t'ao-yüan* ed., 5 a-b). Hung Hao speaks of the Uighurs as of men with curly hair, deep-sunk eyes, thick eyebrows and beard, very clever in trade and industry. The disintegration of these characteristics in a number of Uighurs whom Hung Hao saw was the result of intimate intercourse with the Chinese. At the beginning of the Sung dynasty, many Uighurs had settled in 秦川 Ch'in-ch'uan (=Shàn-hsi and Kan-su), but were later transferred by the Chin to the region of Peking. Hung Hao adds : «At the time they lived in Ch'in-ch'uan, their girls, before they married, had intercourse with the Chinese. Only after they had had several children and when they were nearly thirty, were they allowed to marry some one of their own tribe. When the matchmaker came to discuss the question, the parents [of the girl] would say : 'Our daughter has been intimate with such and such a man'; and the greater the number [of men] the better. Such was the general custom.» The text is certainly of great interest, but does not refer to married women as that of Polo does. It is another case of a practice which has been ascribed to many nations, and which is mentioned by Polo in Tibet (cf. *Y*, II, 48).

## 112. CAMUT

*camoscia* R  
*camu* F  
*camulco* P

*camut* FA, FB  
*camuto* L

*camutto* LT  
*chamuto* TA<sup>1</sup>, TA<sup>3</sup>, VA

BENEDETTO has retained in his edition (*B*, 84) the «camu» of F, without quoting any other reading, and in his translation has adopted «camucca» (*B*<sup>1</sup>, 140, 452) without comment; but «camucca» is certainly wrong. FA writes «chaucelemente de camut qui est bourgal» (*Pa*, 297), and YULE (*Y*, I, 394-396) was quite right, following KLAPROTH, to see in «camut» the *camutum* of *Codex Cumanicus* (ed. Kuun, 106), which is explained by «sagri» in Turkish and in Persian «Sagri», our *shagreen*, means leather made from the croup of a horse, and this is also the meaning of «bourgal» or «borgal», *i. e.* *buryāl*, *buryālī*, a well known metathetic form of *bulyārī*, derived from the name of the ancient Bulyar of the Volga (see «Bolgara»). «Camucca» (or «camocca», etc.) is a quite different word meaning a gold brocade; Polo left several pieces of «chamocho» (cf. vol. I, 556, 557).

On «camucca» («camocatus» in KUUN, *Codex Cumanicus*, 108), see YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Kincob»; HERZFELD, *Iranische Felsreliefs*, 175 (with a wrong derivation from *καυδάχης*);

VULLERS, 938-939; *Fe*, 31; LAUFER, in *TP*, 1916, 477-478, and *Sino-Iranica*, 539 (but 金段 *chin-chia* is a misreading for 金段 *chin-tuan* = 金緞 *chin-tuan*, «gold satin», and Chao Ju-kua writes 花金 *hua-chin*, not *chin-hua*); LOKOTSCH, *Etym. Wörterbuch*, No. 1043, who starts from Pers. *kāmḥā*, itself explained as borrowed from Ch. «*kimša, kinša*» (= 金紗 *chin-sha* or 錦紗 *chin-sha* ?); I have heard myself *kimgāb* in Kāšyar and Turfan. In Turkish, we find Alt. Tel. Kir. Kkir *qamqa* (RADLOV, II, 490; > Russ. *kamka*, «damask», already in THEVET, 16th cent.; cf. BOYER, in *Rec. de mél. orient.*, 1905, 468), Sag. *qamyi*. Čay. *qumqa* of RADLOV, II, 1049, and BLOCHET, *Moufazzal*, 119 (cf. also *Bl*, I, 245) is probably to be read *qomqa* < *qamqa*. The true Chinese etymology has not yet been ascertained. The *chin-sha* quoted by LOKOTSCH on the authority of KARABAČEK is a possible combination, but unattested and phonetically unsatisfactory. One may think of 金錦 *chin-chin* (\**kim-kim*), on which cf. *Br*, II, 125, but this would require that the forms with a labial ending, *kimχwāb*, *kimgāb*, etc., are the most ancient, a conclusion which does not seem to be supported by such evidence as we have. It will be noticed that *qamqa* is not given by Kāšyarī in 1076, although he mentions other Chinese textiles. The word may have come to the West by sea, and only after the 11th cent.; but in such a case, the Turkish forms *qamqa*, etc., would be second-hand borrowings. The whole question must be studied afresh.

On «*sagri*», cf. YULE, *Hobson-Jobson*<sup>2</sup>, 818; LAUFER, in *TP*, 1916, 478, and *Sino-Iranica*, 575; BROCKELMANN, *Kāšyarī*, 169; HOUTSMA, *Turk.-Arab. Glossar*, 81; POPPE, in *Izv. Ak. Nauk*, 1928, 57; LOKOTSCH, *Etym. Wörterbuch*, No. 1760.

For «*camutum*», KUUN (p. 374) has quoted DU CANGE's «*camuzzum*». But the word «*camutum*» occurs in the same form in a document written at Caffa in 1289 (*in zapis centum duodecim camuti*; BRÁTIANU, *Actes des notaires génois*, p. 185). YULE (*Y*, I, 395) has connected «*camut*» with both Pers. *kāmū* *کامو* and *kīmuḥt* *کیمخت* or *kāīmuḥt*; and *kāmū* would seem to give some authority to the «*camu*» of F. But *kāmū* is a doubtful word of lexicographers, and its would-be Turkish form *kāmi* (VULLERS, 783) is otherwise unknown. On the contrary, *kīmuḥt*, «leather made from the croup of a horse or an ass», is common in Persian, and I think it gives the real etymology of *camutum*. *Kīmuḥt* seems to be a true Persian word, and, contrary to the general opinion but in agreement with LOKOTSCH, I believe that *sayrī* (> *šayrī*) was originally Turkish, and simply borrowed in Persian.

Joinville speaks of St Louis's garments in the following terms (DE WAILLY's ed. 1874, p. 667): «*Ses robes estoient de camelin ou de pers; ses pennes de ses couvertours et de ses robes estoient de gamites, ou de jambes de lievres, ou d'aigniaux.*» The word «*gamite*» has been explained as meaning «*chamois*» (cf. GODEFROY, *s. v.* «*gamite*»), but this is not satisfactory from a phonetic point of view and is not even discussed in VON WARTBURG's *Franz. Etym. Wörterbuch* among the forms derived from *camox*. I feel much more inclined to see in «*gamite*» another spelling of «*camut*». More puzzling is the following quotation from FAUCHET's *Antiquitez*, v, 11, which I find in HUGUET's *Dict. de la langue française du seizième siècle*, II, 66: «*Ils [les Sarrasins] vont . . . couverts de sayons de couleurs, qu'encor' aujourd'hui ils nomment Camits.*» If it be the same word, which would thus have still been known in the second half of the 16th cent., we ought to be able to find more traces of it.

## 114. CANOSALMI

*canosalmi* F, P (?), L  
*chalosebini* VA  
*chanosalini* TA<sup>3</sup>  
*chanosalmi* TA<sup>1</sup>

*chansolmin* V  
*conosalmy* FA  
*consalmi* R  
*ganasalin* S

*ganosalini* VL  
*salini* VB  
*tanosalmy* FB  
*tolofornis* LT

I have retained F's reading (there is no text in Z to help us here), but the name is uncertain, and so too is its identification (despite *Ch*, I, 62); cf. *Y*, I, 106. PAUTHIER's «*Khaneh-al-Salam*», «*House of Refuge*», sounds too much like a name suggested by Polo's narrow escape, and ought not to have been mentioned in *B*<sup>1</sup>, 440. To the various hypotheses already proposed may be added the following : the forms in R and LT make an original *-o-* possible in the first syllable, and the name may then have begun with *كهنه* *kohnäh*, «*ancient*».

The «*Ḳanāt-ul-Shām*» quoted by YULE from Idrīsi as a possible original of «*Canosalmi*» appears in fact, in older Arabic geographers, as *Qanāt-aš-Šāh*, «*Royal Canal*» (cf. BARTHOLD, in MINAEV's *Marco Polo*, 52; the place is not mentioned in the *Ḥudūd al-Ālam*). This latter form seems to be the correct one, and YULE's hypothesis must be abandoned.

## 115. CANTAR

*cantar*, *canter* F  
*cantara* LT, VB  
*cantaro* (-i) R

*cantarre* Z  
*cantarum* P  
*chantari* TA<sup>1</sup>, TA<sup>3</sup>

*chantera* V  
*quintau* FA  
*quintaulz* FB

This is Arab. *qinṭār*, which has given our «*quintal*» and is itself indirectly derived from Lat. *centenarius* (cf. LOKOTSCH, *Etym. Wörterbuch*, No. 1178). There are many mediaeval Latin forms, ranging from *cantarum* to *quintale*. In mediaeval Italian, the main form is *cantaro*, but *cantaro*, *cantar*, *cantara*, *cantare* all occur as forms of the singular in Pegolotti (EVANS, 64, 65, 100, 101; in the Index, 408, EVANS adopts *cantare*); *cantaro* survives in Southern Italy and Sicily (*Y*<sup>1</sup>, III, 157).

There were 100 *raṭl* (< Gr. *λίτρα*) or «*pounds*» to the *qinṭār*. *Raṭl* is Pegolotti's *ruotolo* (> *rotolo*), Med. Lat. *rotulus*, *rota*, and it occurs in Portuguese and Spanish as *arratel* and *arrelde* respectively; cf. DU CANGE, *s.v.* «*rotulus*», «*rota*»; YULE, *Hobson-Jobson*<sup>2</sup>, *s.v.* «*rottle*, *rattle*»; LOKOTSCH, *Etym. Wörterbuch*, No. 1708.

The value of the «*cantar*» varied in different places, and sometimes in the same place; for instance there were two kinds of «*cantar*» at Acre, one being five *ruotoli* heavier than the other (Pegolotti, EVANS, 63). The Genoese «*cantar*», the one used in Constantinople and Pera, weighed 150 Genoese «*pounds*» (*libbre*).

In the Genoese system, 100 « pounds » (*libbre*) made a *centinaio*, « hundredweight »; the *centinaio* was 72 *saggi*; the *saggio*, 24 *carati* (Pegolotti, EVANS, 32).

YULE, in his *Marco Polo* (Y, II, 442), rendered « cantar » as « hundredweight »; but in *Cathay* (Y<sup>1</sup>, III, 165, 167), he has retained « cantar » and reserved « hundredweight » for the equivalence of « centinaio ». Although both « cantar » and « centinaio » etymologically mean « hundred [weight] », they have diverged in value to such an extent that the course adopted in *Cathay* seems to be safer.

## 116. CAPDOILLE

*capadocte* P  
*capdoille*, *capdol* F  
*capdoilz* FB  
*capdos* FA

*capi dollio* VB  
*capidioli* L  
*capidolii* L<sup>1</sup>  
*capita olei*, *cauodoio* Z

*chapedoge*, *gapedoge* VA  
*chapidoglie*, *chapodoglie* TA<sup>1</sup>  
*chapidogli* TA<sup>3</sup>

This is of course the word which has survived in Italian as *capidoglio* and *capidogia*; although earlier Italian dictionaries mention *capodoglio* only from Polo's Italian versions, this latter form has been revived in the *Enciclopedia Italiana* and is adopted in modern Italian by BENEDETTO (B<sup>1</sup>, 352, 357). Literally the name means of course « head of oil », « oil-head » (cf. *capita olei* in Z), and it is still the Italian name of the sperm whale or cachalot. At one time it was more or less adopted as a technical term in French; BECHERELLE still listed « capidolio » as being the name of a dolphin among French naturalists.

In Vol. I, 426, mention is made of a suggestion of mine which I wish to develop and also to qualify. I no longer think now that the sentence found only in RAMUSIO is due, in its second part (*e della testa assai botte di olio*), to a « misunderstanding or explanation of *cauodoio* ». It seems more likely that there was there a genuine sentence, omitted in Z, perhaps because it was corrupt in the ms. from which Z was copied or translated. As MOULE has suggested to me, the unintelligible *et quandoque* of Z may belong to that lost sentence, and represent \**et cauodoie*. Whatever the case may be, the fact that oil was extracted from the head of the « capdoille » is of course true, and it accounts for the very name of the animal.

The English names for the « capdoille », *i. e.* « cachalot » and « sperm whale », are not very old. The first occurrence of « cachalot », as a word used in the region of Bayonne and Biarritz, is in 1670, in the Latin *Miscellanea Curiosa*, Francfort, 1670, p. 266; « cachalot » is then found in a French book in 1746, and in English in 1747 (MURRAY). The most probable etymology is to derive « cachalot » from Gascon *cachau*, Catal. *caxal*, meaning « tooth »; the teeth, of which the cachalot makes good use for his defence, are his distinctive characteristic, while the true whale has no teeth.

The other name, « sperm whale », is a survival of the belief that the waxy white substance actually found in a large cavity and in the subcutaneous folds of the head of the cachalot was the sperm of the animal, hence its name *spermaceti*, lit. « whale sperm ». Through the same

abbreviation as in « sperm whale », such terms as « sperm oil » and « sperm candle » have been created as late as the middle of the last century to designate the oil found with the *spermaceti* or candles prepared with *spermaceti*. The word *spermaceti* itself has been traced in English to 1471 (MURRAY).

*Spermaceti* and oil are found mainly in the head of the cachalot, but also, to a lesser degree, in other parts of its body. Moreover, some other whales and dolphins yield the same products, although to a much lesser extent. But « ambergris » is almost exclusively a secretion of the cachalot, so that Polo's « whalefish and capdoille » are more or less pleonastic (see « Ambergris »).

QAZWĪNĪ († 1283) speaks of the oil which was obtained from the head of what certainly is the cachalot (cf. Y, II, 408). We find the same information in Chinese texts. One of the authorities formerly adduced must, however, be left out. The *Yu-yang tsa-tsu* of the 9th cent., which describes a cetacean six to nine yards long under the unexplained names of 奔鯨 *pên-fu* (perhaps for *p'ên* [賁]-*fu*, « spouting *fu* ») and 鯨 *chi*, says that one of these « fish » yielded thirty to forty bushels of oil (膏 *kao*) which could be used to feed lamps; but it was only by punctuating the text erroneously that ROCKHILL made it mean that the oil was obtained from the head of the *pên-fu* (TP, 1915, 158). On the other hand, Chao Ju-kua, writing in 1225, is unambiguous. According to his notice of Chung-li (= Somali coast), huge dead fish thirty odd yards long were stranded there every year. The natives did not eat their flesh, but « cut out their brains, marrow and eyes to get oil (油 *yu*), sometimes as much as three hundred odd 罈 *têng* (from one fish)» (cf. HR, 131). Perhaps the eyes were taken out for another reason than to get oil; at any rate, « whale eyes » were more than once offered as tribute (cf. LAUFER in TP, 1913, 342; add the text of the *Yüan-ho chün-hsien t'u-chih* of the early 9th cent. in which « big fish eyes » [大魚睛 *ta-yü-ching*] are mentioned as a tribute sent to the Court from the modern Hui-chou-fu in Kuang-tung [34, 6 b]). The value of *têng* is uncertain. HIRTH and ROCKHILL (HR, 69, 132, 283) say that, in Buddhist works, it is generally the Skr. *tolā*, a small weight equal to four *māṣa* (but cf. YULE, *Hobson-Jobson*<sup>2</sup>, s.v. « tola » and « mace », where the « tola » is equal to twelve *māṣa*), but must here represent « some other foreign word, Persian probably ». I do not know HIRTH and ROCKHILL's authorities, but cannot trace *têng* as a measure in any Chinese text, Buddhist or otherwise, except that of Chao Ju-kua; moreover the *tolā* is a weight of late date, not likely to appear in Indian Buddhist texts. My own impression is that we have here to deal with a purely Chinese, though dialectical, term for which there was no regular character. Chao Ju-kua wrote it 罈 *têng*, but I think it probable that it is the same word which occurs many times a little more than a century later in the *Tao-i chih-lio*, where it is written with the unauthorized character 罈 (? *têng*). In his partial translation of the *Tao-i chih-lio* (TP, 136, 251, 266), ROCKHILL rendered the latter form as « jar » without any remark. He was certainly right, and the unauthorized character of the *Tao-i chih-lio* is still used now to write *dīng*, « jar », of the Fu-chou dialect (cf. MACLAY and BALDWIN, *An alphab. dictionary of... the Fouchow dialect*<sup>2</sup> [1898], 195). In such a case, Chao Ju-kua would mention a limit somewhat above three hundred jars of oil for a whale; it is much nearer the true figures than the thirty to forty bushels of the *Yu-yang tsa-tsu*.

One difficulty remains, for which I can offer no satisfactory solution. The « tunny » is of

course the bonito, a staple food of the Maldive Islands, where it is also dried and from where it is exported to various ports of the Indian Ocean; it is the «cobily mash», in Maldivian *Kalubili-mās*, «black bonito fish» (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 223-224; *TP*, 1933, 416-418; DALGADO, *Glossário Luso-Asiático*, I, 186-187, «calmelas»; on the word «bonito» itself, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 104-105, and DALGADO, *Glossário Luso-Asiático*, I, 137-138). But nowhere can I find that the cachalot has any taste for the bonito. Nor do I expect to find such information, since the oesophagus of the true whale is only two inches wide, and the cachalot, although his oesophagus is not so narrow, mainly feeds on cephalopods. Some confusion seems to have occurred, but I do not know how nor why.

## 117. CARACHOÇO

### *carachoço* Z

This is Qarā-qōjo or Qarā-ḥōjo, the ancient Uighur capital, about 17 miles east of Turfan. It was at first mistaken by BENEDETTO (*B*, CLXIX) for Qara-ḥoto (see «Eçina»), but the error was immediately corrected (*Journal des Savants*, 1929, 42). The «Carachoço» of Z was altered to «Carachoco» in the late Milan copy (cf. *B*, 46). It is to be regretted that «Caracoco», although this time correctly identified with Qarā-qōjo, should have been retained in *B*<sup>1</sup>, 73, 440.

In 1912, I devoted a whole paper (*JA*, 1912, I, 578-603) to the history of the name (for Qočo in Albirūni and Kāšyarī, cf. *Mi*, 271), and tried to establish the phonetic evolution 高昌 Kao-ch'ang > Qočo > Huo-chou > Ha-la-huo-chou. As a matter of fact, I had been anticipated in deriving Qočo from Kao-ch'ang by a writer of the Mongol period, Ou-yang Hsüan (1273-1357; GILES, *Biogr. Dict.* No. 1593), in his «family memorial» of a noble Uighur family (*Yüan wên lei*, 70, 10 a). We do not know under what influence the epithet *qara*, «black», came to be prefixed to the name. Judging from analogous cases, one may be tempted to believe that it was when the importance of Qočo began to be superseded by the growing influence of Turfan. But although Turfan is a much older place than it is generally taken to be (I think I can trace its name as far back as the first half of the 7th cent.), the form Qarā-qōjo, at least in Chinese transcription, occurs already in the second half of the 13th cent., and the many mentions of that city in the *YS*, to the exclusion of Turfan, leave no doubt that it remained the principal city of the Turfan region down to the end of the 14th cent.

The form Qočo is now well attested in mediaeval Turkish texts. The corresponding Chinese transcription was at first 和州 Ho-chou in 913 (*Liao shih*, I, 3 b; cf. also 36, 10 b), in 1131 (*Chin shih*, 3, 7 a; «1130» is a mistake in *Br*, I, 222), in works of the early 13th cent. (cf. *Br*, I, 16, 32), and even in 1279 and 1282 in the *YS* (10, 8 a; 12, 7 a). Other forms then occur, such as 霍州 Ho-chou in 1281 (*YS*, 11, 5 b), and 火州 Huo-chou which is still to be met much later in *Ming shih*, 329, 8 a. All these transcriptions are at the same time chosen in such a

way that by the use of the character 州 *chou*, the name should acquire a purely Chinese appearance. The result is that Ch'ên Ch'êng, in the *Hsi-yü fan-kuo chih* which embodies the information collected during his embassy of 1414 (ed. of the Pei-ping National Library, 19 a), speaks of Huo-chou as meaning «Fiery district», and as owing that name to its hot climate. But some transcriptions are chosen without any attempt at a semantic value: for instance, 和 綽 *Ho-ch'o*, used by Ou-yang Hsüan in the 14th cent., does not pretend to be anything but a phonetic rendering of Qočo. A Jučen form 高察安 *Kao-ch'a-an* = *Kao-čän*, based on the Chinese name *Kao-ch'ang*, existed during the Ming period (cf. GRUBE, *Die sprache und Schrift der Jučen*, p. 17).

Even when many texts still use the simple form *Ho-chou*, the enlarged *Qarā-qōjo* makes its appearance as 合 刺 禾 州 *Ha-la-[ho-]chou* in 1285 (YS, 13, 9 a; perhaps a misreading for the next form), afterwards 合 刺 和 州 *Ha-la-ho-chou* in 1286 (YS, 14, 4 b), 哈 刺 霍 州 *Ha-la-ho-chou* in the biography of A-shu (see «Agiul»; YS, 128, 3 a) and 哈 刺 火 州 *Ha-la-huo-chou* in 1330, 1347 and in the biography of Ye-lü Hsi-liang (YS, 34, 3 b; 34, 9 a; 41, 5 b; 180, 2 a). The form 合 刺 火 者 *Ha-la-huo-chê* occurs on the Chinese map of c. 1330, of Western origin, and in the corresponding list of YS, 63, 15 b; it may be the one which is at the basis of a misreading *Ha-la-huo* in the text quoted by LAUFER, *Sino-Iranica*, 236. The same form *Ha [哈]-huo-chê* occurs in a Ming itinerary (*China Review*, v, 232). The modern Chinese transcription is 哈 喇 和 卓 *Ha-la-ho-cho* (cf. *Hsi-yü t'u-chih*, 14, 6). It is said in the *Ming shih* (329, 8 a) that *Huo-chou* is also called 哈 刺 *Ha-la*, the same name of course as *Qarā-ḥōjo* (cf. *Br*, 186; *JA*, 1925, I, 582). Naturally enough, BRETSCHNEIDER said that there were «evidently two characters wanting in the Chinese text». But the form of the *Ming shih* is curiously supported by a text hitherto mistranslated. «Sanang Setsen» speaks, under the year 1439, of a land (*yajar*) which SCHMIDT's translation calls «Turufanu Chara» (= *Turufanu Ḥara*), adding that *Turfan* must be meant (*Gesch. der Ost-Mongolen*, 155, 406; SCHMIDT's Mongol text actually gives *Tü*). From this passage BLOCHET (*Moufazzal*, 691) deduced the existence of a «*Turfan the Black*», grammatically impossible. But the text is perfectly clear, and is correctly understood by the Chinese translator of the Mongol chronicle: *Turufan-u Qara nārütü yajar* means «the land of *Turfan* called *Qara*». In other words, the Mongol chronicler of 1662 knew *Qarā-ḥōjo* under the same shortened name *Qara* which is given in the *Ming shih* as an alternative name of *Huo-chou*. The same form *Qara* occurs, also in connection with the events of 1439, in the *Altan tobči*, half a century earlier than «Sanang Setsen» (GOMBOEV ed., 161); but, instead of *Turfan-u*, the *Altan Tobči* gives *Mingan-u*, probably by a scribe's error.

Polo's «Carachoço» is quite correct, and in agreement with the various spellings of *Ha-la-huo-chou* in YS and with Rašidu-'d-Din's *قراخوجو* *Qarā-ḥōjō*, although Juwainī, and sometimes Rašid also, write *قراخواجه* *Qarā-ḥōjah* (*Bl*, II, 85, 502, 595, 609), in the same way as Chinese sources sometimes give *Ha-la-huo-chê*. The latter Turkish form, now prevalent in Chinese Turkestan, is the result of popular etymology, *Qarā-ḥōjah* meaning «Black master». An identification of the *Kc'n* (= \**Kačän*) of an old Sogdian document with *Kao-ch'ang* is not probable (cf. *TP*, 1931, 460).

The name *Kao-ch'ang* goes back to Han times. As *Kao-ch'ang-pi* [壁], or «Wall of *Kao-ch'ang*», it was the designation of a Chinese military colony which first existed for a short time

in 59 B.C., and was re-organized in 48 B.C. Its aim was to strengthen Chinese influence in the territory of the king of Anterior Chü-shih, whose capital was at Yar, to the north-west of Turfan. Kao-ch'ang grew enough in importance to become in 327 the seat of a *chün* («commanderie» in CHAVANNES's terminology), which was created by the irregular dynasty of the Anterior Liang of Kan-su. When the Ta-Chü-chü became independent in the Turfan region, they had their capital at Kao-ch'ang; it was certainly there in the middle of the 5th cent. Later, the Ch'ü dynasty moved its capital to Yar, perhaps already in 497, and in any case before the Kao-ch'ang kingdom was conquered by the T'ang in 640; the accounts of the siege of the capital in 640 refer to Yar, not to Kao-ch'ang = Qarā-ḥōjo as is generally said. After the conquest, the «general protectorate for the Pacification of the West» (*An-hsi tu-hu-fu*) and the civil administration of Hsi-chou («Western District») were at first established at Yar, but were soon transferred (in 651 or 657) to «the ancient land of Kao-ch'ang», *i. e.* most probably to Qarā-ḥōjo. In 658, the «general protectorate for the Pacification of the West» was moved to Kučā, but the civil administration of Hsi-chou remained at Qarā-ḥōjo. About 790, Qarā-ḥōjo was conquered by the Tibetans. After the fall of the Uighur empire of the Orkhon basin in 840-843, a new Uighur kingdom was founded in the Turfan region; its southern capital was Qarā-ḥōjo, the northern one being Beš-baliq, north of the T'ien-shan and to the north-east of Urumchi. Vassal of the Qara-khitai in the 12th cent., the Uighur kingdom of Qarā-ḥōjo submitted to Chinghiz-khan in 1209; the royal family and the grandees, laden with honours, retained a semi-official position; towards 1400, Qarā-ḥōjo was replaced by Turfan as the foremost city south of the T'ien-shan. As the sovereign of the Uighur kingdom of Qarā-ḥōjo bore the title of *idūqt*, *idīqt* (< *idūq-qt*, «Holy Majesty»), the ruins of his capital are now known locally as *Idīqt-šahrī*, «City of the *idīqt*». The wonderful finds of manuscripts and of artistic and antiquarian remains which have in recent years been made at *Idīqt-šahrī* by GRÜNWEDEL and, above all, by VON LE COQ, are well known.

The literature on Kao-ch'ang is voluminous. In the above sketch, I have summed up the conclusions of an unpublished memoir which I have written on the history of the Turfan region. Some of these conclusions are not in absolute agreement with current views. I must abstain, however, from quoting my authorities because they would be useless and sometimes misleading without a critical examination of almost every passage, a procedure which space here forbids.

At the beginning of the Christian era, the population of the region of Turfan must have spoken the same language as the people of Qarāšahr (then Yen-ch'i), a so-called «Tokharian» dialect. Then with the growth of the colony of Kao-ch'ang, Chinese gained ground. The first Kao-ch'ang kingdom of the 5th-7th cents. was in the hands of rulers of Altaic origin who had already turned almost Chinese. A real Turkish influence is manifest in documents of the end of the 6th cent., when the T'u-chüeh empire extended its sway over almost the whole of Central Asia. Then came the Chinese conquest of 640, with a lasting cultural and linguistic action. The Tibetan occupation at the end of the 8th cent. was short-lived, and left but few traces. From the second half of the 9th cent., the Uighur dialect of the Turkish language ruled supreme, and was used by followers of all the various religions which were practised there, Buddhism, Nestorianism, Manichaeism, perhaps Mazdeism, and later Islam. In the Mongol period, the bulk



of the population was Buddhist; many Buddhist works were then translated from Tibetan or Chinese into Uighur, and sometimes from Uighur into Tibetan and Chinese. In 1419-1421, Sāh-Rūh's envoys still found Buddhists at Qarā-ḥōjo. The whole population must have turned Mohammedan in the course of the 15th cent. To the sources generally quoted for that period, two must now be added, the accounts of Ch'ên Ch'êng's mission of 1414, recently reproduced by the National Library of Peiping (*Hsi-yü hsing-ch'êng chi* and *Hsi-yü fan-kuo chih*).

The city of Kao-ch'ang was known in Turkish as Qoço, later Qarā-ḥōjo, but it also had a purely Iranian name, Činānč-kanθ (and Činānč-käθ), «Chinese town» (cf. F. W. K. MÜLLER, *Mahrnāmag*, in *APAW*, 1913, 10, 30 [with an erroneous suggestion that it may be Qomul; see «Camul»]; *Mi*, 271; HENNING, in *BSOS*, ix, 566). Nothing could show better that the Chinese origin of the «Wall of Kao-ch'ang» was then still remembered. North of Činānč-käθ, the *Hudūd al-'Ālam* mentions the طفقان Ṭafqān mountain; Ṭafqān is not a corruption of طرفان Ṭurfān, as supposed by MINORSKY (*Mi*, 195, 271); it is the name which occurs in Chinese sources as Mount 貪汗 T'an-han (\*T'ām-yân), for instance in *Wei shu*, 101, 8 a, and *Hsin T'ang shu*, 217 B, 3 a; cf. CHAVANNES, *Doc. sur les Tou-kiue*, 95, 363.

Polo's chapter on the Uighur kingdom (see «Iuguristan») and on its capital Qarā-ḥōjo is only known from Z, so that, apart from certain brief remarks in *RR*'s introduction and our Vol. I, 49, it has never been commented upon. Although Polo is speaking from hearsay, the account is remarkably correct. The legend that the first Uighur king was born from the swelling of a tree is well known (cf. *Oh*, I, 431-432) and has been studied by MARQUART, *Ĝuwainī's Bericht über die Bekehrung der Uiguren* (in *SPAW*, 1912, 486-502); on the analogous legend concerning the Qipčaq, cf. my remarks in *TP*, 1930, 279-282; see also «Esca», and Ross's comments in Vol. I, 49. The intermarriages between Christians and non-Christians are more than likely, although they were not then peculiar to the Uighurs. As to the quality of the grape-wine produced in the Uighur country, it was praised at the time by Rašidu-'d-Dīn (*Bl*, II, 502; *Y*, III, 133) and by the Chinese (*TP*, 1908, 362); for instance, Ha-la-huo-chou (Qarā-ḥōjo) offered grape-wine to the Court in 1330 (*YS*, 34, 3 b; cf. also LAUFER, *Sino-Iranica*, 236-237).

In 1275 according to GAUBIL, though more probably in 1285, Qaidu and Dua besieged the Uighur *idīqt* in his capital Qarā-ḥōjo (see «Cibai» and «Caidu»). But I find no support for BLOCHET's statement (*Moufazzal*, 608, 683) that the boundary between the territories under Qubilai's direct authority and those in the obedience of Qaidu lay in the middle of Qarā-ḥōjo (which moreover is not «three days» distant from Turfan). Although Blochet does not mention his authority, it seems as though he had misunderstood Rašidu-'d-Dīn's text describing Qarā-ḥōjo as a city of the Uighurs which lay between the dominions of the Great Khan and those of Qaidu and tried to remain neutral (*Bl*, II, 502; *Oh*, II, 640).

Fra Mauro gives «Iuguristan», but not «Carachoço» on his map. On the Catalan Map, there is a place called «Carachoiat», which BUCHON (*Not. et Extr.*, XIV, II, 135) wrongly identified with Qara-qorum (see «Caracorom»; cf. also HALLBERG, 112-113). It may well be Qarā-ḥōjo (or Qarā-ḥōjah). This identification would not in the least imply that the author of the Catalan Map knew a text of Polo which had the chapter on the country of the Uighurs; all the rest of the map shows the contrary, and in as far as Polo's work is concerned, is based on the

French mss., which all lack that chapter. But the Catalan Map has on that part of Central Asia many pieces of information of later date than Polo's travels; the name of «Carachoiant» may be one of them.

## 118. CARACOROM

<i>carachoran, carchoran</i> R	<i>caroccoran, chorochoram</i> VA	<i>characom</i> TA <sup>1</sup>
<i>caracorán</i> Ft, Z	<i>carocoram</i> P	<i>characorón</i> VB
<i>caracoron</i> F, Fr, FA, FB, L,	<i>cathocora, cathogora, catochora</i>	<i>characharom</i> TA <sup>3</sup>
VB, Z	VL	<i>charocharon</i> TA <sup>1</sup> , TA <sup>3</sup>
<i>carathoron</i> FAt	<i>chara</i> (?), <i>characholan, chara-</i>	
<i>carocaton, tartaron</i> LT	<i>choron</i> V	

The «Caracoron» of F, Z, etc., is probably a misreading of \*Caracorō = Caracorum, Qaraqorum. The first Western writer to name the place is Plan Carpine, who stayed in 1246 at Šira-ordu, half a day from «Caracaron» (*Wy*, 30; but «Contracuram» in the Wolfenbüttel ms., *Wy*, III, 805; «Cracurim» in Vincent de Beauvais's abridgment, *Spécul. histor.*, xxxii, 3; perhaps all < «\*Caracorum»). In 1254, Rubrouck spent many days in Qaraqorum. Although the manuscripts of his relation most often give «Caracorum», readings like «Carachorum» and «Caracorum» also occur (*Wy*, 230, 236); I consider them to be the only correct ones. The Catalan Map has «Carachora» (BUCHON, in *Not. et Extr.*, XIV, II, 141; HALLBERG, 112). The name is repeated twice by Fra Mauro, in different inks, as «Charocharā» (= «Charocharam») and as «Characoracur» (such are the actual readings on the map, sometimes misread in the sources of HALLBERG, 113). Kirakos writes «yarayorum» (with the ancient velar *l* passed to *γ*; cf. BROSSET, *Deux historiens arméniens*, I, 115; PATKANOV, *Istoriya Mongolov*, II, 12); the Georgian chronicle gives «Qaraqurum», according to BROSSET (*Hist. de la Géorgie*, I, 485). Persian writers, beginning with Jūwainī and Rašīdu-'d-Dīn, give forms ranging from قاراقورم Qarā-qōrūm to قاراقورم Qarā-qōrūm. The form Qara-qorum occurs in the *Secret History* (§ 273) and, abridged as Qorum, in a Mongol inscription of 1346 (translated in fact from the Chinese; cf. *JA*, 1925, I, 374). The *Yüan shih* (*YS*, 2, 2 b) mentions the place for the first time as 和林 Ho-lin (not «Ha-la-ho-lin», as is said in *Br*, I, 122) in 1235, when Ögödäi ordered it to be walled; later, Ho-lin recurs repeatedly in the official history as well as in other works. The complete form 哈喇和林 Ha-la-ho-lin, Qara-qorum, occurs only once (*YS*, 58, 18 a), as the name of a river from which the city of Ho-lin, Qorum, derived its name. In 1312, the administrative name was altered to the *lu* of 和寧 Ho-ning, which, while retaining something of the original sounds, meant in Chinese «Harmonious peace». The older transcription Ho-lin (then pronounced Ho-lim) was due to the fact that, the Chinese never having had an *r*-, Middle Chinese also lacked all *-um* or *-om* endings.

The history of the name Qara-qorum is not clear; yet the form is certain beyond all doubt.

and it is a pity that so many authorities should persist in tracing it back to a pseudo-Mongol original \*Qara-kürän, which never existed («Qara-kürän» still occurs for instance in *Ch*, I, 164, and *Wy*, 30). According to *YS*, 58, 18 a, [Qara-]Qorum owed its name to a river Ha-la-ho-lin, Qara-qorum, running west of the city. 張德輝 Chang Tê-hui, who visited Qubilai in his camp of Mongolia in 1247-1248 and left a diary, a fresh translation of which is badly wanted, speaks of a road leading to the city of Ho-lin ([Qara-]Qorum). The text goes on to say that there was there a large plain, and PALLADIUS's translation (*Izv. Sibirsk. Otd. I. R. Georg. Obšč.*, Nos. IX-X, Irkutsk, 1867, 582 sq., retranslated into English by SCHUYLER, with notes by YULE, in *The Geogr. Magazine*, 1875, 7 sq.) as well as BRETSCHNEIDER's (*Br*, I, 55) then give : «In the middle of it runs the river Ho-lin». It would thus seem that Chang Tê-hui's account corroborates the information of *YS*. But both translators agree that the river Ho-lin is the Orkhon. Now the Orkhon was known as such at least since T'ang times, and its name can have nothing in common with that of Ho-lin = Qorum. But I think that the translations are not accurate. Chang Tê-hui, after speaking of the large plain, says 中卽和林川也 «in the middle [of the plain] is the valley (*ch'uan*) of Ho-lin». The word *ch'uan* means both a «stream» and a «valley with a stream». In my opinion, Chang Tê-hui simply says that this was the valley in which [Qara-]Qorum lay, but not that [Qara-]Qorum was the name of the river. The result is that the passage of the *YS*, 58, 18 a, stands alone, and it is contradicted by both Chinese and Persian texts. The *YS*, 122, 1 a, at the beginning of the biography of the Uighur *tduqut* Barčuq-art-tegin, gives some information on the ancient seat of the Uighur empire of T'ang times, a region where the Tūla and the Selenga had their sources «in the Ho-lin (Qorum) mountains» (cf. *Br*, I, 247-248; the same text occurs in Yü Chi's *Tao-yüan hsüeh-ku lu*, 21, 8 sq.). A similar tradition was heard by Jūwainī. According to it, the Uighurs originally dwelt on the bank of the Orkhon River, which takes its rise in mountains called Qara-qorum from which Ögödäi's new city derived its name; their first legendary king had been born at the junction of the Tuyla (> Tūla) and the Selenga, both of which take their rise in the Qara-qorum mountains (Mirzā Muḥammad khan QAZWĪNĪ, *Jūwainī*, I, 39-40; *Oh*, I, 430-431; *Br*, I, 254-255). Rašidu-'d-Dīn's account is different from that of Jūwainī; but he also speaks of the mountain Qara-qorum, from which the city founded by the «qañ» (= Ögödäi) took its name (*Ber*, I, 125). With such agreement between Chinese and Persian texts, I feel much inclined to suppose that the «river» Qara-qorum of the *YS* is the result of some confusion. One point, however, must not be forgotten : the Chinese texts which speak of the [Qara-]Qorum mountains as well as the mentions of the Qara-qorum mountains in Jūwainī and Rašidu-'d-Dīn all occur in a traditional account which is less concerned with the Mongol period than with the legend of Uighur origins.

The name being then alive in Uighur tradition, it is no wonder that it should be fundamentally Turkish, and not Mongolian. In Turkish, *qorum* means «rolled stone», «boulder»; the word occurs in the *Qutaḍyu bilig* and in *Kāšyari* (RADLOV, II, 561; BROCKELMANN, 161). Qara-qorum, «Black boulder», is an appropriate name for a mountain. As a matter of fact, the name of the Mongol capital, derived from the Turkish name of a mountain in the Orkhon basin, is identical with that of the modern Qara-qorum Chain, the Karakorum of our maps, at the south-western end of Chinese Turkestan.

We know for certain that Qara-qorum was walled for the first time in 1235, by Ögödäi. BARTHOLD (*12 Vorlesungen*, 181-182) has expressed surprise at this decision of Ögödäi, whose inherited possessions were in the Emil and Qobuq territory and who, by settling in Mongolia proper, seemed to him to trespass on his brother Tului's rights. The explanation, if one be necessary, may lie in the fact that Qara-qorum had already been designated as Mongol capital in the lifetime of Chinghiz-khan. Not that there can be any foundation in CORDIER's assertion (*Y*, I, 227) that Qara-qorum was chosen by Chinghiz-khan in 1206 as his capital. On the other hand, I no longer believe that Rašid in his biography of Chinghiz-khan mentions the name of Qara-qorum in connection with events of 1220 as I said in *JA*, 1925, I, 375; in the text I alluded to (*Ber*, III, 46), I now think that Qara-qorum is corrupt for قاراقوم, Qara-qum, the well-known « Kara-kum » Desert between the Amu-darya and the Caspian Sea (for the same confusion in Juwaini's mss., cf. QAZWĪNĪ, *Juwainī*, I, 69, 70, 279; by an inverse error, VULLERS [II, 717] gives « Qaraqum » as the name of Chinghiz-khan's capital). But the *YS* (58, 18 a), following texts which must be of c. 1330, says that the capital was fixed at Qara-qorum in 1220, and an inscription written on stone by Imperial order in 1346 and erected in Qara-qorum begins with these words : « The 15th year of the Holy and Warlike T'ai-tsu (= Chinghiz-khan), the signs of the year being *kêng-ch'ên* (1220), the capital was fixed at Ho-lin (= Qara-qorum) ». The identical information transmitted by the two texts may be accepted as based on good authority. In all likelihood, although Qara-qorum was walled only in 1235, Chinghiz-khan had already fixed there his main encampment, and perhaps had left there his *oyruq*, i. e. the camps of his wives and personal servants, while he was waging war with the Mussulmans in the West. Prior to Chinghiz-khan's accession, Qara-qorum belonged to the Naiman (*Ber*, I, 108), perhaps also at one time to their immediate neighbours and rivals the Kerait (*Wy*, 207).

Rubrouck was not particularly struck with the size and appearance of the Mongol capital : « It is not as big as the village of Saint Denis, and the monastery of Saint Denis is ten times larger than the palace » (ROCKHILL, *Rubruck*, 220; *Wy*, 285). We know, however, that Ögödäi had ordered trained artisans and painters from China to build it up and adorn it. There were mosques and churches and Buddhist temples. A Buddhist monastery of Qara-qorum, the 太平興國禪寺 T'ai-p'ing hsing-kuo ch'an-ssü, is mentioned in 1247 (cf. *Tōkyō Tripit* of Meiji, 致, XI, 39 a), and in 1256, Mongka had erected in Qara-qorum a five-storeyed stūpa, 300 feet high and with annexed chambers, which was repaired in 1311 and again in 1342-1346 (cf. *JA*, 1925, I, 373).

Qara-qorum remained officially the Mongol capital until Qubilai's accession to the throne in 1260. YULE (*Y*, I, 227) and BENEDETTO (*B*<sup>1</sup>, 440) are mistaken when they say that Qara-qorum ceased to be the capital in 1256 and 1257, respectively. The foundation of K'ai-p'ing in 1256 (see « Chemeinfu ») was intended only to provide a summer residence for Qubilai who was then a prince, not for the Emperor as such. There is something wrong when CHARIGNON (*Ch*, I, 164) re-translating into French what he says is a translation from D'OHSSON's French into Chinese, makes D'OHSSON say that the name of Qara-qorum was transmitted to the West when the Uighurs went there « in the year of the dragon (1208) » to submit to Chinghiz-khan. I do not know the Chinese translation in question, nor do I find anything similar in D'OHSSON's original work.

There must also be some confusion when ELIAS (ELIAS and ROSS, *Tarikh-i Rashidi*, 361) says that « Károrum was, and even is to the present day, known as *Bála Sákun* ».

When the capital had been transferred to Peking in 1260, Ho-lin (Qara-qorum) became the seat of a *hsüan-wei-ssü*, who held at the same time the charge of commander-in-chief (*tu-yüan-shuai-fu*). Later on, the *tu-yüan-shuai-fu* was moved south of the 金山 Chin-shan (= Altai) and Qara-qorum had only a *hsüan-wei-ssü*. In 1289, the *hsüan-wei-ssü* joined in the rebellion of the discontented Mongol princes (see « Caidu »). The following year, a commander-in-chief was re-established. In 1307, a « moving Grand Secretariat » (*hsing chung-shu shêng*; see « Scieng ») was created at Qara-qorum, with a « general administrator » (*tsung-kuan-fu*) of Ho-lin; changed in 1309 to a « moving Grand Council » (*hsing shang-shu-shêng*), it became again a « moving Grand Secretariat » in 1311. In 1312, the « moving Grand Secretariat » of Ho-lin received the new name of « moving Grand Secretariat » of 嶺北 Ling-pei (« North of the Mountains »), and the « general administrator » (*tsung-kuan-fu*) of the *lu* of Ho-lin became « general administrator » of the *lu* of Ho-ning (YS, 24, 7 b; 58, 18 a). This was the last administrative change until the dynasty fell in 1368. In the course of the long war with Qaidu (see « Caidu »), Qara-qorum changed hands temporarily, but as a rule remained in the possession of the Emperor.

D'OHSSON (*Oh*, II, 64), relying on Ĵuwainī (I, 192), said that, although Ögödäi's capital became more famous as Qara-qorum, the name which Ögödäi had given to it was Ordu-balıq. This has been repeated by CORDIER (*Y*, I, 227), VAN WYNGAERT (*Wy*, 30), etc. We have seen that Qara-qorum was a Turkish name; such is also the case with Ordu-balıq, which means in Turkish « City of the [Royal] camp »; the corresponding Mongol name would be Ordu-balyasun. But I am somewhat sceptical about Ĵuwainī's statement. From his own words in that passage and still more from his account of Uighur origins (I, 39-40; *Oh*, I, 430-431; *Br*, I, 254-255), we know that Ordu-balıq was the name of the ancient Uighur capital in the Orkhon region, a name which the Mongols changed to the hybrid Turko-Mongolian name Ma'u-balıq, « Bad city ». That name of Ordu-balıq may not be exclusive; for instance, its Turko-Iranian counterpart Ordu-kánt or Ordu-kánd was a name of Kāšyar (BROCKELMANN, 246; BARTHOLD, 12 *Vorlesungen*, 75; *Mi*, 494). Yet, we know exactly the site of the ancient Uighur capital; it is the present Qara-balyasun (Kara-balgasun), west of the Orkhon, while Qara-qorum lay east of the river (cf. the map of *Y*, I, 229). Ĵuwainī visited the ancient Uighur capital, and saw there ancient inscriptions *in situ*. But these cities, the ancient Uighur capital and the new Mongol one, were not at a great distance from each other. I suspect that Ĵuwainī unduly extended to Qara-qorum a name which properly belonged only to what is now known as « Kara-balgasun » (« Kara-balgasun » is of course a modern Mongol name; BLOCHET is mistaken when he gives it [*Moufazzal*, 691] as the name by which the Uighur capital was called in the 8th cent.).

BRETSCHNEIDER (*Br*, I, 123), followed by CORDIER (*Y*, I, 228), speaks of Qara-qorum as being the place « where, after the expulsion of the Mongols from China, Togon-temur again had fixed the Mongol court ». This is not quite correct. As may be seen from BRETSCHNEIDER himself (*Br*, II, 162), Toyön-tämür, when he fled from Peking in 1368, sought refuge first in Shang-tu (see « Ciandu »), soon fled farther to Ying-ch'ang in South-eastern Mongolia (see « Barscol »), and died in that region in 1370; it was his son who was able to make good his escape

to Qara-qorum. In 1412 or 1413, the great Mongol leader Aruqtai received from the Ming Emperor the title of « prince of Hōning » (*Br*, II, 164); Ho-ning, as we have seen, had been the administrative name of Qara-qorum since 1312. Later Mongol history is practically silent on Qara-qorum. « Sanang Setsen », in connection with Aruqtai, but with the impossible date of 1415, relates a sentence referring to the projected rebuilding of « the cities of Qorumqan » (SCHMIDT, *Gesch. der Ost-Mongolen*, 145-146, 404). Qara-qorum seems here to be meant, but the form is unexplained. As many Mongol names of mountains popularly end in *-qan*, *-han*, « lord », « sovereign », it may be that Qorumqan should be equivalent to « the cities of the [Qara-]Qorum mountains », « cities » in the plural being a plural *majestatis*. The same Mongol chronicle mentions twice Hōning for the years 1552 and 1576 (SCHMIDT, *loc. cit.* 211, 226). So the last two mentions of Chinghiz-khan's and Ögödäi's capital in Mongol texts call it by its Chinese name; that of Qara-qorum had by then sunk into oblivion.

But, if the name was forgotten, the site remained. As is well known, the great Buddhist monastery Erdeni-ju, founded in 1585, was erected on the very ruins of Qara-qorum (*Br*, I, 123).

PENZER (*Pe*, XXXII, XLV, XLVII) still thinks that Polo may have visited Qara-qorum. The little that Polo relates of the city seems, on the contrary, to establish that for him it was only a name.

## 119. CARAGIAN

City		
<i>caracham</i> LT	<i>carayan</i> Z	<i>charaiam</i> VA
<i>caraian</i> F, FA, FB, L	<i>carazan</i> L; R	<i>charayan</i> VB
<i>carayam</i> P	<i>charagian</i> TA <sup>1</sup>	<i>chataian</i> , <i>chiarenza</i> V
Province		
<i>caiaran</i> , <i>carassan</i> , <i>charayan</i> VB	<i>caraian</i> F, Fr, t, FA, FB, L, Z;	<i>charagian</i> TA <sup>1</sup> , V
<i>caraam</i> G	R	<i>charaiam</i> , <i>charaian</i> VA
<i>caracham</i> LT	<i>carayam</i> P	<i>charangia</i> TA <sup>3</sup>
<i>caraçan</i> Z	<i>carazan</i> R	<i>charauian</i> , <i>chataian</i> , <i>chataio</i> V
<i>caragian</i> F	<i>charagia</i> TA <sup>1r</sup> , VL	<i>charian</i> VL, VLr
<i>caraiam</i> F, FA <sup>t</sup> , LT, Pr, VA, Z		

The name is certain and represents the Mong. Qara-Ĵang. Fra Mauro follows the mistaken distinction made in some mss. when he gives on his map two places, « Charazan » and « Charaian » (HALLBERG, 114). On the contamination between « Caragian » and « Calacian » in the Catalan Map, see « Calacian ». Rašīdu-'d-Dīn writes قراجانك Qara-Ĵang (BLOCHET's edition always gives Qara-Ĵang, but Rašīd's mss., here as usual, do not distinguish between چ *j* and چ *ç*, and simply give *j*, which in the present case is the only correct form). The ordinary Chinese transcription is 哈刺章 Ha-la-chang (YS, 11, 2 a; 123, 4 b, 8 a; 125, 1 b, etc.), but other forms occur : 合刺章 Ha-la-chang (YS, 8, 2 b; 14, 1 b; 121, 3 a), 阿刺章 A-la-chang (YS, 14, 2 b), and 哈刺張 Ha-la-chang (YS, 134, 9 a; *Yüan tien-chang*, 3, 2 b [in a document of 1312 in spoken Chinese]).

These too suppose an original Qara-Ĵang. Like so many tribal names, Qara-Ĵang was also used in the Mongol period as the personal name of individuals most of whom did not belong to the Qara-Ĵang tribes (cf. WANG Hui-tsu<sup>1</sup>, 35, 7-8; *JA*, 1927, II, 268).

There cannot be the slightest doubt that, in Polo's time, Qara-Ĵang was the Mongol equivalent of what the Chinese called Yün-nan, a « moving Grand Secretariat » created in 1273 (*YS*, 8, 2 *b*; not 1276 as in *YS*, 61, 1 *a*) and practically extending over the same area as the modern province of Yün-nan. Persian-speaking circles had adopted it from the Mongols, and Polo's nomenclature is once again the Persian one. The Persians even used it after the fall of the Mongol dynasty. In the Persian-Chinese Vocabulary of the Board of Translators of the Ming dynasty, قریانی, with a Chinese phonetic transcription 古兒呀你 Ku-êrh-ya-ni, is given as the Persian name of Yün-nan. The Persian form, which one would normally read \*Qaryani, is certainly corrupt, and so is the Chinese transcription which would suppose \*Quryani; it is clear, however, that this corrupt Persian form goes back to an original Qara-Ĵang. KLAPROTH probably alluded to the same Vocabulary, but with an arbitrary correction, when he said (*JA*, I, 1828, 110; XI, 1833, 458) that « قریان Karâyan » was the name under which Yün-nan Province was known among the Mussulmans of Central Asia. KLAPROTH's statement has been repeated and developed by PAUTHIER (*Pa*, 388) and BLOCHET (*Bl*, II, 375), but YULE (*Y*<sup>1</sup>, III, 127) rightly doubted it. YULE [was equally right (*Y*, II, 74; *Y*<sup>1</sup>, III, 127) in denying any foundation to KLAPROTH's and PAUTHIER's additional statement, also repeated by BLOCHET, that the alleged name of the « Karâyan » was the same as that of the Karens of Burma.

YULE showed the same sure instinct when he was the first to say that Polo's « Iaci » (*q. v.*), the main city of « Caragian », could only be the capital of Yün-nan, the modern Yün-nan-fu. Rašidu-'d-Dīn too gives Yači as the principal city of Qara-Ĵang (*Bl*, II, 494). But Polo goes on to speak of the western part of « Caragian », where another capital of the kingdom existed, and that city was itself called « Caragian »; all are agreed that this western city is Ta-li. I think that here Polo is right in his statement, and that we can account for it by examining the past conditions of the country.

Yün-nan was conquered by the Mongols in 1253-1257, at first under the nominal command of Qubilai, at that time only a prince but all along under the actual leadership of Uriyangqadai. Before the arrival of the Mongols, the country formed the Ta-li kingdom, with its chief capital at Ta-li, and a second capital at Yün-nan-fu. The Mongols, who entered Yün-nan from the north-west, first took Ta-li. It was perhaps at the time of that campaign that the name « Qara-Ĵang », partly Mongol as will be seen below, was first used or at least won popular recognition. Although at a later date it became the Mongol name of the « moving Grand Secretariat » which had its centre at Yün-nan-fu, it was certainly at first the Mongol designation of the Ta-li kingdom, the real capital of which was at Ta-li. But it would not be surprising if the Mongols had used the name of the kingdom as a designation of its capital; this is what Polo's words amount to, and I think I can corroborate this statement from Chinese sources. When in 1273 Sayyid Ajall was sent to establish a « moving Grand Secretariat » in Yün-nan, it was « to govern the barbarians (*man*) of Ha-la-chang (Qara-Ĵang), Ya-ch'ih (Yači), Ch'ih-k'o, Chin-ch'ih, and Ch'a-han-chang » (*YS*, 8, 2 *b* : 統合刺章鴨赤赤科金齒茶罕章諸蠻). Ch'ih-k'o, Čitkör, is the Mongol name of the

Kuei-kuo, the Kingdom of Devils, of the Chinese (see «Cogacin»); the Chin-ch'ih are the Zardandān (see «Çardandan»). On the Ch'a-han-chang, Čayān-Ĵang, more will be said below. Two names remain, Qara-Ĵang and Yači. There can be no doubt that the two names correspond to the two halves of the Ta-li kingdom; and since Yači is acknowledged to designate Yün-nan-fu, the capital of the eastern half, the conclusion is unavoidable that Qara-Ĵang was used among the Mongols as the name of the main capital, situated in the western half, *i. e.* Ta-li. The text concerning the sending out of Sayyid Ajall in 1273 retains in Chinese the greater part of the Mongolian names used in the original Mongol document. A few years earlier, in 1267, a strictly parallel text speaks of the mission of the prince Hügäči (see «Cogacin») to Yün-nan and says that he was «to control the various regions called Ta-li, Shan-shan, Ch'a-han-chang, Ch'ih-t'u-ko-êrh, and Chin-ch'ih» (YS, 6, 5 b : 鎮大理鄯茶罕章赤禿哥兒金齒等處). Čayān-Ĵang, Čitkör, and Chin-ch'ih are common to both lists. Shan-shan was then the Chinese name of Yün-nan-fu, called Yači by the Mongols. The only remaining name is Ha-la-chang in one case, Ta-li in the other; it is evident that the text of 1267 has given the Chinese name of the place, while the text of 1273 retains the Mongol designation. Consequently Polo was amply justified when he gave «Caragian» as being at the same time the name of the «kingdom» of Yün-nan and of the city of Ta-li.

But what is this «Caragian», Qara-Ĵang? *Qara*, «black», is both Turkish and Mongolian, but the Mongol origin of the designation is established by the parallel name Čayān-Ĵang, «White Ĵang», since *čayān* does not exist in Turkish. Both appellations may be descriptive, and may have been suggested to the Mongols, as they were earlier to the Chinese, by the colour of the tribal dress, just as other Yünnanese tribes were then called Chin-ch'ih, «Gold-Teeth», and Iki-büsä (Yäkä-büsä), «Big-Belts». It seems doubtful, however, that such an explanation may account for the various classes of «white» and «black» tribes among the various races of Yün-nan. The Čayān-Ĵang are named more than once in YS, in the forms 茶罕章 Ch'a-han-chang (YS, 6, 5 b [in 1267]; 8, 2 b [in 1273]; 61, 4 a and b) and 察罕章 Ch'a-han-chang (YS, 13, 2 b [in 1284]; 15, 3 b [in 1288]; 61, 4 b; 121, 2 b). Rašidu'-d-Din also speaks of the چغان جانك Čayān-Ĵang (Bl, II, 374, 376). There is no doubt now that the name refers to the Mosso region of 麗江 Li-chiang, in north-western Yün-nan; the administrative title of the «official in charge of the people of Čayān-Ĵang» (茶罕章管民官) was created for a local chieftain in 1254, at the time of Qubilai's campaign, and after the fall of Ta-li (cf. YS, 61, 4 a and b; CHAVANNES, in TP, 1912, 603, 608, 615). To Qara-Ĵang and Čayān-Ĵang I think we must add a third name. In the biography of a man of Uighur origin who served in the reign of Qubilai on the borders of Ssü-ch'uan and Yün-nan, we read (YS, 133, 3 a) that, on one occasion, the «moving Grand Secretariat» of Yün-nan was ordered to detail under that man's command «four hundred men of the Mongol troops [stationed] in the Lo-lo-ssü district and six hundred Lo-lo-chang» (羅羅斯蒙古軍四百人羅羅章六百人). It stands to T'u Chi's credit (47, 4 b) that he has drawn attention to this passage and has connected Lo-lo-chang with Qara-Ĵang and Čayān-Ĵang. Lo-lo-chang can only be explained as Lolo-Ĵang. The name of the Lolo tribes occurs many times in YS, either written 羅羅 Lo-lo (YS, 61, 7 b), or 羅羅斯 Lo-lo-ssü (YS, 9, 7 a; 61, 8 a; 99, 12 a; 100, 12 a; 133, 3 a, etc.), sometimes 羅羅思 Lo-lo-ssü (WANG Hui-tsu<sup>2</sup>, 49, 5 a), once 魯魯斯 Lu-lu-ssü (YS,



121, 3 a); these transcriptions render Lolo and Lolos respectively, the final -s being the mark of the plural in Mongolian. There were, no doubt, Lolo tribes within the modern boundaries of Yün-nan, but the conditions in which the districts of the Lolos and the Lolo-Ĵang are named in the above-mentioned text seem to imply that we have there to deal with the Lolos of the Chien-ch'ang Valley, Polo's «Gaidu» (*q. v.*), which is now in Ssü-ch'uan, but which belonged to Yün-nan under the Mongols. It follows that the Mongols of the middle of the 13th cent. have used the name Ĵang, with various epithets, in reference to the population of the Ta-li kingdom, to the Mosso of Li-chiang, and to the Lolo of the Chien-ch'ang Valley.

The value of Ĵang has been much discussed. I shall waste no time on T'u Chi's explanation of *chang* (= Ĵang) as a barbarian pronunciation of Ch. 人 *jên*, «man» (47, 4 b), or BLOCHET's statement that *jang* has lost in Mongolian its meaning of «primitive population» to retain only that of «nature», «character» (*Bl*, II, 375); *jang* is certainly the transcription of a foreign ethnical name. LAUFER was no doubt right when he recognized in it the Tib. 'Ĵaṅ or 'Ĵaṅs (dialectically also pronounced «Ĵung»?), used in Eastern Tibet as the name of the Mosso region of Li-chiang (*JRAS*, 1915, 781-784). QUATREMÈRE (*Hist. des Mongols*, xc) has already remarked that the name had apparently left some trace in Mongol history since the Kalmuk chronicle Bodi-mör, as partly translated in PALLAS, *Sammlungen historischer Nachrichten* (1776), I, 19, mentions the conquest of the «Jang» people by Qubilai in 1254. As a matter of fact, PALLAS used *i*- in its German value of *y*-, so that the form he really meant would be «Yang» for us. But since both *y*- and *j*-, as initials, are written with the same letter in Mongolian, PALLAS's transcription merely shows that his interpreter had no tradition to fall back on as to the true pronunciation of the name. The same may hold good for a passage which LAUFER quoted from «Sanang Setsen» (*SCHMIDT, Gesch. der Ost-Mongolen*, 239-240) and in which, for events of the second half of the 15th cent., a king (*qa'an*) Šidam of the people (*ulus*) of «Jang» is mentioned. LAUFER supposed that Šidam (written «Sidam») was not the real name of the king, but represented Sadam, the Mosso name of Li-chiang. Li-chiang is a purely Chinese name, which means «Li River» and, properly speaking, designates the Upper Yang-tzü or Chin-sha-chiang; it is another form of the more ancient 麗水 *Li-shui*, also «Li River» (cf. *BEFEO*, IV, 170). In the Mongol period it was adopted as the administrative name of the *lu* of Li-chiang, lit. «district of the Li River», later *fu* of Li-chiang, or, as an abbreviation, simply Li-chiang. The Mosso name Sa-t'am, Sadam, or Sdam may be the same as 三睽, as supposed by CHAVANNES who transcribed the latter form San-t'an, adding that the second character could be read both *shan* and *t'an* (*TP*, 1912, 602). But though this secondary pronunciation *t'an* (\**t'âm*) may be possible, yet it is not given in most dictionaries; for instance the *K'ang-hsi tzu-tien* reads *shan* (\**šjâm*). It is with this latter value that we find it used for instance by the pilgrim Fa-hsien to transcribe the second syllable of Kauśambī (*Fo kuo chi*, ed. Chi ku ko, 33 a). A reading *t'an* could only be due, in my opinion, to a misreading for 賧 *t'an* (\**t'âm*), on which cf. CHAVANNES, in *JA*, 1900, II, 384, and my note (of uncertain value however) in *BEFEO*, IV, 375; but *shan* recurs too often in ch. 61 of *YS* to allow of the hypothesis of such a confusion. Rather am I tempted to trace Sa-t'am back to the T'ang period, c. A. D. 800, and to recognize it as the first element of 三探覽 *San-t'an-lan* (\**Sâm-t'âm-lâm*; cf. *Man shu*, ed. Chien-hsi-ts'un-shê, 17 b, 28 a;

*Hsin T'ang shu*, 222 A, 4b; *YS*, 61, 4b; misread « San-shên-lan » by a slip of CHAVANNES in *TP*, 1912, 612). But although Sa-t'am may actually be an old name, there is still some doubt as to the identity of the name, Šidam, of the king with that of the city. The inclusion of the story relating to that king in « Sanang Setsen »'s Chronicle is also somewhat surprising, but the king's lavish use of gold is quite in agreement with the old reputation of the « River of Golden Sand », and I think that LAUFER's identification of SCHMIDT's people of « Jang » (= Yang), in such a case to be read as Ĵang, has a good chance of being correct. I must add that the name is much older in Tibetan than has hitherto been assumed. In the Tibetan chronicle of the late T'ang period, which I brought back from the Tun-huang Cave, mention is made of Tibetan campaigns against Ĵaŋ; the name certainly existed as early as the 8th cent.

But if I readily admit LAUFER's identification of Mong. Ĵang in Qara-Ĵang, Čayān-Ĵang, and Lolo-Ĵang with Ĵaŋ, 'Ĵaŋ, and 'Ĵaŋs of Tibetan texts, this does not give any definite clue either to the origin of the word Ĵang, or to its original ethnical meaning. In Mongol use, Qara-Ĵang is the designation of the Ta-li kingdom, the former 南詔 Nan-chao kingdom of T'ang times, which is generally supposed to have been Thai (or Tai); the Čayān-Ĵang were Mosso; and the Lolo-Ĵang, of course, Lolo; a motley crowd indeed. In 1904 (*BEFEO*, iv, 159), I had tentatively proposed to see in the Ĵang of the Mongol period the same name as that of the 蠻 Ts'uan of Chinese texts. I expressed myself as follows: « I feel inclined to see in Ĵang a Mongol transcription of the name of the Ts'uan, but to reserve in principle the name of Qara-Ĵang for the tribes of the ancient Nan-chao. The ancient Ts'uan, according to the Chinese, were divided into White and Black Barbarians [白蠻 Po-man and 烏蠻 Wu-man]; although the people of the ancient Nan-chao must not have originally belonged to the Ts'uan tribes, our sources state that they had for a long time intermarried with the Black Barbarians, and even that the Nan-chao were but a branch of the Black Barbarians. The Mongols may have applied to the Nan-chao the inaccurate designation that had become the rule. [In a note I explained that the designation was inaccurate because the real descendants of the Black Ts'uan must have been the Lolo tribes, known under the latter name in Yüan and probably already in T'ang times, who were themselves subdivided into numerous tribes, including White and Black Lolo.] The Qara-Ĵang, in the broad sense of the name, would thus be all the tribes which were included in the Nan-chao kingdom, those of Yün-nan-fu where the Nan-chao had their second capital as well as those of Ta-li where they always had their true metropolis, but the name referred more specifically to the Ta-li Valley, cradle of the Nan-chao. »

CHAVANNES dissented from me on the value of Qara-Ĵang when the term was taken in its restricted sense and not applied to the whole of the Ta-li kingdom. According to him (*TP*, 1904, 471), Qara-Ĵang, in its restricted sense, referred to the thirty-seven tribes which were called the Black Barbarians (Wu-Man), which occupied the eastern half of Yün-nan and were only in a state of relative dependence to the kings of Ta-li. CHAVANNES's arguments are not all of equal value. When he quotes Uriyangqadai's biography in which it is said that the Qara-Ĵang are the Black Barbarians (Wu-Man), this has no bearing on either side of the question, since the biography merely wants to define the Qara-Ĵang in relation to the Čayān-Ĵang, who it says are the White Barbarians (Po-Man); the opposition is between the people of the Ta-li kingdom and the Mosso, not between the respective populations of Ta-li and Yün-nan-fu. In the same way, when the

biography says that Uriyangqadai «reached the city of Yači which is the capital (or a capital) of the Black Barbarians», «Black Barbarians» (Wu-Man) is only the Chinese equivalent of a Mongol original Qara-Ĵang (the Mongol original is shown by the use of Yači), and the sentence simply alludes to the fact that Yün-nan-fu was the second capital of the Qara-Ĵang, *i. e.* the Ta-li kingdom. Nor can the mention of the thirty-seven tribes of the Black Barbarians carry any great weight. The many Chinese texts, ranging from the Six Dynasties to the Yüan period and even later, which concern the Black and White Barbarians have not been studied critically, but amidst their sometimes conflicting statements we clearly find Black Barbarians in western Yün-nan, nay in the very country of the Čayān-Ĵang, or «White Jang», of Li-chiang, from which, in the T'ang period, according to *YS*, 61, 4*a*, the Black Barbarians had ousted the Mo and So tribes; *i. e.* the Mosso. Without being positive as to its absolute value, I think that my theory of 1904 still holds good at the present day, and at any rate it is in apparent agreement with the use of Qara-Ĵang in Polo's text where «Caragian», taken in its restricted sense, is Ta-li. To prevent all misunderstanding, I wish to add that my explanation refers only to the use of Qara-Ĵang, and is not concerned with the location of the Ts'uan tribes.

My hypothetical derivation of «Ĵang» from «Ts'uan» is open to more objection. I had not noticed in 1904, and neither CHAVANNES nor LAUFER remarked on it later, that the phonetic correspondence is not very satisfactory. Ts'uan is an ancient Ts'uan, that is to say the word was never pronounced with the sonant initial of Tib. 'Jañ or Mong. Ĵang. I think, however, that there are fairly strong historical arguments to connect «Ts'uan» with «Ĵang».

The Nan-chao kingdom was actually founded in the first half of the 8th cent. by a petty chieftain whose inherited territory was at Mêng-hua, south-east of Ta-li and south-west of Yün-nan-fu (on the «six *chao*» of Nan-chao, cf. SUZUKI in *Tōyō gakuhō*, XIX [1931], 267-282). Before this, the main power in Yün-nan had been for some centuries in the hands of the Ts'uan, and there is now no doubt that the Ts'uan were Lolo tribes. We know from literary sources that the Ts'uan had a writing of their own as early at least as the Mongol period; two inscriptions in Ts'uan characters were discovered in 1909 at Lu-ch'üan, to the north-east of Yün-nan-fu; they date from the Ming period and are in Lolo writing and Lolo language (cf. *BEFEO*, IV, 155; v, 195-197; also 丁文江 TING Wên-chiang, 羣文叢刻 *Ts'uan wên ts'ung-k'o*, I, 1936, 5, and pls. 1-4).

Of the Ts'uan we possess much earlier epigraphical monuments in Chinese. Two funerary inscriptions of Ts'uan chieftains of the region of Yün-nan-fu, dated A. D. 405 and 458 respectively, have been translated by CHAVANNES (*JA*, 1909, II, 15-41), with a very valuable commentary. In his introduction CHAVANNES says that «Ts'uan is a general term designating the people of the ethnic group which we now call Lolo; the so-called Ts'uan family existed merely as the result of the mistake of the Chinese who made of the name of the nation that of the reigning family. . . The inscriptions of 405 and 458. . . give the first mentions of the name of the Ts'uan who make their appearance in official records only c. A. D. 550.» There is of course a great deal of truth in CHAVANNES's statement; I think, however, that it is not absolutely correct. The *Chan-kuo ts'ê*, written before our era, refers, in the section concerning the 魏 Wei State, to a man called 羣襄 Ts'uan Hsiang (cf. CHANG Chu's *Hsing-shih hsün-yüan*, 34, 4*a-b*). All later mentions of members the Ts'uan family refer to people of the region of Yün-nan-fu. But one of them appears in the

*Hua-yang kuo-chih*, that is, somewhat earlier than the inscription of 405. It may even be, although I have not made any further investigation on the subject, that a man of the surname of Ts'uan is named in the 3rd cent. (cf. *K'ang-hsi tzü-tien*, s. v. «Ts'uan»). CHAVANNES must be right when he says that the surname (except, however, in the *Chan-kuo ts'ê*) is derived from the name of the tribe, but this is not necessarily a mistake of the Chinese. Ts'uan noblemen had taken to Chinese ways, like those whose funerary inscriptions were studied by CHAVANNES, or another who became Chinese governor of Chiao-chou (Kuang-tung and Tonking). I think it was those men themselves who, having to adopt a Chinese surname, deliberately used as such the name of their tribe. It is quite another problem to decide why the Lolo people were first called Ts'uan by the Chinese. If the use of the surname Ts'uan among the Lolo had been more ancient than the use of Ts'uan as an ethnical designation, we might suppose that the character Ts'uan was chosen to render phonetically a native clan name, since it was already used as a surname in the *Chan-kuo ts'ê*. It seems more probable that Ts'uan became an ethnical designation of the Lolo among the Chinese before it was used by any Lolo as a surname. But even then, Ts'uan was probably a transcription of a native Lolo name, and the complicated character may have been chosen by a pedantic Chinese precisely because it occurred already in ancient Chinese literature if not as an ethnical name, at least as a surname. But, and we come back here to *Ĵang*, the Chinese rendering of the native name may have been only an approximation. If the Ch. Ts'uan on the one hand and the Tib. 'Jañ or the Mong. *Ĵang* on the other go back to a common native original which we do not know, neither of these transcriptions needs to be rigorously accurate. On the whole, and without pretending that the case is proved, I think that the leading part played by the Ts'uan Lolo in Yün-nan from the 4th to the 8th cent. makes it a likely assumption that their name was retained among their Tibetan cousins as the common designation of the various ethnical groups of the whole region, that is to say of the Nan-chao and their direct heirs and successors the sovereigns of the Ta-li kingdom (Qara-*Ĵang*), but also of the Mosso (Čayān-*Ĵang*) and even of the Lolo of southern Ssü-ch'uan (Lolo-*Ĵang*).

The use of a common name *Ĵang* for the Nan-chao (and Ta-li), the Mosso, and the Lolo is surprising, however, in view of the fact that the Nan-chao are always said to have been Thai. This is probably true, although the ethnic appurtenance of the Nan-chao has never been established beyond doubt. If they are admitted by general consent to have been Thai, it is because *chao* is said to render a native word meaning «prince», and the word actually means «chief» in all the Thai dialects (cf. *BEFEO*, iv, 163); and also because a word of doubtful reading meaning «district» is supposed to be identical with «the Siamese or Laotian word generally written *Xieng* or *Keng*» (CHAVANNES, in *JA*, 1900, II, 384). But, even granting the identity of the word meaning «district», it may have been borrowed by the Thai tribes. And as to *chao*, though this is less likely, it may have been borrowed from the Thai by the Nan-chao as it has been borrowed by the Burmese (cf. *BEFEO*, iv, 163-168). Without saying that the Nan-chao were not Thai, I have always wondered about some of their ethnical connections. The close intercourse of the Nan-chao with Tibet, which went to the extent of earning for the Nan-chao sovereign the Tibetan title of *blan-pho gčun*, «Younger Brother of the *blan-pho*» (cf. LAUFER, in *TP*, 1914, 83), may simply be the result of the brilliant but short-lived fortune of the Tibetan empire in the 8th cent.

But I have grouped in *BEFEO*, iv, 159-169, a few indications which point to a remarkable similarity of habits and of traditions between the Nan-chao and the Burmese. Further research tends to establish that some at least of these coincidences are no less striking between the Nan-chao and the Mosso. I had shown in 1904 that the curious custom of forming a man's name by taking as first element the last element of his father's name was common to Nan-chao and ancient Burma. But this was also the habit among the Mosso of Li-chiang and those of Wei-hsi (cf. CHAVANNES, in *TP*, 1912, 568). The northernmost of the six *chao* forming the Nan-chao was moreover avowedly peopled by Mosso. The Nan-chao may have been Thai, and this seems to be proved at least for their last royal family (cf. CHAVANNES, in *TP*, 1905, 15), but they must have mixed with members of the Tibeto-Burman branch of the Sino-Tibetan family like the Lolo and the Mosso, and the nearer they may be to the Tibeto-Burman branch, the easier it will be to understand that Nan-chao, Mosso, and Lolo should have been known under the common designation of Jang.

The name of 大理 Ta-li was that of the Nan-chao kingdom at the time of the Mongol conquest and has been retained in Chinese administrative nomenclature down to our days; but its origin is still obscure. It has, in its ordinary form, a good Chinese appearance, but this form is not the oldest. Ta-li was founded at the end of the 8th cent. under the name of 羊苴咩 Yang-hsieh-mieh, clearly the transcription of a Nan-chao name (cf. *BEFEO*, iv, 370, 374, 1103), but a walled city (*ch'êng*) of 大釐 Ta-li had previously been founded, at the beginning of the same century, 40 li north of the present Ta-li (cf. *Man shu*, 23 a, 24 a; *BEFEO*, iv, 1103; SUZUKI, *loc. cit.* 273, 274). About A. D. 860, the Nan-chao sovereign adopted for his kingdom the official name of « Kingdom of 大禮 Ta-li » (*Hsin T'ang shu*, 222 B, 1 b). Although that designation does not seem to have been retained by his immediate successors, there can be no doubt that it inspired the name of « Kingdom of Ta-li », with the modern spelling, which was adopted by the king of the Tuan family in 938. Despite the fact that the first city 大釐 Ta-li of the early 8th cent. was not on the site of the modern 大理 Ta-li, the ancient Yang-hsieh-mieh, I hold it probable that the name of that first city is identical with the two later dynastic designations Ta-li, and that the three Chinese forms are all transcriptions of one and the same original (in which case, *ta* would not be the Chinese epithet « great », regularly prefixed before dynastic designations). It is more difficult to decide about the native meaning of Ta-li (\*D'âi-lji or \*T'âi-lji for the name of the city of the 8th cent. and for the dynastic name of 938; \*D'âi-liei for the dynastic name of c. 860). F. W. K. MÜLLER (*TP*, III, 19) suggested « the Tai (or Thai) of Li », Li being a place-name which should also be recognized in the Chinese name of the Lake of Ta-li, 洱海 Êrh-hai, « Lake Êrh » (\*N'zi). And it is true that, in the Pai-i Vocabulary of the Ming dynasty, the Lake of Ta-li is called in Pai-i « Li-kai », where *kai* transcribes a form borrowed from Ch. *hai*. But the latter fact raises precisely the suspicion that the whole form « Li-kai » may not be due to a partly independent native tradition, but merely renders the Chinese name Êrh-hai. There are many similar cases. The same Vocabulary gives 猛國 Mêng-kuo, in Pa-i writing Mîng-kwē, as the Pai-i name of Ta-li, and in the Pa-pai Vocabulary, Mêng-kuo renders *kuo* alone. *Mêng* is the Thai *mîng*, *miang* (*muang*), « city », « place », and Mîng-kwē is merely a Thai-Chinese hybrid, « The City of the Kingdom », this being a reminiscence of the part played so long by Ta-li as the capital of the Nan-chao and later of

the Ta-li kingdom. On the whole, MÜLLER's hypothesis finds, up to the present at least, no real support in the historical texts or in the vocabularies.

Rašidu-'d-Dīn speaks of the province of Qara-Ĵang which is called in Chinese 大呂 Dai-liu, meaning «Great Realm»; the Indians and Cashmirians call it كندړ Kändār, which has the same meaning, and the Mussulmans قندهار Qandahār (cf. QUATREMÈRE, *Hist. des Mongols*, lxxxix, xciv; KLAPROTH, in *JA*, I [1828], 112-116; *Oh*, II, 317; *Y*, II, 72; *Y*<sup>1</sup>, III, 127). QUATREMÈRE and YULE felt that «Dai-liu» must have some connection with Ta-li. BLOCHET (*Bl*, II, 376), on the faith of Rašid's explanation «Great Kingdom», corrected Dai-liu to 大呂 Dai-kiu, which he interpreted as 大國 Ta-kuo, «Great Kingdom». I do not favour this solution. In Rašid's nomenclature the Chinese *kuo*, «kingdom», occurs as *-gu* in Ĵimingu, «Japan», and as *-guh* (? or *guäh*) in Kāfĵiguh (or *Kāfĵiguäh*), Chiao-chih-kuo, Tonking, so that, even with BLOCHET's correction, the *-i* of *kiu* in the would-be Dai-kiu is not accounted for. Moreover, Ta-kuo cannot be the specific name of any country in Chinese. I rather incline to see in Dai-liu a weakened pronunciation of \*Dai-li-gu = Ta-li-kuo, «Kingdom of Ta-li». Rašid's explanation is true only so far as the *ta* of Ta-li means «great» in Chinese.

The «Indian» (*hindī*) name of Qara-Ĵang is written «Kandar» and «Qandar» by Rašid. It is certainly wrong to correct those forms to «Kandū» and «Qandū», as was done by BLOCHET (*Bl*, II, 365, 376). BLOCHET saw in «Kandū» Polo's «Gaidu» and, through an impossible jumble of Thai and Burmese forms, tried to explain «Gaidu» as also meaning «Great Kingdom». But the latter meaning, given in QUATREMÈRE's translation, does not occur in BLOCHET's own text (*Bl*, II, 376, n. a) and seems to be a wrong reduplication of the gloss on Dai-liu. What Rašid really refers to is Kandar (or Gandar), the Indian form regularly derived from Gandhāra, and this is confirmed by what follows when he says that «we» (*i. e.* the Moslems) call it Qandahār. The name Qandahār has three main meanings (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 154; HALLBERG, 109; *Mi*, 502) : (1) the port of Ghandhār in the gulf of Cambay; (2) Gandhāra, the well-known region of the upper Indus; (3) Candahar in western Afghanistan (in Fra Mauro's map, two at least of these «Candar» or «Chandaar» appear, one being «Chandaar mazor» and the other «Candar menor»; but owing to repetitions by a later [?] hand there are in fact five mentions of the name). A fourth must be added, which Rašid occasionally mixed up with the real Gandhāra, and that is the Ta-li kingdom (cf. YULE, in *JRAS*, NS, IV [1870], 354-356). In *BEFEO*, IV, 157-169, I have shown that not only the name of Gandhāra, but also many other names and legends had been carried from India to the Ta-li kingdom in the early Middle Ages and found pious, though fictitious, identifications in that region of south-western China. The Ta-li kingdom was a Buddhist kingdom, but it owed its Buddhism as much to direct propaganda from India and Burma as to the influence of Chinese Buddhism.

Under such circumstance, it is not surprising that the king of Ta-li should have borne an Indian title. Curiously enough, we do not find it, at least beyond question, in the documents referring to the history of the ancient Nan-chao kingdom, nor to that of the Ta-li kingdom before the Mongol conquest of 1253-1254. (For a possible mention under the rule of the Mêng family, cf. *BEFEO*, IV, 164; *TP*, 1904, 470; CHAVANNES's objections are weakened by the fact that the complete form *mo-ho-lo-ts'o*, and not only *mo-ho-ts'o*, is given in the 1880 edition of

the *Nan-chao yeh-shih*, 2, 41; cf. also SAINSON, *Hist. particulière du Nan-tchao*, 198.) But Rašidu'd-Din says that «the king of Qara-Ĵang bears the title of *mah-ara*, which means 'great prince'» (QUATREMÈRE, *Hist. des Mongols*, XCIV); D'OHSSON (*Oh*, II, 318) gives «Maha Radja» (*i. e. mahārāja*); BLOCHET (*Bl*, 378) corrects to *ماہارا*, *mahāraū* the readings *ماہارا* and *ماہارا* of his mss. Without attempting to establish here what form Rašid actually used, it is clear that the transcriptions represent *mahārāja* or a form derived from *mahārāja* (perhaps *ماہاراز*, *mahārāz*). This title is confirmed by Chinese sources. The campaign of 1253-1257 in Yün-nan was very trying owing to the climate and to the resistance of the population; out of ten *tümän* of troops (100,000 men) which Uriyangqadai had under his command in Yün-nan, only 20,000 men survived (*Oh*, II, 318; *Ber*, 145-146). As early as 1254, Qubilai, perhaps fretting at his subordinate position under Uriyangqadai (if Rašid be right in that respect; the *YS* gives the supreme command to Qubilai), had come back to the Court in Mongolia. According to Rašid, Qubilai brought back with him the *mahārāja*. Chinese texts give a somewhat different account. At the time of the campaign, the power belonged in Ta-li to the Kao clan, under the purely nominal sovereignty of the Tuan family. The Kao were defeated, and mercy was shown to the Tuan; at that time, Qubilai had already left Yün-nan. Uriyangqadai's biography, a not always successful combination of two documents of different origin (see «Iaci»), says that, after Yün-nan-fu had fallen in 1254, Uriyangqadai «captured the king 段智興 Tuan Chih-hsing and the commander-in-chief 馬合剌昔 Ma-ha-la-hsi and offered them [to the Emperor]» (*YS*, 121, 3 a; Tuan Chih-hsing is the form given in this passage and it is retained in *Ch*, II, 221; T'U Chi, 29, 15 b, silently alters it to Tuan Hsing-chih, the form used in the biography of that ruler's son, *YS*, 166, 8 b; CHAVANNES, in *TP*, 1905, 15-16, gives Tuan Hsing-chih without comment; an error in the name as given in the biography of Uriyangqadai would be more probable than in a biography of the Tuan family; but, curiously enough, the form Tuan Chih-hsing is also the one we find in the *Nan-chao yeh-shih* [transl. SAINSON, 104], not always reliable, but generally based on independent tradition). Ma-ha-la-hsi may or may not be corrupt (? for Ma-ha-la-[𠵹𠵹] *cha*; more probably, *hsi* [sī] merely transcribes -s or -z, in agreement with Rašid's *mahārāz*), but certainly it renders *mahārāja*, wrongly taken as a personal name, and still more wrongly given as the name of another man than the actual bearer of the title, *i. e.* the king Tuan Chih-hsing himself. In 1256, Tuan Chih-hsing was received by Mongka who gave him the title of 摩合羅嗟 *mo-ha-lo-ts'o* (*YS*, 3, 3 b), or 摩訶羅嗟 *mo-ho-lo-ts'o* (*YS*, 166, 9 a; cf. CHAVANNES, in *TP*, 1905, 16); we must understand that Tuan Chih-hsing was then confirmed in his pre-existing title. A similar transcription *mo-ho-lo-[𠵹𠵹]ts'o* occurs in an inscription of 1325, also in connection with Tuan Chih-hsing (*TP*, 1905, 28). Here again I cannot examine the minor discrepancies existing between the various sections of the *YS*, including the geographical section (ch. 61), as to the date at which and the conditions under which Tuan Chih-hsing was entrusted as *tsung-kuan*, or «administrator general», with fresh authority over his former subjects; some pertinent remarks have been made by CHAVANNES (*TP*, 1905, 16) and T'U Chi (6, 9 a-b; 29, 15 b; 110, 2 b). But I wish to point out that, while the transcription of *mahārāja* in Uriyangqadai's biography is what we should expect in the Mongol period (except perhaps for the last character), the *mo-ho-lo-ts'o* occurring elsewhere, also adopted in *Yüan shih lei-pien*, 42, 58 b, and in *Nan-chao yeh-shih*

(transl. SAINSON, 110, 198), is of a more ancient type and points to a Sung date. *Mo-ho-lo-ts'o* probably was the traditional transcription of the title in Chinese from the 10th or the 11th cent., but Uriyangqadai's biography, here as in its other portions going back to a Mongol original, gives the Mongol form, which was of course with *-a-* vowels (*mahārāja*; or *mahārāč*, *mahārāj* as in Uighur).

This title, established by Persian and Chinese sources, also seems unexpectedly to occur in the West. In Fra Mauro's map, ZURLA noticed the mention of the «Çardandam» (see «Çardandan») and of «Uocā» (see «Uncian»), but omitted those of «Iāzu» and of «Maharaç», also lacking as a consequence in HALLBERG. «Iāzu» certainly is the «Iaci» of other texts (cf. «Iazo» in V; «Iāzu» is not «Ianzu» Yang-chou, correctly given elsewhere on the map), and «Maharaç», though entered on the map far enough from «Charazan» and «Charaian», can hardly be anything else than the title *mahārāja* of the Ta-li sovereign; Fra Mauro mentions it in a form which is in strict agreement with Rašid's \**mahārāz* and the \**mahārās*, \**mahārāz* (< *mahārāj*) which the transcription used in Uriyangqadai's biography made us suppose. Such must have been the form taken by the title in Mongolian and Persian speech, and consequently it is the one we should expect to find in Polo. But none of our Polo mss. mentions the *mahārāja* of the Qara-Ĵang.

The solution of the difficulty may be sought for in two directions. A number of names on Fra Mauro's map have convinced me that Fra Mauro, a century before RAMUSIO, had knowledge of the complete ms. to which we owe so many paragraphs and even chapters in R and Z, but which is avowedly abbreviated in Z almost until we come to the point where the travellers embark on the homeward journey. There may have been, in the complete ms., a paragraph on the «maharaç» of the Qara-Ĵang. In such a case, the paragraph was omitted by both R and Z, and we could not even suspect its former existence if it were not for the «maharaç» entered on his map by Fra Mauro.

This is the first hypothesis which occurred to me, and I do not reject it altogether; but it has its weak points, particularly when what must be a title is actually marked on the map as the name of a city. Moreover, another explanation is possible. Apart from Polo, Mauro's main modern source for Transgangetic India is Nicolò de' Conti, who travelled in the second quarter of the 15th cent. All our texts of Conti go back to the narrative taken down from Conti's lips and put into Latin, probably in 1439, by Poggio Bracciolini. But Conti, a man of Chioggia, died only in 1469; in the meantime, he went several times to Venice, for instance in 1454 and 1455. The forms of the names which, on Fra Mauro's map, are plainly traceable to Conti are spelt in such a way as to show that Fra Mauro is clearly not dependent on Poggio's text for this nomenclature and must have had direct intercourse with Conti. Poggio's humanism has been more detrimental to Conti's account than Rustichello's taste for romance was to Polo's. The names have particularly suffered in Poggio's text; for instance, the Latin account speaks of «Pauconia» (or «Panconia»), whereas Fra Mauro gives on his map the correct form «Paigu», *i. e.* Malay. Paigū, our Pegu (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 693; LONGHENA, *Viaggi. . . de Nicolò de' Conti*, 147). Unfortunately there is no satisfactory critical edition of Conti's account, and none in particular which studies Fra Mauro's names systematically. Now, there is in Conti a



«great city» called «Maarazia» (*var.* «Maraazia», «Maaratia», «Marahatia»; «Maragama» on the Genoese map; LONGHENA, 137; HALLBERG, 323) for which I find no corresponding name on Fra Mauro's map, except perhaps «Maharaç». A theory may be launched, which would see Sukhothai, the capital of Siam, supplanted in 1350 by Ayuthia, in Mauro's «Sciechutai» (left unidentified by HALLBERG, 463), and Šahr-i-nau («New Town»), *i. e.* Ayuthia, in his «Scierno». This «Scierno», also left unidentified by HALLBERG (p. 463), would be due to Conti and represent another spelling of the «Cernove» adopted by Poggio; YULE has already thought that Conti's «Cernove» might be Ayuthia, and not a city of Bengal, as formerly believed by scholars, including himself (*Hobson-Jobson*<sup>2</sup>, 796; to YULE's quotations of Šahr-i-nau = Ayuthia, add the text of Šadiq Isfahāni [† c. 1680] in *Fe*, 560, where the Persian author understands, and probably misunderstands, the name as Šahr-i-nāu, the «City of the Boat»; cf. also *Fe*, 730). In such a case, the «Ganges» of «Cernove» would be the Menam, and, as a fresh example of what YULE called the «interdivergence of rivers» in mediaeval geography, it would be only a lower part of the Mekong, the latter actually flowing through the country of the *mahārāja* of the Qara-Ĵang. A form «maharaç» = \*mahārāj could be reconciled with a Siamese pronunciation of the word.

That such a theory should discard all previous comments on this part of Conti's journey as complete failures would not be a serious objection, as none of them could pretend even to a semblance of the truth. Conti had travelled in Indo-China. He was the first to mention «Ava» in Burma, and many other Indo-Chinese names on Fra Mauro's map are certainly traceable to him: for instance, such is the case, not far to the west of «Ava», for «Capelang», which HALLBERG (p. 111) merely mentions, but which is accompanied by a note «qui nasie rubini», and so clearly ought to be inserted in *Hobson-Jobson*<sup>2</sup>, 159, at the beginning of the quotations on «Capelan», the unidentified mountain of Burma where the rubies purchased at Pegu were said to be mined. Unfortunately Conti's vague geographical notions have become almost unintelligible in passing through Poggio's «editing». Conti speaks of China from hearsay, and also probably of Yün-nan, if his «Maarazia» is to be located there. Granting all that, there are here, however, serious objections. One, which is as true for Conti as it would be for Polo, is that he gives as the name of a city what was the title of a prince. But what is more serious is that the title of *mahārāja*, which really existed in Polo's time, must have long been extinct in Yün-nan when Conti was in Indo-China a century and a half later; even supposing that the twelve *tsung-kuan* of the Tuan family had retained the title, the last of them was captured and beheaded by the Ming in 1382. To account for any later use of the title, we ought to assume, in the geographical nomenclature of the Siamese or, less likely here, of the Burmese, a survival analogous to that which makes modern Burmese texts speak of the Chinese Emperor as *udi-bhva* (< *udaya-bhva* = 東帝 *tung-ti*), «Emperor of the East», or as Gandhālarāj (pronounced Gandhālayit), «King of Gandhāra», both being ancient designations of the early Nan-chao sovereigns of Yün-nan (cf. YULE, in *JRAS*, NS, IV [1870], 356; *BEFEO*, IV, 158, 162-164). But one does not see that «Maharaç» could have been a popular designation of Yün-nan in Ming times, nor does it occur then in any of the vocabularies, either Pai-i, or Pa-pai, or Siamese, or Burmese.

Yet it would be too great a coincidence if an unidentified town of «Maharaç», perhaps to be looked for, as is usually done, somewhere in India, had happened by mere chance to be

located by Fra Mauro on his map just north of the «Çardandam», that is, with the spelling and on the spot we should expect if Polo had spoken of the *mahārāja* of Ta-li. Therefore, while I readily admit that Conti's «Maarazia» has a fair chance of being the basis of Fra Mauro's «Maharaç», I do not preclude the other possibility, to wit, that the latter name might be of Polian origin.

While in the country of the Qara-Ĵang, Polo had heard of Bengal. This is only too natural, in view of the intercourse which, since the beginning of our era, was conducted across Upper Burma between north-eastern India and Yün-nan (cf. *BEFEO*, iv, 142-152, 183, 412). Rašidu-'d-Din describes both the sea-route from India to China and the land-route *via* Bengal and Burma. The itinerary by land is very corrupt. YULE had studied it in the first edition of *Cathay* with the translation given in the first edition of ELLIOT's *History of India*; but, when ELLIOT's first volume was revised and re-edited by DOWSON, YULE published in *JRAS*, NS, iv [1870], 340-356, a fresh commentary, much more accurate. It is a pity that this paper of 1870 should have been overlooked by CORDIER when preparing the second edition of *Cathay*. The result is that in *Y<sup>1</sup>*, III, 131-132, we still see the itinerary from India to Yün-nan reach first the Uman, that is, Wu-man or «Black Barbarians», afterwards the Zardandān (and finally the Qara-Ĵang, omitted from the quotation in *Cathay*). The «Wu-man», however, are the Qara-Ĵang themselves, and could only be mentioned after the Zardandān (moreover, the Chinese name «Wu-man» does not seem to have passed abroad). But in his paper of 1870, YULE here read with DOWSON ارمن Arman, not اومن Uman, and was probably right when he saw in Arman a transcription of the native name of Burma (see «Mien»). A comprehensive monograph on all the texts referring to the ancient trade route between India and Yün-nan is badly wanted.

## 120. CARAMANI

*caramani* Z; R

Only Z and R give the name, as «Turcomani qui vocantur Caramani» (Z), and «i Turcho-mani, che si chiamano Caramani» (R). Although he knew R, YULE (*Y*, I, 43) has tacitly left out the name of the «Caramani». *RR*, 20, and *B<sup>1</sup>*, 19, have translated «that part of Turcomania called Caramania»; but the text concerns the name of a people, not the name of the country. In *RR*, 416, it is added that «Caramania» is mentioned only in Z (our *Z<sup>1</sup>*), and that the other texts have Konieh instead. Nor is this correct; instead of the name of Konieh (Quniyäh), Z does not have «Caramania» but what seems to be a clerical corruption «Turchia» (see «Como»). As a matter of fact, there are two redactions of that passage, one being that of F, etc., the other that of Z, V, and R. RAMUSIO was wrong to include both, and I think there was no more reason, in translations like those of *RR* and *B<sup>1</sup>*, which do not give the different redactions of one and the same passage, to follow here RAMUSIO's example and reproduce twice the list «Caesaria, Sevasta», with an arbitrary interval of some twenty lines.

RR, 416, substituting «Caramania» for «Caramani», says it is the district of Qaramān in Asia Minor, and there is of course an identity of name. In modern geography, that part of Anatolia is usually called Caramania (in French, Caramanie). The name goes back to the Türkmän prince Qaramān or Qarāmān, who, after the fall of the Seljukids, established there a Qaramān dynasty c. A. D. 1223; its first centre was at Lāranda, north of Armanak, and it was removed to Konieh about A. D. 1330. Abū-'l-Fidā speaks of the «Qaramān mountains which are inhabited by the Türkmän tribes now ruled over by Qaramān's descendants», and elsewhere mentions in the same region «The Türkmän of the son of Qaramān (Ibn-Qaramān)» (REINAUD, *Géogr. d'Aboulféda*, II, I, 35; II, II, 133).

Now Z and R's «Caramani» can be understood in two different ways, either as a Latin plural of «Caramanus», when we should adopt «Caramans», just as we have in FA «Turquemans» instead of the Latin «Turcomani»; or «Caramani» may be conceived as an ethnical name \*Qaramānī derived from Qaramān, in which case it should be retained. I would incline towards the second solution (as Polo does not speak, apparently, of the reigning family only, but of the whole of the tribe), if I could find \*Qaramānī used ethnically elsewhere. But I find only Qaramān (cf. LS, 144, 145, 148), applied to the whole «emirate» and even to its capital. The epithet Qaramānī of men like Qaramānī Meḥmed Pasha † 1481, so called because he was born in Caramania, can only be cited as a parallel with some qualification. In Osmanli Turkish, the ethnical name derived from Qaramān is naturally Qaramānli.

On Caramania, cf. EI, s. v. «Qaramān-ili» and «Qaramān-Oghlū», by J. H. KRAMERS; also BLOCHET, *Moufazzal*, 459, 738.

## 121. CARAMORAN

<i>cakomoya</i> , <i>caramora</i> G	<i>caramota</i> , <i>carcamoran</i> , <i>catha-</i>	<i>charamoira</i> , <i>charamoiram</i> , <i>cha-</i>
<i>caracoron</i> Fr, t	<i>metam</i> LT	<i>ramoram</i> VA
<i>caramoram</i> FA, FBt	<i>caromoram</i> P	<i>charamoran</i> , <i>charchoron</i> , <i>gran-</i>
<i>caramoran</i> F, FB, VB, Z, L,	<i>coromoran</i> P <sup>5</sup> , S	<i>moian</i> V
R	<i>carramorán</i> Z	<i>charomoran</i> , <i>choromoran</i> VL
<i>caramoron</i> L	<i>charameran</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>chaumera</i> TA <sup>3</sup>

Qara-mörän, «Black River», is well known as an ancient Mongol name of the Huang-ho or Yellow River of the Chinese. It is used by Odoric (*Wγ*, 470, «Caramoram», a copyist's misreading of «Caramorā» = Caramoran), by Marignolli (*ibid.* 533, «Caramora», also altered from «Caramorā» = Caramoran), and often by Rašidu-'d-Dīn (ex. gr. *Bl*, II, 181, 488 : قراموران Qara-mörän; also in his unpublished *History of China*). The three Western travellers use *mörän*, which must be the correct pronunciation, but *mürän* is also met with, although only later (the Chinese transcriptions, however, are always based on *mürän*). For Polo and Odoric, as well as for Rašid, Qara-mörän is the name of the river along the whole of its course, but later it

seems to have acquired a more restricted sense. In 1420-1422, Šāh-Rūḥ's envoys, who crossed the Huang-ho at least twice each way, name the Qara-mörän, written as with Rašīd, only when they cross it at Lan-chou (Y<sup>1</sup>, I, 278, 286). And a Sino-Turkish itinerary of the Ming dynasty (the Turkish being written phonetically with Chinese characters) from the collection of vocabularies of the former Morrison Library (now in the School of Oriental Studies in London) gives 曷刺木連 Ko-la-mu-lien, Qara-mürän, as the equivalent of 藍州 Lan-chou (read 蘭州 Lan-chou; the error shows that the writer no longer distinguished between the finals *-m* and *-n*, both still existing in Northern China at the beginning of the Ming dynasty). Further on, for 河南 Ho-nan, the same itinerary gives a 克兒帖木連轟 K'o-eul-t'ie-mu-lien-nieh which must be \*Kärtä-mürän[-ä], unknown to me, but showing that some at least of the Mongol names of places in Northern China remained in use among the Persians and the Turks long after the collapse of the Mongol dynasty.

Qara-mörän was still known in 1662 to «Sanang Setsen» (or Sayang Säcän) as an old name of the Yellow River (SCHMIDT, *Gesch. der Ost-Mongolen*, 103), but the modern Mongol name is Ḥatun-mürän, or Ḥatun-γöl, or sometimes Ḥatun-äkä, meaning «the River of the Princess» (cf. SCHMIDT, *ibid.*, 103, 360, 388; KOVALEVSKIĪ, *Dict. mong.-russe-français*, 781).

Qara-mürän (Ha-la-mu-lien) appears as the name of a place in YS, 100, 1b, and is altered to \*Tara-mürän in YS, 100, 2a (if it be not the reverse, though this is improbable; cf. also *Br*, I, 185; *Ch*, II, 173; and see «Caccia modun»). YANAI gives on his map the Ha-la-mu-lien of Chinese texts of the Yüan period, not as the equivalent of the Ḥara-mürän (< Qara-mörän) = Huang-ho, but as the modern Ḥara-mürän of Eastern Mongolia, a small northern tributary of the Šara-mürän (cf. also POPOV, *Mên-gu-yu-mu-czi*, 256, 260, 269); but, from his text (p. 618), it appears that his identification applies strictly only to the Ha-la-ho («Qara River» = Qara-mürän) of YS, 154, 3b, which is not necessarily the same as the Qara-mürän of YS, 100, 1b.

To-day, Ḥara-mürän (with the corresponding form 黑河 Hei-ho in Chinese) is also the name of a tributary of the Huang-ho (cf. POPOV, *ibid.*, 301).

## 122. CARAUNAS.

*camouas* P  
*carans, carcionas* FB  
*caraonas* F, FA  
*caraoor* VB

*caraunas* F, L; R  
*charaoni* VL  
*charaunas* V  
*charoonas* VA

*ischerani* TA<sup>1</sup>  
*scarani* LT  
*scharani* TA<sup>3</sup>

The problem of the «Caraunas» is one of the most difficult with which a commentator of Marco Polo is confronted, partly on account of contradictions in the Oriental sources, and partly also owing to the slipshod manner in which it has been approached by HAMMER, HOWORTH and others. The best contributions on the subject are due to QUATREMÈRE, *Not. et Extr.* XIV, 282-284, and YULE (Y, I, 100-106, supplemented in III, 21-24), but they still leave much to be

desired. Unfortunately, too many texts remain unpublished, or poorly published, or not indexed, or untranslated. While I hope to clear the way by disposing of a number of wrong forms and mis-statements, the solutions I shall propose have themselves, on some essential points, only a provisional character.

One thing is certain. Polo's «Caraunas» is an absolutely correct transcription of قراواناس Qaraunas, the very form most frequently used by Persian historians of the Mongol period for a particular group of troops living then in Persia. The late spellings قراواناس Qarāunas (or \*Qarōnas < Qaraunas) of the *Zafar-nāmā* (cf. QUATREMÈRE, *loc. cit.*, 282) and قراواناس Qarāwānās of the *Ta'riḥ-i Rašīdī* of 1547 (transl. ELIAS and ROSS, *A History of the Moghuls*, 148) have no primary authority. The name occurs also once in Armenian chronicles, under the form «Karavunas» (cf. PATKANOV, *Istoriya Mongolov*, I, 57, 95; BROSSET, *Hist. de la Siounie*, 260).

The first to tackle the question of the Qaraunas with the use of Persian texts is HAMMER who mentioned the «Karawinas or Karawunas» in the *Jahrbücher der Literatur* of January-March 1837 (t. 77, 8), with a note the substance of which is that according to Waśśāf (so in HAMMER's note; I do not know why PAUTHIER, *Pa*, 78, says Rašīdu-'d-Dīn), the «Karawinas» were the «artificers» (Feuerwerker) of the Mongol army, and that, «in all likelihood», they had given their name, at the time of the Mongol invasions in Europe, to the «carbineers» (Karabinire). In all his later works, HAMMER used «Karawinas» to the exclusion of «Karawunas», of course to suit his etymology of «carbineers»; the «Karawinas» had become in the meantime «throwers of naphtha fire» (cf. *Ha*<sup>3</sup>, 214; *Ha*<sup>1</sup>, I, 17, 270, 309; *Ha*<sup>2</sup>, 223, 260). HAMMER's etymology of «carbineer», repeated by ERDMANN (*Temudschin*, 183) and HOWORTH (III, 389), has been admitted as possible by YULE (*Y*, I, 101). CORDIER ought to have suppressed it. Qaraunas was never pronounced «Qarawinas». As to «carbineer» and «carbine», the words do not go back, even in the forms «carabineer» and «carabine», farther than the 16th cent., and certainly are not derived from an Oriental word. Above all, neither QUATREMÈRE nor D'OHSSON has alluded to any text mentioning the Qaraunas as «artificers» or «throwers of naphtha fire». I strongly suspect that the whole story was suggested to HAMMER by his etymology of «carbineers».

Apart from Qaraunas, the name appears often in Persian texts of the Mongol period with another form, قراونه, which QUATREMÈRE transcribed «Karaveneh». With all his immense erudition in Semitic languages and in Persian, QUATREMÈRE knew no Mongolian. It is evident that this second form must be transcribed Qarauna, and represents a singular, the plural of which is regularly Qaraunas in Mongolian.

Many attempts have been made to explain «Qaraunas».

MARSDEN's Skr. *karāṇa* (and Hindi *karāṇī*), BIRD's «KOPANO» on Indo-Scythian coins (adopted in *Pa*, 78-79, where a whole theory is based upon it, and retained in *Ch*, I, 61), have been justly discarded by YULE as etymologies of Qaraunas (*Y*, I, 101; YULE's note in *Hobson-Jobson*<sup>2</sup>, s. v. «cranny», is to say the least ambiguous); but CORDIER ought to have suppressed «KOPANO» itself; it has been a well-known fact for more than forty years that the third letter is not a Greek P, but a special letter representing a sort of š, and that the word is simply the equivalent of Kuṣaṇa,

QUATREMÈRE thought of a connection of «Qaravnas», as he read the name, with the Qonyrat tribe of the Qaranut, mentioned by Rašid, the name of which, once introduced into Persia, would have changed its Mongol form Qaranut into «Qaravnas». Moreover, the name (*i. e.* both Qaranut and «Qaravnas»?) could, according to QUATREMÈRE, be derived from what may have been the place of origin of the tribe, «the town called Karaoun-khidoun (the black city), now Karahotun, at a short distance from the Great Wall of China». Confusions were excusable when QUATREMÈRE wrote in 1843, but one cannot read without amazement BLOCHET's long note of 1911 (*Bl*, II, App. 43-45; cf. also II, 367-368) on «Karakhon khidon» being the «Manchu equivalent» of «Mongolian Kara Khitat», a monument of pretentious nonsense. «Karaoun-khidoun» never existed; it is a misreading, due to a misplaced dot in Persian texts, for Qaraun-jidun, which form occurs in the *Secret History* (§§ 183, 206; there are practically no *h* or *ḥ* in the Persian transcriptions of purely Mongol forms; as a rule, they are misreadings for *č* or *j*). The true form was adopted by D'OHSSON (I, 67, «Caraoun Tchidoun»), and passed from him to HAMMER, who, independently of QUATREMÈRE, and on the sole phonetic analogy, sought in Qaraun-jidun the cradle of the Qaraunas (*Ha*<sup>1</sup>, I, 17, 309). HOWORTH (III, 388) reverts to the Qaranut to say that if we take off the *-t* of the Mongol plural ending, «we have a name singularly like that of the Qaraunas». And as the Qaranut were a Qonyrat tribe, HOWORTH calls attention to the fact that in Persian a Qonyrat is mentioned as an *emir* of the Qaraunas. The theory is accepted as a fact by YULE (*Y*, I, 101) : «They [*the Qaraunas*] are described as having had their original seats on the mountains north of the Chinese wall near *Qaraún Jidun* or *Khidun*»; moreover YULE considers as possible a connection between the Qaranut and the Qaraunas. Western scholars have not been aware of BEREZIN's attempt (*Ber*, I, 241) at deriving «Qaraun» or «Qaraut» (!; BEREZIN always misread the name without the final *-a*; cf. *Ber*, III, 237<sup>2</sup>) either from Mong. *qarau*, *qara'u*, «mutual defence», which would mean that the Qaraunas would be a confederation of allies, or from *qarayuna*, name of a tree, from which a mountain mentioned in «Sanang Setsen» derives its name; the Qaraunas may have lived originally near that mountain, but BEREZIN thinks his first solution preferable.

I shall deal first with the Qaranut. Such a form cannot be the plural of a \*Qaran (which would be \*Qarat) or of a \*Qaranu (it would be \*Qaranus); Qaranut can only be the plural of a \*Qaranun, \*Qaranur, or \*Qaranul; all this leaves us far enough from Qarauna, pl. Qaraunas. Moreover, the Qaranut are known only through Rašidu-'d-Din, and although this is the form adopted by QUATREMÈRE and ERDMANN (*Vollständ. Uebersicht*, 86, 92; *Temudschin*, 168) as well as by BEREZIN (*Ber*, I, 148, 153, 154), there are many different readings in the mss.; the four used by BEREZIN have more than once Qaratut, all of them in one case (*Ber*, I, 153; Pers. text, 204); so we may have to do with a Qaratut, pl. of \*Qaratun. Even retaining Qaranut, there can be no phonetic connection with Qaraunas. That a Qonyrat should have been an *emir* of the force of the Qaraunas does not mean, contrary to HOWORTH's view, that the Qaraunas were a branch of the Qonyrat. Rašidu-'d-Din, in his notices of the tribes, names one *emir*, two chiliarchs, and one myriarch of the Qaraunas (cf. *Ber*, I, 63, 143, 151, 174; p. 143, read «Qarauna Čuban», instead of «Qarauba-Čuban», and cf. III, 138; I have admitted that this «Qarauna Čuban», a chiliarch, was a chiliarch of the Qaraunas, but this is not certain), and the *emir* is a Qonyrat

but the two chiliarchs and the myriarch are a Tatar, an Uryangqit, and an « Ildürgin ». Moreover the Qonyrat were renowned for their beauty among the Mongols, and we shall see farther on that the Qaraunas are described in terms which imply just the opposite of beauty.

« Sanang Setsen » names a mountain Qaryuna-qan (not Qarayuna-qan as BEREZIN says; Qaryuna-qan of SCHMIDT's translation is confirmed by the Chinese version, although SCHMIDT gives in the Mongol text « Qaryuna nutuq », the « Camp of Qaryuna », *Gesch. der Ost-Mongolen*, 108-109). But the single mention of that mountain in the epic lament on Chinghiz-khan's death can give no indication as to an original seat of the Qaraunas. It may be that Qaryuna is but another form of *qarayuna*, the name of a tree, just as there is a secondary form *qaryana* (> Russ. *khargana*) for the tree *qarayana*; but if a mountain may easily be named after the trees that cover it, that same name of the tree is not likely to become the tribal name of the people who live on the mountain. The hypothesis rests on nothing.

The same may be said of Qaraun-ǰidun, or more exactly Qara'un-ǰidun (written Qarayun-ǰidun, with an intervocalic -ǰ- in value of hiatus). Although classical Mongolian knows only *qara'u*, *qarau*, « mutual defence », *qara'un* (and, with the usual slurring of the final -n, *qara'u*) has been a regular Mongol word, derived from *qara*, « black », and *qara'u* also meant « black » (perhaps « blackish »); in the *Secret History* of 1240, *qara'u* is used once alone poetically in the sense of « black cart » (§ 55), but mostly with the adjectival suffix -tai in the full expression *qara'utai tǰrgǰn*, « black cart » (§§ 6, 100, 244). Similarly, in Qara'un-ǰidun, *qara'un* must be an epithet of *ǰidun*. The meaning of *ǰidun* (or *ǰidün?*) has not been ascertained, but both Mongol and Persian texts know another name of similar formation, Qara'un-qabčal, which Rašid translates by « Black Forest » (cf. *Secret History*, §§ 150, 177; *Ber*, II, 110, 135; *Oh*, I, 73; ERDMANN, *Temudschin*, 289, 594), although the real meaning, confirmed by the Chinese version of the *Secret History*, is « Black Pass ». Even in the case of a common root between the adjective *qara'un* and the name of the Qara'una, Qara'unas, there is not the slightest reason to try to connect them otherwise and to deduce in that way, either from Qara'un-ǰidun or from Qara'un-qabčal or from any similar name that may turn up in texts or in actual use, what was in the early 13th cent. the original seat of the Qaraunas. The texts say nothing and we know nothing about it.

Qara'una (> Qarauna), pl. Qara'unas (> Qaraunas), is certainly a true Mongolian name, and QUATREMÈRE was wrong in supposing that it may have undergone some alteration in Persia. Even the Armenian transcription « Karawunas » is regular, as the intervocalic -ǰ- or -g- (when it is not a true -ǰ- or -g-, which then is retained as such) can develop, mainly before a labial vowel, into a -w-, instead of leaving simply a hiatus; the case is the same as in Hülǰgü = Hülǰü, which gives finally Hülǰü, but also Hülǰwü (see « Ulaun »). It is probably derived from *qara*, « black », and this would be in agreement with the depreciatory sense which attached to the name even in the days of Polo. The words derived from *qara*, both in Turkish and in Mongolian, often take on a pejorative tinge: *qaravaš* (< *qara-baš*, lit. « black-head ») meant a « slave-girl » in both Coman and ancient Osmanli; Mong. *qaraliq* (> *haralikh*) is « slave », *qaraču* is « commoner ». In a passage surely original, but which has been preserved in its full form only by RAMUSIO, Polo says that the name « Qaraunas » means « half-breed » (*meschiati*, confirmed by « *gasmulli over bastardi* » in VB, B, 248), and that

the Qaraunas are so called because their white Tartar fathers have intermarried with black Indian mothers. Of course, in Polo's time, the contacts between the Mongols and India were too recent and too sporadic to let us admit of the existence of such a mixed race, and Polo may have been influenced by *qara*, «black». But the idea that the Qaraunas were half-breeds has been adopted again by BARTHOLD for the following reason (*12 Vorlesungen*, 215). In the 15th and 16th cents., the former Čayatai empire had split up in two halves, one including Russian Turkistan and Afghanistan, the other consisting mainly of Chinese Turkistan; the first one retained the name of Čayatai; the second was known as Moyolistan. But they were jealous of each other, and in the *Ta'riḥ-i Rašīdī*, completed in 1547, we are told that the Čayatai people called their cousins of Moyolistan «Jätä», to which the Moyols of Moyolistan replied by calling the Čayatai people «Qarawānās» (= Qaraunas; cf. ELLIAS and ROSS, *Ta'riḥ-i Rašīdī*, 148). Now *jätä* (or *čätä*?) is the same as Osm. *čätä*, «thieves», and BARTHOLD, evidently on Polo's authority, says that the rejoinder meant «half-breeds». He may be right, but the argument is not decisive. As we shall see farther on, the Qaraunas, who were settled in the territory of the Čayatai (as a matter of fact in Afghanistan), had a well-established reputation for violence and highway robbery. This would be enough to explain that to the epithet of «Jätä», «thieves», the Mongols would reply by «Qaraunas», «brigands». At the present stage of our information, I am inclined to think that the name of the Qaraunas, *Qara'una*, is really identical with the word *qarayuna* which is not only the name of a tree, but is also used as an epithet for a water-fowl with black markings (but I have no information on the real pronunciation of that common noun written *qarayuna*, that is to say I do not know whether it is *qarayuna* or *qara'una*). Both the proper and the common nouns must have originally meant «black» or «blackish», like *qara'un*. The name may have been given to the Qaraunas on account of their black complexion, or as a depreciatory designation. The fact that the Qaraunas appear nowhere in Rašīd's account of the tribes as forming part of any of them would almost lend colour to the latter hypothesis. In such a case, they may, after all, have been originally half-breeds of some sort. One may think of half-breeds of Mongols and Qara-Qitai people, but without any serious ground apart from the destruction of the Qara-Qitai empire by the Mongols in Chinghiz-khan's time.

The name of the Qaraunas, which appears only after Hülägü's Mongols had settled in Persia, is not known in Mongolia nor does it occur in Far Eastern texts. But in Persia, Qarauna has been the personal name of certain individuals. An *emir* whose name was Qarauna was put to death in 1319 (*Ha*<sup>1</sup>, II, 277); another *emir* called Qarauna lived at the beginning of the 15th cent.; but this does not imply, as QUATREMÈRE thought (*Not. et Extr.* XIV, 283), that a man bearing such a name has more chance than anybody else to belong to the Qaraunas; he was named after them, sometimes on account of his mother's origin, but in most cases on quite fortuitous grounds.

The name seems to have disappeared now, as a tribal as well as a personal one. There is very little likelihood that the modern «Karwānis» represent Polo's «Caraunas», as suggested by P. SYKES (*Y*, I, 102). As to the Moyol «Kárnās» of MAULA BAKSH (ELIAS and ROSS, *A History of the Moghuls*, 491-492), whose name is said to mean «archers» or «hunters» (by a false



etymology based on Mong. *qarba-*, «to shoot»?), the information is too scanty to allow of any conclusion.

YULE says (*Y*, I, 101) that, according to Waśśāf, the Qaraunas are «a kind of goblins rather than human beings, the most daring of all the Mongols», and adds that Mirhōnd speaks in like terms. The quotation is taken from HAMMER (*Ha*<sup>1</sup>, I, 309, 344; *Ha*<sup>2</sup>, 223), who speaks of «devils» (Dämonen), although he translates the same words of Waśśāf which QUATREMÈRE (*Not. et Extr.* XIV, 282) has rendered by «who are like apes»; the word *nāsnās* means at the same time a kind of ape, and a human monster hopping on one leg. But we must not attach too much importance to Waśśāf's words. That «prince of rigmarole», as YULE calls him, would not miss the chance to make a pun, and what he really says is that the Qaraunās are similar to apes (*nāsnās*), not to men (*nā nās*). The only conclusion we can draw is that the Qaraunas must not have had a very attractive appearance.

Apart from their revolts and inroads, no information has come to us on the Qaraunas, except in a passage which QUATREMÈRE (*Not. et Extr.* XIV, 282) has quoted from the *Nuzhatu'l-Qulūb* of Qazwīnī, completed in 1340 (*Bibl. Nat.*, Persian, Anc. fonds 139, p. 173). The passage occurs in the botanical section, still untranslated. According to QUATREMÈRE, Qazwīnī, «speaking of the tree called *عرعر*, 'boxwood', says *آذرا سرزکوهی خوانند وقرانده اورس گویند* 'It is called *sārv-i kohī* (lit. «mountain-cypress»), and the Qarauna name it *oros*.' QUATREMÈRE admitted that he did not know what was the language of the Qaraunas, and that he could make nothing of «*oros*». In the zoological portion of his work, which was published in 1928 by J. STEPHENSON and to which I have devoted a long paper (*BSOS*, VI [1931], 555-580), Qazwīnī quotes Arabic, Persian, Turkish, and Mongol words, but never any «Qarauna» word, and why should he if the Qaraunas spoke ordinary Mongolian? On the other hand, the word quoted is not known in any language under a form «*oros*». QUATREMÈRE has said *عرعر* '*ar'ar* meant «boxwood», but this seems to be a slip, as '*ar'ar* really means «juniper-tree», and such is also the sense of *sārv-i kohī* in Persian. Now the word for «juniper-tree» is well known in Altaic languages; in Eastern Mongolian it is *arča* (> *arca* [*c = ts*]); in Kalm., *arc*<sup>a</sup>; in Buriat, *arsa*; these forms are hardly reconcilable with the Arabic spelling of Qazwīnī (I do not know the form used by the Moghols of Afghanistan, if they still have the word). The Turkish forms are widely divergent, ranging from Uigh. *artuč* to Alt. Tel. *arčēn*; a form *artīš* exists in the Kazan and Čayatai dialects. Something must be wrong in QUATREMÈRE's quotation. At my request, H. MASSÉ was good enough to examine the ms., and, after consulting with Muḥammad Khan QAZWĪNĪ and Abbas IQBĀL, he came to the conclusion that Qarauna was a misreading for *قزوانه*, an Arabic plural of Qazwīnī, meaning «the people of Qazwīn». As to the name of the tree in the Qazwīn dialect, it is not *oros* as QUATREMÈRE read it, but *اورس* *avirs*, which is known in Persian as one of the names of the juniper-tree. I have no doubt that MASSÉ is right; thus no text quotes any word special to the language of the Qaraunas.

We hear first of the «army» (*laškar*) of the Qaraunas; at a date which seems to be 1282-1283, they were organized into a *tümän* or myriarchy (see «toman»; cf. *Not. et Extr.* XIV, 282; *Ber.* I, 174), and YULE may be right (*Y*, I, 101) in supposing that the existence of this *tümän* is responsible for the average number of «ten thousand» men which Polo attributes to the gather-

ings of the Qaraunas; nevertheless I doubt if the *tümän* of the Qaraunas existed as such when Polo had trouble with those highwaymen on his outward journey to China; if it did not, there is no real allusion to the *tümän* in the text, unless Polo has here mixed up with his former experience information which he acquired on the return journey. The *tümän*, or myriarchy, must have been formed of ten *hazāra* or chiliarchies, and we have seen that Rašid, in his notices of the tribes, mentions at least two, perhaps three *emirs* of the *hazāra* of the Qaraunas.

As far as I can see from the texts at present available, the first mention of the Qaraunas is when Baraq (see « Barac »), who ruled over the house of Čayatai, says in 1270 that, at a critical moment, a Qarauna called Sali was the only one who gave him his horse (*Ha*<sup>1</sup>, I, 270; cf. also *Oh*, III, 349); that Qarauna would thus seem to belong to the dominions of the house of Čayatai, not to those of the *ilkhans* in Persia. But we must not forget that, in 1270, Baraq had occupied most of the Ḥorāsān, with the exception of Herat, the governor of which refused to open the gates of the city to Baraq's officers. The Mongols had already been settled in Ḥorāsān in 1241, and had advanced to the Sind. In 1252, Mongka sent from Qara-qorum the Tatar Sali *turyaq* to conquer North-Western India and Kashmir, and Sali's army set out in 1253; Mongka had told him he would remain in those parts to the end of his life (cf. *YS*, 3, 2 *a-b*; T'u Chi, 6, 6 *b*; *Br*, I, 138, with wrong restitutions; *Ha*<sup>1</sup>, I, 8, 87; *Oh*, II, 280; QUATREMÈRE, *Hist. des Mongols*, 131). Later, Sali's grandson \*Bäktut was *emir* of the Qaraunas (*Ber*, I, 63). It is very tempting to identify the Qarauna Sali of 1270 with the Tatar Sali. It might be objected that the Tatar Sali, also called Sali *noyan*, ought to have been dead in 1270, since, according to D'OHSSON (II, 280), the intercession of Chinghiz-khan's Tatar wives saved his life when Chinghiz-khan almost exterminated the Tatar. But this is an error of D'OHSSON (repeated by HOWORTH, III, 184) : the man whose life was spared in Chinghiz-khan's time was not Sali, but his father \*Qara Mängätü Uha (whom Mongka had sent to Badaḥšān and the borders of India; cf. *Ber*, I, 61-64). Nevertheless, Sali must be the same as Sali *noyan*, and if so he had already invaded North-Western India in 1246 (cf. the chronicle of Herat in *JA*, 1861, I, 442-445, but with some impossible data, such as the death of Čayatai in 1247 instead of 1242; HOWORTH, III, 99-100). We must only suppose that in the meantime he had returned to Mongolia, but that sent back to Eastern Iran and Afghanistan in 1253, he was still alive in 1270. It was at the request of \*Bäktut's father, Üldü (> modern Mong. *ildü*, « sword »), that the *il Khan* again formed a *hazāra*, or chiliarchy, with what remained of the Tatar in Eastern Persia; and they stayed with Üldü (*Ber*, I, 63). These Tatar may have been near cousins to the Qaraunas.

At any rate, in the following years, the Qaraunas were in the district of Bady'is of Ḥorāsān, the main town of which was Herat; in 1278, it was at Herat that the *emirs* of the Qaraunas made their submission to Abaya (*Ha*<sup>1</sup>, I, 309); but they were already there in 1272, if I am right in believing that it was on the outward journey that Polo almost fell a victim to one of their forays. Either in 1278, or after a campaign of 1279, Abaya must have taken with him, in his guard, an important portion of the Qaraunas, who lived during the winter in Bagdad and spent the summer on the Siyāh-koh (« Black Mountain », on the verge of the Great Desert north-east of Ispahan). When, in 1282-1283, Aryun spent the winter in Bagdad, he formed with these Qaraunas a *tümän*, and appointed Tayačar (see « Tagaciar ») to command it (*Oh*, III, 581; *Ha*<sup>1</sup>, I, 344); Tayačar still

held the same command in 1295 (*Ha*<sup>1</sup>, II, 26). But part of the Qaraunas had remained in Ḥorāsān, and perhaps the *tümān* itself or some of its chiliarchies were transferred there after 1283, since Rašid names the *emir* of a *hazāra* of Qaraunas who had settled in the region of Badyīs, and even a « secretary » Hindu, who was *emir* of the *tümān* of the Qaraunas in the region of Badyīs in Ḥorāsān (*Ber*, I, 151, 174). In the meantime, some of the Qaraunas had gone to different parts of the dominions of the *ilkhans* : in 1290-1291, the Qaraunas revolted in Merv. Above all, they often raided the country as freebooters, indulging in all sorts of excesses. In 1284, when Aḥmad was defeated by Aryun, the Qaraunas sacked his camp, entering the tents of the women and tearing off their jewels and their clothes; Aḥmad's mother and two other princesses were stripped to the skin. Polo's account of the misdeeds of the Qaraunas are fully borne out by the Persian historians.

As to the inroads made according to Polo by the Qaraunas into India, this can only be discussed in connection with the identity of their « king » « Negodar ». YULE (*Y*, I, 101) has already called attention to a passage in Ibn Baṭṭūṭah (transl. DEFRÉMERY and SANGUINETTI, III, 201) : « The Sultan Tuyluq was one of those Turks who are known under the name of قراونا Qarāuna, and who live in the mountains between the Sind and the land of the Turks ». Tuyluq (not « Taylaq », as is often said), sultan of Delhi, a former slave, ruled from 1320 to 1324, of course long after Polo's travels; he was probably a Turk, not a Mongol. But it is a remarkable fact that Ibn Baṭṭūṭah, travelling in India a few years later, should have heard the name of the Qaraunas, and as a designation of people located between North-Western India and Turkistan. This goes a long way to show, as we shall see, that right or wrong, Polo's association of the Qaraunas with « Negodar » cannot merely be an error of the Venetian traveller.

Unfortunately, the case of « Negodar » is so intricate that the difficulties attending a discussion of the Qaraunas are almost inconsequential in comparison. Several quasi-homonymous individuals, beginning with the Sultan Aḥmad, are involved, and for all of them, confusion prevails in Persian texts, because the absence of diacritical marks on consonants or the change of a dot makes it impossible to tell at once which one is a تگودار Tägüdär and which one a نگودار Nägüdär. For the Sultan Aḥmad, a sort of agreement appears to have been reached to call him not Nägüdär as the Persian historians do, but Tägüdär on the faith of contemporary Armenian and French transcriptions (see « Acmat<sup>2</sup> »); but this does not help us much in the present discussion, since our Persian sources speak often of a « Prince Nägüdär » which may be either Tägüdär, *i. e.* Sultan Aḥmad, or another prince Tägüdär or Nägüdär; finally the name of another man, who was not a prince, comes in also with the same ambiguity of spelling.

According to Polo, « Negodar » went with 10,000 men to the Court of Čayatai, « own brother to the Great Khan », and his uncle. But he left his uncle who was then in « Greater Armenia », and, through Badaḥšān, invaded North-Western India. It has often been said that by « Negodar », Polo meant a prince « Nigudar », son of « (the younger) Juji », son of Čayatai, who was the leader of the Čayatai troops put at the disposal of Hülägü by Mongka (cf. *Ha*<sup>1</sup>, I, 86, 284; *Oh*, III, 516; HOWORTH, III, 96; *Y*, I, 103); according to RAMSTEDT (*Mogholica*, I), the Moghols of Afghanistan are the descendants of « the lost army of Chinghiz-khan's grandson Nukodar, about the fate of which the Chinghizkhanid epigons even now often inquire in Mongolia »; BЛОЧЕТ, in his edition

of Rašid (*Bl*, II, 158; also III, 39, 48 [unpublished proofs]), prints Nāgüdār as the correct form of the name of Čayatai's grandson and adds that he is Polo's «Negodar».

The question is not so simple. A preliminary point should be made clear: «Juji» (= Jöči), as the name of Čayatai's eldest son, is a misreading by HAMMER for Moči (Mong. *moči* means «carpenter»; cf. *Secret History*, § 223; in modern Mong., *modonči*); Plan Carpine's «Moucy» represents the same name and probably applies to the same man (cf. *Wy*, 67, etc.; «Moucy» is a better reading than «Maucy», and Moči was the eldest son of Čayatai, not the second one). Abū'l-Faraǰ's بوحى «Buḫī» (*Hist. dynast.*, ed. ПОСОКЕ, 503; transl. 329), also corrupt, is less altered as to the initial consonant. An account of Moči and of his eleven sons is given by Rašid (*Bl*, II, 158-161). A general «Negoudar», of whom I shall have to speak later, is said by D'OHSSON (*Oh*, III, 380) to have been in command of some of the troops of the branch of Jöči who came to Persia with Hülägü; but it seems to be partly as the result of a confusion with HAMMER's misreading for the name of Čayatai's eldest son that PAUTHIER (*Pa*, 82) sees in that «Negoudar» a son or a grandson of Jöči of the Golden Horde. It is only by an oversight that Nāgüdār (or Tägüdār) is said to be a «son» of Čayatai by BROWNE, *A Hist. of Pers. Lit.* III, 25, or a «great-grandson» by HOWORTH, III, 310.

To identify Polo's «Negodar» with Čayatai's grandson Nāgüdār (or Tägüdār), it is necessary to amend considerably the account given by our traveller. Čayatai was not really brother to the Great Khan, if we are to understand by «Great Khan» Qubilai; but this is the usual confusion in Polo (see «Ciagatai»). Nāgüdār (or Tägüdār) was not Čayatai's nephew, but his grandson; and here we cannot take advantage of the double meaning of «neveu», which means both «nephew» and «grandson», since Polo, in Rustichello's words, speaks of Čayatai as being «Negodar's» «uncle» (unless we are to put the confusion to the account of Rustichello misunderstanding Polo). Moreover Čayatai died early in 1242, and could therefore have taken no part in the campaign of Hülägü which took place many years later. Even in his lifetime, Čayatai was not in «Greater Armenia». And we cannot think either of the Čayatai princes who succeeded Čayatai, since their dominions were always far to the north-east, towards Russian and Chinese Turkistan (RAMUSIO's text has here a «Turchia maggiore», «Greater Turkey», which perhaps takes the place of «Greater Armenia»), and that name, used for what we now call Turkistan, would be correct for the dominions of Čayatai and of his successors; but although there is original information going back to Polo in this chapter of RAMUSIO, I dare not here prefer his reading to that of the other texts, with which, moreover, there is not here a strict parallelism in RAMUSIO).

But even making allowances for inaccuracies in Polo's genealogical account of «Negodar», the identification with Čayatai's grandson raises other difficulties. YULE (*Y*, I, 103) has already called attention to the fact that the grandson of Čayatai whom he calls «Nigudar» had revolted under Abaya, sought refuge in Georgia and was captured by Abaya's men. Abaya spared his life, but the rebel prince's troops were dispersed among Abaya's army (*Oh*, III, 433-435, quoting Rašid; cf. also *Ha*<sup>1</sup>, I, 261). According to Rašid, that Nāgüdār (or Tägüdār), Čayatai's grandson, died childless (*Bl*, II, 159; but D'OHSSON, III, 592, speaks of 'Omar, son of the prince Nāgüdār, as being on Aryun's side against Aḫmad-Tägüdār in 1284, and HAMMER [*Ha*<sup>1</sup>, I, 352] says that this 'Omar is the son of Čayatai's grandson Nāgüdār [although he does not mention him in his

genealogical tables]). HAMMER (*Ha*<sup>1</sup>, I, 284), speaking of events of 1278-1279, in which « Nigudarian bands » took part, adds that these were formed of the former troops of the Čayatai prince « Niguder » after that prince's death, and that they were a rabble of Šul, Mongol, Türkmän, and Kurd bandits. YULE has already noted that HAMMER gives no authority for the origin of the « Nigudarian bands » (*Y*, I, 103), but accepts (*Y*, I, 102) HAMMER's data as to their motley ethnical constituents. The statement that the « Nigudarian bands » were formed of the subordinates of Čayatai's grandson Nägüdär (or Tägüdär) who, after his disgrace, had retired to Seistan, appears also in D'OHSSON (*Oh*, III, 516), whose authority is here Waśśāf (in a section not yet translated); but in D'OHSSON, more reliable than HAMMER, the Šul, Mongol, Türkmän and Kurd people, instead of forming the « Nigudarian bands », are the troops of the province of Fars who try to oppose their inroad. There is nevertheless some contradiction in D'OHSSON's text when he says first, quoting Rašid, that « Nägüdär »'s troops were incorporated in 1269 into Abaya's army and that « Nägüdär » himself, according to Waśśāf, was put under the custody of the *noyan* « Couroumisch » (*Oh*, III, 435; read « Qurumši », and cf. *TP*, 1938, 150-152), and later, referring to Waśśāf again, represents « Nägüdär » as being no longer in bondage and, having settled in Seistan with his « vassals », as being strong enough to constitute the « Nigudarian bands » (*Oh*, III, 516). It is not easy to find one's way through the untranslated portions of Waśśāf, and I am not able to say anything more on this point for the present, except that according to Rašid (*Bl*, II, 159; and in the history of Abaya, BN, Pers. 209, f. 301 a), Čayatai's grandson used to be present in the Imperial camp (*ordu*) of Abaya until he died; the death of Nägüdär or Tägüdär must then have taken place before 1281.

All modern scholars have taken for granted that the name of Čayatai's grandson was Nägüdär, not Täküdär; Rašid (*Bl*, II, 158) adds to his name an epithet <sup>ابو</sup> of doubtful reading (? Yäbä = Jäbä; cf. also *Bl*, II, 167); the epithet « Aghul » or « Oghlan » in *Y*, I, 103, is of course Turk. « *oyul* » or « *oylan* », « son », with the meaning of « prince » (cf. *Pe*, 168). But here again we are not on safe ground. In 1595, Abū'l-Fazl speaks of the Hazāra of Afghanistan as the descendants of the Čayatai troops sent by Mongka with Hülägü under the command of Nägüdär-oylan, and YULE has said (*Y*, I, 103) that Čayatai's grandson is evidently the man to whom Abū'l-Fazl alludes. But YULE has omitted two words (« his son »); Abū'l-Fazl expressly states that this Nägüdär-oylan is Hülägü's son (GLADWIN, *Ayeen Akbery*, II, 163; JARRETT, *The Āin i Akbari*, II, 401-402), that is to say he mistakes him for the *ilkhan* Aḥmad, originally named Tägüdär, but misread as Nägüdär in most Persian histories (see « *Acmat*<sup>2</sup> »). When Thomas HERBERT, in 1628, saw at Kāšān a tomb which was said to be that of « Nycador-Oglan the Usurper » (ed. W. FOSTER, 219, and cf. *TP*, 1930, 439), the epithet implies that he believed it to be that of Aḥmad-Tägüdär, for whose name he thus uses the same form as Abū'l-Fazl. The form of the name of Čayatai's grandson would seem to be settled by RAMSTEDT's sentence about Mongols of the present day still inquiring about the fate of the army of this grandson « Nukodar »; but with all due respect for RAMSTEDT's Mongol scholarship, I am afraid that there is some misunderstanding here. The Mongols of Mongolia, as far as I am aware, have no direct tradition about the Čayatai troops which were sent with Hülägü to Persia; if some of them know even of their former existence (and one would wish more precise information on the point), the knowledge must have come to them through Mussulman authors,

and consequently would carry no weight in so far as the true form of the name, Nāgüdār or Tāgüdār, is concerned. The confusion was moreover too easy. Only a dot differentiates the two names; both princes had their name followed by the epithet «oyul» or «oylan»; even in their history there are some common points. Aḥmad-Tāgüdār also does not seem to have always been on good terms with his brother Abaya; and just as Čayatai's grandson sought refuge in Georgia, Aḥmad-Tāgüdār, on Abaya's death in 1281, came to the Imperial camp straight from Georgia (cf. Rašidu-'d-Din's unpublished account of Aḥmad's reign). In some cases, I suspect that texts which have been taken as referring to Čayatai's grandson really concern Aḥmad-Tāgüdār. We should have a somewhat safer basis in this respect if it were true that as HAMMER has it (*Ha*<sup>1</sup>, I, 322) Aḥmad-Tāgüdār had remained in Mongolia and was sent to Persia by Qubilai under Abaya's reign, that is to say after 1265; but although we know that Hülāgü had only taken with him, when he started for the West, his two elder sons Abaya and Yošmut (QUATREMÈRE, *Hist. des Mongols*, 147), I cannot trace the origin of HAMMER's statement about Qubilai and Abaya.

Apart from occasional confusions between Aḥmad-Tāgüdār and the grandson of Čayatai in late Persian texts and among Western scholars, the form of the name of Čayatai's grandson requires further investigation. While Aḥmad-Tāgüdār's name is generally written Nāgüdār in Persian sources, the name of Čayatai's grandson occurs twice in Ĵuwaīnī, and the readings of the best mss. are once كڭدار Tāgüdār (III, 91) and once ڭڭدار Tāgüdār (III, 107); and it is also Tāgüdār which we find in a parallel passage of Abū'l-Faraĵ, drawing in fact from Ĵuwaīnī (*Hist. Dynast.*, ed. ПОЦОКЕ, text, 503; transl., 329). But there is something more, which has been overlooked by YULE and others. Čayatai's grandson, when he revolted against Abaya, went to Georgia, where he fought long enough to become well known to the Georgians; the Georgian chronicle has a long account of all these events, with many details not to be found elsewhere, and the name of the prince is always written «Thaguthar» or «Theguthar» (cf. BROSSET, *Hist. de la Géorgie*, I, I, 575-583; the note on p. 576 teems with confused statements). «Thakudar» is also the only reading in the Armenian account of Malakia, an author of the late 13th cent. (BROSSET, *ibid.* I, Add., 455, 465-466; PATKANOV, *Ist. Mongolov Magakii*, 31-32, 52-54). Now Armenian transcriptions have been accepted as decisive for «Tāgüdār» against «Nāgüdār» in the case of Aḥmad-Tāgüdār. Similarly, the conclusion seems inevitable that, for Čayatai's grandson, Georgian and Armenian transcriptions must prevail, and that his name was Tāgüdār, not Nāgüdār.

But what then about Polo's «Negodar»? Of the various individuals who could be considered, only one remains, and it is one whose case is still mysterious. In the course of his note, YULE (*Y*, I, 103) has quoted a general Nāgüdār who, according to D'OHSSON, was in command of some of the troops of the house of Ĵöči sent by Mongka with Hülāgü. When war broke out in 1262 between Hülāgü and Bärkä (then the head of the house of Ĵöči; see «Berca»), Nāgüdār and his colleague «Onguĵa» escaped to Ḥorāsān, pursued by the troops of Hülāgü, and thence eastward, where they seized upon Ghazni and other districts bordering on India (cf. *Oh*, III, 379). Unfortunately, D'OHSSON does not mention his source. This Nāgüdār cannot of course be the prince, grandson of Čayatai, who was still in Georgia in 1268-1269. But I have not succeeded in tracing him, no more than his colleague «Onguĵa», although «Onguĵa» (cf. Mong. *ongyoča*, «boat») is probably the same man whose name has been altered in Abū'l-Faraĵ's text

into انكوجنا «Otguhanā (read انكوجا Onguča?; *Hist. Dynast.*, text, 508; transl., 332). We may suppose that this one was really a Nāgūdār, not a Tāgūdār, and he at least is said to have invaded the borders of India; nevertheless the possibility of a confusion remains, since the Čayatai prince Tākūdār was also involved in the intrigues of the princes in 1262, and is said to have then saved his life by submitting to Hülāgū (BROSSET, *Hist. de la Géorgie*, I, Add., 455; PATKANOV, *Ist. Mongol. Magakii*, 31-32). Nāgūdār or Tāgūdār, we should want more precise information about him to enable us to determine whether he can still be the same as the *emir* Nāgūdār (or Tāgūdār?) who, according to the chronicle of Herat, had settled in Herat with 300 adventurers from 'Iraq and created trouble in 1298 (*Y*, I, 103), or the Nāgūdār-bahadur (or Tāgūdār-bahadur?) who, in the next year, is named among the *emirs* of the predatory Qutluyšāh (*Ha*<sup>1</sup>, II, 104).

If the *emir* of 1262 is really a Nāgūdār, he is likely to be the eponym of the Nāgūdārī bands; these freebooters did much harm in Eastern Persia at the end of the 13th cent. and in the beginning of the 14th. To the references already given by QUATREMÈRE (*Not. et Extr.* XIV, 284) and by YULE (*Y*, I, 102-104), we may add a curious text of Mufazzal on one of their campaigns against the kingdom of Delhi (BLOCHET, *Moufazzal*, 556-557). Waśśāf says that the Nāgūdārī are a people of the Seistan; Rašīd speaks of their army (*laškar*). According to a text of Mirḥōnd, Nāgūdārī had been transferred to 'Iraq by Ghazan, with summer and winter encampments, which sounds very much like a repetition of what Aryun had done with the Qaraunas some fifteen years earlier; these Nāgūdārī, in Mirḥōnd's account, form one of several chiliarchies (*hazāra*). The texts generally agree to call these freebooters «Nāgūdārī», although they occasionally give «Tāgūdārī». HAMMER (*Ha*<sup>1</sup>, I, 284, 309) has blamed D'OHSSON for confounding the Qaraunas of Ḥorāsān with the Nāgūdārī of Seistan; but as we have seen, an *emir* Nāgūdār and his men had settled at Herat in Ḥorāsān, in the very region where we hear most of the Qaraunas. The Mongol Amaji-Nāgūdārī of the chronicle of Herat (*JA*, 1861, I, 456; *Oh*, IV, 184) may be the same as the «Aladu» of *Ha*<sup>1</sup>, I, 13, «Alaju» of *Oh*, IV, 46, who had at one time Qaraunas under his command, and anyhow that Nigudarian was in the region of Herat, not in Seistan. Probably no strict distinction was made between them, and it may be that Qaraunas was the name used regularly by the Mongols, while Nāgūdārī was practically its later (?) Persian equivalent. Polo's only mistake was then, owing to the phonetic resemblance, to associate unduly the Nāgūdārī, *alias* the Qaraunas, with the Čayatai prince Tākūdār, as the Persians did themselves later, on account of graphic analogy. The Qaraunas, *alias* Nāgūdārī, of Ḥorāsān and Seistan seem at the same time to have been the ancestors of the Nāgūdārī > Nāgdārī whom we find in Bābur's time in the region of Kabul; some of them, and also some of the Hazāra (cf. *Hobson-Jobson*<sup>2</sup>, s. v. «Huzāra»), spoke Mongolian according to Bābur (transl. A. BEVERIDGE, 207); the Hazāra, *i. e.* Chiliarchies, owe evidently their name to the former organization of the Mongol army (it is surprising that no such use of this Persian word should be mentioned by VULLERS), and they still speak an archaic Mongolian dialect, although they are becoming more and more iranized (cf. RAMSTEDT, *Mogholica*). Although A. BEVERIDGE, in the translation of the *Memoirs of Bābur*, transcribes the name of the Nāgūdārī by «Nikdīrī» as well as by «Nikdīrī» (pp. 196, 200, 207), there is no -ī- in any ms. after the *d*; all the mss. give نكدري Ngdrī, *i. e.*

Nägdārī or Nāgūdārī (cf. ILMINSKII, *Baber-namē*, 153, 156, 161; PAVET DE COURTEILLE, *Mémoires de Baber*, I, 273, 278, 287; *The Bábar-Nāma*, facsimile of the Hyderabad ms., Gibb Memorial, I, 125 b, 128 a, 131 b).

YULE has not even mentioned the possibility of tackling the problem of the Qaraunas and of Nāgūdār by supposing that Čayatai is really meant, that is to say that the Qaraunas are men who separated from him in his lifetime, while he was, if not in «Greater Armenia», at least in Persia. Nāgūdār may then have been an officer of Čayatai himself. This was the view taken by PAUTHIER (*Pa*, 80), and it is upheld by BENEDETTO (*B*<sup>1</sup>, 430), when he remarks that Čayatai died in 1242, so that, in Polo's time, Nāgūdār must have been an octogenarian. I have no great confidence in the traditional view that the Qaraunas left Armenia in 1269 or even in 1262; we have seen that the first mention of a Qarauna, in 1270, occurs really in a region open to inroads of the Čayatai princes, and that the last mention of the Qaraunas, in 1547, refers to the people of the Čayatai dominions. At the same time, a more remote date would render less surprising Polo's belief in Indo-Tartar half-breeds; the Mongols had occupied Eastern Persia and begun their inroads in India more than thirty years before Hülāgū reached Iran. They had sacked Lahore in 1241. and, as a consequence, a revolution occurred in Delhi (cf. *Oh*, II, 280-281); some account of these events may have lingered in Polo's memory, and 'Alā'u-'d-Dīn, who began to reign in Delhi in 1242, would, phonetically, not be a much worse original than Ghiyāṭu-'d-Dīn (yiyāṭu-'d-Dīn) for Polo's «Asidin» (see «Asidin soldan»). But the fact remains that no mention of the Qaraunas has yet been found before 1270. Contrary to BENEDETTO (*B*<sup>1</sup>, 446), according to whom several nephews of Čayatai are called Nāgūdār, I have not been able to discover a single one. Moreover, Polo speaks of «Negodar» as of somebody alive and active in his own time. And also I have a suspicion that the name of Čayatai, even after his death, continued to be used loosely for his successors, in the same way as, in the beginning of the 14th cent., we still hear of the realms of Qaidu and Dua long after both had died (see «Caidu»). On the whole, and despite all difficulties and uncertainties, I think that Polo's account of the Qaraunas and of «Negodar» refers to a date posterior to Hülāgū's arrival in Persia.

I have to add three more remarks on Polo's chapter on the Qaraunas. First, the reader may notice that there is no trace in the translation of the «a scaranis» of Latin version (LT; < TA : *dagli Ischerani*), which YULE (*Y*, I, 100-101) thought represented Italian *scherani*, «bandits». YULE is right since we already find *ischerani* in TA<sup>1</sup>, *scherani* in TA<sup>3</sup>; but I suspect that *scherani* itself has taken the place of *carauanas*, and that *et melandrinis* (? < \**i. e. melandrinis*) has been added as a gloss to *a scaranis* in LT.

The second remark relates to the magical practices of the Qaraunas by which they were said to produce darkness in the day-time. According to RAMUSIO, much more detailed here than the other texts, the Qaraunas learned that art in India, when they reached «Malabar». RICCI and ROSS (*RR*, 43, omitted from the index) have changed this name into «Maabar», which seems to have been BENEDETTO's first idea, and has left traces in *B*<sup>1</sup>, 430. But the correction is arbitrary; in Polo's time, the Mongols had reached neither Malabar, nor Ma'abar. Nevertheless it is possible that owing to some similarity of name, RAMUSIO here «edited» his source. In RAMUSIO, Polo's «Maabar» (= Ma'abar) and «Melibar» (= Malabar) both become «Malabar»; moreover, RAMUSIO



gives here «Dely», instead of the «Dilivar» (*q. v.*) of F. Whether «Dilivar» be Delhi or not, the fact that there is in RAMUSIO a chapter on «Dely» (which is not Delhi; see «Eli») followed by a chapter on «Malabar» (=Malabar) may have influenced his readings in the chapter on the Qaraunas; in other words, both his «Dely» and his «Malabar» of the chapter on the Qaraunas are open to suspicion. I think it probable, in the present case, that RAMUSIO's «Malabar» was altered from the name which reads «Dilivar» in F; this is also the solution adopted on second thought by BENEDETTO (*B*<sup>1</sup>, 43, 430).

Finally, I must explain why, in the above notes, I have supposed that Polo had been assaulted by the Qaraunas on the outward journey. On the return journey, the three Polos, conveying the two princesses, travelled in state until they had met Ghazan, and it appears very unlikely that they should have been then attacked by the Qaraunas. Later on, they were always in North-Western Persia, far from «Camadi» and from the Qaraunas. On the other hand, they passed through the regions infested by the Qaraunas on the outward journey, and they travelled then as private individuals, perhaps with a caravan of merchants; I think it was then that the attack took place, in 1272.

### 123. CASCAR

<i>calscar</i> FAt	<i>cascar</i> F, FA, FB, L, LT, P,	<i>chasar, chascar</i> VA
<i>carschar</i> Z	VL, Z	<i>chaschar</i> V, VA
<i>casar</i> FB	<i>caschar</i> V, VB, VL; R	<i>chasciar</i> TA <sup>1</sup> , TA <sup>2</sup>

This is the well-known كاشغر Kāšyar. The earliest Western mention of the modern name which I know of occurs in the Latin translation, made at Damietta in 1221 from the Arabic, of a document concerning the Mongol campaign against the Mussulmans; there it is written «Chassar», «Casahar», «Chasahar», etc. (cf. ZARNCKE, *Der Priester Johannes* II, 32, 34, 46, 49). «Caschat» (read «Caschar») is next mentioned in the letter of Sembat the Constable dated February 7, 1248 (in Vincent de Beauvais, XXXII, 92; D'ACHERY, *Spicilegium*<sup>2</sup>, III, 624-628; *Y*<sup>1</sup>, I, 162, 263; on the date, cf. *Pel*, 130). Fra Mauro's «Chaschar» (HALLBERG, 121) comes from Polo. In RICCI's arrangement of GOES's Diary, we find the same spelling «Cascàr» as in Polo (cf. TACCHI-VENTURI, *Opere storiche del P. Matteo Ricci*, I, 536).

On Kāšyar, cf. CHAVANNES, *Doc. sur les Tou-kiue*, 121-122; *Y*, I, 182-183; STEIN, *Ancient Khotan*, I, 47-72; «Kāshghar», by BARTHOLD in *EI*; *Br*, II, 45-47, 230-234; *Mi*, 200-281.

The ancient names of Kāšyar, and «Kāšyar», itself have already been much discussed, but it is now possible to be more precise on some points.

Kāšyar is first mentioned in Chinese texts under the name 疏勒 Shu-lo (\*Sj<sup>1</sup>w-o-lək); it goes back to c. 130-125 B. C. and remained the only one in current use among the Chinese down to c. A. D. 1000. Some Buddhist pilgrims however, from c. A. D. 400 to the end of the 8th cent.,

adopt a transcription 沙勒 Sha-lo (\*Ša-lək; cf. STEIN, *Ancient Khotan*, I, 48). Various attempts have been made in China and in Europe to account for this name.

The oldest one is due to Hsüan-tsang, who, in a note of his *Memoirs* of 646, states that Shu-lo is an inaccurate rendering of the name of the capital, and that the correct form of the latter name is 室利訖栗多底 Shih-li-ki-li-to-ti (\*Šjēt-lji-kjət-ljēt-tā-tiei). JULIEN (*Mémoires*, II, 219) restored this as \*Śrīkrīṭati; if we abide by Hsüan-tsang's rules of transcription, the original ought to be \*Śrīkrīṭātī. Of course, Hsüan-tsang was himself mistaken when he tried to explain Shu-lo by the beginning of his \*Śrīkrīṭātī. The latter name, to judge from Hsüan-tsang's words, must be a sanskritized name of the capital, being some epithet beginning with *śri-*, «fortunate», though the second part remains unexplained. FRANKE's «Śrīkrīṭadhi» (*SPAW*, 1903, 738; STEIN, *Ancient Khotan*, 50) is not acceptable. The variants mentioned by LÉVI (*BEFEO*, v, 261), and which have been taken into account by MARQUART, *Wehröt und der Fluss Arang*, 68, are valueless: the second 利 *li* is a wrong duplication of the first one, and 乾 *ch'ien* is a wrong duplication of 訖 *chi*.

Another explanation was proposed at the beginning of the 8th cent. by 慧苑 Hui-yüan, a priest of Hsi-an-fu, in his commentary on the *Avataṃsaka* (Lo Chên-yü is mistaken when, in his 丙寅稿 *Ping-yin kao*, 2 b, he identifies this Hui-yüan with a Tun-huang priest Hui-yüan for whom a document was written by Tu Mu c. 845; neither the date nor the place will fit). Finding the name Shu-lo in the new translation of the *Avataṃsaka* made in 695-699, Hui-yüan commented on it as follows (Tōkyō *Tripit.* of Meiji, 爲, x, 121 a, 140 b): «The correct name is 佉路數怛勒 Ch'ieh-lu-shu-ta-lo (\*K'ja-luo-šju-tāt-lək). This country (= China) has from old preserved it in the abbreviated form Shu-lo (\*Šj'o-lək), in which, moreover, the sound 數 *shu* (\*šju) was altered to 疏 *shu* (\*šj'o). This name, however, is the name of a mountain in that country, and the designation was made on account [of the mountain]. Moreover, the name is sometimes translated 'Kingdom of Wicked Nature'; it is because the nature of the people of that country is mostly savage and cruel.» The changes made to Hui-yüan's text in later compilations are few and unimportant.

S. LÉVI discovered this passage in 1902, and drew from it conclusions which were sharply opposed by O. FRANKE and PISCHEL (cf. *BEFEO*, II, 246-255; III, 339-341; IV, 543-579; *SPAW*, 1903, 184-196; 735-745; 1905, 238-248; J. HALÉVY, *Le berceau de l'écriture kharoṣṭrī*, in *Rev. sémitique*, 1903, reprint of 15 pp.). Two different problems were concerned: the restoration of the «correct» name proposed by Hui-yüan, and its application.

LÉVI had restored Ch'ieh-lu-shu-ta-lo to Kharoṣṭra. FRANCKE and PISCHEL suggested \*Kaluṣāntara, or \*Kaluṣadhara, or \*Kaluṣottara, all entirely unlikely forms, which ought not to have been accepted, as they were more or less, in STEIN, *Ancient Khotan*, I, 49. LÉVI's Kharoṣṭra is the regular original suggested by the Chinese transcription, except in two points: 怛 *ta* (\*tāt) generally renders a form in *-t*, not in *-t*; 勒 *lo* (\*lək) is abnormal for *-ra*. LÉVI's explanation of the final character as rendering a locative Kharoṣṭre of the Sanskrit original is unfounded. My own view is that we have here to deal with an iranized form like \*Kharoṣṭrag. We can now appreciate better than thirty-five years ago the important part played by the Iranians in the diffusion of Hindu doctrines in China. The texts of the *Avataṃsaka* show a strong influence of

Central Asia, and Hui-yüan, their commentator, was very probably in touch with Buddhists of Iranian origin. Such cases are not exceptional. Among the *yakṣa* of the *Mahāmāyūri*, one is called Kharaposta, translated «Donkey-skin» in Chinese; this is a purely Iranian name (cf. S. LÉVI, in *JA*, 1915, I, 74), the exact equivalent of the *Χαρπῶστ* of JUSTI, *Iranisches Namenbuch*, 171.

LÉVI had first thought that «Kharoṣṭra» was a real name for Kāśyar; further research led him to suppose that it had a broader meaning, and that it had been the designation of all the Buddhist countries north of India and west of China. Without entering into the whole discussion in detail, I must state that, contrary to LÉVI, I do not believe that «Kharoṣṭra» occurred in the Sanskrit text of the *Avataṃsaka*; all translations, Chinese as well as Tibetan, show that the text spoke of the Khaśa, which of the three translators, the translator of 696-699 alone interpreted as Shu-lo, Kāśyar; and it was this Shu-lo of the translation of 696-699 which furnished the occasion for Hui-yüan to speak of «Kharoṣṭra» as being the etymology of Shu-lo. Whether he was or was not the initiator of this wrong etymology is almost immaterial. What matters, and it is to LÉVI's credit to have drawn attention to it, is that there was then a name «Kharoṣṭra» or \*Kharoṣṭrag, which Hui-yüan considered as a geographical name. I have a suspicion that Hui-yüan's commentary is not as plain as its wording seems to show. Hui-yüan knew the Khaśa of the Sanskrit text and the translation Shu-lo adopted in 696-699. My impression is that his «name of a mountain» actually refers to Khaśa, which we shall find again below as the name of certain mountainous districts and at the same time as a name applied to Kāśyar. It is only \*Kharoṣṭrag or «Kharoṣṭra» which was explained by some as meaning «Kingdom of Wicked Nature», unless even this, perhaps, also relates to Khaśa.

The vivacity of the contest for or against «Kharoṣṭra» was mainly due to its repercussion upon the name of the mythic ṛṣi Kharoṣṭha, the eponymous creator of the *kharoṣṭhī* or *kharoṣṭrī* writing once used in north-western India and western Chinese Turkestan. Kharoṣṭha means «Donkey's-lip», and is so translated in Chinese texts; but for the name of the writing, the Indian mss. hesitate between *kharostī*, *kharoṣṭī* and *kharostrī*. I shall not dwell upon the respective fortunes of the rare compound *kharoṣṭha* and the much more frequent *kharoṣṭra* in Indian literature, discussed at length by LÉVI and PISCHEL. But, apart from the fact that *oṣṭha* and *uṣṭra* give the same form in some Indian dialects, for instance in Pāli *ōttha*, we must not forget that the *kharoṣṭhī* or *kharoṣṭrī* writing originated in countries which were strongly under Iranian influence, and that the only Old-Iranian form corresponding to Skr. *oṣṭha* is *aoṣtra*. Under these conditions, an Iranized form equivalent to Skr. *Kharoṣṭha* would precisely be \*Kharoṣṭrag. In a Tibetan text relating to the history of Khotan, the name occurs in a form which is not quite certain; THOMAS gives it as Khare'ustra in *Asia Major*, II, 256, but as Khare'usteñ in *Tibetan Texts and Documents*, I, 93; owing to a very common confusion in Tibetan writing, <sup>o</sup>steñ may be for <sup>o</sup>ster. In such a case, the Tibetan form would be based on \*Kharostrā.

Such a view is confirmed by a name which I am surprised to have scarcely seen mentioned in the discussions of 1902-1905. The ṛṣi Kharoṣṭha is not only the eponymous creator of the *Kharoṣṭhī* writing, he is also an astronomer; LÉVI alluded to this (*BEFEO*, IV, 564-565), but many more Chinese texts could be adduced. The ṛṣi «Kharuṣṭa», an astronomer, addresses the congregation in a text in «mixed» Prākṛit of Khotanese origin (HOERNLE, *Manuscript Remains*

of *Buddhist Literature*, 121, 725). Now, VASIL'EV drew attention many years ago to an Armenian text in which Mekhitar of Airivang mentions Kharustr as the initiator of astronomy in Chaldea (cf. SCHIEFNER, *Wassiliew's Vorrede*, 30); VASIL'EV thought of connecting Kharustr with Zoroaster. WEBER (*Ind. Streifen*, III, 8-9; *Hist. of Indian Literature*, 247-248) preferred to establish a relation between Kharustr and a Krauṣṭuki mentioned as an astrologer in the *Atharva-pariśiṣṭa*. But it seems to me almost evident that we cannot distinguish between the astronomer Kharustr of Armenian sources and the astronomer Kharoṣṭha or, in Iranian form, \*Kharoṣtrag. There may not have been a country called Kharoṣtra or \*Kharoṣtrag, but there was a legendary name Kharoṣṭha or Kharoṣtrag, perhaps of Iranian origin, with a wide range of application in the countries adjacent to India in the north-west. It never was a name of Kāśyar, but we are indebted to Hui-yüan's unfortunate etymology for the possibility of a more correct appreciation of its past significance.

The Tibetan text mentioned above says that, after the Buddha Kāśyapa had preached the law and before the Buddha Śākyamuni was born in India, the sages Kha-ra-śva and Kha-re'u-steñ settled on a mountain of the country of Khotan (THOMAS, *Tibetan Texts and Documents*, I, 93). Just as Kha-re'u-steñ is obviously identical with \*Kharoṣtrag, which is given by Hui-yüan as the «correct» name of Shu-lo (Kāśyar), Kha-ra-śva (certainly the «Kharasha» of S. C. DAS in *JASB*, LV [1887], 194) seems to represent the same name as 迦邏沙摩 Chia-lo-sha-mo (\*Ka-lā-śa-muâ), which occurs in the *Sūryagarbha* as the name of Khotan at the time of the Buddha Kāśyapa (cf. LÉVI, in *BEFEO*, II, 250; v, 257). On the other hand, this last name has a fair chance of being the same which, in the glosses devoted by the *Fan Fan yü* to Fa-mêng's lost *Wai kuo chuan* (beginning of the 5th cent.; cf. *BEFEO*, III, 431-434), appears as 迦羅奢末 Chia-lo-shê-mo (\*Ka-lā-śja-muât) in an edition used by LÉVI (*JA*, 1915, I, 77), but as Chia-lo-shê-mu [木] (\*Ka-lā-śja-muk) in *Taishō Tripit.* 54, 1037. The translation of the name given in the *Fan Fan yü*, which is said to be provided by Fa-mêng himself, is 滿鷹金國, «Kingdom (kuo) full (man) of eagles (ying) and gold (chin) [or of eagle-gold?]». As one of the meanings of *khara* in Indian lexicons is «eagle», though not attested in any text, and although neither transcription shows an aspirated initial, LÉVI restored the first part of the name to Khara<sup>0</sup>, and this at least is confirmed by the Tibetan Kha-ra-śva. According to LÉVI, the whole name would be Khara-Śyāma, and represent the name of a village Khara, coupled with that of the country Śyāma or Śyāmāka. On the correctness of that restoration, LÜDERS (*Weitere Beiträge zur Gesch. und Geogr. von Ostturkestan*, in *SPAW*, 1930, 39) has expressed certain doubts which are based on the Chinese translation «full of eagles and gold»; I think that the very principle of the equivalence is unacceptable. In the *Sūryagarbha*, the village of Khara is reached by Kātyāyana when he leaves Rauruka; he then passes through the country of Lampāka, and only afterwards arrives at Śyāmāka. Khara and Śyāmāka occur in the same text, but they are not connected. Moreover, Chia-lo-sha-mo and Chia-lo-shê-mo suggest \*Kharāśma<sup>0</sup> rather than Khara-Śyāmaka; only the final is uncertain. The Chinese translation is difficult to interpret; many of the glosses are corrupt, and even when they are not, those attributed to Fa-mêng are often as fantastic as those which seem to be due to the compiler of the *Fan Fan yü*. My impression is that Fa-mêng took the name to have been formed of two elements, one being *khara*, which he interpreted as «eagle», and the other in which he saw some

Prākṛit form of *suvarṇa*, «gold» (for *-s-* and *-ś-*, cf. *soma* «moon», given as the original of a \**śūma*<sup>0</sup> or \**śoma*<sup>0</sup>, *ibid.* 1038; and for *-m-* and *-v-* cf. a \**suman*<sup>0</sup> interpreted as «gold» *suvarṇa*, *ibid.*, 1039). We can establish from another source that \**Khara-Śyāmāka* must be abandoned. As remarked by THOMAS (*loc. cit.* 93), the Tibetan translation, in the passage corresponding to the text of the *Sūryagarbha* which mentions Chia-lo-sha-mo, gives rDo-rcub «Hardstone», clearly representing an original Kharāśman. THOMAS adds that possibly there is some relation between the name Kharāśman and that of the sage Kha-ra-śva (Kharāśva); moreover, he is inclined to connect these names with that of Hwārizm. I do not feel much inclined to accept the second hypothesis; and I have no doubt that the first one is correct. Kharāśva and Kharāśman (Kharāśma) are in fact one and the same name, and we have only here one more case of the confusion between *-śva-* and *śma-* to which we owe the double forms *aśvagarbha* and *aśmagarbha*, *Aśvaparānta* and *Aśmāparānta*. They may be due to the hesitation of Buddhist compilers when rendering into Sanskrit Prākṛit forms in *-ssa-* (cf. *BEFEO*, iv, 379; *JASB*, 1925, 48-51). Whatever the case may be, the various texts concerning \**Kharoṣṭra* (\**Kharoṣtrag*) and \**Kharāśman* provide an interesting example of the way in which, in the first centuries of our era, traditional names which had perhaps quite a different value originally became connected with Chinese Turkestan, with occasional precise references to Khotan and even to Kāśyar.

The name of Shu-lo has been adduced by MARQUART (*Erānšahr*, 282-284; cf. also STEIN, *Ancient Khotan*, I, 56) to explain the name of the convent of the kingdom of Kapiśī which is said in Hsüan-tsang's *Life* (JULIEN, *Vie*, 71) to have been built when a son of the Chinese Emperor was there as a hostage, and which was known under the name of 沙落迦 Sha-lo-chia (\**Ṣa-lāk-ka*). But, at the same time, MARQUART saw in Sha-lo-chia, restored as Šaraka, a name of the Chinese. The apparent contradiction seems to have been solved, in MARQUART's mind, by his idea that the name, to be read as \**Šaraka*, originally meant «Prince of Kāśyar», but in Hsüan-tsang's time was misunderstood as meaning «Chinese». As a matter of fact, the contradiction is in the names themselves. The name of Sarag, Skr. Saraga, is now well attested as that of Lo-yang, the eastern capital of the Han and the T'ang, but Sha-lo-chia can only be \**Šaraka* or \**Šalaka*; to obtain the name of China, or of Lo-yang, we ought to correct 沙 *sha* to 莎 *so* (\**śud*) or rather to 娑 *so* (\**sā*). If we retain Sha-lo-chia and connect it with the form Sha-lo used by Buddhist pilgrims for Shu-lo, \**Šaraka* will hold good, but then the difficulty is that the *Life* speaks of a son of the Chinese Emperor, not of a son of the king of Kāśyar. I still hesitate between the two solutions (cf. my remarks in *JA*, 1927, II, 139-141, and also *Mi*, 225; MINORSKY's objection to the surprising transcription of *g* or *gh* as ɟ by early Arab writers may perhaps be answered by supposing that the Arab transcription is based on \**Sarak*). I do not believe in MARQUART's \**Šarakavati*.

I should perhaps feel more inclined to accept \**Šaraka* as a form derived from Shu-lo or Sha-lo if we knew more about this name (MARQUART's restoration as \**Šarak* in *Wehröt und der Fluss Arang*, 68, is possible, but not proved). It occurs in Tibetan texts of about the 10th cent. as Šu-lig (cf. THOMAS, in STEIN, *Ancient Khotan*, 581, 582), or sometimes Šu-leg (THOMAS, *Tibetan Texts and Documents*, 118), but these appear to be forms merely borrowed from the Ch. Shu-lo. BAILEY (*BSOS*, VIII, 883) says that Sūlya «seems, in a document from the Khotan region, to mean the people of Kāśyar». We must wait until we know more about it, since the initial is neither

that of Ch. Shu-lo or Sha-lo, nor of Tib. Šu-lig (BAILEY speaks of «Tib. Su-lig»; it is a slip or a misprint). We should rather expect Sūlya to be a designation of the Sogdians.

A last explanation of Shu-lo or Sha-lo has been proposed by HERRMANN in Sven HEDIN, *Southern Tibet*, VIII, 448 : that name of Kāšyar would originally represent «Sogdak (Sogdian-City)». It is true that Suydī had become Sulī when Hsüan-tsang passed through ancient Sogdiana in the 7th cent., and attempts have been made to trace to the name of the Sogdians that of the Çulika of Indian texts. But we must not forget that the form Shu-lo goes back to the 2nd cent. B. C., and that the forms Suyd, Suydak (or Soyđ, Soydak) are the only attested ones, particularly by Chinese transcriptions, down to c. A.D. 500. On the other hand HERRMANN's hypothesis does not find the slightest support in what we know of the history of Kāšyar.

Several other names have been said to be names of Kāšyar without sufficient reason. Such is the 伽舍羅 Ch'ieh-shê-lo (\*G'ja-śja-lâ) of the *Shui-ching chu* equated with Kāšyar by HERRMANN, in *Southern Tibet*, VIII, 438. The name must have been known in China in the second half of the 4th cent., but it is of doubtful form, being also written Chia [迦]-shê-lo (\*Ka-śja-lâ) and even Ch'ieh-shê-lo-shih [迦] (\*G'ja-śja-lâ-źjâi), abbreviated to Lo-shih (Lâ-źjâi); cf. *Shui-ching chu*, ed. of WANG Hsien-ch'ien, 2, 5 a. The location is no more certain than the name, but the description in the *Shui-ching chu*, quite independent of that devoted further on to Shu-lo (2, 10 a), makes it very unlikely that Shu-lo was meant.

The same objection applies to 奇沙 Ch'i-sha, in which CHAVANNES proposed to see another name of Kāšyar (BEFEO, III, 432-433). In CHAVANNES's translation, the pilgrim Chih-mêng had at Ch'i-sha, with the bowl of Śākyamuni, the same miraculous experience which Kumārajīva had with the same bowl, at about the same time, at Sha-lo, i.e. Kāšyar. The coincidence appeared striking enough to carry STEIN's conviction, who declared that CHAVANNES's argument left no room for doubt (*Ancient Khotan*, I, 67). I hold, however, a different view. The name Ch'i-sha (\*G'jje-ša) is probably a transcription, though it has the disadvantage of having too obvious a meaning «Marvellous Sands» in Chinese. A kingdom of Ch'i-sha is mentioned in the *Pei shih* (97, 10 a-b; *Wei shu*, 102, 8 a), in connection with other unknown names of Western countries. When China created in 659-661 a new administrative nomenclature for the countries situated to the west of Chinese Turkestan and resorted for that purpose to old geographic names which she applied in a more or less haphazard manner, the name of Ch'i-sha was given to a district corresponding to Gūzgān, between Marv and Balkh (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 71, 278). The authors of that new nomenclature were more probably indebted for the name to the notice in the *Pei shih* (as in many other cases) than to Chih-mêng's little-known account. We have no direct indication as to the true location of Chih-mêng's Ch'i-sha, which occurs in the text incidentally, among place-names of northern India, not of Chinese Turkestan. If CHAVANNES thought of Kāšyar, against what the text, as he translated it, would seem to imply, it was only because an identical legend was attributed to Sha-lo (Kāšyar) in Kumārajīva's life and to Ch'i-sha, according to CHAVANNES, in Chih-mêng's fragmentary account. Nothing could be built on such a coincidence. The text of Chih-mêng's account which is given in his biography of 519 is only a summary, clearly corrupt in some cases (there are many independent quotations of the original account, which would be worth collecting); on the other hand, Kumārajīva's biography abounds in legendary

details. There was a tradition in the Buddhist world concerning the miracle of Buddha's bowl. Different places claimed to possess the bowl, and it was only natural that every one of them should insist on the miraculous property which was a proof of the authenticity of *their* relic.

There is, however, a detail which may provide a solution, or at least a clue towards it. According to CHAVANNES, Chih-mêng not only saw in Ch'i-sha Buddha's bowl, but also his spittoon. A few years earlier, Fa-hsien had seen the bowl at Peshawur, but the spittoon at 竭叉 Chieh-ch'a (\*G'jät-tš'a). Here again, CHAVANNES thought that Chieh-ch'a was Kāšyar and his deduction met STEIN's approval. But such a result is only reached by doing violence to Fa-hsien's text. The trend of the itinerary is quite clear, and leads directly from Khotan to the Pamir without going north to Kāšyar. Moreover, Fa-hsien's description of Chieh-ch'a is that of a mountainous district, «in the midst of the Onion range» (LEGGE, *Travels of Fâ-hien*, 23). I think therefore that Kāšyar is to be excluded.

It is probable, however, that Chih-mêng's Ch'i-sha (in such a case it is out of order where it appears in the biography) and Fa-hsien's Chieh-ch'a are one and the same place, and they may perhaps, as supposed by HERRMANN (*Southern Tibet*, VIII, 438), be identical with the valley (谷 *ku*) of 岐沙 Ch'i-sha (\*G'jię-ša) of the *Shui-ching shu* (2, 5 a). That valley would probably suit the location which we must roughly suppose for Fa-hsien's Chieh-ch'a. Unfortunately, the passage on the valley of Ch'i-sha belongs to the obscure section which also mentions Chia-shê-lo. But whether in the Pamir or on its eastern outskirts, the valley of Ch'i-sha cannot be Kāšyar.

The true explanation is, in my opinion, the one which LÉVI gave in *BEFEO*, v, 296-297; CHAVANNES's translation is not accurate. In Chih-mêng's biography, the pilgrim arrives at the kingdom of Chi-pin (Kashmir and Gandhāra); the biographer then recalls that Chih-mêng «formerly» had seen Buddha's spittoon at Ch'i-sha and goes on to say that «in this country», *i. e.* in Chi-pin, he saw the bowl. The word «formerly» occurs in the account of the *Ch'u san-tsang chi-chi*, which is of the same date as the *Kao sêng chuan* translated by CHAVANNES; but LÉVI is right when he says that even without it, the same translation would be necessary. This explains why, in the summary account which has come down to us, the kingdom of Ch'i-sha occurs in a part which refers to north-western India. It was not in India, but neither was it in Kāšyar. Chih-mêng, like Fa-hsien, saw the spittoon in the region of the Pamirs. As to the bowl, like all the other pilgrims of the 5th cent., he saw it in Chi-pin. The only exception occurs in the biography of Kumārajīva, which speaks of the bowl as being at Shu-lo (Kāšyar); but the text is of very doubtful authority.

After such negative results, we arrive at a name the identification of which is not doubtful, although, in my opinion, the facts have not been shown in their true light. Hsüan-tsang describes the country the traditional name of which was Shu-lo, *i. e.* Kāšyar, under the name of 去沙 Ch'ieh-sha (\*K'ja-ša), which undubitably, for that very strict phonetician, renders an original Khaša (cf. JULIEN, *Vie*, 272, 277; *Mémoires*, II, 219; it is to be regretted that CORDIER should have reproduced in *Y*, III, 41, the absurd note in which PARKER questions the identity of Hsüan-tsang's Ch'ieh-sha with Kāšyar, seeing in Ch'ieh-sha a name of Keš; but Hsüan-tsang's Śitā is the Tarim River; as to Ch'ieh-sha being given in the *Hsin T'ang shu* as another name of Keš [cf. CHAVANNES, *Doc. sur les Tou-kiue*, 146], it is probably an error of the compilers of the

*Hsin T'ang shu* who, under the influence of phonetic analogy, duplicated the name which they found in Hsüan-tsang for Kāšyar). Hsüan-tsang's notice, in this case as in many others, has been taken over by the compilers of the *Hsin T'ang shu*, who say that Shu-lo (Kāšyar) was also called Ch'ieh-sha, but add that the king resided in the city of 迦師 Chia-shih (\*Ka-ši); cf. CHAVANNES, *ibid.* 121 (the 迦瑟 Chia-sê in STEIN, *Ancient Khotan*, 66, is a slip; Chia-sê is quite another name, which just comes before Chia-shih in the Index of CHAVANNES's book, p. 336). STEIN (*loc. cit.* 48) says that in Ch'ieh-sha «it is impossible to mistake the phonetic rendering of some form connected with the present Kāshgar», and that the name of the capital Chia-shih «is evidently also closely related in origin». But I doubt that there should be any original connection between Ch'ieh-sha and Chia-shih.

The original of Ch'ieh-sha, Khaṣa, is perfectly plain; it is the name so frequently found in Indian texts in the form Khaśa, Khaṣa, Khaṣa, as a designation of a number of hill-tribes of the Himalayan regions. Already in the 18th cent., scholars connected the name with Ptolemy's *Κάσια ἔρη*; nowadays, the Khakha tribes to the west and north-west of Kashmir are Khaṣa; the Gurkhas of Nepal call themselves Khas and their language is known under the name of the Khas language as well as under that of Parbatiya (STEIN, *Ancien Khotan*, 50-51; LÉVI, in *BEFEO*, iv, 556). BURNOUF had thought of connecting the name of the Khaśa with that of Kāšyar. STEIN is right in his objection that Kāšyar is far from the ordinary seats of the Khaśa tribes, but the fact is that a moment came when the equivalence Khaśa or Khaṣa = Kāšyar became current in Central Asia. As I have said above, the original word of the *Avatamsaka* which was rendered Shu-lo (Kāšyar) in the translation of 696-699 must have been Khaśa; but the earlier translation of the 5th cent. rendered the same name with a vague 邊夷 *pien-i*, «Barbarians of the frontier», and a parallel text of the *Sūryagarbha*, translated c. 600, gave Yü-tien, Khotan; the Tibetan version of the *Sūryagarbha* specifies that Khotan was in the land of the Khaśa (cf. LÉVI, in *BEFEO*, iv, 546-547, 556). In the list of *yakṣa* of the *Mahāmāyūrī*, the name of the land of Khaśa is simply transcribed 迦舍 Chia-shê (\*Ka-śja) in the translation of the beginning of the 6th cent. (due to a bad phonetician), but is rendered Shu-lo (Kāšyar) in the translation of I-ching (705) and half a century later by Amoghavajra (cf. LÉVI, in *JA*, 1915, i, 52, 102). This equivalence did not originate with Hsüan-tsang, since in the list of writings of the *Lalitavistara*, the translation of 587 transcribes Khaṣa as 珂沙 K'o-sha (\*K'â-ṣa), but adds that it is Shu-lo, Kāšyar (cf. LÉVI, in *BEFEO*, iv, 575; the translation of 308 transcribes 怯沙 Ch'ieh-sha and that of 683 可索 K'o-so [\*K'â-sâk], both without translation). The form used in the translation of 308 shows that Hsüan-tsang was not the first to use those two characters for Khaṣa. I think it is even more ancient than 308, and goes back to the version of the *āgama* made in the 3rd cent. (cf. NANJĪŌ, No. 547; and the quotation in ch. 43 of the *Fa-yüan chu-lin*).

While the name of the Khaṣa often occurs in Chinese translations from the 3rd cent. onwards, we have so far no indication that it was brought into connection with Kāšyar before the end of the 6th cent. The new identification was evidently not due to purely geographical or historical reasons, but must have been suggested by phonetic analogy between the names. The inference may be drawn that the name «Kāšyar», the first part of which resembles Khaśa, Khaṣa, is perhaps not much older than A. D. 600.



It is that name Kāšyar which appears in the *Hsin T'ang shu* in the form Chia-shih, but this is an apocopated rendering which we can trace back to its full form. In the account of the travels in India of 慧超 Hui-ch'ao which I brought back from the Tun-huang caves, it is said that Hui-ch'ao, in 727, arrived at «Shu-lo, the native name of which in foreign countries is 伽師祇離 Ch'ieh-shih-ch'i-li (\*G'ia-ši-g'jiē-ljiē»); cf. *Tripit.* of Taishō, 51, 979. Hui-ch'ao's work, of which we have only this incomplete and sometimes faulty manuscript, has been briefly commented upon by 慧琳 Hui-lin (737-820), himself a Kāshgarian, in his *I-ch'ie ching yin-i* completed in 817 (ch. 100; Tōkyō *Tripit.* of Meiji, 爲, x, 104 b). There the name of Kāšyar is written 迦師信梨 Chia-shih-chi-li (\*Ka-ši-g'jēt-liei), and I have no doubt that Hui-lin has preserved the correct form, at least as far as the first character is concerned. It is indirectly confirmed by the apocopated form which is given in the *Hsin T'ang shu* and which probably comes from Hui-ch'ao's account. Hui-lin adds that it is a 胡 Hu (at that time generally = Iranian) name, and that the Chinese («T'ang») name for it is 蔥嶺鎮 Ts'ung-ling-chên, «Garrison of the Ts'ung-ling». Kāšyar was one of the «Four Garrisons» of the T'ang in Turkestan, although it had already been lost to China when Hui-lin wrote his commentary; and it was the «Garrison» which was particularly intended to protect the Ts'ung-ling or «Onion Range». Hui-ch'ao uses «Ts'ung-ling-chên» in a general way, but I do not think that there has ever been such an official name, and Hui-lin may have meant to give an explanation of \*Kašgiri, not its equivalence with Shu-lo already expressed in Hui-ch'ao's text. The more so since, at the end of the subsisting portion of Hui-ch'ao's account, we find a list of the «Four Garrisons» where that of Kāšyar appears under its ordinary Chinese name Shu-lo.

Ch'ieh-shih-ch'i-li or Chia-shih-chi-li must evidently be restored as \*Kašgiri, and this text of 727 provides us with the earliest mention of the very name «Kāšyar»; we have seen above, however, that it must then have been in existence for more than a century. Various attempts have been made to explain it. The one quoted by CORDIER (*Y*, I, 183), according to which Kāšyar is formed of «*kash*, fine colour, and *gar*, brick house», is the one started in 1763 by the compilers of the *Hsi-yü t'ung-wên chih* (III, 13-15), who write the name Qašiqar in Mongolian, but Qašqār in Turkī, and go on by saying that, in Turkī, \*qaši means «motley», and \*qar, «brick-house». WATTERS (*On Yuan Chwang's Travels*, II, 292) commented on the Chinese explanation. According to him, «there is a Turkī word *Kasha* (or *Kashka*) which means 'variously coloured', but *gar*, in Mongolian *ger*, stands perhaps for the Chinese interpretation of the Hindu word *ghar* which means 'a house'. But there is no word *kaša* meaning «variously coloured» in any Turkī dialect, and *qašqa* (also *qačqa*, *qasqa*; *qašya* in Kāšyari) is only used for piebald animals. I think that, in spite of the «Mongolized» spelling (see *infra*) adopted in Turkī by Ch'ien-lung's Commissioners for the name of Kāšyar, they were given a «pandit's» etymology actually based on the true *k-* initial, and that their \*qaši is the Pers. کاشی *kāshī*, «enamelled brick». \*Qar, «brick-house», is unknown too in Turkī. One may think of Mong. *gār*, «house» (but not «brick-house»), but *gār* can of course have nothing to do with Hindī *ghar* (<Prākṛit *ghara*; cf. Skr. *grha*), «house». Since Mong. *gār* is not of the same «class» as *-qar* (*-yar*) of Qašqar, Kāšyar, and also for want of a suitable word either in Turkī or in Persian, I incline to the view that the word meant by «brick-house» may be the Hindī *ghar*, carried to Kāšyar by Hindu traders; but

one would like to find elsewhere some confirmation of the fact that *ghar* was more or less known in the region towards the middle of the 18th cent. Whatever the case may be, neither Pers. *kāshī*, nor Mong. *gār* or Hindī *ghar* can actually be the source of the name « Kāšyar ».

VIVIEN DE SAINT-MARTIN's remark (in JULIEN, *Mémoires*, II, 427) that « *gar* is the common ending of a great number of place-names in the dialects of northern India, with the meaning 'city' » has been simply repeated by FRANKE (*SPAW*, 1903, 186, who erroneously credits JULIEN with it), but is ignored by STEIN, and rightly, I believe; the name has no chance of being Indian. THOMAS (*Tibetan Texts and Documents*, I, 166) also speaks of « *gar* », which is « perhaps fundamentally the same as Tibetan *sgar*, *gar*, 'encampment' » and « was, no doubt, widely used in Tibeto-Burman dialects »; Kāšyar is one of the names adduced. I am afraid that much of THOMAS's list must be left out, for instance « rGya-gar », India, « Pho-dkar », which I believe to be Bokhara, and « Kāšyar » itself.

RICHTOFEN's attempt at explaining the first part of « Kāšyar » from the Turk. *qaš*, « jade », was opposed by STEIN (*loc. cit.* 50), who however, like RICHTOFEN, felt inclined to connect with *qaš* the name of the *Κάσια ὄρη*. STEIN's opinion was that the name of the *qaš* stone may have come to the Greeks from the Great Yüeh-chih « who probably spoke a language of the Turki-Mongolian family ». MARQUART, on the other hand, saw in *qaš* the outcome of « an old *vrddhi*-form \**khāša* = stone from the Khaša or *Κασία* land » (*Wehröt und der Fluss Arang*, 68; *Ueber das Volkstum der Komanen*, 201). HERRMANN (art. « Kasia » in PAULY-WISSOWA) repeats MARQUART's etymology *Khāša* > *qaš*, and considers as established that the *Κάσια ὄρη* owed their name to *qaš*, jade (cf. also my remarks in *TP*, 1930, 299, and HERRMANN, *Das Land der Seide und Tibet*, 141-142). I do not believe in the Turco-Mongolian appurtenance of the Great Yüeh-chih, nor do I think it probable that *qaš* is derived from \**khāša*. It would rather appeal to me simply to connect Khaša and the *Κάσια ὄρη*. As to the word *qaš*, it occurs for the first time in 1076 in Kāšyarī (BROCKELMANN, 150); but its earlier history is still unknown (see also « Cotan »).

BURNOUF had the intuition of what I believe to be partly the true etymology of the name « Kāšyar », when he suggested to HUMBOLDT \*Khašagairi, « Hill of the Khaša » (cf. STEIN, *Ancient Khotan*, 50-51). Not that I think that *kāš-* has anything to do with Khaša, but the second half of the word seems to me to have the greatest probability of corresponding to Skr. *giri*, Avest. *gairi*, Khotan. *gari*, Wakh. *yar*, Yavn. *gor* < \**gar*. This may even be what Hui-lin meant when he rendered \*Kašgiri in Chinese as « Garrison of the Onion Range » instead of « Garrison of Shu-lo ». A natural inference would be to look also into the languages of the Pamir for a word meaning « onion » as the first component of \*Kašgiri. It just happens that the word for « onion » is *kašu* in Šhiṅ'a, *yašu* in Burušaski (cf. LORIMER, *The Burushaski Language*, III, 182). But these are not Iranian languages, and they have no word resembling *gairi*, *giri*, for « mountain ». Hui-lin's gloss is not bound to give us the literal sense of the whole name, and I prefer to consider the first element of \*Kašgiri as still undetermined.

But, alongside of \*Kašgiri, a short form based only on the first element seems to have been current in Sogdian. This is at least the conclusion we must accept if, as appears very probable, Kāše is to be understood as < Kāšak and means « of Kāšyar », « Kāshgarian », while the Q's'nč = Kāšānč of *Mahnāmag*, line 146, would represent its feminine form (cf. HENNING, in

*BSOS*, IX, 566-567). In the modern work *Hsin-mao shih-hsing chi* (5, 22 a) it is said that nowadays the Turks of Kāšyar often call the town in an abbreviated form K'o-shih (*i. e.* \*Qaš or \*Qaši?); this is adduced to explain the « short » forms Ch'ieh-sha (Khaşa) and Chia-shih of the T'ang period. But the author, who went from Tientsin to Urumči in 1891 never visited Kāšyar, and I hold the modern abbreviated form, which I never heard on the spot, to be artificial.

STEIN says (*Ancient Khotan*, 47, 48) that, in Mohammedan works, the name Kāšyar occurs in different spellings : كاشغر Kāšyar, كاشگر Kāšgar, كاشكر Kāškar, قاشقر Qāšqar, « in accordance with the phonetic latitude allowed by Turkī languages in respect of certain consonants ». But we must keep in mind that the spelling Kāšyar is the one used in Arabic by Ibn Al-Aθīr, in Persian by the author of the *Hudūd al-'Ālam*, in Turkish by the author of the *Qutadūyū bilig* and by Kāšyarī, that is to say by the most ancient sources. Forms like Kāškar, Kāšgar and Qāšqar, when they are not misreadings (few mss. distinguish between *k* and *g*, and *-q-* is easily confused with *-γ-*), far from being the result of a « phonetic latitude » in Turkish, are more likely to be the result of the general reluctance to have in Turkish or Mongolian, in the same word, sounds of different classes like *k* (or *g*) and *q* (or *γ*). On the other hand, changes between *k* and *q* or *h* are common in Persian. In the beginning of the 14th cent., Abū-'l-Fidā gives the two spellings Qāšyar and Kāšyar, but all the ancient quotations given in his notice only have Kāšyar (REINAUD, *Géogr. d'Aboulféda*, Arabic text, 504-505; transl. II, II, 229-230). Secondary forms كاشجر Kāčyar and كاشجر Kāčyar are quoted by VULLERS (II, 772) from the *Burhan-i Qāfi'*. I do not know any instance of « Kāčyar », which may be only due to the influence of the sonant *γ* on the preceding *š*. But « Kāčyar » is attested at an early date in an adjective of origin Kājyarī (? or Kāčyarī). The father of the first historian of Kāšyar, who outlived his son by ten years and died in 1093, was called Ḥusain al-Alma'ī al Qājyarī (cf. BARTHOLD, « *Qāshghar* », in *EI*). I have no theory to offer to explain « Kāčyar ».

Before and during the T'ang dynasty, the royal family of Kāšyar, like those of Khotan, of Kučā and of Qarā-šahr, had received or adopted, in conditions which are still unknown to us, a regular Chinese surname, that is to say one of those in use among the Chinese themselves. The surname of the royal family of Kāšyar was 裴 P'ei. In the case of Khotan, the surname seems to have been chosen in more or less close agreement with a native dynastic title (that of Vijaya, hence the Chinese surname Wei-ch'ih or Yü-ch'ih; see « *Cotan* »); we do not know whether anything similar occurred in the case of Kāšyar. It has often been said, and is still repeated in STEIN, *Ancient Khotan*, 62, that Kāšyar was conquered by the Arabs under Qutaiba in 715. But GIBB has tried to show (*BSOS*, II, 467 sq.) that the whole account of the campaign is legendary, a view endorsed by BARTHOLD (« *Qāshghar* », in *EI*).

When Chinese domination had been finally eliminated from the western part of Chinese Turkestan and a Turkish dynasty installed with one of its capitals at Kāšyar, the city received the new Turkish name of Ordu-Känd, « City of the Royal Camp ». This new name is mentioned by Al-Muqaddisī (cf. BROCKELMANN, 246), Al-Birūnī (cf. *Mi*, 280), Kāšyarī (BROCKELMANN, 246) and Abū-'l-Fidā (REINAUD, transl. II, II, 230); but it never superseded « Kāšyar » itself. HARTMAN, followed by BARTHOLD, maintains that from the Mongol period, the capital of the khans was not at Kāšyar, but at Yärkänd, and that Kāšyar's importance in modern times only dates from the

Chinese re-conquest in 1877. This view may be too radical, but the capital certainly was at Yärkänd at the time of Goes's visit in 1604.

Two of the earliest monuments of the Turkish language are connected with Kāšyar : the *Qutađyu bilig*, or «The Lore of the Royal Fortune» was written at Kāšyar in 1069, and the first great dictionary of the Turkish language, the *Divān lughat at-Turk*, completed in 1076, is the work of a man from Kāšyar, Kāšyarī.

The *Secret History*, compiled in 1240, gives the name of Kāšyar in the form «Kisqar» (§ 263); but is probably a misreading of the transcribers of the second half of the 14th cent. They did not recognize or did not know the name, and Mongolian writing does not distinguish between *-s-* and *-š-*; on the other hand, no distinction was then made in transcription between *-γ-* and *-q-*. As to the *-i-* of the first syllable, it might be considered as an easy misreading *-i-* for *-a-*, but in such a case we ought to assume the same misreading in one of the forms occurring in YS. It is not surprising, since in Mongolian *ki-* is «neutral» and can occur in words containing *-γ-* or *-q-*, whereas *ka-* is excluded. The form really given in the original text of 1240 must have been either Kišyar, or more probably Kāšyar.

The transcriptions in YS are many (cf. *Br*, II, 45-47), but resolve themselves to two original forms : 可失合兒 K'o-shih-ha-êrh (YS, 15, 5 a), 可失哈耳 K'o-shih-ha-êrh (YS, 63, 15 b and map of c. 1330), 可失哈兒 K'o-shih-ha-êrh (YS, 120, 7 a), 甲石哈 Chia-shih-ha (YS, 122, 1 a-b) and 可失哈里 K'o-shih-ha-li (YS, 180, 2 a; *Br*, I, 162) render Kāšyar; 合失合兒 Ha-shih-ha-êrh (YS, 8, 4 a) represents Qāšyar, altered according to the requirements of Mongol euphony. The abnormal 乞失哈里 Ch'i-shih-ha-li, apparently Kišyar, of YS, 123, 2 a, is probably due to a misreading of an original Kāšyar in Mongolian writing.

Polo says that Kāšyar was subject to the Great Khan, as he does in the case of Khotan, but states that Yärkänd was subject to a nephew of the Great Khan who must be Qaidu (see «Cotan», «Yarcān», «Caidu»). Surprise has been expressed at Polo's statement, for the reason that the boundary between Qaidu's territory and the Great Khan's lay between Qarā-šahr and Qomul, so that the whole of the western part of Chinese Turkestan must have belonged to Qaidu (*Y*, I, 183; *Br*, II, 47). This is not quite a true representation of the situation which obtained in Polo's days. It is true that Rašidu-'d-Dīn speaks of Qarā-ḥōjo (to the east of Turfan) as keeping somehow neutral between Qubilai and Qaidu, and also that, on the map of c. 1330, Kāšyar and Khotan are listed among the possessions of Düräi-Tämür, the then head of the house of Čayatai. But it is no less certain that, to the north of the T'ien-shan, Qubilai's forces held Almaliq in Ili, and that the Great Khan had established postal relays on the whole route from Tun-huang to Kāšyar *via* the Lop-Nör (see «Ciarcian», «Lop»); he could even send men to work for him in the mines of Badaḥšān (see «Badascian»). Under such conditions, the surprise should not be so great to be told that Kāšyar and Khotan belonged to the Great Khan as to hear of a different regime for Yarkänd. Prince Hoqu's rebellion, which for a time wrenched the region of Khotan from the rule of Qubilai, must have taken place shortly after the passage of the Polos (see «Cotan» and «Badascian»), and, moreover, would not affect the respective status of Yärkänd and Khotan. Perhaps Yärkänd, while included within the dominions under the direct administration of the Great Khan, had been given as an appanage, at least for

the revenue to be derived from it, to a prince of the collateral branches. An error of Polo is also possible.

From the middle of the 14th cent. down to 1514, Kāšyar was ruled by the Duylat family; a new dynasty then began, and maintained itself until the second half of the 17th cent. I cannot enter here into the details of the modern intercourse and the conflicts of Kāšyar with the Kalmuks and the Chinese. The main source for the history of Kāšyar in the 14th-16th cents. is Mirzā Ḥaidar's *Ta'riḥ-i Rašidi* (transl. by N. ELIAS and D. ROSS, *A History of the Mongols of Central Asia*, 1895); cf. also BELLEW, *History of Kashgar*; Br, II, 245-246.

The local rulers of Kāšyar exchanged embassies with the Ming dynasty. In its paragraph on 哈實哈兒 Ha-shih-ha-êrh, Qāšyar (the «Mongol» form of the name), the *Ming shih* (332, 9a) mentions diplomatic intercourse in 1408, 1413, 1426-1435 and 1463 (cf. Br, II, 245). In the Ming itinerary preserved (already corrupt) in the *Pien chêng k'ao* of 1547 (Peiping National Library ed. 8, 8a), a city 怜失怜力 Lien-shih-lien-li is mentioned, and 50 li to the west of it a city 失哈力 Shih-ha-li. There is no doubt that Lien-shih-lien-li is corrupt for 哈失哈力 Ha-shih-ha-li, Qāšyar, and the correct form is given in the independent, though later copy of the same itinerary translated by BRETSCHNEIDER in *China Review*, v, 235. Although the next name, Shih-ha-li, is given in both texts and may be correct, I do not entirely dismiss the idea that it may also be altered from Ha-shih-ha-li, and represent a wrong duplication of Kāšyar.

From the end of the 17th cent. onwards, the Chinese name of the city has been 喀什噶爾 K'o-shih-ko-êrh, which is no longer based on the Mongol «Qāšqar», but renders the Turkish Kāšyar. Nevertheless, while retaining the transcription K'o-shih-ko-êrh, the *Hsi-yü t'ung-wên chih* (3, 13-15) gives the following forms in the different languages: Mong. Qašiqar, Kalm. Qašiyar, Tib. Kha-ši-kar, Turki Qašqar. As a matter of fact, the modern pronunciation is Kāšyar in Russian Turkestan, but the name is generally spelt Kāšqar in Chinese Turkestan, and I have heard a «Sart» master say at Tashkend that the proper form should be Qāšqar, i. e. with the Mongol pronunciation, a survival of the Kalmuk rule (cf. also SHAW, *Vocabulary*, 154). «Qašiqar» is given in KOWALEWSKI's dictionary, drawing perhaps from Sino-Mongolian dictionaries. In Tibetan, the earliest mention I know of the modern name occurs in the chronicle translated by SCHLAGINTWEIT, *Die Könige von Tibet (Abh. d. Bay. Ak. d. W., x [1866], No. 3, 847)*, which speaks of «O-don-kas-dkar», i. e. «Odon (Khotan) and Qasqar (Kāšyar)».

According to Polo, there were in Kāšyar some Nestorian Christians, with a church (or with churches according to Z, which may be correct); the «many» Christians of Y, I, 182, based only on FB, is a variant which should not be retained. V's addition that these Nestorians observed «the Greek rule» is certainly arbitrary. As is usual in its text, Z says that these Nestorians were Turks; this detail may be original, as it falls in with Oriental texts which have a tendency to speak of the Nestorians of Central Asia as being Turks. RICCI-ROSS (*RR*, 63) and BENEDETTO (*B<sup>1</sup>*, 63) have translated «Among the Turks in this land...». This rendering is probably due to the idea that, all the inhabitants of Kāšyar being Turks, it would be absurd to give the impression that there were only in Kāšyar a few Turks, the Nestorians. But, if the many mentions of «Turks who are Nestorian Christians» in Z actually go back to Polo, we must consider that Polo does not speak of the Mussulmans of Central Asia as «Turks»; for him, the «Turks» of Central Asia seem

to be the non-Mussulmans, among whom there were many Christians. It is in fact quite possible that the Nestorians of Kāšyar were to a great extent Uighurs, who may have still used the Uighur writing instead of the Arabic script of the Mohammedans. In any case, « Among the Turks in this land . . . » is a correction, not a translation.

Syriac documents testify to the importance of the Nestorian community at Kāšyar. A list of the Nestorian metropolitan sees dating from the middle of the 13th cent. gives as the nineteenth see « Kāšgar » and as the twenty-fifth « Kāsemgar and Nuāketh »; another list of c. 1349 gives as the nineteenth see « Tark » (read « Turk ») instead of « Kāšgar » and as the twenty-fifth see « Kašimghar » and « Navaketh » (cf. *Y*<sup>1</sup>, III, 22-24; *Br*, II, 47; Stein, *Ancient Khotan*, 71-72; *Mo*, 21). There can be no doubt that a wrong duplication has taken place, and probably Kāšyar occurs only as the twenty-fifth see, or rather as one of the two seats of that see (such double seats of bishoprics or archbishoprics often occur in the Nestorian church). « Nuāketh » or « Navaketh » is of course a place the Iranian name of which means « New City ». The *Hudūd al-Ālam* mentions a city Navākat in the neighbourhood of Tūnkat, in the valley of the Chu River. MINORSKY (*Mi*, 289) correctly identified Tūnkat with the 頓建 Tun-chien (Tūnkānt) of Chinese texts, but could not locate Navākat. It is however evident that it is the 新城 Hsin-ch'êng of Chinese texts, 60 li east of Tūnkat, the name of which means « New City » (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 10). But this « New City » is in Russian Turkestan, and the joint see of Kāšyar might be expected to be entirely in Chinese Turkestan. In the *Hudūd al-Ālam*, a city « Navīkath » is mentioned « on the bank of the river », after other places in the region of the Lop-Nōr. MINORSKY (*Mi*, 234-235) has felt some hesitation as to its location, but rightly recognized that it was a Sogdian form, Nōč-kāθ, « New City », and thought of the old Sogdian colony south of the Lop-Nōr to which I have devoted a short paper in *JA*, 1916, I, 111-123. But it escaped him that in my paper (p. 119, 121-122), there were two mentions in the Lop region of a city 新城 Hsin-ch'êng, « New City », also called 弩支城 Nu-chih-ch'êng, « Nu-chih City ». *Nu-chih* must be a rendering of *nōč*, and the whole Sogdian name must have been Nōč-kāθ. One of the Chinese texts is of c. 750, the other of the end of the 8th cent., while the *Hudūd al-Ālam* was completed in 982-983. The « New City » had been founded by Sogdian immigrants south of the Lop-Nōr in the first half of the 7th cent. Although we cannot follow its history after the 10th cent., it is quite possible that, owing to its Sogdian origin, it remained a partly Christian centre for several centuries longer, and was with Kāšyar the joint seat of the Nestorian metropolitan see in the Tarim basin. « Nuāketh » or « Navaketh » would represent the Persian form of the Sogdian Nōč-kāθ. The only difficulty is that, with the exception of the Syriac list (if the identification be correct), we have no other mention of the city in the Mongol period, at least under that name.

As to « Kašimghar » or « Kāsemgar », something must have gone wrong with the text. The form Kāšyar, which I have quoted above from early Mohammedan authors, is also the only one used in the Mongol period by Ĵuwainī, Rašīdu-'d-Dīn, Waśśāf, the author of *Nuzhat-al-Qulūb*, etc. What is more, the Syriac writers were aware of it. Not only have we « Kāšgar », correctly written, though wrongly inserted in the Syriac list of the middle of the 13th cent., but the name occurs for instance in the history of Mār Yahbalaha III (cf. BUDGE, *The Monks of Kublāi Khān*, 139 : « Kashkār »). It seems impossible that the Nestorian patriarchate should not have had a

correct list of the names of its metropolitan sees. ASSEMANI's compilations have been of the highest service, but one cannot help wishing that such important documents as these lists were critically examined anew.

Polo's notice of Kāšyar raises another interesting problem. The people of the country, according to him, spoke a language of their own. STEIN (*Ancient Khotan*, 70) wondered at this, because the fact that the *Qutaδyu-bilig* had been written in Turkish at Kāšyar in 1069 should have led us «to conclude that Turkī was in Marco Polo's days, as it is now, the language current in Kāshgar». Polo says nothing of the same sort for Yārkānd, Khotan or Čärčän.

Different solutions may be suggested. Several mss., omitting «the people of the country», make the remark on the language apply to the Nestorian Christians (cf. vol. I, 143). As MOULE says, they «may possibly be right», although I do not think it is very probable. I feel more inclined to the view that Polo, arriving from Iranian-speaking countries, found in Kāšyar people who spoke Turkish, and noticed the change of language. He abstained from doing the same afterwards, because Yārkānd, Khotan and Čärčän spoke the same language as Kāšyar.

But there is also the possibility, however remote, that a native Kashgarian tongue, not Turkish, should have still been used at Kāšyar in Polo's days. While we now have very precise information as to the native languages spoken at Kučā and at Khotan in the second half of the first millennium A. D., the early ethnic or linguistic connections of the Kashgarians in the same period still escape us almost completely. Hsüan-tsang says of the people of Kāšyar that they tattoo their bodies and have green eyes (文身綠睛 *wen-shên lü-ching*; cf. JULIEN, *Mémoires*, II, 220). The pilgrim mentions this last characteristic only for another nation, the people of Waḥan in the Pamir, «the majority of whom have blue-green eyes (碧綠 *pi-lü*), by which they are different from other people» (JULIEN, *Mémoires*, II, 201; both passages have been copied in the *Hsin T'ang shu* [cf. CHAVANNES, *Doc. sur les Tou-kiue*, 121, 165], but only with *pi*, «blue-green», as the colour of the eyes in both cases). Hsüan-tsang adds that «the language they [*i. e.* the Kashgarians] speak and the pronunciation are different from those of other countries». Such a statement falls in remarkably well with Polo's remark, but we must add that, in Hsüan-tsang's time, Yārkānd and Khotan also had languages of their own, duly noticed by the pilgrim, while Polo speaks of a special language only for Kāšyar. Moreover, six centuries had elapsed between the travels of Hsüan-tsang and those of Polo. It so happens, however, that the gap between the two is bridged over by a man who had authority to speak of Kāšyar since he was born there, Kāšyarī. If Polo by the «language of their own» of the Kashgarians really meant something else than Turkī, Kāšyarī cannot be said in a sense to confirm such a statement, since in Kāšyar itself, according to Kāšyarī, «royal» (*ḥāqānī*) Turkish was used. But Kāšyarī adds that in the districts of Kāšyar, a non-Turkish language was spoken, called *kānjāki* (cf. *Mi*, 280; BARTHOLD, *12 Vorlesungen*, 82, 135; or *kānčäki*?). In the Mongol period, *Kānjäk* or *Kānčäk* is frequently mentioned as a place which was somewhere to the north-west of Talas (cf. Waśśāf, in *Ha*<sup>2</sup>, 24 [misread «Kānjäl»], 127 and 128 [misread «Künjük»]; Juwainī, II, 248 [left as كجك by the editor]); it must be Kāšyarī's *Kānčäk-sāngir* (BROCKELMANN, 245). Although he speaks of the *kānjāki* (or *kānčäki*) as a non-Turkish language, Kāšyarī includes the *Kānčäk*, whom he often mentions, among the Turkish tribes. They were known to him to the extent of listing a special verb *kānčäklänmäk*, «to dress

like the Känčäk» (BROCKELMANN, 104). Part of the Känčäk may have settled in the region of Talas, but Kāšyari's text leaves no doubt that Känčäk people also lived in the districts of Kāšyar, and this is confirmed from other sources. The name occurs after that of a man from Qaml = Qamul (see «Camul») in the Manichaeon fragment studied by HANEDA (*Mem. of the . . . Toyo Bunko*, No. 6, 3), although the editor does not seem to have seen in it a proper name. In the Tibetan chronicle of Khotan, *The Annals of the Li Country* (THOMAS, *Tibetan Texts and Documents*, I, 118), the «king of the Ga-'jag» invades Khotan territory, but is defeated; the king of Khotan sends him back to Šu-leg, i. e. Kāšyar. That Ga-'jag is meant to render a pronunciation \*Ganjag, and that the king of the Ga-'jag is the king of Kāšyar do not seem open to doubt, and CLAUSON (*JRAS*, 1937, 178) has already connected Ga-'jag with the name given by Kāšyari to the language spoken in the Kāšyar area. CLAUSON adds that, as a consequence, the name in Kāšyari ought to be transliterated Ganjak, and no longer Känčäk. «The name», CLAUSON adds, «is clearly Iranian, and the same as Ganja, the name of another well-known town at the other end of the Iranian area in the Southern Caucasus». I am afraid that such far-reaching conclusions are still premature. It is true that there are no -j- in Kāšyari's Turkish, or, to be more correct, that Kāšyari does not distinguish between چ č and چ j, and always writes j, but with the value of č; j was foreign to Middle Turkish. Consequently, we are right when we read Känčäk in Turkish that which is written Känjak by Kāšyari, even though the original name, if it be non-Turkish, may have actually been Känjak (moreover, a foreign -j- often gave in Middle Turkish -ž- or -z-). Very much the same may be said of g and k; an initial g- is quite exceptional in Middle Turkish; but in such a case, and also sometimes to prevent a confusion between words written identically, Kāšyari indicates that the word must be read with g-, not with k- (cf. BROCKELMANN, v, 61); no such remark is made about Känčäk. On the other hand, the Tibetan spelling does not permit a choice between Känčäk and \*Ganjag. There are many cases when a surd initial of a foreign name is rendered as a sonant in Tibetan; for instance the very name of the Turks becomes Drug, Dru-gu, Drug-gu in Tibetan. Tibetan writing does not admit of any final surd occlusives, so that a final -k is always transcribed -g in Tibetan. The vocalization Ga-'jag and not \*Ge-'jeg may be more significant; still, Tibetans have not always been very strict in that respect; their e is not the Turkish ä, and they have borrowed Uiy. *ämči* with the two spellings *am-či* and *em-či* (LAUFER, in *TP*, 1926, 489). In my opinion, Känčäk may represent an original Iranian name, which in its turn may be \*Kanjak or \*Kanjag, but this is only an hypothesis, and we have no reason to connect it with a name of the far-away Caucasus.

Of that ancient language of Kāšyar, Iranian or not, we do not possess any text, or none at least has been recognized as such hitherto. As far as I can remember, only one word is anciently said to be a specifically Kāšyarian form, and even this case is open to some doubt. As is well known, the Skr. *upādhyāya* > Prākr. *uwaḥhāa*, «master», through an apheretical form of the same type which has given Tamil *vāddyar*, is the remote original of 和尚 *ho-shang* or 和上 *ho-shang* (\**yuā-ziang*), the respectful title with which Buddhist monks are usually addressed in Chinese. From the point of view of Chinese phonetics, *ho* (\**yuā*), which is pronounced *wa* in Japanese, always renders a *va-* of Central Asian languages (but with a prothetic laryngeal opening); the transcription was made on a form with a nasalized final. Although the derivation is not open



to doubt, an original «munshee» (an Arabic word!) was proposed by EITEL (*Handbook*<sup>2</sup>, 186) and repeated by LEGGE (*Travels of Fâ-hien*, 58); an impossible derivation from Pers. *ḥwājah*, *ḥōjah*, «master», started many years ago by WATTERS (*Essays on the Chinese language*, 358), has recently again found its way into a memoir by TAKAKUSU (*BEFEO*, xxviii, 441); and the new *Dictionary of Chinese Buddhist Terms* by SOOTHILL and HODOUS (p. 253) still proposes «*vandyā* (Tibetan and Khotanī *ban-de*)».

It must be admitted, however, that Chinese Buddhist scholars of early date were also puzzled by the transcription. I ought rather to say the transcriptions, since, apart from the two spellings of *ho-shang* which are used almost indifferently and go back to the beginning of the 5th cent. (for instance in Fa-hsien's travels, LEGGE, 58, or in the 1st chapter of both NANJĪŌ, Nos. 1117 and 1125, with the corresponding *yin-i* by Hsüan-ying, reproduced by Hui-lin), there is also a form 和闐 *oh-shê* (\**yuâ-d'z'ia*; with a purely graphic variant 和闐 *ho-shê*), occurring in NANJĪŌ, No. 1082. The translation of the latter work is of uncertain date (there is a contradiction between what NANJĪŌ says in his notice of No. 1082 and his statement in App. II, 79; cf. also BAGCHI, *Le canon bouddhique*, 375; the attribution of the translation to Guṇavarman may be due to a confusion with the work of similar title described by BAGCHI, 373-374, as No. 1), but certainly prior to the 7th cent.

Hsüan-ying, who wrote in the middle of the 7th cent., seems to have taken it for granted that *ho-shê* and *ho-shang* were corrupt forms of *upādhyāya* current in «the kingdom of Khotan and others» (cf. Tōkyō *Tripit.* of Meiji, 爲, VII, 36 a, 58 a, 66 a, reproduced in Hui-lin's more comprehensive work, *ibid.* IX, 146 b, 173 a-b).

The pilgrim I-ching, towards the end of the same century, gives a different explanation in his *Nan-hai chi-kui nei-fa chuan* (translated by TAKAKUSU, *A Record of the Buddhist Religion*, 117-118). His text runs as follows: «*Upādhyāya*. . . In the Western countries (西方 *si-fang*), when recklessly addressing (汎喚 *fan-huan*) men of great learning (博士 *po-shih*), people always call them 烏社 *wu-shê* (\**uo-z'ia*); this is not a duly recognized term (典語 *tien-yü*). In all the Sanskrit texts of the *sūtras* and the *vinayas*, the term used is *upādhyāya*, the translation of which is «the master [giving] personal instruction». The kingdoms of the Northern countries (*pei-fang*) all address [the masters] as *ho-shang*, with the result that the translators became accustomed to that corrupt sound (訛音 *ê-yin*).» There is no doubt, from what follows in the text, that by «Western countries» I-ching means India, and by «Northern countries», the 胡 Hu countries of Central Asia, mainly Iranian, but also «Tokharian». A later work quoted in ODA Tokunō's *Bukkyō daijiten*, 1853<sup>3</sup>, although evidently based on I-ching's text, gives the Indian form 烏邪 *wu-hsieh* (\**uo-z'ia*); it goes on to deplore that a term referring to lay scholars should have come to be used for Buddhist masters, and insists that the proper form to be used is 拔底耶 *pa-ti-yeh*. As a matter of fact, this other apheretical form of *upādhyāya*, unknown or almost unknown in China, has been more or less in use in Japan, where it is pronounced *pateiya* (cf. ODA Tokunō, 1452<sup>2</sup>; *Hōbōgin*, 58).

The same Hui-yüan who wrote the note on \*Kharoṣṭrag, a contemporary of I-ching, has a note on *ho-shang* (Tōkyō *Tripit.* of Meiji, 爲, X, 115 a, 135 a; reproduced by Hui-lin, *ibid.* VIII, 139 b; used by Hsi-lin, *ibid.* VIII, 16 a): «The refined language of the Five Indies says *upādhyāya*.

But in the vulgar practice of those countries (*i. e.* India), [the *upādhyāya*] are called 嚧社 *wu-shê* (\**uət-źia*; *yün*[殞]-*shê* in EITEL and LEGGE is a misreading). In Khotan and Shu-lo (Kāšyar), people say 嚧社 *hu-shê* (\**yuət-źia*). Now this country (*i. e.* China), with a corrupt sound, says *ho-shang*. . . »

At the end of the 8th cent., the pilgrim Wu-k'ung came back from India; while in Kashmir, he had become a full monk. Speaking of his *upādhyāya*, the biography inserted at the beginning of the translation of one of the works he had brought back from India says that «at 安西 An-hsi, [for *upādhyāya*] people say *ho-shang*» (JA, 1895, II, 353); since the protectorate of An-hsi had its seat at Kučā, CHAVANNES concluded (TP, 1904, 380) that the form *ho-shang* had originated there.

A last text remains to be mentioned. Tsan-ning, who compiled by Imperial order the great collection of Buddhist biographies called *Sung kao-sêng chuan* (NANJIŌ, No. 1495), completed in 988, wrote a final dissertation to conclude the biographies of the translators. We find in it the following passage (Tōkyō *Tripit.* of Meiji, 致, IV, 81 a; cf. LÉVI, in BEFEO, IV, 562-563): «For instance, the Sanskrit says *upādhyāya*, Shu-lo (Kāšyar) says *hu-shê* (\**yuət-źia*), and Khotan, *ho-shang*. . . » Here, a form is expressly stated to be the one used in Kāšyar, and this is the reason for the present notes on *upādhyāya*. But is the form *hu-shê* specifically Kashgarian?

First of all, I-ching was mistaken when he distinguished *wu-shê* from *upādhyāya*. If «men of great learning» (*po-shih*) were popularly addressed in India by a Prākṛit form of *upādhyāya*, it is the exact counterpart of what occurred in Central Asia when the same *po-shih* used in I-ching's text passed into Uighur as *bahši* and became a designation of Buddhist masters (see «Bacsi»). Hui-yüan, who had not travelled in India like I-ching, had however in the present case a more correct idea of the real facts. I-ching's *wu-shê* (\**uo-źia*) would suppose \**uḥhā*, and Hui-yüan's *wu-shê* (\**uət-źia*), \**uḥhā*; that is to say, the reduction is stronger than in the ordinary Prākṛit form *uajjhā*; but *uḥhā* is precisely another attested Prākṛit form of *upādhyāya* (cf. PISCHEL, *Grammatik der Prākṛit-Sprachen*, pp. 116-117).

On the other hand, the note in Wu-k'ung's biography is not to be understood, as it was by CHAVANNES, as meaning that the form *ho-shang* started from Kučā. As it is clear from a following sentence concerning *karmācārya*, the biographer, when speaking of An-hsi, means the whole territory of An-hsi and the Four Garrisons, which include Khotan and Kāšyar as well as Kučā. As a matter of fact, the forms of *upādhyāya* known in «Tokharian I» are *uḥhā*, *upādhyā*, *opādhyā* (SIEG and SIEGLING, *Tocharische Grammatik*, §§ 93, 121, 149), that is to say, are not based on the Prākṛit forms, and I see no reason why the case should have been different in the closely cognate Kuchean. All that we can conclude from the note in Wu-k'ung's biography is that the form *ho-shang* was current in part at least of western Chinese Turkestan. We naturally turn then to Kāšyar and Khotan.

We have no text in Kashgarian, and I do not find a form of *upādhyāya* in the published texts in Khotanese. The *hu-shê* (\**yuət-źia*) which Hui-yüan gives as the form in Khotanese and in Kashgarian would suppose \**uḥhā*, and *ho-shang*, which he gives as a Chinese corruption of *hu-shê*, would be \**vajhā*; in other words, the Chinese «corrupt» form would come nearer the classical Prākṛit *uajjhā* than the intermediate form from which it is supposed to have been

derived. The same may be said of course of *ho-shê* (\**yuâ-d'z'ia*), \**vajhā*, not mentioned by Hui-yüan, and certainly as ancient as *ho-shang*. This leads us to think that *ho-shê*(\**vajhā*) and *ho-shang* (\**vajhañ*) may not have been borrowed from the Central Asian language which had *hu-shê* (\**ujjhā*). We are thus tempted to admit that Tsan-ning may not be wrong when he says that *hu-shê* (\**ujjhā*) is Khotanese and that *ho-shang* (\**vajhā*) is Kashgarian. Tsan-ning wrote in 988, at a time when the intercourse with Central Asia was no longer as active as it had been in pre-T'ang and T'ang times; but he was a competent Buddhist scholar, and had access to a number of earlier works which have now disappeared. There are in Khotanese many examples of apheretical borrowings of the type *upa* > *va-*, and of *-d-* > *-j-*, so that \**vajhā*, \**vajhā* might in principle be Khotanese forms. But we are told that the Khotanese form was in fact \**ujjhā*. Provisionally, I accept it as such, and take \**vajhā*, \**vajhā*, as being probably the only example of a Kashgarian form which has been determined hitherto. We must note that, while neither I-ching nor Hui-yüan prefix an *alif* or a laryngeal opening to the Prākṛit forms used in India, they give it both for the Khotanese and for the Kashgarian. This is, moreover, an almost general feature for Central Asian languages, and the Chinese transcriptions also show it for the Sogdian and for the Turkish.

## 124. CASSES

### *casses* F

The Calif of Bagdad took counsel *cum seç regisles et cum seç casses* (F); BENEDETTO (B, 20) has corrected it to *cun sez regulés et cun sez sajes*. FA has simply *à ses prestres de sa loy*. The correction to «regules» (=«regulés») had already been proposed by YULE (Y, I, 70), and is certain. But there is no support for «sajes», and I agree with YULE in seeing in «casses» an Oriental word. YULE, while inclining to *kašiš*, admitted the possibility of *qāqī*; I think *kašiš* alone can be accepted. It is true that the Arabic *qāqī*, Mussulman «judge», is met with in transcriptions such as «cazee»; but the mediaeval forms are in favour of a transcription «cadi», which is also represented by *ha-ti* of the Chinese. I think Polo would have pronounced it «cadi», and it may even be that the «chadi» of a passage in VB represents a genuine Polian term. Moreover the double *s* of «casses» is not in favour of an original with a *-d-* heard like a *-z-* (see «Chadi»).

But there is another reason for adopting *kašiš*. This word, borrowed from the Syriac, is *qiss* or *qasīs* (or *qisīs*) in Arabic, *kašiš* in Persian (YULE wrongly says in Arabic). Although applied mainly to Christian priests (cf. Pers. «chasis» translating *presbyter* in KUUN, *Codex Cumanicus*, 77), it is used, even in Persian, as a designation of the clergy of other creeds, and YULE has shown (Y, I, 70; *Hobson-Jobson*<sup>2</sup>, 169-170) that Western travellers have very often applied it to Mussulman divines (as in Clavijo, in a letter of Giovanni da Empoli, and constantly in Portu-

guese texts). Strictly speaking, a *kašiš* is a priest of the secular clergy. When speaking of Hang-chou, Waššāf mentions there the non-Mussulman clergy of «priests» (*kašišān*) without faith and «monks» (*rahbān*) without religion (cf. *Ha*<sup>2</sup>, 42; *Y*, II, 213). I think we have the same distinction in Polo. His «regules» (he uses the word elsewhere) are the «regular» clergy, the monks, and the «casses» must consequently be the «secular» clergy, the *kašiš*, the priests. But it is more difficult to say where he drew the line in Islām between «priests» and «monks».

The only point which is more or less embarrassing is that Polo rarely uses Oriental words without stating them as such or giving their explanation (except as proper names or parts of proper names, e. g. «cingsan», «sangon»). It may be that something is missing here or elsewhere in our mss. in regard to «casses». Moreover, I am not certain how we should interpret Polo's transcription. The Arabic قيس would be *qiss*, not *qass*, and, as a rule, Polo would have used the Persian form *kašiš*. But I cannot say whether we should take «casses» for \*«cascis» = *kašiš*, or whether the final -s is a European plural. I myself incline rather to the first solution.

## 125. CASVIN

*casibin* R  
*casium* FB  
*cassum* VL  
*casum* F, P

*causuin* L  
*causum* LT  
*chascim* FA  
*chasom* VA

*chasuin* Z  
*chasuni* V  
*chausom* TA<sup>3</sup>, VB  
*chauson* TA<sup>1</sup>

The well-known city of قزوين Qazwīn. Cf. BARBIER DE MEYNARD, *Dict. hist. de la Perse*, 441-445; *LS*, 218-220; *Mi*, 36, 132. Fra Mauro's «Chassu» (HALLBERG, 125) is probably \*Chassuin > \*Chassum (°um occurs in Polo's F and other mss.) > \*Chassū > Chassu.

Qazwīn is transcribed 可疾云 K'o-chi-yün, Qāzwin, on the Chinese map of c. 1330 and in the corresponding list of *YS*, 63, 16*b* (cf. *Br*, II, 110). It is also the 阿郎民 A-chi-min (read K'o [柯 or 可]-chi-min), between Sultāniya and Tabrīz, of the Ming itinerary translated by BRETSCHNEIDER in *China Review*, v, 239, which he left unidentified. K'o-chi-min supposes \*Qāzmin, and Qazmin is the form of the name in Georgian (cf. BROSSET, *Hist. de la Géorgie*, I, 472, 601).

In the *Times Literary Supplement* of 1929, p. 946, ROSS has expressed himself strongly in favour of the reading «Casum» = \*Casun, which would represent the popular pronunciation of the name of Kāšān. He must have changed his view later, since in his translation of 1931 (*RR*, 416), he adopts «Casvin» = Qazwīn. Moreover, Kāšān is correctly named as «Caxan» in Z (see «Caxan»).

## 126. CATAI

<i>alcatay</i> Z	<i>catagio</i> , <i>catayo</i> <i>chatagio</i> , <i>cattayo</i> , <i>ouchach</i> , <i>ouchacha</i> R	
<i>alchay</i> , <i>catha</i> , <i>cathayum</i> , <i>cay</i>	<i>chatalio</i> VL	<i>charesse</i> , <i>chatay</i> TA <sup>1</sup>
LT	<i>catai</i> F, Fr, FA, L, VB	<i>chatai</i> F, VA
<i>auchara</i> , <i>chattai</i> , <i>chattay</i> TA <sup>1</sup> ,	<i>cataio</i> VB, VL; R	<i>chataio</i> V, VL
TA <sup>3</sup>	<i>catay</i> F, Ft, FA, FB, L,	<i>chatan</i> V
<i>carcai</i> , <i>cartai</i> FA	LT, P; G	<i>chattau</i> TA <sup>3</sup>
<i>castay</i> FB	<i>cathay</i> FB, LT, P, Z; G	
<i>cata</i> F, FA, FB		
(c. 24)		
<i>achata</i> (per) TA <sup>1</sup>	<i>anchasa</i> (per) V	<i>cata</i> (au) F, FA
<i>achate</i> (in) VA	<i>atan</i> (auch) G	<i>catha</i> (per) LT
<i>aghata</i> (per) TA <sup>3</sup>	<i>aucata</i> (en) FB	<i>la ducata</i> (per) VL
<i>alochayray</i> (per) Z	<i>cairo</i> (al) R	

«Catai» is the mediaeval name of China, and more particularly of northern China which we have long become accustomed to write «Cathay». «Alcatay», «Auchara» (read «Auchata») is the same name with the article; but I hold it to be here a Western article, not the Arabic one (see also «Calif»). The Catalan Map gives «Catayo»; Fra Mauro, «Chataio». HALLBERG, *s. v.* «Cataia» (p. 125) merely refers the reader to an «Appendice I» of which I find no other mention and which does not seem to have been published.

The earliest Western mention of the name which I can trace is the «Chata» of the document describing the Mongol campaign against the Mussulmans which was translated from the Arabic into Latin at Damietta in 1221 (cf. ZARNCKE, *Der Priester Johannes*, II, 50). The vicinity of this «Chata» to Ḥwārizm seems to me to imply, however, that it is not China we have to deal with here, and that we should take the name with its other mediaeval value of «Black Cathay», on which more anon. The «Catha» or «Chatha» of Sembac the Constable's letter of February 7, 1248, more probably refers to true Cathay, *i. e.* North China (cf. D'ACHERY, *Spicilegium*<sup>2</sup>, III, 624-628; Y<sup>1</sup>, I, 162, 263). Plan Carpine often speaks of the «Kitai», and also of the «Karakitai» or «Black Kitai» (*Wy*, 48, 53-55, 88, etc.). Rubrouck has much to say of «Cataia» and of «Caracatai» or «Black Catai» (*Wy*, 181, 206-207, 236, etc.). Hethum begins his *Flor des estoires de la terre d'Orient* with a chapter on «Cathay». The *Gestes des Chiprois* speak of «Hata» (*Hist. des Croisades*, Arm., II, 841). Further mentions are: «Cathay» in Monte Corvino (*Wy*, 352); «Catay» in Odoric (*Wy*, 471); «Katay» in Marignolli (*Wy*, 542); «Catay» in Marino Sanudo's map (in BONGARS, *Gesta Dei per Francos*, at the end of the *Secreta*). Pegolotti's «Gattaio» (EVANS, 401) shows the same softening of the initial as in «Gamalecco» = Ḥān-baliq, or in «Gostantinopoli»; this form makes it somewhat doubtful that «cattua» or «cattua», used three times in Pegolotti for silk coming from afar, should represent «Cathayan» as admitted by HEYD (*Hist. du Commerce*, II, 673) and EVANS (p. 399).

It has long been recognized that «Catai» represents the name of the 契丹 Ch'i-tan (\*K'jet-tân). This Altaic tribe founded in northern China, in A. D. 907, the dynasty afterwards known

as the Liao, which was destroyed by the Jučen or Chin (see « Giorcia ») in 1125, but it had in the meantime provided the name under which North China came to be designated during the Middle Ages by the nations of Central and Western Asia whose intercourse with China was carried on by land.

That name had of course been preceded by others. The earliest designation by which China was known in Central Asia was the one which we still use, and which is derived from the Chinese state of Ch'in (see « Cin »). This was replaced in the 5th-6th cents. by that of Tabyač, Таβγач. The latter name most probably renders the original form of the name of the Altaic tribe which founded first the 代 Tai kingdom in the northern part of Shan-hsi (c. A. D. 315), and afterwards the dynasty of the Northern Wei (386-556); the native name of the Wei nation occurs in Chinese texts in the metathetic transcription 托跋 T'o-pa (\*T'āk-b'uât; cf. *TP*, 1912, 732; 1936, 366). Theophylactus Simocatta (VII, 9, 6-9) gives Ταυγάσι (for \*Ταυγάσις); China is called Tabyač in the Turkish runic inscriptions of the Orkhon (8th cent.), and Tavγač in Uighur texts from Turfan (cf. BANG and VON GABAIN, *Analyt. Index*, in *SPAW*, 1931, 502). For Kāšyari, in 1076, Tavγač is the designation of South China, under Sung rule, while North China is 𠵼itai, i. e. Cathay (BROCKELMANN, 250; BARTHOLD, *12 Vorlesungen*, 97-98). At the same time, the old name of Šin = Čin, « China », had been taken over by the Qarākhānids of western Chinese Turkestan, and that entitled them to be referred to as khans of Tavγač in the *Qutadγu-bilig*, which was completed in 1069 (cf. RADLOV, III, 952; BARTHOLD, *loc. cit.* 98). Ch'iu Ch'ang-ch'un, who travelled in Central Asia in 1221-1224, still heard the people of Ili call the Chinese 桃花石 T'ao-hua-shih, i. e. \*Tōyaš (for \*Tōyač; cf. *Br*, I, 71). A Uighur Tao-wa-ch'ih Sa-li, son of Qitai Salī, and brother of Uiyur Salī and Aryun Salī, was probably a \*Dōwač Salī = Tōyač Salī (T'u Chi, 154, 24 a-b). In mediaeval Arabic and Persian works, the name occurs as Tamyaj and Tōyač (cf. *Oh*, I, 203; *Y*<sup>1</sup>, I, 33, 256). When Clavijo says that the Čayatai people call the Emperor of China « Tangus, which means Pig Emperor », I agree with YULE, (*Y*<sup>1</sup>, I, 33, 174, 264) in his suspicion that, despite the fanciful translation, the word has nothing to do with Turk. *tonguz*, « pig », but is a misreading for \*Taugas = Tōyač. The name no longer survives as an ethnic name, but survives, in my opinion, as the name of the red *Prunus* known in Turkī as *tōyač*, which would indicate that the fruit originally came from China (the word is not in RADLOV's dictionary, but it is given in SHAW, *Vocabulary*, 219, and I have often heard it at Kāšyar).

KLAPROTH adduced a text according to which the Ch'i-tan would seem to have been already known in A. D. 233-239 (cf. HOWORTH, in *JRAS*, 1881, 128-130). But this is only due to a misleading note of the 13th cent. commentary on the *Tzū-chih t'ung-chien*, s. a. 405, 12th month (114, 19 b). There is no authority for a direct connection between the chieftain K'o-pi-nēng killed c. 235 and the future Ch'i-tan tribes (cf. *San-kuo chih*, Wei chih, 30, 3 b-4 b). According to the *Pei shih* (94, 8 b; cf. also *Liao shih*, 63, 1 b) the Ch'i-tan were soundly beaten by 慕容晃 Mu-jung Huang; this can only be an abnormal spelling for the name of 慕容皝 Mu-jung Huang († 348; cf. GILES, *Biogr. Dict.* No. 1544), a chieftain of Hsien-pei origin who had established an independent Court of his own. But, although Mu-jung Huang led a campaign against the K'u-mo-hi (*Wei shu*, 100, 5 a), who were closely connected with the Ch'i-tan, no mention of

the latter occurs in his biography (*Chin shu*, 109, 1-5), and I wonder whether the name in the *Pei shih* is not an error for 慕容熙 Mu-jung Hsi († 406 or 407; cf. GILES, *Biogr. Dict.* No. 1543), whom we know to have defeated the Ch'i-tan (*Chin shu*, 124, 6 b) in a campaign which took place in the 12th month of the 1st *i-hsi* year (January 6-February 3, 406), according to the *Tzū-chih t'ung-chien* (114, 19 b).

The *Hsin T'ang shu* (219, 1 a) is probably right in saying that the name Ch'i-tan was adopted only at the time of the Wei dynasty (386-556), or at least that it did not come to the knowledge of the Chinese before that period. As we have seen, it is attested for A. D. 405-406 in Mu-jung Hsi's biography; but there must be some error in L. GIBERT's assertion (*Dict. hist. et géogr. de la Mandchourie*, 454) that, from the time of the Wei Emperor T'ui-tsu (386-409), the Ch'i-tan began to come to the Court every year to present a tribute of horses : the first embassy of the Ch'i-tan to the Wei Court recorded in the official history of that dynasty was in 468 (*Wei shu*, 6, 2 b).

As a result of their defeat by Mu-jung Huang (or Mu-jung Hsi?), the Ch'i-tan are said to have sought refuge in the great « Pine Plain » (松漠 Sung-mo) extending north and north-east of Jehol, up to the Khingan mountains and the Šira-muren (Šara-mürän; cf. also MULLIE, in *TP*, 1933, 188, 190, 210). Despite minor moves, it was in that region that the T'ang established in 648 a « Government General » (*tu-tu-fu*) of Sung-mo which was allocated to the chiefs of the Ch'i-tan (*Hsin T'ang shu*, 43 B, 4 a-b; 219, 1 b). Other Ch'i-tan tribes, among those who had submitted to China, were organized in « vassal (*chi-mi*) *chou* » scattered in the northern part of the present province of Ho-pei. Independent Ch'i-tan groups must have lived still farther north, in the region of the Khingan. The *Hsin T'ang shu* has preserved the names and the location of the various vassal Ch'i-tan tribes (cf. also *Liao shih*, 37, 1 a-b). Roughly speaking, we may say that the great « Pine Plain » was the main site occupied by the Ch'i-tan at the time when they appear, as « Qitaï », in the Turkish inscriptions of the Orkhon basin in the 8th cent. (cf. THOMSEN, in *ZDMG*, 1924, 172). The Tibetan translation (unpublished) of a Uighur inquiry on the people of the North (No. 246 of the Tibetan mss. which I brought back from Tun-huang) speaks of the « Ge-tan » or « Ge-taŋ », with the usual Tibetan change of a surd initial to a sonant. Rašidu'd-Dīn knew the Chinese form « Ch'i-tan » and renders it as « Ḥidan » (QUATREMÈRE, *Hist. des Mongols*, xcii).

The early Ch'i-tan, like several other ethnical groups of north-eastern Asia, used to let their dead rest on high trees and decay for three years before they collected and burnt the bones. They used carts (*Wei shu*, 100, 5 b; *Pei shih*, 94, 9 a; *Hsin T'ang shu*, 219, 1 a). During the T'ang dynasty, they numbered eight great tribes; this division seems to have already existed in the 4th-5th cents. (cf. also the legend of the Ch'i-tan's origin, in *Liao shih*, 37, 4 b). The supreme authority was for a time in the hands of the 大賀 Ta-ho (\*T'ái-γá) clan, but there are obvious contradictions in the accounts of the *Hsin T'ang shu* (219, 1 a, 3 b) and of the *Liao shih* (1, 1 a-b; 63, 4 a) as to the manner in which the power passed into the hands of A-pao-chi, the head of the Yeh-lü clan and the founder of the Ch'i-tan dynasty in northern China. The connection which has been suggested between the name of the Ta-ho clan and that of the modern Daḥur (*JRAS*, 1881, 128) is not acceptable.

The linguistic appurtenance is still debated. Most authors say that they were Tungus.

But the Ch'i-tan words which have been transmitted to us in Chinese transcriptions of the Sung period leave no doubt that the Ch'i-tan spoke a Mongol language, with some archaic features and much palatalization. The purely Tungus words given by WYLIE and HOWORTH (*JRAS*, 1881, 123-126) as occurring in the Ch'i-tan language are fanciful restitutions made by Ch'ien-lung's Commissioners. I cannot dwell here on the problem, which moreover will be susceptible of a more precise solution when some progress has been made in the deciphering of the newly discovered inscriptions in Ch'i-tan writing.

The «Upper Capital» (Shang-ching) of the Ch'i-tan was in Jehol Province; in 938, they established their southern capital at Peking, for which they revived in 1012 the old name of Yen, calling it Yen-ching (see «Cambaluc»). In the meantime, they had adopted, in 947, a Chinese dynastic name, 遼 Liao (*Liao shih*, 4, 7a). Although there is no mention of it in the *Liao shih*, it is stated in the *Tung-tu shih-liao* that the Emperor Shêng-tsung reverted in 983 to «Ch'i-tan» as the dynastic name, and that «Liao» was not adopted a second time until 1066; this is confirmed, to some extent at least, by contemporary inscriptions (cf. *Ssü-k'u... t'i-yao*, 46, 20a). It has often been said, for instance by D'OHSSON (*Oh*, I, 115), YULE (*Y*, I, 12; *Y*<sup>1</sup>, I, 147) and HOWORTH (*JRAS*, 1877, 269), that «Liao» meant «iron» or «steel», but this seems to me to rest on a misinterpretation. The dynastic title «Liao» was clearly adopted by the Ch'i-tan because the original seat of their power was in the region of the «Liao» River, a name which goes back at least to the 3rd cent. B.C. On the other hand, neither Liao in its usual form nor any ancient variant of the character is in the slightest degree connected with iron or with steel. A confusion with 鏢 *liao* is not possible; moreover this *liao* could only mean «fine silver». The text which forms the basis of YULE's and HOWORTH's argument is the edict by which the Jučen Emperor adopted in 1115 the dynastic title of Chin, «Gold» (see «Ciorcia» and «Roi Dor»). The text says: «The Liao had adopted for their designation (*hao*) 寶鐵 *pin-t'ieh*, on account of its hardness. But although the *pin-t'ieh* is hard, finally it also alters and decays; gold alone does not alter nor decay. The colour of gold is white (in the theory of the five elements, gold, *i. e.* metal, corresponds to the West, the colour of which is white), and the Wan-yen tribe (the ruling Jučen tribe) venerates the white colour. Thereupon the dynastic designation was Ta-Chin («Great Gold»). The *pin-t'ieh*, in which MAILLA (*Hist. gén. de la Chine*, VIII, 374) erroneously saw «iron from Pin-chou», is the kind of steel of which I speak at some length in another note (see «Andanique»). Since there is no possible connection between «Liao» and «steel», we must suppose that the dynastic appellation of the Liao referred to in the text is not «Liao» itself, but some native name, perhaps the very name «Ch'i-tan» which was in fact twice adopted as dynastic title, and which may have had some traditional meaning which has not come down to us. I must add that, although this passage is given in the *pên-chi* of the *Chin shih* as part of an Imperial edict, I entertain some doubt as to its real value: it seems that the name «Chin», «Gold», adopted by the Jučen is exactly of the same nature as that of «Liao» chosen by the Ch'i-tan, that is to say it is of geographical origin (see «Roi Dor»).

It was of course during the 10th cent. that the name of the Ch'i-tan began to supersede that of Taβyač as the designation of China among the nations of Central and Western Asia. But while the Chinese said Ch'i-tan (\*K'jət-tân), and the Orkhon inscriptions «Qītaŷ», the basic form



in Mussulman countries was ختای *Hītāi* or خطای *Hīṭāi*. *Hītāi* maintained itself in Turkish (BROCKELMANN, *Kāšyari*, 251) and in Persian, and passed to the countries which heard of China through a Turkish channel like Russia («Kitāi») and Greece («Kitāia»). As the first vowel was never written, خطای *Hīṭāi* > خطا *Hīṭā* (sometimes قتا *Qitā*) was often read «*Hāṭāi*», «*Hāṭā*», particularly by the Arabs (cf. the various forms given by FERRAND, in *JA*, 1919, II, 184), and this explains why the countries of Western Europe which first heard of China from Arabs in the Near East adopted «Cata», «Catai», «Cathay» (where *-th-* simply represents *-t-*). Plan Carpine's isolated «Kitai» bears evidence once again to his use of Russian interpreters. In an Uighur Manichaeic manuscript published by VON LE COQ (*Türk. Manichaica aus Chotscho*, I, 29, 44, 45; BANG, *Manich. Hymnen*, in *Muséon*, XXXVIII, 47), a note has been added by a Manichaeic «hearer» \*Yapyun or \*Fapḥua on his return from «*Hītai*» (or «*Qtai*»), *i. e.* from northern China. If the name be \*Fapḥua, the first part would represent Ch. 𐰉𐰺 *fa* (\**pī<sup>w</sup>op*), and the fact that the final labial was still heard would point to a date not later than the 10th cent.; this would be the earliest mention of the modern name in Central Asia. But \*Fapḥua is a doubtful reading. At any rate, it is interesting to find «*Hītai*» (or «*Qtai*»); the writing does not distinguish between *h* and *q*) written in Uighur without the first vowel as is done in the Arabic transcriptions.

The final element is more embarrassing: *\*tan* according to the Chinese, *\*taŋ* in the Orkhon inscriptions, *\*tai* in Uighur and Persian forms. Here certain observations are necessary. We generally transcribe such Turkish finals as *-ai*, but the last element is actually a semi-vowel, and the proper transcription would be *-ay*. As to the Orkhon inscriptions, the «runic» letter which THOMSEN finally transcribed as *-ŷ* is of a somewhat doubtful value, but certainly contained a nasal element. It occurs in Toḡuquq, the first syllable of which is transcribed *tun* in Chinese; in *qoŷ*, «sheep», later *qoi*, but in Mong. *qoni*, etc. The presence of a nasal element in the name of the Ch'i-tan is indirectly confirmed by the Mongol form, «Kitad» > Kitat. This is a plural, but it can only be accounted for by starting from a singular \*Kitan (< \*Qitan; cf. VLADIMIROV, in *Doklady Ak. N.* 1929, 172; RAMSTEDT, *Kalm. Wörterbuch*, 233), identical with the Chinese transcription and very close to what *Qitay* must have really sounded. An Uighur pronunciation \*Qitan is required by the forms «Ge-tan» and «Ge-taḥ» of the Tibetan translation mentioned above. The alternative \*Qitan/\*Qitai is more or less similar to that of «*altan*» and «*Altai*» (see «*Altai*»). This phonetic link also connects the Ch'i-tan with the Mongols. I do not feel inclined to accept MOSTAERT's view (*Bull. Cath. Univ. of Peking*, No. 9 [1934], 40) that \*Qitan is an ancient plural of *Qitai*.

According to BRETSCHNEIDER (*Br*, I, 265), «the first emperor of the Liao dynasty received an embassy from the Ta-shih in 924, when he was sojourning near the ancient capital of the Hui-hu»; in principle the Ta-shih are of course the Arabs; the ancient capital of the Hui-hu or Uighur is Qara-balyasun in the Orkhon basin (cf. HOWORTH, in *JRAS*, 1881, 175, who also speaks of the embassy as coming to the camp in Mongolia, but believes that the Ta-shih here referred to were not the Arabs, but Persians). BARTHOLD (*12 Vorlesungen*, 120) also speaks of this meeting of Mussulmans and the Ch'i-tan in Mongolia, on the sole authority of BRETSCHNEIDER. Here again, I am afraid that the text has been misunderstood. In the *pên-chi*, under the year 924, we hear of the various stages of T'ai-tsu's advance into Mongolia, but life went on in the meantime as

before at his Court, where he had left a regent, and the various items of information listed in the *pên-chi* come in succession in chronological order, irrespective of the place where the event took place. The *pên-chi* (*Liao shih*, 2, 2 b), having just spoken of a canal dug in the region of the Šira-muren, go on to say : « [In the third *t'ien-tsan* year, in the ninth month,] on the *kuei-hai* day (October 28, 924), the kingdom of the Ta-shih (Arabs) came to offer tribute. » There is no reason to believe that this « embassy » did not come to the Court, at Shang-ching, in the north of Jehol Province. Moreover, this was not the first diplomatic intercourse between the Liao and Mussulman countries : an embassy from Po-ssü, *i. e.* Persia, had come to the Ch'i-tan Court in 923 (*Liao shih*, 2, 2 a; 70, 1 b). According to Mussulman sources, the first embassy from the Qitā-khān that is recorded is the one which came to Ghazna in 1026-1027. The principal text referring to this embassy is the one which appears in the *Tabā'i' al-ḥayawān*, completed c. A.D. 1120; attention has been drawn to it by MINORSKY (cf. *Comptes rendus de l'Ac. des Inscr.* 1937, 317-324), though it has not yet been published.

The last Ch'i-tan Emperor was dethroned by the Jučen in 1125; but one of the members of the Imperial family who would not submit to the conquerors fled to the West with part of his tribe; he finally settled in the region of the Chu River, and founded the empire known as Qarā-Ḥitai, « Black Ḥitai », which lasted almost a century. But the history of the Qarā-Ḥitai, although known in broad outline, is crammed with uncertainties and contradictions which come to one's notice as soon as one tries to make sure of a fact, a name, or a date.

The founder of the Qarā-Ḥitai empire is generally called in Chinese 耶律大石 Yeh-lü Ta-shih, sometimes 大石林牙 Ta-shih Lin-ya (cf. *Liao shih*, 29, 2 a; 30, 2 a; *Chin shih*, 3, 7 a; *Br*, 1, 211; GILES, *Biogr. Dict.* No. 2452; GIBERT, *Dict. . . de la Mandchourie*, 974). Yeh-lü is the clan name of the Ch'i-tan Imperial family, also spelt 移剌 I-la (*TP*, 1930, 48); two forms 益律子 I-lü-tzū and 亦剌思 I-la-ssü seem to transcribe a plural of the same name (*TP*, 1931, 118, 469). The connection established in some Chinese works like the *Ch'i-tan kuo chih* between « Yeh-lü » and a place-name 世里 Shih-li is rightly distrusted in *Liao shih*, 116, 1 b (it is accepted by GIBERT, *Dict. . . de la Mandchourie*, 113; D'OHSSON's « Ché-liou » [*Oh*, 1, 113] and HOWORTH's « Sheliu » [*JRAS*, 1881, 142] render the same Shih-li, but altered by a slip of VISDELOU). We do not know the true Ch'i-tan original, and \*Iru, plur. \*Irus, with an alternative form \*Ira, plur. \*Iras, are not the only possible restorations (for other hypotheses, cf. HOWORTH, in *JRAS*, 1881, 144). « Lin-ya » is not ambiguous; it was the Ch'i-tan term for a man with a *han-lin* degree (cf. *Liao shih*, 45, 6 a-b; 116, 4 b); *lin* is the *lin* of *han-lin*, and *ya* may be used as in the modern term *ya-mên* (this *ya* goes back to T'ang times). Yeh-lü Ta-shih, well versed in the Ch'i-tan and Chinese scripts, had received his doctor's degree in 1115. The case of « Ta-shih » is less simple. Although it looks like a man's personal name, I have a strong suspicion that it is a title. Yeh-lü Ta-shih had been 節度使 *chieh-tu-shih* (Commissioner High-Commander) of an important army, and we know from the *Chin shih* (1, 2 b) that the Liao gave to the *chieh-tu-shih* the name of 太師 *t'ai-shih*, which was certainly pronounced *taisī* in Ch'i-tan, as it was afterwards in Mongolian. Now, *taisī* represents both Ch. *t'ai-shih* and Ch. 太子 *t'ai-tzū*. and I have quoted elsewhere a re-transcription 大石 *ta-shih*, to be read as 太石 *t'ai-shih*, which renders a Mongol form *taisī* itself originating from the Chinese (*TP*, 1930, 44-45). The case may be the same here,

and Yeh-lü Ta-shih, to be read Yeh-lü T'ai-shih, may simply mean « Yeh-lü the *t'ai-shih* ». This old Chinese title of *t'ai-shih*, « Great Instructor », correctly explained as such by Rašidu-'d-Dīn, was given under the Ch'i-tan, the Jučen and the first period of the Mongols to all kinds of high officials, both civil and military.

But the name given by Rašidu-'d-Dīn as that of the founder of the Qarā-Ĥitai Empire is different from all the forms occurring in Chinese sources. From the available readings in Rašid's mss. and in those of works derived from his own, it seems practically certain that the form intended by the Persian historian is نوسی طایفو Nūši Ṭāifū. BRETSCHNEIDER (*Br*, I, 224), noticing that D'OHSSON spoke once of « Tūši Ṭāifū » (*Oh*, I, 163) and another time of « Nūši Ṭāifū » (*Oh*, I, 443), supposed that the second form was a misprint. But it is in this second passage that D'OHSSON followed the mss.; in the first one, he tacitly corrected the reading to « Tūši Ṭāifū » so as to bring the form into some agreement with the second element of « Yeh-lü Ta-shih ». HOWORTH's hypothesis (*JRAS*, 1876, 263) that « Nūši » is « Jučen » (what a name at such a date for a Ch'i-tan!) and that Ṭāifū is the « Chinese » translation of a « native » (*i. e.* Ch'i-tan) title *taiši* is of course unacceptable. « Ṭāifū » transcribes the Chinese 太傅 *t'ai-fu*, « Grand Tutor », formerly a high civil title like *t'ai-shih*, but independent from it. D'OHSSON added in a note that *t'ai-fu* meant « commander-in-chief », and BRETSCHNEIDER (*Br*, I, 225), who only thought of 大夫 *ta-fu* or *tai-fu*, « great officer » (also an ancient civil title) questioned the validity of the explanation. But as I shall show in another note (see « Thai »), the Ch'i-tan gave the old Chinese title of *t'ai-fu* to men who were in fact army commanders. As to the first part of the name, and even granting that Ta-shih may perhaps not be the title *taiši* which I believe it to be and correcting Nūši to \*Tūši as D'OHSSON did, Ta-shih can only be \*Taiši, which could not give \*Tūši. Yeh-lü Ta-shih was perhaps given or took the title of *t'ai-fu*, although the Chinese texts make no mention of it. On the other hand, if I am right in supposing that Ta-shih merely renders the title *taiši*, Nūši might well be the real personal name, though unknown to Chinese sources. Yeh-lü Ta-shih was an agnate of the Ch'i-tan Imperial family, a descendant at the eighth generation of the founder of the dynasty; but his lineage is otherwise unknown (he was not a brother of the last Emperor as is said by BARTHOLD, *12 Vorlesungen*, 122), and he only came into prominence on account of his Western adventure.

Even then, the names and dates are often doubtful, and there are strange discrepancies between different passages in the *Liao shih* itself, and between the *Liao shih* and the *Chin shih* or other Chinese works (in particular Wu-ku-sun Chung-tuan's account of his mission of 1220-1222 in *Br*, I, 28-29), not to speak of the contradictions between Chinese data and those of Mussulman authors.

If we abide by the text of the *Liao shih*, Peking was taken by the Jučen in 1122 (not in 1120 as is said in *Br*, I, 211); Yeh-lü Ta-shih was captured by the Jučen in 1123 (*Liao shih*, 29, 3a; BRETSCHNEIDER [*Br*, I, 220] is mistaken when he says that this capture is not mentioned in the *Liao shih*), but was set free soon afterwards; he then repaired to the Ch'i-tan Emperor, but not feeling safe, proclaimed himself « king » (*wang*) in 1124 and left for the West (*Liao shih*, 29, 3a-4b). Yeh-lü Ta-shih's surprising release by the Chin is almost in favour of Wu-ku-sun Chung-tuan's account, according to which Yeh-lü Ta-shih had at some time betrayed the Ch'i-tan, at least,

ostensibly, and taken service with the first Chin Emperor, who gave him a Chin princess to wife; but he took his family and fled as soon as he had a chance (cf. *Br*, I, 28). At the head of 200 horsemen, he travelled first to the north for three days, crossed the 黑水 Hei-shui or Black River, met Čongyur, the head of the « White Tatar » (see « Ung ») and proceeded west to 可敦城 K'o-tun-ch'êng, the « City of the *Qatun* ». He stopped at the « Protectorate General » (*tu-hu-fu*) of 北庭 Pei-t'ing and there convened a meeting of the heads of seven « districts » (*chou*) and eighteen tribes (*pu*). BRETSCHNEIDER (*Br*, I, 212) has taken it for granted that the meeting took place at Beš-baliq (near Ku-ch'êng, to the north-east of Urumči). Now, it is true that, under the T'ang, the Protectorate General of Pei-t'ing was at Beš-baliq, but it is no less evident, from the names of the districts and tribes the heads of which attended the meeting and from what follows in the account, that Yeh-lü Ta-shih had not then even come near the Uighur country and that the place referred to must be sought for in Eastern Mongolia (the name of the first « district », 威武 Wei-wu, equated to « Uighur » without comment by BRETSCHNEIDER, seems to be a purely Chinese name and never occurs as a transcription of « Uighur »). Either the name « Protectorate General of Pei-t'ing » has in the text another value than it had in T'ang times, or it is corrupt. « The following year, in the second month, on the *chia-wu* day », Yeh-lü Ta-shih sacrificed a grey (*ch'ing*) ox and a white horse to Heaven, the Earth and to his ancestors and set out for the West. This would seem to have occurred in 1125, but there is no *chia-wu* day in the second month of 1125, and either the year is wrong (for 1124 or 1126?), or 甲午 *chia-wu* is corrupt for 甲子 *chia-tzũ* (March 28, 1125). It was then that Yeh-lü Ta-shih, going out of Mongolia, asked permission to pass through the dominions of the Turfan Uighur, and proceeded towards the territory of the Qarakhanids (Ili and the western part of Chinese Turkestan). BARTHOLD has deduced from Mohammedan sources that the advance of the Ch'i-tan was made in two directions : part of them went through the Uighur country, and suffered a crushing defeat at the hands of the Khan of Kāšyar; but the other group advanced successfully through Mongolia to the Upper Yenisei, turned to the south-west towards Čugučak and reached the land of the Qarakhanids (cf. BARTHOLD, *12 Vorlesungen*, 122). No indication of that double progress is found in the Chinese texts, according to which Yeh-lü Ta-shih himself was accompanied by the Uighur king when passing through his dominions. It is stated in the *Chin shih*, 3, 7a, that in 1131 (not « 1130 » as in *Br*, I, 222), « the Uighur of Ho-chou » (*i. e.* of the Turfan region; see « Carachoço »), having captured a partisan of Yeh-lü Ta-shih, sent him to the Chin. The date seems to be too late to refer to a member of the party of 1224-1226; but a biography in the *Chin shih* (121, 2b; cf. *Br*, I, 221) shows Yeh-lü Ta-shih as still being in the region of Turfan in 1130. Any attempt to reconcile Chinese and Mohammedan sources will prove futile until the whole material is carefully studied and discussed.

On the whole, I have a suspicion that the Chinese accounts of Yeh-lü Ta-shih's progress to the West, of his reign there and of those of his successors are not much to be trusted. The Liao dynasty fell at the time of Yeh-lü Ta-shih's departure, and from that date on, no independent Ch'i-tan historiography exists; all fresh information of course went to the reigning Chin dynasty. Now it is a striking fact that the Chin were long unaware of Yeh-lü Ta-shih's movements; and the little they heard falls in with the Mohammedan sources which bring down Yeh-lü Ta-shih's advance

to the West by a few years. But the founder of the Qara-Ĥitai Empire had become a legendary figure, and what we have in the *Liao shih* may well be a romanced biography, with more or less fictitious reign names (*nien-hao*) and a fanciful chronology. For instance, it is clearly impossible that Yeh-lü Ta-shih, who was still in northern China in 1124, should have taken the Imperial title in Russian Turkestan in the second month of that very year (*Liao shih*, 30, 3*a*; *Br.* I, 216). Yeh-lü Ta-shih was already dead in 1144, but his name Ta-shih remained in China as a designation of his successors, and is even said in the *Chin shih* (121, 3*a*) to have been so used by the non-Chinese nations of Central Asia. As late as 1161-1163, Ch'i-tan who had rebelled in Manchuria wished to go and join «Ta-shih of the Western Liao» (cf. *Yüan shih hsün pien*, 17, 4*a*).

A few names which stand out in the Chinese account of Yeh-lü Ta-shih are however, quite genuine. One is that of his capital, written 虎思斡耳朵 *Hu-ssü-wo-êrh-to* in *Liao shih*, 30, 3*a*, and 骨斯訛魯朵 *Ku-ssü-ê-lu-to* in *Chin shih*, 121, 2*b* (the first form is badly altered in GILES, *Biogr. Dict.* No. 2452, and in TCHANG, *Synchronismes chinois*, 367). In the Mongol period, the same name occurs as 虎司窩魯朵 *Hu-ssü-wo-lu-to* in Yeh-lü Ch'u-ts'ai's *Hsi-yu lu* (*Br.* I, 18) and as 谷則斡兒朵 *Ku-tsê-wo-êrh-to* in *YS*, 120, 7*a*. These transcriptions represent an original «Quz-or-do» or «yuz-or-do». «Or-do» is of course the «Mongol» pronunciation of Turk. *ordu*, «camp», «royal camp». The vocabulary which constitutes ch. 116 of the *Liao shih* correctly explains *ordo*, but says (9*a*) that 虎思 *hu-ssü*, also written 虎斯 *hu-ssü*, means «strong» (有力 *yu-li*). This has been taken to be the Ch'i-tan word corresponding to the Manchu *husun*, «strength», since the Ch'i-tan, «as is known, belonged to the same Tungusic stock as the Manchus» (*Br.* I, 18). It may be added that the Jučen word for strength was already at that time \**husun* (cf. GRUBE, *Die Sprache und Schrift der Jučen*, 27, 93) or \**husu* (as in the unpublished vocabulary belonging to the Ecole Française d'Extrême-Orient). But this has no bearing on the linguistic appurtenance of the Ch'i-tan, because the same word for «strength» exists in Turk. *küç* (also *Uiy. küsün* in MÜLLER, *Uigurica*, IV, 54<sup>1</sup>; *küsin* in BANG and VON GABAIN, *Analyt. Index, SPAW*, 1931, 485) and Mong. *küçün*, and above all because «Quz-or-do» is not a Ch'i-tan name. If the vocabulary of the *Liao shih* explains the name as being Ch'i-tan, it is because *Hu-ssü* alone, with the second spelling, occurs elsewhere as a Ch'i-tan name (cf. *Liao shih*, 116, 17*b*). But the name Quz-or-do, or rather «Quz-or-do» existed at least half a century before the arrival of the Qarā-Ĥitai, and is already given in 1076 by Kāšyarī (BROCKELMANN, 248). It was the Turkish name of Balāsāyūn (this is perhaps a Sogdian name), the capital of the Qaraghanids on the Ču River. MARQUART has said more than once (the last time in *Ungar. Jahrbücher*, IX, 97-98) that the Chinese transcriptions represented yuz-or-do, for \*Oyuz-or-do, «the camp of the Oyuz». It is true that the Turkish Oyuz tribes have often been simply called yuz (*yuzz*) in Mohammedan sources, and also that the alternative name of Balāsāyūn in Ĥuwainī (I, 43<sup>3</sup>; II, 87<sup>8</sup>), long misread غوباليق *γū-bāliq* (*Oh*, I, 442; *Br.* I, 226-227), is in fact غزباليق *yuz-bāliq*, «yuz City». But, although the history of the name Oyuz and of its application is still obscure (see «Iuguristan») and although Kāšyarī gives as tribal names both the forms Oyuz and *yuzz* (BROCKELMANN, 243, 246), the fact that he only gives Quz-or-do for the alternative name of Balāsāyūn and Quz-uluš for the region of Balāsāyūn does not favour an equation of the name with Oyuz or yuz, *yuzz*. «Quz» remains, however, unexplained (cf. BARTHOLD, in *Izv. Ak. Nauk*, 1931, 396).

The Chinese account of Yeh-lü Ta-shih also says that he took the title of 葛兒罕 *Ko-êrh-han*, written 闊兒罕 *K'uo-êrh-han* in one of the biographies of *YS*, 120, 7 *a*; according to the vocabulary of the *Liao shih*, 116, 9 *b*, *ko-êrh-han* is the title of the sovereigns «north of the Desert», *i. e.* in Mongolia. The two Chinese transcriptions represent respectively \**gôrhan* and \**kôrhan*, but seem to have been made by people who found the title in Uighuro-Mongol writing, and had no tradition as to its correct pronunciation; the original should be read *gürhan* or *kürhan*. Speaking of the Qara-Ĥitai, Ĵuwainī (II, 86<sup>17</sup>) gives it as كورخان *gür-hān*, and adds that it meant *hān-i hānān*, «khān of khān» («king of kings»); cf. the «*chanchana*» of a Latin translation made in 1221 from the Arabic in ZARNCKE, *Der Priester Johannes*, 31-32.

It would not be necessary to denounce the confusion made by HAMMER and already dispelled by ERDMANN (*Temudschin*, 577-581) between *gür-khan* and Mong. *kürägän* or *kürgän*, «son-in-law», and especially «son-in-law of the Emperor» (Ch. *fu-ma*), if it had not been repeated by HOWORTH (*JRAS*, 1876, 274) and in BRETSCHNEIDER's index (*Br*, II, 338). The two words can have nothing in common.

BARTHOLD, who by some oversight states that the title *gür-hān* is only known in connection with the Qara-Ĥitai, suggests that the first part may represent the old Turkish word *kür* or *kül*, known from the Orkhon inscriptions, and also from Gardēzī and Kāšyari. It is true that there is a Turkish word *kül*, occurring for instance in a name like *Kül-tegin* or a title like *kül-čor*; on the other hand, the name is given as *Kür-tegin* by Gardēzī (BARTHOLD, *Otčēt o poézdké*, 90, 114-115), and the title as *kür-söl* by Ṭabari (cf. MARQUART, *Histor. Glossen*, 181-182; CHAVANNES, *Doc. sur les Tou-kiue*, 285; *Mi*, 301). Kāšyari only knows a word *kür*, «brave» (BROCKELMANN, 117), but says that the Uighur sovereign bears the title of «Köl-Bilgä khan», «the khan as wise as a lake» (BROCKELMANN, 245). It may be that *kül* was the northern and eastern form of the word which became *kür* in the west, but it is also possible that an independent word *kül*, unknown in the west, was contaminated by the better known *kür* in Gardēzī's source, and was misunderstood as *köl* in the Uighur title by Kāšyari; I do not doubt that the title of the Uighur sovereign was *Kül-Bilgä khan*, and that it had nothing to do with *köl*, «lake».

But there is no reason, in my opinion, to seek for a Turkish explanation of *gür-hān*. While it is true that the title is unattested before the Qara-Ĥitai, it is well known as the title of the Kerait sovereigns, and we also find it adopted in the first period of the history of Chinghiz-khan by his Mongol rival Ĵamuqa (cf. *Secret History*, §§ 141, 150, 177, 198; also § 203; *Oh*, I, 63; *Ber*, II, 124). In the case of Ĵamuqa, Rašidu'd-Dīn adds that *gür-hān* means «Emperor of sultans and kings». In the *Secret History*, the *gür* of *gür-hān* is interpreted as 普 *p'u*, «general», «universal». I have no doubt that D'OHSSON gave long ago the true explanation (*Oh*, I, 99) when he said that *gür*, in Mongolian, meant «whole», and that the true meaning of *gür-hān* was «universal khan». Uighuro-Mongol and Arabic writings do not distinguish between *g*- and *k*-, and the Chinese transcriptions of the *Secret History* often give *g*- for words which are actually pronounced with a *k*- in Mongolian (for instance *gür*-, «to reach», instead of *kür*-; *güčün*, «strength», instead of *küčün*, etc.). Now, there is in Mongolian a word *kür*, generally rendered as «crowd». RAMSTEDT (*Kalm. Wörterbuch*, 246) says that the word is onomatopoeic for a «great noise» and that the meaning crowd is secondary. But the term *kür*

*ulus*, « all the people » is well known. It occurs in literary texts, for instance in the historical colophon published by LICETI (*TP*, 1930, 131, where it is transcribed *gür ulus*). In the preamble of Güyük's letter to Innocent IV, the Mongol Emperor calls himself « the Oceanic khan of the whole great nation » (*kür uluy ulus-nung talui-nung han*; cf. *Pe*, 22). RAMSTEDT's remark seems to be arbitrary. But whatever the case may be, and whether we read *gür-han* or *kür-han*, the fact remains that *gür* or *kür* is a good Mongolian word, and that the title *gür-han* or *kür-han* was in use among Mongol tribes. That it should have made its first appearance with the Qara-Ĥitai should therefore cause no surprise if we remember that the Ch'i-tan must have spoken a Mongol dialect.

Rubrouck refers to a sovereign « Coirchan » (var. « Concham »; but also « Coirchan » in Roger Bacon's extracts) to whom the « Turks » sent for succour at the time when the Franks took Antioch, as is said « in the history of Antioch »; he adds that « Coir » (or « Con ») is a proper name, that « chan » (or « cham ») means « soothsayer » and that the said sovereign was a « Caracatai », i. e. a Qara-Ĥitai (ROCKHILL, *Rubruck*, 108-109; *Wγ*, 205-206). It has been taken for granted that « Coirchan » (or « Concham ») was intended by Rubrouck to represent *gür-hān*, and this is probably the case. Yet there are errors and difficulties in the text. Rubrouck confuses *han*, « sovereign », « khan », with *qam*, « soothsayer ». Moreover, at the time of the capture of Antioch by the Franks (1098), there was not yet any *gür-han*. It seems that Rubrouck wrongly associated a « Coirchan » whom he found mentioned in a history of Antioch with the *gür-han* he heard of in Central Asia. Unfortunately, it has been impossible so far to identify the *Hystoria Antiochie* alluded to by Rubrouck (cf. also ZARNCKE, *Der Priester Johannes*, II, 87-96).

It is said in the *Liao shih* (30, 3 a) that when Yeh-lü Ta-shih reached Samarkand, « the various kingdoms of the western countries raised 100,000 soldiers and called up 忽兒珊 Hu-êrh-shan to come and oppose him in the field (舉兵十萬號忽兒珊來拒戰) . . . Hu-êrh-shan was utterly defeated . . . ». VISDELOU and BRETSCHNEIDER (*Br*, I, 215) are in substantial agreement with the above translation. HOWORTH (*JRAS*, 1876, 272), relying on DE MAILLA's editor and on DOUGLAS, felt no hesitation in declaring it wrong, and so did MARQUART, on DE GROOT's authority (*Ueber das Volkstum der Komänen*, 142) : for them Hu-êrh-shan was not the name or the title of the commander-in-chief, but a designation of the soldiers. But while HOWORTH saw in Hu-êrh-shan the name « Ĥorāsān », MARQUART identified it with Ar. <sup>هُزَات</sup> *γuzāt*, « soldier fighting for the faith ». Despite HOWORTH, MARQUART and their authorities, those who have seen in Hu-êrh-shan the name, title, or epithet of the commander-in-chief have the support of the *Liao shih* itself, where it is said (ch. 116, 9 a) that Hu-êrh-shan was « the name (*ming*) of the commander-in-chief of the western countries ». On the other hand, MARQUART's explanation is phonetically unacceptable : in the Mongol period, 珊 *shan* still sounded *san*, and represented *san* or exceptionally *sal* in transcriptions (cf. *TP*, 1930, 43); the original, with some uncertainty as to the sound -u- or -o- in the first syllable (cf. above for *ko-êrh-han*) can only be \*Ĥursan (or \*Ĥursal). BRETSCHNEIDER (*Br*, I, 215) suggested the Ĥwārizm-šāh, without finding elsewhere a confirmation of a battle fought between him and the Qara-Ĥitai; but he had also obviously thought of « the Seldjuk Sultan Sangiar of Khorasan » since he mentioned him, and was only

prevented by chronological reasons from deciding in his favour. We have seen, however, that the chronology of the Chinese account could not be trusted. Wu-ku-sun Chung-tuan makes Yeh-lü Ta-shih wander several years before he was able to enter Ili territory (cf. *Br*, I, 28-29). On the other hand, the crushing defeat which Sanġar, the Sälġük Sultan of Ĥorāsān, suffered in 1141 north of Samarkand at the hands of the Qara-Ĥıtai was the capital event which created new conditions in Central Asia for the next two generations. In my opinion, HOWORTH was right in thinking that Hu-êrh-shan, \*Ĥursan, was Ĥorāsān. But the equivalence does not imply, as he believed, that the name is an epithet of the soldiers : it is the Sultan of Ĥorāsān himself who is designated in the Chinese text by the name of his country. The identification is of special interest if we remember that, precisely on account of the victory he won over Sanġar, the *gür-han* of the Qara-Ĥıtai was considered by OFFERT and ZARNCKE as having provided the prototype of the famous Prester John (see « Prester Iohan »).

I have said that the chronology and the reign titles of the Chinese text concerning the Qara-Ĥıtai were not to be trusted, but the objection may be raised that a number of banknotes of these reigns are reproduced in the numismatic work *Ch'üan-pu t'ung-chih* and have been made the subject of a monograph by H. A. RAMSDEN in *Chinese Paper Money*, 1911. My answer will be brief : those banknotes are glaring and clumsy forgeries, which have been wisely omitted by Lo Chên-yü from his *Ssü-ch'ao ch'ao-pi t'u-lu*.

We do not know why the Ch'i-tan who migrated to the West came to be known as Qarā-Ĥıtai, « Black Ch'i-tan ». The Chinese texts generally speak of them as Hsi-Liao, « Western Liao ». But the name of 黑契丹 *Hei Ch'i-tan*, « Black Ch'i-tan » is also met with occasionally it occurs, together with an illustration, in a geographical work of the early Ming period (cf. GILES, *Adversaria Sinica*, I, 268-269; MOULE, in *TP*, 1930, 188); the figure of the *Hei Ch'i-tan* passed into the *San-ts'ai t'u-hui*, from which it has also been reproduced in the *T'u-shu ch'ich'êng*, Pien-i-tien, 129, 8 a, at the end of the chapter devoted to the Ch'i-tan. This is not surprising, since the name of the « Black Ch'i-tan » had passed into Mongolian, and often occurs in the *Secret History*. But, while the *Secret History*, in the Chinese phonetic transcription, speaks of the Ch'i-tan of China as « Kitat » (§§ 132, 247, 250, 251, 263, 266, 271, 272), once as « Kitan » (§ 55), it always mentions the Qara-Ĥıtai as « Qara-Kidat » (§§ 151, 152, 177, 198, 247, 248), except once where we find « Qara-Kitat » (probably the result of a Chinese textual corruption). As there can be no foundation for such a distinction, I believe that the transcribers, at the end of the 14th cent., of course knew the Mongol pronunciation of « Kitat », alive in Mongolian current speech, but had no tradition about the Qara-Ĥıtai and misread as *-d-* the letter of the Uighuro-Mongol writing which has the double value of *-t-* and *-d-*.

Since Ĥıtai had become in Mohammedan countries the name of China herself, Persian authors were embarrassed when they had to speak of the Ch'i-tan who had remained in China. Rašidu-'d-Dīn solved the difficulty by transferring to those Ch'i-tan the name of « Qarā-Ĥıtai » as well; it is not surprising that the double value thus acquired by Qarā-Ĥıtai should have given rise to some misunderstanding (cf. *TP*, 1930, 42-49).

Ĥıtai (or Ĥatai) is the normal appellation of North China in Rašidu-'d-Dīn, but it is not the only one. QUATREMÈRE (*Hist. des Mongols*, LXXXVI and CXI) has quoted two parallel texts on



the subject, one from Rašid himself, the other from Bānākātī who copied Rašid. Rašid says : «The country of Hītai is known among the Mongols under the general name of جاوقوت Jāvqūt. Hītai is called in Chinese خانزی Hānzī. . . ». Bānākātī's text reads follows : «The region [of Hītai] which has almost always been the residence of the kings bears in Chinese the name of خانز چو خون قوی Hānzu-čū-hūn-qūi, in Mongolian that of جاوقوت Jāvqūt. . . ». In the first case, QUATREMÈRE corrected «Hānzī» to جانزی Jānzī, which he took to be the province of Shan-hsi. In the second QUATREMÈRE read جانزی و چون قوی «Jānzī ū Čūn-qūi», which he interpreted as «Shan-hsi and 中國 Chung-kuo (Middle Kingdom)». D'OHSSON (*Oh*, I, 120) retained «Hānzī», without explaining it. «Hānzī», has passed as «Kansi» into *Hist. des Croisades*, Arm., II, 261. QUATREMÈRE was mistaken, and «Hānzī» is absolutely correct. Opposed by Rašid to «Mānzī», Man-tzū, Southern China (see «Mangi»), it renders 漢子 Han-tzū, «Chinese», and we have many texts showing that «Han» was used in the Mongol period as the official designation of the people of Chinese descent in Northern China, as distinguished from the Ch'i-tan, Jučen and Tangut people on the one hand, and from the «Man» or Southern Chinese on the other. I do not believe either in the correction «Jūn-qūi» = Chung-kuo; the word *kuo* is always transcribed by Rašid with *k-* (*g-*), not with *q-* (see «Caugigu», «Çipingu»). Bānākātī's text, however, is obviously corrupt. As there is no probability that it could give a name which does not occur in Rašid's text, I think that his چو خون قوی is merely a wrong duplication, by a copyist, of the following جاوقوت Jāvqūt. The latter name, said to be Mongolian, is puzzling. As already said by QUATREMÈRE, Rašid uses it more than once; it occurs for instance in *Bl*, II, 323, 374, 380, 383, generally applied to troops and in contradistinction to the Mongol forces. The natural deduction is that it was the general name in Mongolian for all the people of North China, excepting the Mongols. BLOCHET (*Bl*, II, 323) says that it is a plural of 兆戶 *chao-hu*, «million of families». The hypothesis is hardly worth refutation; suffice it to say that it is wholly arbitrary, and that a Mongol plural of a transcription of *chao-hu* would have in principle, c. A. D. 1300, ended in *-s* and not in *-t*. D'OHSSON (*Oh*, I, 120) considers «Čauqut» as meaning «country of Chao», adding that the name had probably been borrowed from the Chinese; but he does not explain how *-qut* can mean «country». My own view is as follows. Although BLOCHET always writes «Čauqut», Rašid's mss. do not distinguish between *č-* and *ʃ-*; both sounds, as a rule, are written *ʃ-*. In the *Secret History*, there are two forms which might be taken into account. One is Jaqut (or Jayut) occurring twice (§ 281), with the translation «Chin», *i. e.* Jučen. But one does not see why it should be used as a designation of the Northern Chinese, and in a wider sense of all people of Northern China except the Mongols; above all, the first *-u-* of «Jāvqūt» is missing in Jaqut. The other form is twice transcribed 趙官 Chao-kuan (§ 251), and translated 宋 Sung (=the Sung dynasty). «Chao-kuan» renders in principle a Mongol form \*Jaugon, and since the Sung Emperors belonged to the Chao family, there can be no doubt that it is their surname which constitutes the first part of the Mongol term. It is more difficult to be sure of the second. 官 *kuan* means «official», «mandarin», but 官家 *kuan-chia* was used in China for many centuries as a popular designation of the Emperor (cf. *TP*, 1921, 326, 328). So it is quite possible that «Chao-kuan» was really used in the sense of «Sung»,

and passed as such to the northern neighbours of China. But «Chao-kuan» would give ǰaugon, not ǰauqut; in the Mongol period at least, we should not expect the *k-* of *kuan* to be rendered as *q-*, and the finals are moreover different. But the Mongols have given forms of the plural to similar borrowed names; for instance Nan-chia occurs in Mongolian first as Nangias, later as Nangiad (see «Mangi»), that is to say, only in plural form; and a Mongol plural of ǰaugon would be \*ǰaugot. For the *-q-*, we must not forget that, in such transcriptions, *-q-* can always have the value of either *-q-* or *-γ-*; moreover, the transcription was not made by Rašid direct from a Chinese original, but represents a form which had already passed into Mongolian. Finally, it may be that the Mongols themselves did not take the name direct from the Chinese, but inherited it from the Ch'i-tan or from the Chin. This seems to be almost certain because, when the Mongols entered the field, the Sung had already been driven away from North China for too long a time to give it its Mongol name. My conclusion is that if «ǰauqut» actually goes back to «Chao-kuan», the term, and its plural form, must be due to the Ch'i-tan, or less probably to the Jučen. The very form of the plural, which is Mongolian, causes no surprise, since the Ch'i-tan spoke a Mongol dialect.

Rašidu-'d-Dīn says that the southern limit of Hītai was the Qara-mörän, *i. e.* the Huang-ho (cf. QUATREMÈRE, *Hist. des Mongols*, XCII-XCIII). Such was also Polo's notion of «Catai». We must remember, however, that the course of the Huang-ho was then different from what it is now; from east of K'ai-fêng-fu it deflected to the south-east, instead of the north-east as at present.

The equivalence of «Catai» or «Cathay» with China was forgotten in Europe after the Mongol period. RICCI, who had soon recognized it, as early at least as 1596, had great difficulty in convincing his brethren, the missionaries of north-western India. BENTO DE GOES's journey was especially intended to solve the problem, and no doubt on the point should have survived it (cf. WESSELS, *Early Jesuit travellers in Central Asia*, 3-6). However the desire to discover somewhere the prosperous Christian communities of Cathay spoken of by Polo was so great that belated attempts continued to be made to distinguish Cathay from China. After the first journey of Antonio DE ANDRADE to western Tibet in 1624, the account of his experiences, written by himself at Agra on November 8, 1624, was still published in 1626 under the misleading title *Novo Descobrimto do gram Cathayo*.

## 127. CATORS

*cators* F, TA<sup>1</sup>  
*chartos* TA<sup>3</sup>  
*chatorni* V  
*coturnices* LT

*perdelis* FB  
*perdris* FA  
*pernici et quaglie* R

*pernise* VL  
*pernix* VA  
*quattros* VB

Polo, who is enthusiastic about the water-fowl of Ciagannor (*q. v.*), speaks of its cranes, pheasants and partridges («perdrices»), describes the five kinds of cranes to be found there, and

goes on to mention a near-by valley where Qubilai had had little houses built to keep «grandismes quantité de cators, que nos apellon les grant perdris». Most texts either omit the latter part of the paragraph, or speak only either of «partridges» (FA), or of «quails» («coturnices», LT). RAMUSIO, however, gives «pernici, et quaglie» («partridges and quails»), and I have little doubt that, like LT, he understood as «quails» the «cators» of F and TA<sup>1</sup>. In the chapter on Yazd, Polo mentions «pernis et quatornis» (F); «de perdrix et de contornis» (FA; but misread «contorvis» in the Glossary of the 1824 ed., 524, and «concornis» in *Pa*, 71, with a wrong explanation «pheasant» which has passed into Polo's text in MINAEV, 48, and *Ch*, 55); «perdriz cailles» (FC<sup>1</sup>); «perdices et coturnices» (Z); «et pernici et quaglie» (R). It seems to be obvious that, in the second passage, «quatornis» is Lat. *coturnix*, Ital. *coturnice*, «quail», and the natural inference would be that F's and TA<sup>1</sup>'s «cators» (plural form), V's «chatorni», and VB's «quatros» represent the same word, rightly understood as «quail» by LT and RAMUSIO. It is accordingly rendered «quails» in *RR*, 96, and *B*<sup>1</sup>, 98 («coturnici»).

Yet there is a difficulty : it sounds absurd to say that Europeans called the quail, which is a much smaller bird than the partridge, «great partridge». This may be the reason why YULE (*Y*, I, 297-298) suspected that the name had become corrupt owing to the attraction of *coturnix*, and suggested that the original form may have been \*çacor or \*ciacor, «viz. *chakór*, a term applied in the East to more than one kind of 'Great Partridge'». According to YULE, «*chakór*» is the usual designation of a red-legged partridge, larger than the European one; this is the «francolin» of MOORCROFT, the *Caccabis Chukor* of GRAY. But in Ladakh the name is sometimes used as a designation of the big bird, «as large as a hen-turkey», which goes by the scientific name of *Tetraogallus himalayensis*.

Let us first dispose of this second identification. CORDIER has added to YULE's note a note by M. E. D. MORGAN which is full of inaccuracies : instead of «*Megaloperdrix tibetanus*», we should read *Megaloperdix tibetanus*; instead of Turk. «*ullar*», *ular*; instead of Mong. «*hailik*», *käklik*; instead of Tib. «*kung-mo*», *goñ-mo*. «*Chakór*» may have been used in Ladakh for the bird, but the Turkish name of the two species *Tetraogallus himalayensis* and *Tetraogallus tibetanus* is *ular*, occurring already in Kāšyari (BROCKELMANN, 229, with several misprints). The pronunciation *aular*, copied by RADLOV (I, 77) from SHAW's *Vocabulary* (p. 209) is not correct, as I can testify from personal experience. Moreover SHAW himself transcribes «*ulār*», p. 26; but I have also heard *ölār* at Kuča. It has passed into Mongolian as *ularu*, but as the designation of a grouse (cf. KOWALEWSKI, 401; *Ssū-t'i ho-pi wên-chien*, 30, 31 b [corresponding to Ross, *A Polyglot List of Birds*, No. 123]). It is a glaring error in the Turkī section of the Polyglot Dictionary to give *ular* as the Turkī name of a stork (*Ssū-t'i ho-pi wên-chien*, 30, 26 b; Ross, *loc. cit.* No. 15). On the other hand, *käklik*, as will be seen below, is the Mongol name of the «*chakór*», though in the usual value of the latter name, not when it is applied to a *Tetraogallus*. As to Tib. *goñ-mo*, it is a general name for «grouse». But the *ular* is a mountain bird of Central Asia, which would make it out of the question for Čayān-nōr.

YULE's idea was that «*chakór*» could originally have been a Mongol word, «not improbably» *čogor* (> *coxor*; *c = ts*), «dappled or pied», which had spread to Persia, India and Tibet. YULE found it mentioned by Bābur, and also by the Hindī poet Chand. «If the latter passage is

genuine», YULE added, «it is adverse to my Mongol etymology, as Chand lived before the Mongol era».

CORDIER has no comment on this etymology, which YULE, however, had rightly renounced later in *Hobson-Jobson*<sup>2</sup>, s. v. «chickore». The reasons for this change of opinion were twofold: SCHIEFNER had told YULE that Mong. *čogor* was only applied to horses; and secondly, «*chakór*» seemed to go back to Skr. *cakora*. The first reason had no great weight: *coqor* also occurs in the names of fish (cf. KOWALEWSKI, 2196) and of birds (cf. *Ssü-t'i ho-pi wên-chien*, 30, 43 a: *čogor boljumur*, corresponding to Ross, *loc. cit.* No. 344). But the derivation of *čakor* from Skr. *čakora* is evident, and confirms the doubts which the use of *čakor* by Chand had already raised in YULE's mind.

Even then, however, YULE maintained in *Hobson-Jobson* his hypothesis that Polo's *cator* stood for \**čacor* or \**ciacor*. This would imply that *čakor*, instead of passing direct from Central Asia to India, had spread at an early date from India to Persia and Central Asia. But this again finds no support in the texts. Between the date of Polo's *cator* and the Anglo-Indian quotations of the 19th cent., YULE could only adduce a passage in Bābur's *Memoirs* as translated from the Persian by ERSKINE (p. 282), and this at least would carry us back to the beginning of the 16th cent. Unfortunately the mention of the word in Bābur's *Memoirs* is extremely doubtful. In the Turkī original text (ILMINSKIĪ ed., 321; Haidarabad ms., 249 a), Bābur speaks not of *čakor*, but of چيرکە *čüürkă*, clearly identical with Turk. *čigürtkă*, «locust» (on which cf. my note in *BSOS*, VI, 567), and correctly translated as such by PAVET DE COURTEILLE (*Mémoires de Bāber*, II, 122). A. S. BEVERIDGE (*The Memoirs of Bābur*, 422) did not translate the word, leaving a blank space, since she was in doubt whether «locusts» could be found in the mountains in January. But the Turkī text is quite unambiguous, whereas the Persian translation, as is seen from Mrs BEVERIDGE's own note, betrays the embarrassment of a translator confronted with an unknown or corrupt word.

It may be that «chikur» or «chakor» is now more or less in use in eastern Persia (cf. ABBOTT, in *JRGS*, XXV, 41, alluded to in *Y*, I, 298, and more completely quoted in *Hobson-Jobson*<sup>2</sup>, 195), but the fact remains that no form of the word is known to Persian lexicographers, nor is it found in any Turkish or Mongol dialect. Only the Tibetans know of *ca-ko-ra* in a bookish way (S. C. DAS's *Dictionary*, 995; KOWALEWSKI, 695). I must add, however, that Chinese nomenclature here raises a curious problem. Our dictionaries (GILES, TARANZANO, etc.) agree in giving 鷓鴣 *chê-ku* (\*t'sja-kuo) as the ordinary modern name of the «common partridge». The term occurs first in post-Han, though pre-T'ang times as that of a bird of the «southern countries» (*nan-fang*) which «flies towards the sun» and is afraid of the dew and the hoar-frost (the Mongol, Tibetan and Turkī names given in *Ssü-t'i ho-pi wên-chien*, 30, 41 b, and Ross, *loc. cit.* No. 314, seem to be fanciful terms based on that tradition). The Indian tradition is that the *cakora* drinks in the moonbeams (cf. the water produced by the rays of the moon in Chinese texts?). Quite evidently, legends were current about both birds, but it is difficult to connect the names; the phonetic resemblance may perhaps be rather of an onomatopoeic order. Nor can the fact that the *cakora* is a sort of francolin and that *chê-ku* is also sometimes used, according to TARANZANO, as a name of the Chinese francolin be adduced outright as an additional link between

*cakora* and *chê-ku* : the *cakora* is a bird of Central Asia and eastern Mongolia, while the Chinese francolin, *Francolinus chinensis*, only occurs in south-eastern China and Indo-China (cf. DAVID and OUSTALET, *Les oiseaux de la Chine*, Nos. 572, 579). One would, however, like to know more about the history of the word *chê-ku*. J. DELACOUR and P. JABOUILLE's great work, *Les Oiseaux de l'Indochine française*, 1931, 4 vol., unfortunately does not give native names.

At any rate, I think it possible to establish that the bird referred to by Polo's «cator» is not the *cakora* or «*chakór*», or the *Caccabis chukar*, to use the incorrect form of the name which has prevailed among naturalists. The *Caccabis chukar*, with its red beak and legs, is no other than the bird so well known in Chinese Turkestan as ككلكك *käklik*, already mentioned in the 11th cent. by the *Qutad̄yu bilig* and Kāšyari (BROCKELMANN, 102), and still so called in various Turkish dialects (RADLOV, II, 1063); the name has passed into Kalmuk as *kek'lij* and *häkilij* (RAMSTEDT, *Kalm. Wörterbuch*, 176, 223, who also gives a form *käkilij* in written Mongolian, for which I find no authority); the Chinese name is 石雞 *shih-chi* (cf. ROSS, *loc. cit.* No. 126). As YULE says, this is the bird which was referred to as «francolin» by MOORCROFT. But, more than five centuries before MOORCROFT, Polo mentioned in Persia francolins «different from the other francolins of the other lands, for they are black and white mixed together and they have the feet and the beak red» (see Vol. I, 120). These «francolins» are mentioned a second time, again in Persia (Vol. I, 123). A third mention occurs in that section of the description of Hang-chou (see «Quinsai») which is only found in RAMUSIO («...pernici, fagiani, francolini, coturnici...»); cf. Vol. I, 328). PAUTHIER (*Pa*, 76) explained «francolin» as «gelinotte» (hazel-grouse), *Tetrastes bonasia*, which, owing to the geographical distribution of this bird, is certainly not right. According to YULE (*Y*, I, 99), Polo's «francolin» is «the *Darrāj* of the Persians, the *black partridge* of English sportsmen, sometimes called the red-legged francolin»; CORDIER adds as the scientific name *Tetrao francolinus*. On the other hand, YULE says in *Hobson-Jobson*<sup>2</sup>, 99, that «black partridge» is the popular Anglo-Indian name of the common francolin of south-eastern Europe and western Asia (*Francolinus vulgaris*). But Polo makes a positive statement to the effect that the Persian «francolin» is different from those of other countries. It seems beyond doubt that Polo's «francolin» actually is, as YULE had it, what the Persians call *durrāj* (originally an Arabic word), a red-legged partridge (cf. VULLERS, I, 819; J. L. SCHLIMMER, *Terminol. médico-pharmaceutique*, Teheran, 1874, 66), which must be similar to, but not identical with the European francolin. Bābur, when describing the birds of India, connects the *durrāj* with the *käklik* as to size and song, but considers them different birds (BEVERIDGE, *The Memoirs of Bābur*, 496-497). In fact, the two species are near enough to have often been called by the same name; «red-legged partridge» and «*chakór*» have both been used for the «black partridge» as well as for the *Caccabis chukar* or *käklik*. I am convinced that if Polo had meant to speak of «*chakór*» at Čayān-nōr, he would have simply called them «francolins». As to the *francolini* of «Quinsai» mentioned in RAMUSIO's text, if they really have a Polian origin, it may be that Polo anticipated modern naturalists in recognizing a francolin in our *Francolinus chinensis*; it is surprising, however, that the latter bird should have lived in a wild state as far north as Hang-chou.

But if we dismiss YULE's correction of «cator» to «çacor», we are left with an original text which makes the quail bigger than the partridge. I have no certain solution to suggest. Although

BENEDETTO has in his *Indice* (452, 455) separate entries for «coturnici» and for «quaglie», I do not think that the difference in the words implies for him a difference in meaning; both words occur in F, which he followed in his translation. In the case of F's «cators, que nos apellon les grant perdris», it may be noticed that this is the only occurrence of the form «perdris» in F; in the two other cases where F speaks of partridges, it uses «pernis» (*B*, 27, 31), which is an italianism. So the question may be raised whether «que nos apellon les grant perdris» is not an early interpolation, analogous to the *cioè contornici* added in TA<sup>1</sup>, and due to a copyist who misunderstood the meaning of «cators». In such a case, the term would simply imply quails which were fed for the Emperor's use. There is nothing impossible in this. I may add that YULE's text relating to this subject is not complete, leaving out all the details, certainly genuine, which are to be found only in RAMUSIO.

But there is perhaps another solution. BOERIO's *Dizionario del Dialetto Veneziano*<sup>2</sup>, under *pernice*, says that this is the «common partridge» (*pernice comune*, or red partridge); under *quagia*, that it is the *quaglia*, or *coturnice*, or *cotornice*, viz. the quail. But, under *cotórno*, we are told that this is a name used by sportsmen as a designation of the *Starna maggiore* or *coturnice*, commonly called *Starna di Piacenza* in Tuscany, LINNAEUS's *Tetrao rufus*. *Starna*, not used in the Venetian dialect, is the Italian name of a kind of partridge, and I think that VB's *stornelli* actually represents \**starnelli*, «great partridges», which is also the meaning of *starna maggiore*, given by BOERIO as an equivalent of *cotórno*. It would thus seem that *cators*, *quatornis* (in the chapter on Yazd), etc., may well have to be taken with the Venetian value of *cotórno*, and, in spite of the etymology, be the designation not of the quail, but of a kind of partridge.

## 128. CAUGIGU

<i>caigu</i> Fr	<i>cauçugu</i> , <i>cauzugu</i> Z	<i>gangigu</i> [mellichac (74 r <sup>o</sup> )] V
<i>cangighu</i> , <i>changighu</i> , <i>changigu</i>	<i>caugigu</i> F, FA, FB, P, L	<i>gaugigu</i> F
<i>changiu</i> TA <sup>3</sup>	<i>chaugigu</i> TA <sup>1</sup>	<i>talugigla</i> LT
<i>cangigu</i> Ft, L, R	<i>cjngo</i> VB	<i>tangigu</i> VA

I cannot see why we still find «Cangigu» in *B*, 125, and *Pe*, 293, 378, nor why B<sup>1</sup>, 440, while now adopting «Caugigu», adds that the reading is uncertain. *Ch*, II, 262, is absurd. The name, as already suspected by QUATREMÈRE and D'OHSSON, is evidently 交趾國 Chiao-chih-kuo, «Kingdom of Chiao-chih», Tonking, often so called in YS as an alternative name for An-nan (Annam, but in the sense of modern Tonking); cf. for instance YS, 10, 9 a; 11, 3 b, 4 a, etc. The etymology is confirmed by Rašīdu-'d-Dīn's كَفْجَه كَفْجَه كَفْجَه Kaf'jäh-guh (Y<sup>1</sup>, III, 130; *Bl*, II, 451-452, 499). «Caugigu» and «Kaf'jäh-guh» are Chiao-chih-kuo, just as «Çipingu» and «Ĵimingu» are Jih-pên-kuo (see «Çipingu»). It is well known that the very old name Chiao-chih, through an intermediary Malay form, survives as the first element of our «Cochinchina» (a name which was applied to Middle Annam until the end of the 18th cent.). In 1554, Sidī 'Alī names كَوْجِي Kōjī (for Kōčī),

and writes also كوشى Kōšī (*Fe*, 500, 515, 517, 519). The rich « island » called « Cauchī » of which LEGAZPI heard soon after his arrival at the Philippines in 1571, and the name of which has puzzled Father BERNARD, *Les Iles Philippines*, Tientsin, 1936, 7, is no other than Chiao-chih, *i. e.* Annam.

But, if the etymology is certain, it must also be admitted that Polo's description of the country is rather baffling, and this explains why commentators have thought of Upper Laos (*Pe*, 293; *B*<sup>1</sup>, 440). My own solution is this : while Rašīdu-'d-Dīn has a more correct notion of « Chiao-chih-kuo », Polo heard of it in Yün-nan, as is shown by the order of his narrative, and what he was told refers to the conditions of the Upper Red River, contiguous with Yün-nan; the Upper Laos properly so called was independent of « Annam ». On the other hand, Polo, misled by the second name An-nan (then read An-nam) of modern Tonking, thought that the two names represented different countries, so that he was under the impression that An-nan, but not Chiao-chih, touched the Gulf of Tonking (see « Amu »).

I am one of those who think it possible, and even probable, that Chiao-chih, very early attested in Chinese texts, is also the first component of Ptolemy's « Cattigara » (cf. *TP*, 1932, 181).

Barros has connected his tattooed and cannibal « Gueos » of Laos with Polo's tattooed people of « Caugigu »; it is by some confusion that DAMES (*Barbosa*, II, 167) has thought that Barros's allusion referred to Polo's tattooed cannibals of Fu-chien.

Fra Mauro's « Chauzuzu » (not « Chanzuzu » as in *Zu*, 40, and HALLBERG, 136) is in agreement only with *Z*.

## 129. CAULI

*carli* R  
*cauli* F, LT, P, VL  
*cauly* FA

*causy* FB  
*chalulon* V  
*chauli* VA

*chauly* TA<sup>1</sup>, TA<sup>3</sup>  
*guli* S

Ch. 高麗 Kao-li, Corea. But it appears very doubtful that Nayan should have wielded any authority even over the north-western part of Corea. Cf. *Y*, I, 345; *Y*<sup>1</sup>, I, 303.

Rubrouck writes « Caule » (*Wy*, 270-271); Rašīdu-'d-Dīn, كولى Kaūli (or Kōli?; *Bl*, II, 486, 498). The « Caulij » of the Catalan Map, although wrongly located, and bearing an epithet not traceable to Polo, seems also to represent Corea; cf. HALLBERG, 126.

Rašīd, who sometimes gives Kaūli (or Kōli) alone, uses in his description of the Chinese provinces one of those repetitive forms which YULE (*Y*<sup>1</sup>, III, 125) calls a « double jingle », but which would require a more pertinent explanation. Moreover, in spite of the versions published by HAMMER, KLAPROTH, D'OHSSON and BLOCHET, we still lack a critical study of this description. As to the name of the « province » of Corea, previous editors have read the double name as كولى و كوكولى « Koli and Ukoli »; nevertheless, no « Ukoli » reading is quoted by BLOCHET (*Bl*, 496) from his mss., and he corrects the second form into كوكولى Kokuli = 高句麗 Kao-kou-li, although there is no indication that this old name of Corea had survived in the Mongol period and could have come to the knowledge of the Persian historian. There is for Corea another ancient name which

was known in Central Asia and may have survived in some circles; towards the end of the T'ang dynasty, this was written Mukuri in Sanskrit, and Mug-lig in Tibetan (cf. BACCHI, *Deux lexiques sanskrit-chinois*, revised edition [1929], I, 295). There is just a possibility that Rašid's second name for Corea might have had some connection with this mysterious name, which is also perhaps the origin of the obscure «Muc» used by Rubrouck (*Wy*, 235).

The name of Corea reappears in the West with the «Gori» of Giovanni da Empoli (1514). And, in spite of many controversies, I believe that the name of the «Gores», in Western texts of the 16th cent., has the same origin (cf. Ch. HAGUENAUER's various papers, lastly in *JA*, 1935, 67-115, and 1936, 392-395; cf. also *TP*, 1931, 157; 1932, 190). Fra Mauro's «Gori» (*Zu*, 38; HALLBERG, 231) would appear to be the same name, the knowledge of which should in such an event have reached Europe at least in the middle of the 15th cent.

## 130. CAUYU

*caim* VB, R  
*caiu* F, Fr, FA  
*cauyu* Z  
*caym*, *chaym* LT

*cayn* FB, L, VL, S  
*cayu* F, Ft, FA, P  
*chailu*, *chailu* (?) VA

*chaim* VB  
*chauin*, *pauin* V  
*chayn* TA<sup>1</sup>, TA<sup>2</sup>

This is the excellent reading of Z, more correct than F's «Caiu», and it represents exactly 高郵 Kao-yu, south of Pao-ying, as has been noticed by former editors. «Cauyu» is also given by Fra Mauro (this reading is omitted by ZURLA and HALLBERG).

The name of the *hsien* of Kao-yu goes back to Han times, and lasted until 1368. But the seat of the *hsien* became also the seat of a military area (*chün*) in 971, which continued until 1277, when the *chün* was changed to a *lu* and this in turn to a *fu* in 1284. In 1368, the Ming lowered the status of the *fu* to that of a *chou*, and the *hsien* was merged into the *chou*; this new status of Kao-yu was maintained until the fall of the Ch'ing (*YS*, 59, 11 a; *Ta-Ch'ing i-t'ung chih*, 66, map, 2 b-3 a).

Polo, who never mentions the *chün* or the *lu*, but often the *fu*, did not call it «Kao-yu-fu», although he wrote after 1284; this tends to show that the place continued to be generally known simply as Kao-yu.

## 131. CAXAN

*caxan* Z  
*chasa* V

*chasan* L, L<sup>1</sup>

*chasson* L<sup>4</sup>

This is كاشان Kāšān, also written قاشان Qāšān, south-east of Savah and Qum; cf. on it BARBIER DE MEYNARD, *Dict. géogr.*, 434-435 and 474; *LS*, 209; *Mi*, 80, 133. Fra Mauro gives «Chascian» (not «Chascion» as in HALLBERG, 137).



According to Polo, it was the place of origin of the third of the Magi Kings. Odoric locates at Kāšān («Casan») the place from which the Magi Kings started towards Jerusalem (cf. *Y*, I, 81; *Y*<sup>1</sup>, II, 106-107; *Wy*, 429; HALLBERG, 123-124).

W. JACKSON had supposed that Polo's «Cala Ataperistan» should be identified with Kāšān, but, when he wrote, the name of Kāšān had not yet been traced in Polo himself. JACKSON'S view seems no longer acceptable, although «Cala Ataperistan» must have been in the neighbourhood.

Polo says that the Magi, on their arrival, found Jesus thirteen days old; in Odoric, the length of thirteen days for their journey is given only in John the Long's French version, and it has been taken over by Maundeville. But the coincidence of the thirteen days mentioned by both Polo and Odoric is not due to local information; it represents the thirteen days between the traditional date of the birth of Christ (December 25) and the Epiphany or «Jour des Rois» (January 6), both days included.

BENEDETTO (*B*, 25) is surely mistaken when he introduces a spelling «Cashan» into his text of *F*; *sh* for *š* never occurs in the Polian mss.

Kāšān appears, transcribed 柯傷 K'o-shang (\*Kašang; for the nasalization of *-ān*, see «Badascian»), on the Chinese map of c. 1330 and in the corresponding list of *YS*, 63, 16 *b*; cf. *Br*, II, 112.

Thomas HERBERT mentions at Kāšān, in 1628, a tomb of «Nycador oġlan», though no mention of it is to be found elsewhere (see «Negodar»).

## 132. CENCHU

*cenchu* R

I agree with MOULE'S explanation that this must represent Ch. 千戶 *ch'ien-hu* «chiliarch» (*JNCB*, 1915, 32; 1927, 26-27); that is in exact agreement with Polo's statement that the «Cathayan» whom he calls «Cenchu» «had under him a thousand men». Cf. the similar case of «Vanchu». It is a pity that this explanation should have been ignored in *B*<sup>1</sup>, 440, who adopts «Cenciu» instead of the «Cenchu» of R, our only source, and thinks that the name represents 張易 Chang I, one of the conspirators. But «Cenchu» (where *ch* before *u* = *k*) cannot be reconciled phonetically with Chang I; moreover, it was 王著 Wang Chu who was the initiator of the plot, and we know from Chinese texts that he was really a *ch'ien-hu*. See «Vanchu» and «Acmat».

## 133. CHADI, see ESCEQE

## 134. CHAIERO

*caiaro* VB<sup>2</sup>*cairo* R*chaiero* VB (in *B*)

I think «Caire» must have been Rustichello's French spelling for Cairo; cf. «Kaire» and «Kayre» in Hethum (*Hist. des Crois.*, Arm., II, 230, 232, 347, 348), «Chayre» on the Catalan Map.

Polo generally uses «Babilonie» instead of «Caire», so that YULE suspected an editorial change in RAMUSIO's text which gives «Cairo» in the passage relating to the navigation on the Nile (*Y*, II, 439). *RR*, 353, and *B*, 369, have changed «Caire» into «Babilonia», evidently impressed by YULE's idea that Polo never mentions the name Cairo. But since the name of Cairo is given in *VB*, and so cannot be attributed to RAMUSIO, I prefer to retain it. Polo repeats here oral information of Mussulman origin and uses the Arabic words «çerme» and «Calizene»; it is quite natural then that he should also keep the name Cairo used by his informants.

The question may be raised whether he does not use it elsewhere. In the passage on the bishops, etc., sent abroad by the Nestorian Patriarch, the mss. of groups *F* and *TA* have «Cata» and the like, *i. e.* Cathay, but *Z* writes «Alochayray» and *R* «al Cairo». A mention of Cairo would not now be excluded *a priori*, since we have another in the passage relating to navigation on the Nile. But I agree with BENEDETTO that Cathay must here be meant, especially because there were no Nestorian activities in Egypt in the 13th cent.

To my knowledge, the name of Cairo occurs only once in Chinese texts, *i. e.* in 1225, when Chao Ju-kua writes it 汴野 Chieh-yeh, and calls it a «district» (州 *chou*), not a «city» as in the translation. The transcription, which would suppose \*Këyã, is unsatisfactory, as also the others in that chapter which relate legends of unknown origin (cf. *HR*, 144-145).

## 135. CHARIZIERA

*chariziera* V (and see BERCA)

The name is doubtful, and difficult to identify. One is tempted to connect it with «Tharzara», which occurs in *V* as a corrupt reading for «Berca» (*q. v.*); but Bärkä, who died in 1266 at the latest, is of course impossible here. BENEDETTO (*B*<sup>1</sup>, 429) thought that Baraq (see «Barac») was actually meant. Yet there are serious difficulties in such an identification. Baraq was defeated in 1270 and died *c.* August 1271; but these events took place in eastern Persia and western Afghanistan, and ought not to have exerted a belated and far-away influence upon the progress of the Polos from the Mediterranean to Mesopotamia and western Persia in October 1271.

Another hypothesis would be to connect the Polos' stop at «Laias» with the Mongol advance towards northern Syria in the second half of October, 1271. But the chief officer in command

was Samayar (see «Samagar»; cf. *Oh*, III, 459; *Ha*<sup>1</sup>, I, 286; *Y*, I, 23). There had also been about the time of the campaigns against Baraq several revolts of the princes of the Mongols of Persia. One may have been led by the prince whose name appears in V as «Chariziera», and this name is in its turn almost suggestive of the name of Ḥwārizmī, Qurumši, lit. Khwarizmian, which we know to have been borne by several Chinghiz-khanids (cf. *TP*, 1938, 151). But V, despite its importance, is too corrupt to permit of any safe conclusion when no confirmation is available from other sources. The supposed Christian faith of this grandson (or nephew?, *nievo*) of the Great Khan provides no sufficient clue for further inquiry.

## 136. CHEMEINFU

*chemeinsu* TA<sup>1</sup>

*chemeissu* TA<sup>3</sup>

*chememsu*, *clememsum* P<sup>5</sup>

*clemanfu* L

*clemeifu* Fr

*clemeinfu* F, Ft, FA, FB

*clememfu* L<sup>1</sup>

*clemenfu* R

*clemenifu* VA

*clemensu* LT, P

*clemetissa* VB

*demenif* VL

*glinfu* V

This is the form under which 開平府 K'ai-p'ing-fu was known in Persian-speaking circles, as is shown by Rašīdu-'d-Dīn's کیمینفو Kemin-fu (*Bl*, II, 387, 391, 462, 464, 542). In Waśśāf (*Ha*<sup>2</sup>, 32; Pers. text, 32), کنچافو, wrongly transcribed «Kendschaku» by HAMMER, is altered from کیمینفو Keminfu.

False notions about K'ai-p'ing-fu are prevalent. The site of K'ai-p'ing, north of the Luan river, was chosen and walled, and a palace erected there in 1256 (*YS*, 4, 2*a*), as a summer residence for Qubilai, then Heir-Apparent, but it is a mistake to say, as does for instance *B*<sup>1</sup>, 440, 441, that «Chemeinfu», alias «Ciandu» (see «Ciandu») was the Mongol capital from 1257 to 1264. The capital remained officially at Qara-qorum until Qubilai's accession to the throne in 1260. The princes elected Qubilai at K'ai-p'ing-fu on May 5, 1260, but the place where the diet was held has no bearing on the site of the capital. The geographical section of *YS*, 58, 18*a*, says that, in 1260, Qubilai changed the capital from Qara-qorum to 興 Ta-hsing, that is to say to Peking. Although I do not find any official mention of a change of capital in the *pên-chi* and in spite of many errors in that geographical section, I think the indication is substantially correct. My reasons are twofold. One is that from 1260, all mentions of 京師 *ching-shih*, «the Capital» (see «Quinsai») in the *pên-chi* apply to Peking. The other is that from that date, Qubilai's moves are indicated in relation to Peking: the Imperial car leaves *for* K'ai-p'ing-fu, and arrives *from* K'ai-p'ing-fu (*YS*, 1261, 2nd moon; 1263, 2nd moon; 8th moon). The chroniclers do not think it necessary to mention that it is from Peking that the Emperor leaves and at Peking that he arrives, because it is understood as a matter of course; from 1261, and throughout his reign, Qubilai generally left Peking in the 2nd moon; in the early years of his reign, he returned only towards the beginning of the 9th moon. On the news of Qubilai's

election, the princes of the old Mongol party elected his brother Arīq-bögä in a diet held near the \*Altan river, west of Qara-qorum, towards the beginning of June 1260. On August 18, 1260, Qubilai left K'ai-p'ing-fu and marched against Arīq-bögä; while staying at \*Jöldürgä, Qubilai solemnly denounced Arīq-bögä as a traitor (October 8, 1260). Finally, leaving the leadership of the campaign to his lieutenants, Qubilai, coming back from Qara-qorum, « stopped temporarily » (鞫蹕 *chu-pi*) at Yen-ching, *i. e.* Peking, on January 14, 1261; he took the field again in the course of the year. It seems that this « temporary stay » marks actually the change of the capital. From that time, the central Imperial administration was established in Peking, although this former capital of the Chin and its Imperial palace were then partly in a state of ruin as a result of the Mongol conquest of 1215 (see « Cambaluc »). Qubilai always spent part of the year at K'ai-p'ing-fu, a greater part so long as his new palace in Peking was not completed; but K'ai-p'ing-fu was never his administrative capital, no more than Windsor was of England, or even Versailles of France.

The YS, 58, 3 *a*, says that the name of K'ai-p'ing-fu was only given to Qubilai's summer residence in 1260, and that would leave us in the dark as to its name between 1256 and 1260; but this is an error, as has been shown by WANG Hui-tsu<sup>2</sup>, 8, 1 *b* (to which other mentions of K'ai-p'ing-fu prior to 1260 may be added, as YS, 4, 2 *b*, at the end of 1259). The name must have been K'ai-p'ing-fu already in 1256.

It is generally said that the name of K'ai-p'ing-fu was changed to Shang-tu (see « Ciandu ») in 1264; this is due to a misinterpretation of a sentence in YS, 58, 1 *b*, and to an error in YS, 58, 3 *a*, where the change is attributed to the 5th year *chung-t'ung*, *i. e.* to January-August 1264. But K'ai-p'ing-fu had become Shang-tu before then, on June 16, 1263 in fact (cf. YS, 5, 6 *a*, and the *k'ao-chêng* at the end of the ch.; also WANG Hui-tsu<sup>2</sup>, 8, 1 *b*).

The Mongol tradition (cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 113, 137) speaks of K'ai-p'ing-fu under the name of Šangdu-Käibüng-Kürtü-balyasun, or simply Šangdu-Käibüng. Šangdu is of course Shang-tu, and Käibüng is K'ai-p'ing (on the pronunciation of *p'ing* with *ü*, see « Pianfu »). As to Kürtü-balyasun, it is given in *Ta-Ch'ing i-t'ung chih* (409, II, 2 *a*) as the local name of the 桓州 Huan-chou of the Yüan, a little to the south-west of K'ai-p'ing-fu; the Mongol chronicler thus seems to have connected wrongly that third name with K'ai-p'ing-fu. SHIRATORI, followed by YANAI, 633, has explained *kürtü* by Mong. *qoriya*, *qoryan*, etc.; this is impossible, owing to the difference of « class ». The *kürdü*, « circle », « wheel », of KLAPROTH and of BLOCHET (*Bl*, II, 387, 462, and App. 42) would be preferable phonetically, but does not make much sense. The name might represent *kürtü* < *kä'ürtü*, [« place » with corpses]; then Kürtü-balyasun would mean « City of the Dead »; but in such a case, we should expect \*Kä'ürtü in the Mongol chronicle. For the geographical identification of K'ai-p'ing-fu, see « Ciandu ».

It is somewhat surprising that in the narrative of the voyage, Polo should speak of « Chemeinfu », and of « Ciandu » in his description of the country and of its customs, without stating anywhere that both names are equivalent. But nowhere does he say either that his Sea of Sarai (see « Saray ») and his Sea of « Gel or Chelan » (see « Gel or Chelan ») are the same as his Sea of Baku (see « Bachu »). Perhaps, although he knew the identical value of these various names, he failed to explain them to Rustichello, who naturally could not go beyond

Polo's words. Polo heard of course the name of « Ciandu » at Qubilai's court, but « Chemeinfu » remained in use in Persian-speaking circles, and it is only this last name which occurs in Rašidu-'d-Dīn.

## 137. CHEMIS

<i>charanis</i> VA	<i>chemisi</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>chenus</i> LT
<i>chemenis</i> V	<i>chems</i> Port.	<i>guemis</i> FB
<i>cheminis</i> VL	<i>chemurs</i> R	<i>quemis</i> FA
<i>chemins</i> F, L, P		

Although we generally refer to mare's milk as *qumīz* (*kumīz*, *kumis*), it is the form *qīmīz* which was given in 1076 by Kāšyarī (BROCKELMANN, 154), and which is represented by Polo's « chemis ». In 1248, Simon of Saint-Quentin wrote « kamous » (Vincent de Beauvais, *Spec. hist.*, xxx, 78). In Rubrouck's text, the reading *comos* is certainly correct, rather than the generally accepted « cosmos ». The word is Turkish (cf. also RADLOV, s. v. *qīmīz*, *qīmīs*, *qumis*, and TP, 1930, 287; I think the form with -u- belongs originally to Eastern dialects). It has passed into Persian and Arabic under the forms قمیز and قمر, *qīmīz*; cf. *Bl*, II, 65<sup>4</sup> (but قموزچیان *qumūzčīān* in *Bl*, II, 283-284, seems to me a wrong correction; the form *qobūzčīān* of the mss. gives a good sense, « player of *qobuz* »), BLOCHET, *Moufazzal*, 610, and QUATREMÈRE, *Hist. des Sultans Mamlouks*, I, II, 147 (important for the use of that drink among the Mamluks of Egypt). It is *qīmīz* which is misread قمیر *qīmīr* in VULLERS, II, 743. Cf. also Y, I, 259, and on *qara-qīmīz* (« caracomos » of Rubrouck), *JA*, 1920, 169-171. In Mongolian, the word for mare's milk is *ūsük* in the *Secret History* (§§ 28, 31), and *ūsük* in « Sanang Setsen » (SCHMIDT, *Gesch. der Ost-Mongolen*, 60); but there is also a Mongol word *kimur* corresponding to Turk. *qīmīz* (cf. RAMSTEDT, *Kalm. Wörterb.* 231). Russian chronicles mention *kumīz(a)*, *komuz(a)*. s. a. 1185 (cf. P. MELIORANSKII, I, in *Izv. Otd. R. yaz. i slov. I. Ak. N.* 1905 g, x, No. 4, 122, who derives the word from Turk. *qtm*, « to shake »). Cf. also LOKOTSCH, *Etym. Wörterbuch*, No. 1242.

## 138. CHERMAN

<i>cherman</i> , <i>chiermain</i> R	<i>crema</i> TA <sup>1</sup>	<i>crerina</i> VL
<i>chermam</i> Z	<i>cremain</i> F, L, V	<i>crermain</i> F, Ft
<i>chrerina</i> , <i>trierianam</i> VA	<i>cremam</i> LT	<i>crerman</i> F
<i>cinance</i> , <i>cremen</i> , <i>querman</i> FA	<i>creman</i> F, Fr, FA, FB, L, LT,	<i>cretina</i> G
<i>craman</i> (cor.), <i>creimain</i> , <i>etiman</i> ,	P, TA <sup>1</sup> , TA <sup>3</sup> , VB	<i>quermam</i> FA, FB
<i>raman</i> , <i>tremain</i> V	<i>cremina</i> TA <sup>3</sup>	

Ya'qūt writes كرمآن *Kārmān*, adding that it is also pronounced *Kirmān*, which seems to him less correct (BARBIER DE MEYNARD, *Dict. hist.* 482). The modern form is *Kermān* or *Kirmān*, a province in Persia. On the city which is referred to by Polo in *Kermān*, and which is the

present city of that name, cf. *Y*, I, 91-92; *LS*, 299-321; *Mi*, 373-376. From misreadings in Polian mss., the name has become «Creman» on the Catalan Map, «Chreman», «Chremanía» and «Charmania» in Fra Mauro (cf. HALLBERG, 119-120).

Hethum mentions in «Media» the two towns of «Seras» (= Šīrāz) and of «Queremen» (*Hist. des Crois.*, Arm., II, 127, 267); the last name has been adopted by Fra Mauro (HALLBERG, 423). Hethum's editors say that «Queremen» is the mediaeval Kirmānšāhān, now called Kirmānšāh (on this city in Chinese texts, cf. *Br*, II, 126-127). But Kirmānšāh is far in the north-west of Persia, and the limits given by Hethum, as also its mention alongside of Šīrāz, make me think that «Queremen» is simply Kermān, thus appearing twice, with different spellings, on Fra Mauro's map. Since Hethum knew Kermān already as the name of the city, we can understand why Polo does not name the city; he knew it under the same name as the province (cf. also *LS*, 303).

Two parallel Chinese texts name 乞里彎 Ch'i-li-wan (read Ch'i-li-[蠻]man?), Kirmān, and its great city of 拔里寺 Pa-li-ssü (Bardsīr; on this city, cf. *LS*, 303; *Mi*, 375; the Chinese transcription represents a form Bardsīr, not Bardasīr or Bardasīr; the text stops short after that name), when speaking of Hülägü's campaign (cf. *Br* II, 147). A mediaeval list of precious stones mentions the turquoises called *ch'i-li-ma-ni*, *kirmāni* (cf. *Br*, II, 176), the same stones that are praised by Polo. It is more doubtful whether the country Ch'i-li-ma-êrh, which sent envoys to China in the beginning of the 15th cent., is Kirmān (cf. *Br*, II, 290); the final -r cannot be accounted for.

Polo must have passed through «Cherman» twice, during the outward journey in 1272, and while escorting the princess «Cocacin» in 1293. The rulers of Kermān, under the suzerainty of the Mongol *ilkhān*, were then the descendants of the Qara-Ĥitai Qutluy-ĥan (cf. on them *EI*, notice «Kutluĥ-Khān», by MINORSKY).

### 139. CHESCEMIR

<i>casimur</i> , <i>chasimur</i> , <i>chisuamor</i> ,	<i>chesimur</i> FA, FB, P, VA	<i>kesimur</i> F, Ft
<i>quesmun</i> V	<i>chesmir</i> Z; R	<i>quechimur</i> FA†
<i>ceschamur</i> , <i>chesemir</i> VB	<i>chesmur</i> Fr; R	<i>quesimur</i> FA, FB
<i>chesciemur</i> , <i>chesimur</i> F	<i>chessimum</i> , <i>chessimun</i> TA <sup>3</sup>	<i>quesmur</i> F, L, VB
<i>chesiemar</i> FB	<i>chiesiema</i> FA	<i>thasimur</i> VL
<i>chesimior</i> G	<i>kesimui</i> L	<i>thesimur</i> LT
<i>chesimu</i> , <i>chesimun</i> TA <sup>1</sup>		

(c. 36)

*a a nom chiesiemar* FB  
*aanonaarion chiesiemar* FB<sup>1</sup>

*a non Ariora chiesiema* FA  
*arionichisor ennor* VA

*ariora chiesiemar* FA<sup>2</sup>

I had first adopted «Chesmir» which is given in Z (hence «Chesmir» in R and on Fra Mauro's map) and which is a fairly correct representation of Kašmir, Kashmir; but I now think it

more probable that Polo or Rustichello wrote « Chescemir », so as to show that the name had -š- and not -s- (cf. the case of « Abasce »).

The « Ariora Chiesiemar », etc., which has been adopted in YULE, and for which STEIN'S identification with Agror has been accepted in *Y*, III, 22, 24, and in *Pe*, XLI, is an *idolum libri*; « ariora » is altered from « a a non », « is named » (cf. *B*, LXXVIII), and must be put on a level with « Anonichesiemur » and « Chiamala Chesmur » in HALLBERG, 122.

In 1076, Kāšyarī mentions separately Qašmīr, our Kashmir (BROCKELMANN, 247), and Kāšmir, a city in the land of the Turks, built by Salomon (*ibid.* 245). A. HERRMANN (*Die älteste türkische Weltkarte*, p. 28; cf. also *TP*, 1936, 361-363; the identification of Kāšmir with Chih-shih-mi of CHAVANNES, *Doc. sur les Tou-kiue*, 13, is hardly possible phonetically) has accepted the distinction, about which I am still doubtful; other writers spell indifferently Kašmir and Qašmir for our Kashmir; cf. *Mi*, 494.

Kashmir has been known to the Chinese from the Han times, first under the name of 罽賓 Chi-pin. For the T'ang period, cf. CHAVANNES, *Doc. sur les Tou-kiue*, 336. It is mentioned many times in the Mongol period, at first in the accounts of the campaigns in Central Asia, later on because of the influence of lamaism and Indian sorcery. In the *Secret History*, the Mongolian form of the name is Kāšimir (§ 262, 270). For the Chinese texts, cf. *Br*, I, 137-138; II, 26; many others could be added.

For the order given in 1252 by Mongka to Sali *noyan* to conquer Kashmir, cf. *Br*, I, 138, and see « Caraunas ». In the *Secret History*, completed in 1240, the name of « Kāšimir » appears, strangely enough (§ 262, 270), in the list of the countries of the Caucasus, Western Siberia, and Eastern Europe which were conquered by Sübötai. But the first Mongol inroad into Kashmir seems to be of 1241 (see « Caraunas »); moreover, Kashmir was quite out of the area of Sübötai's activities (cf. *Br*, I, 305). If we could really prove the existence of the city of Kāšmir in the lands of Turks mentioned by Kāšyarī, this might perhaps help us out of the difficulty; in such a case, it should also be with that « Turkish » Kāšmir that we ought to identify the « Cosmir » which Plan Carpine in 1246 names among the countries conquered by the Mongols (*Wy*, 89).

## 140. CHEYNAM

*cheinan* R

*cheynam* Z<sub>1</sub>

Z has « Cheynam », R « Cheinan »; they go back to a common source, and the original form was very probably \*Cainam, *i. e.* 海南 Hai-nan (still pronounced Hai-nam in the Mongol period). The term, which means « South of the Sea », is well known as being now the name of the Island of Hai-nan; YULE (*Y*, II, 266), still followed by *Pe*, 239, was wrong in thinking that « Cheynan » (« Cheynam » < \*Cainam) may represent « An-nan » (= Tonking).

We should not suppose, however, that Polo's \*Cainam is derived direct from a true name of the island. Hai-nan, as applied to the island, is a vulgar, unofficial name, and the date at which

it came into common use has not been ascertained. Nevertheless, it goes pretty far back, and its use under the Sung is proved by *Yü-ti chi-shêng*, 124, and in 1225 by Chao Ju-kua (*HR*, 175-190).

In the Mongol period, from about 1278, Hai-nan was officially associated with 海北 Hai-pei, «North of the Sea», in the title of a commissioner (*hsüan-wei-ssü*) named «Commissioner of North of the Sea and South of the Sea», who was stationed on the mainland, at Lei-chou (*YS*, 63, 7 b-8 a). And already in the beginning of 1279, we find an edict mentioning «the districts (*chiün*) of Ch'üing, Yai and Wan of South of the Sea»; these districts were all in the island of Hai-nan. The popular name thus fell in with the official designation. Hai-nan by itself is referred to in 1329 (*YS*, 33, 5 b), and a work of 1349-1350 speaks of «cottons of Champa and Hai-nan» (*TP*, 1915, 123). So Polo was amply justified in applying the name to the whole Gulf of Tonking which, with the island of Hai-nan, was the «South of the Sea» section of the Lei-chou Commissariat.

The Catalan Map of 1375 shows the island of «Caynam», with the correct form of the distorted name as it appears in Z and R; BUCHON took it to be the Andaman Islands, but the true identification with Hainan has already been adopted in *Y*<sup>1</sup>, I, 301, and CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 10, 23-24, 45. Opposite the island and on the mainland, the Catalan Map has an important town, with the legend: *Cjutat de caynã açj finis catayo*. The second part of this corroborates the identification, as Hai-nan is the southernmost portion of China. But the city of Caynã = Caynam raises a difficult problem. CORDIER disposed of it by saying that the city was a fancy of the cartographer. The question is not so simple.

Rašidu-'d-Din mentions in Kafjäh-guh, *i. e.* Chiao-chih-kuo, Tonking (see «Caugigu»), two towns neither of which has been satisfactorily identified. BLOCHET (*Bl*, II, 499), following QUATREMÈRE (*Hist. des Mongols*, xcv; cf. also *Y*<sup>1</sup>, III, 30), reads one of the names as خينام Hāinam; but while QUATREMÈRE took this name to be that of the island of Hai-nan, BLOCHET identifies his Hāinam with «Qainam», marked in MARTINI's *Atlas Sinensis* as a town of «Kiaochi, sive Couchinchina». The reading of the name in Rašid is doubtful. D'OHSSON (*Oh*, II, 640) adopted hypothetically «Djessam»; BLOCHET's mss. write حسام, صيام and صيام; *h* is of rare occurrence in Rašid's transcriptions of Mongolian and Chinese names, and one might think of readings like \*Činam, etc. Nevertheless, Hāinam is probably correct. YULE (*Y*<sup>1</sup>, III, 131) has already quoted from the first edition of the first volume of ELLIOT's *History of India* a passage where Rašid describes a sea-route from India to China by way of Champa, «Haitam» subject to the Great Khan, and Mahačün (=Canton). «Haitam» YULE took hypothetically for Hai-nan, and this was confirmed in ELLIOT's revised edition of the first volume (p. 71), where we find Hāinam, not Hāitam.

Of course, MARTINI is no authority for Indo-China; his second-hand information of the 17th cent. carries no weight for the 13th or 14th; and there was no city of «Qainam» south of Tourane in his time. But MARTINI never meant to locate a «Qainam» in Annam. In his system of spelling, *q* is always followed by *u*, and in spite of a blurred engraving, the form he meant is in fact «Quinam», that is to say, the name then given in European documents to Annam in general, or to the port of Faifo; cf. *Bull. des amis du Vieux Hué*, I, 337-340, 347-351; *BEFEO*, xxxvi, 114.

But the curious case of the «city» of the Catalan Map remains unexplained. The tradition of such a city lingered for two centuries. In 1554, Sidi 'Ali Čäläbi mentions a port of اينم Aīnam



between Šanbā (Champa) and the Gate of China, and this is likely to be Hai-nan (cf. *Fe*, 501, 684; *Instr. naut.* III, 165-166); at that period, the initial *h-* (*h-*) of Chinese names was sometimes dropped in transcriptions, as in the Portuguese *aytão* instead of \**haytão*, 海道 *hai-tao*, « sea-commissioner » (cf. DALGADO, *Glossário Luso-Asiático*, I, 18<sup>a</sup>).

It seems difficult to separate the names given by Rašīd, in the Catalan Map, and last by Sīdī ‘Alī, although their use of these names may be somewhat different. It is possible that a port on the island of Hai-nan is meant by Sīdī ‘Alī, if that port was then a regular port of call for the Mussulmans. If the « city » of the Catalan Map has not been placed on the mainland by mistake, it ought to be Lei-chou, and we must then suppose that the seat of the Commissariat, although on the mainland and consequently « North of the Sea », had come to be popularly named « South of the Sea », because the « South of the Sea » districts of the Commissariat were the most active in foreign trade. Either of the two values would do for Rašīd’s sea itinerary from India to China. But the Persian historian must be mistaken when, elsewhere, he makes Hāinam a city of Annam-Tonking and not of China, unless in this last case our reading of Hāinam be itself an error.

#### 141. CHISCI

*achisi*, *quinsi* LT  
*altriso*, *quisai* S  
*chis*, *chysci* Z  
*chisci* Z, V, L  
*chisi* F, FA, TA<sup>1</sup>, LT, P, V,  
 VA, VL, S; R

*chisin*, *quixi* VA  
*chisti* R  
*chisy* FA  
*chysi* FB  
*cisy*, *cysi*, *quiz* FB

*ochoisi* VB  
*risi* VL  
*qisci* F, L, Z  
*quis* FA, L  
*quysi* G

The island of Kīš. The mss. authorize only « Chisci », but I think « \*Chisce » has a fair chance of being the original spelling, of the same type as « Abasce », « Chescemir ». In any case, I prefer « Chisci » to the « Kisi » of RR, 425, and the « Chisi » of B<sup>1</sup>, 441.

The Persian form is کیش Kīš, in Arabic قيس Qīs and more often Qāis; cf. BARBIER DE MEYNARD, *Dict. hist.* 468, 499; it seems that the Arabic pronunciation Qāis is now the usual one. « Guase » and « Kena » of Y<sup>1</sup>, I, 144, « Ghes » and « Kenn » of Y, I, 64, and of Y<sup>1</sup>, I, 145, « Geise » or « Kena » of Br, II, 130, « Ghez » or « Kem » of RR, 425, do not exist as names of the island (cf. DAMES, *Barbosa*, I, 80-81). Polo speaks of Kīš from hearsay, and not very accurately (cf. Y, I, 65; II, 452-453); for mediaeval mentions of Kīš, cf. also *Fe*, 68, 389; *LS*, 257; and the notice « Kāis » of EI, by STRECK.

The island of 記施 Chi-shih, Kīš, was described by Chao Ju-kua in 1225 (*HR*, 117, 133-134). The Chinese map of c. 1330 and the corresponding list of YS, 63, 16 b, name 怯失 Ch’ieh-shih, Keš (= Kīš); cf. *Br*, II, 129-130.

Guillaume Adam, in 1316-1318, mentions « Chyx » (= Kīš), wrongly identified with Kīšm in the Introduction (*Hist. des Crois.*, Arm., II, cxcii, 553).

The Catalan Map of 1375 has « Chis »; the « Raff » or « Kaff » of Schiltberger seem to be misreadings for « Kais » (HALLBERG, 142, 431).

The Portuguese text of Barbosa does not mention Kiš, but it appears in the Spanish version and in RAMUSIO's edition as « Cuyx » and « Quixi »; DAMES (*Barbosa*, I, 80) thinks that the name has been added by the Spanish editor, familiar with earlier narratives such as that of Polo. It may be so, but the Spanish version and RAMUSIO have in this section other names which are not in the Portuguese version and do not go back to earlier travellers; why should Kiš be an exception?

The supposed omission of Kiš by Barbosa is attributed by DAMES to the fact that it had been replaced in importance by the greater island of « Kishm » (Kišm and Qišm), the « Queixime » of Barbosa. According to DAMES, the name of Kišm or Qišm was still unknown in the 13th cent., and YULE, *Hobson-Jobson*<sup>2</sup>, 485, cites no mention of it earlier than 1538; cf. also « Kishm », by Cl. HUART, in *EI*. Nevertheless, it may have existed almost in Polo's time. In 1307, Hethum names a « Quissim » or « Quissan », which the editors have identified with Kiš (*Hist. des Crois.*, Arm., II, 127, 267; cf. also HALLBERG, 33, s. v. « Aqvissa »), and so does HALLBERG (p. 429-430) in the case of Fra Mauro's « Quissan », evidently borrowed from Hethum's Latin version. But Schiltberger, who mentions a « Kaff » probably corrupt for « Kais », Kiš, names also a « Reschan » or « Keschon », different from the precedent, in which the editors have seen Kišm (cf. HALLBERG, 433). But « Keschon » (read « Kescham ») must be identical with Hethum's « Quissim » or « Quissan » (read « Quissam »?), and, in such a case, the name of Kišm would really go back to the Middle Ages. It would even be necessary to admit, although it sounds surprising, that the name of Kišm alone, and not that of Kiš, came to Hethum's ears.

## 142. CHONCHA

*cancha* TA<sup>1</sup>

*changha* TA<sup>3</sup>

*choncha* F, FB, L, V, VA, Z

*chonka* FA

*chontan* G

*concha* LT, P, P<sup>5</sup>, VB; R

*thoncha* FB

*toncha* FA, VL

The name occurs also in Fra Mauro's map, as « Choncha » (not « Concoa » as in *Zu*, 38, and HALLBERG, 143).

According to most mss., « Choncha » was the name of the « kingdom » the main city of which was Fu-chou (see « Fugiu »). Several explanations have been suggested : 江浙 *Chiang-chê* by Klaproth and Neumann, 建國 *Chien-kuo* by Pauthier (*Pa*, 526) and Blochet (*Bl*, II, 493), both rightly rejected by Yule (*Y*, II, 232). Yule also mentions the hypothesis of a derivation from the name of Ch'üan-chou (see « Çaiton ») and the phonetic resemblance of « Choncha » with Chung-kuo, the « Middle Kingdom », China, but himself recognizes that this leads nowhere. I may add that, contrary to what Yule believed, « Chung-kuo » probably does not occur in Persian sources (see « Catai »). Charignon's 諸家 *Chu-chia*, « Chu people » (*Ch*, III, 109-110), is entirely unsupported and not worth refutation. A colleague has suggested to me a correction

\*Chochan, which would approximately represent the Fuchienese pronunciation of 福建 Fu-chien itself. I had myself thought of a somewhat analogous solution: «Choncha» may be < \*Chocha = 福家 \*Hok-ka, «Fu-chien people», a possible synonym of the modern Hok-lo, and built up in the same way as for instance Nan-chia, «Southerner», or the modern Hak-ka. But I do not know of any certain case when a name in Polo is to be explained from a Chinese dialectical pronunciation. One may also imagine a misapplication by Rustichello of a name which actually referred to the Canton province, not otherwise mentioned by Polo, and which would correspond to the كونگی Kōngī, لومکالی Lūmkali or کوبلیکی Kūilki of Rašidu-'d-Dīn (*Y*<sup>1</sup>, III, 126; *Bl*, II, 493, 499), itself unexplained (see «Cin»).

But I am afraid that no derivation whatsoever can be valid, since I doubt the very existence of the name «Concha». This is the view taken by BENEDETTO. Polo speaks several times of the «kingdom of Fugiu», and only once says that the «city of Fugiu» is the capital of a kingdom called «Concha». Now, there are in Polo quite a few passages where we are told that a city bears a certain name and that the province or the kingdom bears the same name; but more than once the readings of the name are so much at variance that they have been accepted by some editors as representing different originals. Such is the case for instance for «Cuigiu» (*q. v.*), in which PAUTHIER (*Pa*, 432-433) saw a province of «Cuigiu» and a city of «Fungul». BENEDETTO suggested (*B*, 156) that «Choncha» was corrupt for «Fugiu», and that *com ele*, «like it», or some similar expression had been dropped. Consequently «Choncha» has disappeared from his translation (*B*<sup>1</sup>, 268); and while it is retained by RICCI-ROSS, it is said in their Index to be a mistake for «Fugiu» (*RR*, 258, 417). I incline to the same opinion. An intermediary form seems to be provided by VB's «Cagui» or «Cangui» as the name of the city «Fugiu», which form has passed into a passage of RAMUSIO as «Cangiu»; and the name of the «kingdom» is «Cancha» in TA<sup>1</sup>. But the reading «Choncha» must have been a very early corruption to have become so general. It is just because it is so general that I still hesitate to reject it altogether.

### 143. CIAGANNOR

*ciagannuor* F, L  
*ciagatimor* LT  
*ciagnuorum* VL  
*ciagunor* VA  
*ciagamor* P

*cianganor* R  
*cyagannor* FA  
*gauor* TA<sup>1</sup>  
*gharuor* TA<sup>3</sup>

*gianga* V  
*gianganor*, *zanganor* VB  
*goangamor* Pr  
*siasamor* FB

This name has long been recognized to be Čayān-na'ur > Čayān-nōr; it means in Mongolian «White Lake», as correctly stated by Polo. Fra Mauro's map gives «Zaganaor» (*Zu*, 35; HALLBERG, 173), and it is possible that the correct reading in Polo's text was \*Ciagannaor, partly supported by forms like «Ciagannuor» in F and L. On the Catalan map, there is a city the name of which is written «Cinginar» in BUCHON's text (*Not. et Extr.* XIV, II, 143), but «Cingonor» on

his facsimile map (after p. 132) and also by RUGE (HALLBERG, 150). It was equated with Hsi-an-fu by BUCHON, and this has been repeated by HALLBERG; but the identification, based on phonetic analogy, is valueless since the name Hsi-an-fu did not exist in Polo's time, and the city was then known as «Quengianfu» (*q. v.*). In the meantime, CORDIER, whose facsimile map gives «Cinganar», had said that this «Cinganar» was Polo's «Ciagannor» (*L'Extrême-Orient dans l'Atlas Catalan*, 20). I do not think that CORDIER was right. Although I cannot identify «Cinganor» or «Cinganar», the location seems to be too southern for «Ciagannor». Moreover, CORDIER left out of account a name «Ciagamor», the location of which fits «Ciagannor» much better and which was already identified with «Ciagannor» by BUCHON (cf. BUCHON's map after p. 132, and the text p. 134; RUGE reads «Ciaganor»; cf. HALLBERG, 173).

The name Čayān-nōr is fairly common (nine Čayān-nōr are listed in the Index to POPOV, *Mên-gu-yu-mu-czi*, 73), and in Mongol times we find a Čayān-nōr («Čayān-nāūr») as far away as Persia (*Oh*, III, 415). Two Čayān-nōr are mentioned in North China in Polo's time. One, where the prince Ananda resided for a time, was in «Tangut»; it is known mainly from Rašidu-'d-Dīn's account who calls it چغان ناور Čayān-nāwūr (*Bl*, II, 496, 501; *Y<sup>1</sup>*, III, 127 [where «Fanchán Náúr» is a corrupt reading], 132; *iv*, 162 [which I cannot trace]). Its location is still doubtful. It could not lie as far north as lat. 45° 45' (with east long. 96°) as was supposed by YULE (*Y<sup>1</sup>*, III, 133) and given as a fact by HOWORTH (*I*, 176). Still less could it be lat. 48° 10' and long. 99° 45', as given without comment by PENZER (*Pe*, XLIV), which would carry us north of the Altai, far to the north-western corner of Mongolia. I think that PENZER's «48° 10'» is a misprint for «45° 10'», and that the location is simply due to the fact that there is a Čayān-nōr with approximately such coordinates on our maps. But this Čayān-nōr is out of the question here. Prince Ananda's palace must have been in Shàn-hsi. This makes it difficult, however, to account for the following passage in Rašidu-'d-Dīn as translated by QUATREMÈRE (*Not. et Extr.*, XIII, 1, 235; I cannot trace the original Persian text): «When one descends below the Čayān-nōr, one is near the city of Qarā-Ḥwājah (see 'Carachoço') in the Uighur country, where they make good wine.» If I am right, the Čayān-nōr of «Tangut» must have been at a great distance from the region of «Carachoço» or Turfan. Unfortunately, I cannot find any certain mention of the Tangut Čayān-nōr in Chinese sources of the Mongol period (CHANG Hsing-lang, *Chung-hsi chiao-t'ung shih-liao hui-p'ien*, *iv*, 276, ignores it entirely; *YS*, 100, 2 *a*, is at least ambiguous). The place remained known, however, at the beginning of the Ming dynasty, and it seems to have been in the region of the Ordos, inside the great bend of the Yellow River, somewhat west of Yü-lin and north of the district of Huai-yüan (now Hêng-shan); cf. *Ti-ming ta tz'ü-tien*, 1086. In 1370, when the Ming armies were active in Shàn-hsi and Kan-su against the last upholders of the Mongol dynasty, a Chinese general, after Lan-chou had been conquered, entered Ning-hsia, and thence proceeded to Čayān-nōr, Tung-shêng (*i. e.* Tohto, at the north-eastern angle of the great bend of the Huang-ho), Ta-t'ung and Hsüan-fu (*i. e.* Hsüan-hua; see «Sindachiu»). This text of the *Ming shih* (126, 6 *a*; cf. also 124, 2 *a*) seems decisive in favour of the location adopted in the *Ti-ming ta tz'ü-tien*, and is supported, moreover, by a proposal made in the middle of the 15th cent. to remove to Čayān-nōr a neighbouring garrison of the Ordos.

While much remains to be elucidated in connection with the Čayān-nōr of Tangut, we can reach

a fairly accurate solution in regard to the Čayān-nōr referred to in Polo's text. Chinese sources often speak of it as 察罕腦兒 Ch'a-han-nao-êrh, sometimes 茶罕腦兒 Ch'a-han-nao-êrh (cf. WANG Hui-tsu<sup>2</sup>, 49, 6 a), or, translating the name, as 白海 Pai-hai, «White Sea» (i. e. White Lake; cf. YS, 160, 7 a; 166, 5 a). A «temporary palace» (行宮 *hsing-kung*, sometimes 行殿 *hsing-tien*, or 行營 *hsing-ying*), with clay walls, had been erected there in 1280 under the supervision of 蔡珍 Ts'ai Chên (YS, 11, 2 a; 12, 3 b; 166, 5 a). The *Ti-ming ta tz'ŭ-tien* is evidently mistaken when it takes the erection of the palace of 1280 to refer to the Čayān-nōr of Tangut.

Different identifications have been proposed for Polo's Čayān-nōr. I leave out of account the one which is given by BENEDETTO (*B<sup>1</sup>*, 441) as being due to PENZER and which would locate Čayān-nōr at lat. 48° 10', long. 99° 45'. As I said above, this is the identification adopted by PENZER for the other Čayān-nōr, and, although it is wrong, PENZER is not responsible for a bad blunder which would carry Polo, while on his way from Ning-hsia to Shang-tu, to the other end of Mongolia (cf. moreover *Pe*, 194). CORDIER's assertion (*L'Extrême-Orient dans l'Atlas Catalan*, 20) that the ruins of Polo's Čayān-nōr are at Čayān-ḥoto, «near Kōkō-Ḥoto or Kuei-hua-ch'êng» (i. e. near Sui-yüan, just beyond the north-eastern angle of the great bend of the Huang-ho), is also a glaring error. Neither can RICCI-ROSS's suggestion of an unknown «Hsin-hua» (*RR*, 416), whether it be a slip for «Hsüan-hua» or for «Hsing-ho», be retained. DEVÉRIA tentatively identified with Polo's Čayān-nōr the Čayān-tsang where the Emperor Jên-tsung (Buyantu-khan) signed an edict in the seventh month of 1314 (*JA*, 1896, II, 398) and this has been repeated by CHAVANNES (*TP*, 1904, 426). It is true that Jên-tsung, who had left Peking for Shang-tu (see «Ciandu») in the second month of 1314, only returned to Peking in the eighth month (YS, 25, 1 a, 2 a), and he may well have gone in the course of the seventh month from Shang-tu to Čayān-nōr for hunting purposes. But DEVÉRIA's hypothesis merely rests on the fact that there is a common element *čayān*, «white», in both Čayān-nōr, «White Lake», and Čayān-tsang, «White Granary». This is not enough to establish a real connection between the two names. ▼

A long-accepted theory, partly based on the opinion of Chinese geographers of the 18th cent., located Polo's Čayān-nōr immediately east of the «Anguli-nōr» of our maps, at a place called «Tsagan balgassu» (= Čayān-balyasun, «White City»). This is still the view maintained by YULE (*Y*, I, 297) and by PENZER (*Pe*, 194). If YULE had lived long enough himself to prepare for the press the third edition of his *Marco Polo*, I have no doubt that he would have changed his opinion in view of the information given by BRETSCHNEIDER in *Recherches archéol. et histor. sur Pékin*, 91, 93, 129-131.

One point must be made clear : «Anguli-nōr», though the form is corrupt, represents the very name under which this lake was known in Mongol and even Chin times. In the *Chin shih* (24, 7 a), mention is made of it as 昂吉灤 Ang-chi-po, «Ang-chi Lake» (BRETSCHNEIDER's «Ang-chi-[里]li» is the result of some oversight), also called 鴛鴦灤 Yüan-yang-po, «Brahminy-duck Lake». As BRETSCHNEIDER says, the second name is the translation of the first. *Ang-chi*, then pronounced Ang-ki, is the transcription of the Altaic name of the brahminy duck, *Casarca rutila*, and it is sometimes also applied to the cognate species «mandarin duck», which is properly the *yüan-yang* of the Chinese. The oldest Turkish form is *angit* in Kāšyari (BROCKELMANN, 9, where it is left unidentified); in Mongolian, we find *anggir* in the *Secret*

*History* (§ 78). Later Turkish dialectical forms are *angar*, *angyar*, *angqir*, *angqur* (RADLOV, 1, 184, 186, 187), *angqut* (PAVET DE COURTEILLE, *Dict. turc-oriental*, 38), *hangyut* (SHAW, *Vocabulary*, 215), *hangyirt* (at Turfan; cf. VON LE COQ, *Sprichwörter*, 98), *hang-yirta* (read *hangyirt*; in ROSS, *A Polyglot list of birds*, No. 157). I shall not discuss here the various finals *-i*, *-r*, *-rt* in Turkish; in Mongolian, the word is always *anggir* (Kalm. *ängr*), plur. *anggit*. An *-u* vowel in the second syllable occurs only in western Turkish forms, and I suspect that the modern Chinese transcription 昂古里 Ang-ku-li is the result of a clerical error for Ang-chi[吉]-li, rendering an original *anggir*. It is this *anggir* which occurs already as *ang-chi* in the *Chin shih*, but I do not think that it is the transcription of a word really used in Jučen. In Manchu, *anggir niyehe*, «*anggir duck*», was borrowed from the Mongolian at a late date. We have two Jučen vocabularies, one edited by GRUBE, the other unpublished: both give for *yüan-yang* a word \**guyahu* or \**guyahung* which has nothing to do with *anggir*. We are thus led to believe that the *Chin shih* gives us the Mongol name of the lake, either because the Chin inherited this name from the Ch'i-tan, or because the population of the region spoke Mongolian. At any rate, the persistence of the Mongol name from the 12th cent. down to our days precludes the possibility that it might have been superseded by that of Čayān-nōr in the 13th and 14th cents.

Still more than the name, the study of the postal stages and the itineraries between Peking and Shang-tu in the Mongol period proves that Polo's Čayān-nōr cannot be the ruined city east of the Anguli-nōr which was spoken of by TIMKOVSKIĪ, PALLADIUS and YULE, and which is called Čayān-balyasun in Mongolian, and 白城子 Pai-ch'êng-tzū in Chinese, both meaning «White City». The discussion, started by BRETSCHNEIDER, has been pursued in much greater detail by YANAI, 752-768. It seems clear that Pai-ch'êng-tzū is not Čayān-nōr, but rather the place which in Mongol times was known as 昌州 Ch'ang-chou, 寶昌州 Pao-ch'ang-chou, and also as 中都 Chung-tu (of course different from the old Chung-tu = Peking; see «Cambaluc»). The lake called Čayān-nōr by Polo must be the «Pain Čayān-nōr», about 80 li north-east of the «Anguli-nōr», and the «palace» with clay-walls lay probably just south-east of the «Pain Čayān-nōr».

YANAI (pp. 684, 761, 763) has also tried to identify with the palace of Čayān-nōr another *ordo* or *hsing-kung* mentioned in YS and certain other Chinese sources. In YS, 30, 4 b, it is said that, in 1326, «the 清寧殿 Ch'ing-ning-tien ('Ch'ing-ning Hall') of Shang-tu was removed to the 伯亦兒行宮 Pai-i-êrh *hsing-kung*». In 1327, «the 欽明殿 Ch'in-ming-tien ('Ch'in-ming Hall') was completed at the Pai-i *wo-êrh-to (ordo)*» (YS, 30, 7 a), and «camels and oxen were given to Pai-i *wo-êrh-to (ordo)*» (YS, 30, 8 a). The name of the Pai-i *wo-êrh-to* also occurs in the *Ching-shih ta-tien* and in the 禁扁 *Chin pien*. YANAI explains the «Pain» of «Pain Čayān-nōr» as being the Mong. *bayan* «rich», and thinks that the same word is meant by Pai-i-êrh or Pai-i. The equation of «Pain» with *bayan* is by no means certain. On the other hand, Pai-i-êrh cannot render *bayan*, nor be identical with «Pain»; but if we take into account that Pai-i-êrh occurs only once, it may be that *êrh* unduly crept into the text (perhaps through a contamination due to *wo-êrh-to*), and that the true reading is Pai-i as in all other passages. But even then the phonetic resemblance between the name of the Pai-i *ordo* of the Mongol period and the modern name «Pain Čayān-nōr» is not enough to

establish an identification. We shall soon see, however, that there may be some collateral support for YANAI's view.

A passage of the *Ching-shih ta-tien* preserved in the *Yung-lo ta-tien* (ch. 19422, 4 b) gives as the number of animals and cars assigned to the postal relay of Čayān-nōr : «The original numbers were 150 horses, 50 cars and 200 oxen, to which were later added 58 horses, 30 cars and 120 oxen.»

Qubilai was at Čayān-nōr when the startling news of Aḥmad's assassination reached him (April 10, 1282; see «Acmat<sup>1</sup>», and cf. *YS*, 12, 1 b; 160, 7 a; 205, 3 b).

If YANAI be right in his identification of the Pai-i *ordo* with Čayān-nōr, the palace of Čayān-nōr was later the scene of an event of some importance in the intercourse between the Mongols and the West. On August 19, 1342, the Papal Legate Marignolli presented a superb horse to the Emperor Shun-ti, who was then sitting enthroned in the 慈仁殿 Tz'u-jên-tien («Hall of Mercy and Benevolence»). On August 22, an order was issued from the 龍光殿 Lung-kuang-tien («Hall of Dragon Brightness») that Chou Lang should draw a picture of the horse (cf. *Mo*, 256-257). From *YS*, 40, 6 a, we know that Shun-ti was then residing, in principle at least, at Shang-tu; but the *YS*, as a rule, takes no notice of the moves of the Emperor from Shang-tu to Čayān-nōr or *vice-versa*; he is regarded as staying at Shang-tu so long as he does not come back to Peking. For instance, if we had only the *pén-chi* of the *YS*, we should not know that Qubilai was at Čayān-nōr, and not at Shang-tu, at the time of Aḥmad's murder. Now, it is said in the *Chin pien* (cf. YANAI, 761) that both the Tz'ü-jên-tien and the Lung-kuang-tien were halls of the Pai-i *ordo*, and there is no reason to doubt the accuracy of such a statement. The Pai-i *ordo* cannot have been at Shang-tu itself, since we have been told that «a hall had been removed from Shang-tu to Pai-i *ordo*». Since Čayān-nōr was a favourite resort of the Mongol Emperors when the Court was at Shang-tu, and is in fact the only one of which we have a distinct mention in connection with Qubilai both in Polo and in Chinese sources, there is a fair chance that it received a new name when it was enlarged into a real *ordo* in 1326, and that Marignolli actually had his audience at Čayān-nōr. In such a case, YANAI may well be right in connecting the «Pai-i» of Pai-i *ordo* with the «Pain» of Pain Čayān-nōr.

#### 144. CIAGATAI

<i>agathay, ciagatay, cyachactay</i>	<i>ciagaci, cilgatay, sigatai, sigatay</i> FA	<i>çagathay Z</i>
LT		<i>digatai, draganti, gigatai</i> VB
<i>agati, cigataio, gegitan, rigatai</i> V	<i>ciagatai, ciagati</i> F	<i>gighatta</i> TA <sup>1</sup> , TA <sup>2</sup>
	<i>ciagaty</i> FB	<i>gisghatta</i> TA <sup>1</sup>
<i>chagatai, chiagati, cigati, zigatai</i> VA	<i>cigatai</i> F, L, V	<i>gyanchai</i> G
	<i>cygastay</i> P	<i>sygatay</i> FA, FB
	<i>cygathai, cygathay</i> VL	<i>zagathai</i> R

This is a very accurate rendering of the name of Chinghiz-khan's second son «Čayatai». It was first mentioned in the West by Plan Carpine, who gives «Chiaaday» (*Wy*, 65, 66; Plan

Carpine often uses *chi-* with the value of č. Hethum has « Chagaday » (with *ch-* = č; *Hist. des Crois.*, Arm., II, 157, 163, 296). The name occurs several times in Fra Mauro's map — in the present case derived from sources other than Polo — always in the form « Čagatai » (not « Zagatai » as in *Zu*, 33, nor « Cagatai » as in HALLBERG, 92, 346). We find « Chacatay », and sometimes « Checatay » in Clavijo (*ch-* = č; SREZNEVSKIĪ, 424; not « Zagatay » as in HALLBERG, 347; nor « Chagatay » as in LE STRANGE, *Clavijo*, 366); « Zekathey » (var. « Zekatay »; with German *z-* = *ts-*) in Schiltberger (LANGMANTEL, 61, 127); « Zacatai » in the *Libellus de notitia orbis* (cf. A. KERN, in *Arch. Fratr. Praed.* VIII [1938], 96, 100). Juwaini, Rašidu-'d-Din, Abū-'l-Ghāzī all spell the name چغتای Čăyātāi (with the usual double value of چ as *j-* and č; the form چغتای with č in *Bl*, II, *passim*, is BLOCHET's editing); Waśśāf (*Ha*<sup>2</sup>, Pers. text, 20) gives however چغتای Čăyātāi, which is more satisfactory from an etymological point of view; the form Čăyātāi may be the result of the Mongolian slurring of a second unaccented vowel. The Armenian form « Čayata » is somewhat ambiguous (cf. PATKANOV, *Istoriya Mongolov*, II, 13; BROSSET, *Deux historiens arméniens*, 115). Bar Hebraeus transcribes in Syriac « Šăgātāi » (BRUNS, *Chronicon Syriacum*, 439).

« Čayatai » is a purely Mongolian name. Ch'ien-lung's Commissioners (*Yüan shih yü chieh*, I, 16 b) changed it to « Čayantai », which they explained as čayān, « white », with the adjectival suffix *-tai*, « having » (cf. also *Bl*, II, 153; the « \*Čayantai buqa » which BLOCHET adduces from the same work, 17, 9 a, is valueless, being an absurd restoration of a name the first part of which has nothing to do with Čayatai or \*Čayantai). Although the correction to \*Čayantai is arbitrary and useless, I think that the derivation is correct : Čayātai, also read Čayā dai, must be čayān + *tai*, in the same way as Ulātai (see « Oulatai »), Ulā dai, Hulātai or Hulā dai is formed with *ula'an* > *ulān* or *hula'an* > *hulān*, « red », and *-tai*. This type of derivation is no longer alive in Mongolian, and our Mongol dictionaries give « Čayadai », as KOWALEWSKI reads it, only as the name of Chinghiz-khan's second son.

« Čayātai » and « Čayā dai » are indistinguishable in Mongolian script, since the same letter serves for *t* as for *d*. On the other hand, Mongolian *-y-* can represent both a real *-y-* or merely an intervocalic hiatus of the type \*Ča'ā dai or \*Ča'ā tai. Curiously enough, the latter value, which is suggested by Plan Carpine's « Chiaaday », was also the one adopted by the transcribers of the *Secret History*, who always read « Ča'adai » (many mentions of the name occur in §§ 242-280). In the same way, they read « Ča'alun » the feminine name formed with čayān and the ancient feminine suffix *-lun* (§ 157). A still more contracted form is represented by 察帶 Ch'a-tai, \*Čā dai, in *YS*, 63, 15 b, if Čayatai is meant as I believe him to be and if a character has not been dropped between *ch'a* and *tai*. At any rate, 察阿台 Ch'a-a-t'ai (= Ča'ā tai) occurs twice under A. D. 1228-1229 in *YS*, 31, 1 b, 2 b.

All other Chinese transcriptions are based on Čayatai and Čayadai. We find 察合台 Ch'a-ha-t'ai, Čayatai (in *YS*, 1, 7 a [s. a. 1213]; 8 b [s. a. 1221]; 107, 5 a; and in *Shêng-wu ch'in-chêng lu* [WANG Kuo-wei ed. 50 b]); 察哈台 Ch'a-ha-t'ai, Čayatai (in *YS*, 120, 2 a; 124, 6 b); 茶合帶 Ch'a-ha-tai, Čayadai (in *YS*, 2, 3 a [s. a. 1236]); 察合帶 Ch'a-ha-tai, Čayadai (in *YS*, 68, 3 a; 69, 5 a; 72, 1 b; 74, 1 b); 茶合解 Ch'a-ha-tai, Čayadai (in *YS*, 95, 2 b, and in *Hei-Ta shih-liao* [WANG Kuo-wei ed. 18 b]); and 察哈歹 Ch'a-ha-tai,



Čayadai (in *YS*, 119, 8 a). In the *Secret History*, Čayatai is often respectfully addressed as « Ča'adai aqa », « Elder brother Čayatai »; the *YS* too, 124, 6 b, speaks of the grandsons of « Čayatai aqa ».

In Osmanli Turkish, partly perhaps because of the double value *y* and *č* of  $\zeta$  of Persian sources, « Čayatai » has become « Ĵayatai », both as the name of Chinghiz-khan's son and as that of the language later spoken in the dominions of his branch. « Ĵayatai » is the only form given in RADLOV's great Turkish dictionary (iv, 15). It is « Čayatai », however, which is given with both meanings in Persian dictionaries (VULLERS, I, 580). I can vouch for the fact that « Čayatai » was the pronunciation used among the Turks of Chinese Turkestan. Although the word has practically died out there either as a personal name or as that of a tribe or of a language (cf. SHAW, *Vocabulary*, 94), I have heard at Kučā *čayatai ādam*, « čayatai man », used with the meaning of « a violent man », « a man with a bad temper » : a clear echo of the time when the Turks of Chinese Turkestan fought many a battle against their cousins of Russian Turkestan who had come to be known more particularly as Čayatai. The name, moreover, did not become obsolete so soon in Mongolia : a modern 察哈泰 Ch'a-ha-t'ai, Čayatai (or Čaqatai), has a biographical notice in ch. 270 of the *Kuo-ch'ao chi-hsien lei-chêng ch'u-pien*.

We are told by Rašidu-'d-Din that the name « Čayatai » was tabooed after Čayatai's death in 1242 : a Sünit who was called Čayatai and known as Čayatai Küčük, « Smaller Čayatai », had then to abandon his true name to be henceforth called by a mere tribal epithet, Sünitäi (*Oh*, II, 108; *Ber*, I, 47). It may be so, and the fact is that we hardly know of any « Čayatai » other than Chinghiz-khan's son. A second Čayatai named alongside of the prince Čayatai in *YS*, 2, 3 a (s. a. 1236), is probably due to a clerical corruption (cf. T'u Chi, 4, 11 b). The « prince Čayatai » mentioned in *YS* under 1228-1229 may be, as will be shown further on, an example of the survival and extension of the name of the true Čayatai, and not the personal name of a real homonym. Only one case remains to be considered : the *capitaneus*, Batu's *consanguineus*, to whom Rubrouck carried a letter in 1253 and whose name, to judge from the various readings of the mss., he seems to have written \*Scacatay. The most natural restoration of such a name would be Čayatai, and this would show the name Čayatai in actual use thirteen years after prince Čayatai's death. But no safe conclusion can be based on such an isolated and doubtful instance (cf. *TP*, 1930, 203, 207).

Čayatai was the second of the four sons whom Börtä bore to Chinghiz-khan; he was younger than Jöči, but older than Ögödäi and Tolui. Qubilai, son of Tolui, was Čayatai's nephew, so that Polo is mistaken when he speaks of Čayatai as being Qubilai's own brother (Vol. I, 449). He is no less wrong when he represents Qaidu (see « Caidu »), a grandson of Ögödäi, as grandson of Čayatai (Vol. I, 447). We may of course suppose that Rustichello sometimes misunderstood Polo. For instance, Čayatai, whose name already occurred in the narrative, may have unduly taken Ögödäi's place in reference to Qaidu. On the other hand, Čayatai actually was the Great Khan's own brother (i. e. Ögödäi's), and the assertion becomes wrong only when it is added that this Great Khan was Qubilai. Polo himself may have known better, though this is by no means certain, for a confusion of the two persons runs throughout his whole text. It would in a way

become easier to understand if the custom already obtained in Polo's time to use Čayatai's name as the designation of his branch.

Moreover, we must not be too much surprised at Polo's errors when we see those made by other mediaeval travellers and historians. Plan Carpine (*Wy*, 65) knew that Chinghiz-khan had four sons, but he gives them in the following order : Ögödäi, Jöči, Čayatai and a last one (= Tolui) whose name he was not told. Kirakos gives to Chinghiz-khan three sons in the following order : Čayatai, a second son unnamed (Jöči or Tolui) and Ögödäi (BROSSET, *Deux historiens arméniens*, I, 115; PATKANOV, *Istoriya Mongolov*, II, 13). In Hethum, Čayatai becomes the third son of Ögödäi (*Hist. des Crois.*, Arm., II, 157, 163, 296). Even the Mongol chronicler « Sanang Setsen » is mistaken and gives Čayatai as Chinghiz-khan's eldest son (SCHMIDT, *Gesch. der Ost-Mongolen*, 111). European scholars are not always more accurate : Čayatai becomes Chinghiz-khan's third son, instead of second, in RADLOV's dictionary (IV, 15), in SREZNEVSKII (*Clavijo*, 425) and in LE STRANGE (*Clavijo*, 350). In 1819, 'JIGS-MED NAM-MKHA (HUTH, *Gesch. des Buddhismus*, II, 29) gives the four names in the correct order, but attributes to Čayatai five fanciful sons who became kings of western Asia and India, one of them being « Kodkhar » (read « Koŋkhar »), king of « Rom », and residing in « Stambhola », which in a note to the text is supposed to be part of the mythical land of Śambhala !

On Čayatai, cf. Juwainī, I, 226-232; Rašidu-'d-Dīn in *Bl*, II, 153-197 (Rašid left this section incomplete); T'u Chi, 148, 32, 1a-2b; 148, 39a; *Oh*, II, 99-108; *EI*, s. v. « Čaghatai-khān », by BARTHOLD.

We do not know in which year Čayatai was born, but he was older than Ögödäi, who was born in 1186. It is not likely that there should have been a great difference of age between them, and we may assume that Čayatai was born c. A. D. 1185. He took part in the campaigns of his father against the Jučen and against the Mussulmans. After Chinghiz-khan's death (1227), Čayatai remained most of the time in his own appanage which extended from Bokhara and Samarkand to the region of Beš-baliq (north-east of Urumchi). In the summer, he used to reside at قُبَّاسْ Quyās (? \*Quyāš) near Almaliq, and in the winter at a place the name of which has been read مَرَوَزِيكْ اِيلا \*Mārāuzik-Īlā, but which may as well be \*Māzūrāng-Īlā, \*Mārūrāng-Īlā, \*Mārūzik-Īlā, etc. None of the names has as yet been identified, but both places were certainly in the Ili region (cf. Juwainī, I, 31, 227; Kāšyari, in *Brockelmann*, 247 [« Qayas »] and 248 [« Quyas »]; *Oh*, II, 107; BARTHOLD, *12 Vorlesungen*, 76, 181; *Mi*, 298, 301); T'u Chi's identification (32, 1a) of the winter residence with « Maryinan » (the « Margelan » of our maps; cf. *Br*, II, 54) is not acceptable. Rubrouck mentions a city of « Equius » which has been equated with Kāšyari's « Iki-ögüz » by BARTHOLD (*12 Vorlesungen*, 95; cf. *TP*, 1936, 363); although this is a possible solution, I am not convinced that an identification of « Equius » with « Quyas » is not just as likely. Apart from his dominions in the West, we know that Čayatai had been given as appanage 47,330 families of T'ai-yüan-fu in Shàn-hsi in 1236 at the time of the repartition of lands in North China among Mongol princes and grandees (*YS*, 2, 3a; 95, 2b; *Oh*, II, 70); in 1238, he received an additional grant of 10,000 families of Chên-ting (see « Achbaluch ») and 深州 Shên-chou (now Shên-hsien; cf. *YS*, 95, 2b).

In the *Secret History*, written when Čayatai was still alive, he is represented by Chinghiz-

khan himself as an obstinate and high-tempered man. He violently opposed his elder brother Jöči, but agreed to the appointment of his younger brother Ögödäi and his lineage to succeed Chinghiz-khan. He remained to the last true to the laws which his father had laid down, and, when Ögödäi died at the end of 1241, Čayatai seems to have been instrumental in leaving the supreme power in Ögödäi's branch. Moreover, he did not long survive Ögödäi : like him, he had taken to drink, and died in 1242, when he must have been still under sixty. Despite Polo, he was never a Christian.

When in 1266 Qubilai installed the eight « chambers » (室 *shih*) of the Ancestral Temple, the fifth « chamber » was that of Čayatai and of his principal wife 也速倫 Yeh-su-lun (Yäsülün; *YS*, 74, 1*b*; she is the يسولون Yesülün of Rašidu-'d-Dīn, in *Bl*, II, 154). The *YS* (69, 5*a*) has preserved the text of the Chinese song which was sung at the Ancestral Temple in honour of Čayatai.

The best genealogical table of Čayatai's branch down to 1300 is that given in Rašidu-'d-Dīn (*Bl*, II, 153-177). The *YS* (107, 5*a-b*, 7*b*) goes quite astray owing to a confusion which makes it transfer part of Čayatai's branch to the line of Tolui (see « Cibai and Caban »). An unpublished Sino-Uighur inscription of 1326 gives at least a correct genealogy of Čayatai's sixth son Baidar : Baidar's son was Aluyu (not « Naliyu » who is another man, despite *Bl*, I, 120-121; II, 176), father of Čübäi (Polo's « Cibai »), father of Nom-qulī, father of Nom-taš (or Nom-daš?). The current tables given for Čayatai's branch in the first half of the 14th cent. are often erroneous, and could be corrected to a certain extent with the help of Chinese sources. But such a discussion would be out of the scope of the present notes.

Western sources of the early 14th cent. speak of the dominions of Čayatai's branch as « Medium Imperium », sometimes, by a curious mistake, as « Medorum Imperium » (cf. HALLBERG, 343-345); we also find, for the allied houses of Ögödäi and Čayatai, the term « Empire of Qaidu and Dua » used as a sort of territorial designation even after the death of both Dua and Qaidu (see « Caidu »). Although BARTHOLD (in *EI*, s. v. « Čaghatāi-khān ») considers Dua to be the « true founder » of the Čayatai Empire, it was not his name which was ultimately retained, but that of Čayatai himself, which has survived to designate both the country and the Turkish dialect spoken by its inhabitants. This extended use of the name of Čayatai was already well established at the end of the 14th cent., as is shown by the accounts of Clavijo and Schiltberger, and by the *Libellus de notitia orbis* written in 1402 by John III, archbishop of Sultaniyah (cf. KERN, in *Arch. Frat. Praed.* VIII, 96, 100); it occurs also in the 15th cent. in Josafa Barbaro (RAMUSIO, II, 105-106). But the question may be raised whether it did not begin much earlier. When the *YS* speaks of « the princes of the north-west, Čayatai and others » in 1328-1329 (cf. above), we have no indication that a prince whose name was Čayatai should have then existed. I am greatly tempted to interpret this « Čayatai » as meaning the princes « of the branch of Čayatai », i. e. with the value which the name certainly had half a century later. Ultimately, the use of the name « Čayatai » was restricted to the western half of the Moyol, the name « Moyol » being retained by the eastern half. Both branches had dwindled to insignificance when Mirzā Muḥammad Haidar completed his *Ta'riḥ-i Rašidī* (cf. the translation by ELIAS and ROSS, 148).

## 145. CIAMBA

<i>çamba</i> Z	<i>cianban</i> F, L, VA	<i>ziamba, ziambi</i> R
<i>chaiabia</i> TA <sup>3</sup>	<i>cinaba</i> Fr	<i>zianban</i> V
<i>chimban</i> TA <sup>1</sup>	<i>çinba</i> F	<i>zinba</i> VA
<i>ciaban</i> VL, S	<i>cyam</i> Pr	[ <i>accambale</i> R
<i>ciamba</i> FA <sup>t</sup> , TA <sup>1</sup> , V (cor.)	<i>cyamba</i> FA, FB, P	<i>azambale, azamballe, aziam-</i>
<i>ciambam</i> TA <sup>1</sup> , LT	<i>cyambu</i> G	<i>balle</i> VB]
<i>ciamba</i> F, Ft, TA <sup>3</sup> , LT	<i>zaban, zanba</i> VB	

I retain this spelling, although the initial ç- of Z, z- of VB and R, would suggest «Giamba» or the like. In any case, Polo's transcription must be interpreted phonetically as \*Ĵamba rather than \*Ĉamba. The country meant is the middle and lower Annam of the present day, the port of call, in Polo's time, being the modern Quinhon (cf. BEFEO, iv, 205). The Skr. form of the name is Ćampā and the native name of the people is Ćam, the usual transcriptions being, conventionally, Champa and Cham. The Chinese transcriptions of the Mongol period, 占八 Chan-pa and 巠八 Chan-pa (cf. BEFEO, iv, 243, and suppress the doubt in note 9), presuppose a pronunciation \*Ĵamba, identical with that heard by Polo. We may, for the initial consonant at least, say the same of Odoric's «Zampa» (var. «Çampa»); this double spelling reappearing in Fra Mauro, but «Zampa» is there on the island of «Taprobane») and for «Janpa» of the Catalan Map. The pronunciation Ćampa (not Ćampā) is represented by the early Arabic transcription صنف Šanf (cf. Fe, 695; Mi, 240), probably by Jourdain Cathala's «Champa» (and «Chapa»; read Ćhāpa), which seems to apply to the Indo-Chinese Champa, and by Conti's «Ciampa» (cf. HALLBERG, 173-175).

Our knowledge of Champa has greatly increased in the last thirty years, and much of YULE's commentary in *Y*, II, 268-272, calls for important changes. I cannot enter into details here, and must refer readers to Georges MASPERO's *Le royaume de Champa* (repr. from *TP*), 1914, 8vo. (new edition in 1928).

One detail must, however, be pointed out: the king «Accambale», whose name has been used in connection with Polo's visit to Champa and who is still mentioned in YULE's edition (*Y*, II, 267, 270-271) and in G. MASPERO's work, p. 233, is a misreading due to VB and copied by RAMUSIO. Such a person never existed in fact, and the name has been duly suppressed in *RR* and *B*<sup>1</sup>. The date of Polo's visit to Champa varies in the different texts. Z gives none, and judging from it alone, we might think that Polo speaks of his visit to Champa when his ship touched there on the homeward journey. *RR* and *B*<sup>1</sup> adopt «1285» on the authority of F; but FA gives «1280», LT «1288». YULE has decided in favour of 1288 because it is the nearest to Polo's final departure from China, and we know from the Prologue that he was then just back from a mission to the Indies. In principle, I agree with YULE, although even «1288» seems rather early, and one might suggest «1290».

## 146. CIANDU

*chiantai* VA*ciadu* FA*ciandu* F, FA, FB, L, LTr, P*cyandai* G*cyandu* LT, P*giandu* TA<sup>1</sup>, TA<sup>3</sup>*iadun* V*siandu* FA<sup>1</sup>*xandu* Z; R*ziandu* VB, VL

Transcribes 上都 Shang-tu. I do not find the name in Rašidu-'d-Din, who uses only Keminfu. Odoric writes «Sandu», var. «Zandu» (*Y*<sup>1</sup>, II, 227; *W**y*, 475). The «Ciandu» of F is not a good transcription, as *ci-* in Polo's transcriptions generally represents *č* and not *š*; but the case of «Cielstan» (*q. v.*) is more or less similar.

The name of K'ai-p'ing-fu was changed to Shang-tu on June 16, 1263 (see «Chemeinfu»). A mention of Shang-tu occurs earlier in *YS*, 5, 1 *b*, under the date March 5, 1262, but the whole sentence is full of anachronisms in the names (some of them, though not this one, have been noticed in the *k'ao-chêng* of ch. 5, and by WANG Hui-tsu<sup>2</sup>, 1, 5 *a*), and it is clear that the nomenclature has been modernized. In the same way, the *lu* of Shang-tu mentioned incidentally in 1262 in *YS*, 58, 3 *b*, is wrong; the *pên-chi*, in the corresponding passage (*YS*, 5, 5 *a*, 12th moon), has correctly K'ai-p'ing-fu (cf. WANG Hui-tsu<sup>2</sup>, 8, 2 *a*).

Shang-tu, «Upper Court», «Upper Capital», has been said by PAUTHIER (*Pa*, 21, 223-224, 265) to mean «Main Imperial Residence», in opposition to 中都 Chung-tu (see «Cambaluc» and «Taidu») which he renders «Secondary Imperial Residence», and CHARIGNON (*Ch*, I, 261) likewise translates Shang-tu «Main Capital». It would take too long to examine in detail the use of the names Shang-tu and its synonymous 上京 Shang-ching, of Chung-tu and Chung-ching, and even of 下都 Hsia-tu, «Lower Capital», from the time of the Chou and principally from that of the T'ang dynasty. Let it suffice to remark that the Liao and the Chin each had five capitals, but the Shang-ching, the northernmost capital, was not the one where the dynasty actually ruled (cf. CHAVANNES, in *JA*, 1897, I, 377-378); the Chin ruled in Peking, to which they had given in 1153 the name of Chung-tu, «Middle Capital». The case of Shang-tu is analogous. It never was the real capital. But owing to the fact that it was Qubilai's summer residence, and although Peking had been since 1260 and was to remain the real capital, K'ai-p'ing-fu was given in 1263 the honorary name of Shang-tu, «Upper Capital», perhaps partly because it lay to the north of Peking, as is admitted by POZDNEEV (II, 300).

The ruins of K'ai-p'ing-fu, alias Shang-tu, still exist north of the Luan river, in the region generally called by Europeans Dolôn-nôr (the «Seven Lakes»); but the *locality* called to-day Dolôn-nôr is actually south of the river. According to the *Ta-Ch'ing i-t'ung chich* (409, II, 1 *b*; *Bl*, II, App. 42; YANAI, 631-632), the real name, two centuries ago, was 巴哈呼爾虎 Pa-ha-hu-êrh-hu, Baya-hurhu, and the popular local name was Ja'u-naiman-sümä, «Hundred and eight temples». This last name is well known in the West thanks to D'ANVILLE's maps. As to Baya-hurhu, it may mean «Small enclosure», as is said in YANAI, 632, but the vocalization in *u* instead of *o* is surprising; I have not found the name elsewhere.

To the bibliography on Shang-tu given in *Y*, I, 306, must be added mainly : A. POZDNEEV, *Mongoliya i Mongoly* (1898), II, 300-310; VAN LERBERGHE's booklet *Au pays des Centaures* narrating his trip of 1910, which I saw some twenty years ago but to which I cannot now refer, and which is quoted in *Ch*, I, 262; and IMPEY in *Geogr. Review*, 1924, [xv,] 584-604.

## 147. CIANGA

<i>caiugan</i> F	<i>çangan</i> Z	( <i>quisai</i> TA <sup>3</sup>
<i>ciangan</i> F, L	<i>siangu</i> FA	<i>alia ciuitas</i> LT)
<i>cianghin, cinghan</i> TA <sup>1</sup>	<i>syangan</i> FB	

Probably the *chên* of 長安 Ch'ang-an, north of Hang-chou. See under «Vugiu». The *chên* of Ch'ang-an, at 25 *li* north-west of the *hsien* of Hai-ning, was important for both land and river transport. The Sung had built up a bund there. In 1341-1368, a new bund was built west of the old one and is now known as 長安壩 Ch'ang an-pa, «Ch'ang-an weir»; it remained an active trade centre throughout the Manchu dynasty. Bayan halted at the *chên* of Ch'ang-an early in 1276, waiting for the impending submission of the Sung Emperor (cf. *YS*, 9, 1 b; *Ti-ming ta tz'ü-tien*, 551).

## 148. CIANGIU

<i>chinchingui</i> P, P <sup>5</sup>	<i>tanchin</i> Ft	<i>tingigui</i> VA
<i>chinginguy</i> FB	<i>tingçu</i> Z	<i>tingingui</i> L (L <sup>1</sup> , L <sup>4</sup> omit)
<i>cinghigium</i> TA <sup>3</sup>	<i>tinghingiu</i> F	<i>tingiungiu</i> VL
<i>cinghingiu</i> TA <sup>1</sup>	<i>tinghingui</i> F, LT	<i>tinguigui</i> R
<i>cyngui</i> G	<i>tingiggui</i> Fr	<i>tinguiguy</i> FAt
<i>çingigi</i> VBm	<i>tingigi</i> VB	<i>tygui</i> P <sup>20</sup>
<i>guinguagui</i> V		

All authorities are agreed that the place referred to is 常州 Ch'ang-chou, which was already called by that name in Polo's time (cf. *TP*, 1915, 407, 412). The city was really besieged by Bayan, and stormed and taken on December 6, 1275 (see «Alains»). But the massacre of the Alans, as I have shown many years ago (*TP*, 1914, 641-642; *Mo*, 140-141, 262, and see «Alains»), occurred not at Ch'ang-chou, but north of the Yang-tzū, at 鎮巢 Chên-ch'ao, to-day Ch'ao-hsien (between Lü-chou and Wu-hu). None of the transcriptions given in the mss. however represents in a satisfactory way a rendering of Ch'ang-chou, or even of the intruding Chên-ch'ao (adopted in *RR*, 415). CHARIGNON (*Ch*, III, 69) has sponsored, after MARTINI, an explanation

of Polo's form by 晉陵 Chin-ling, a name which was borne by Ch'ang-chou in the 5th and 6th cents., and for a short time in the 8th, and which continued, until the end of the Mongols, as the name of one of the two *hsien* established at Ch'ang-chou (YS, 59, 2*b*; *Ta-Ch'ing i-tung chih*, 60, 1*a*). But Chin-ling has never been a *chou*, the *l* is not represented in the transcriptions, and there is no doubt that, for centuries before, the usual name had been and was Ch'ang-chou. Polo's «normal» transcription would be the «Ciangiu», already adopted for instance in *B*<sup>1</sup>, 239. I do not exclude, however, the possibility of a «Cianggiu» (or even perhaps of «Cinggiu»); the theory that Polo never gives the final *-g* sound of Chinese words does not always hold good; cf. «Cianglu», «Ciangli». It would be futile to try to explain the transcriptions «Tinghingiu», «Cinghingiu», etc., by supposing that the original is not Ch'ang-chou, but 新安 Hsin-an, a *chên* then existing south-east of Wu-hsi on the main road from Chên-chiang to Su-chou; apart from all the other difficulties, Hsin-an was not a *chou* but a *chên*, and I do not know of any case where *chên* itself is transcribed at the end of a name in Polo.

## 149. CIANGLI (&lt; \*CIANGLIN)

*cenglisu, zengli* VB*ciagli* TA<sup>1</sup>*cianghi* TA<sup>3</sup>*ciangli* F, Ft, FA, LT, Pr,Z, L, L<sup>1</sup>, R*ciangli* Z*ciangluy* FAt*cinagli* F, Fr*cyangli* P*cyangly, cynangly* FB, FB<sup>2</sup>*cynanchy* FBt*ziangli* V*zinuangli, zinuanglu* VA

The mss. have «Ciangli» and the place has been identified with Chi-nan-fu by PAUTHIER, YULE, PENZER, RICCI-ROSS, and BENEDETTO; CHARIGNON's Ts'ang-chou (*Ch*, III, 7) is out of the question (see under «Cianglu»).

According to Polo, after travelling south of «Ciangli» for five days, one reached Tundinfu, which is certainly Tung-p'ing-fu; on the other hand, there was at Ciangli a large river, used for much traffic of goods between north and south.

From Peking to Chi-ning (see «Singiu matu»), there were two roads in the Mongol period: one via Cho-chou, Ho-chien-fu, Tê-chou, and Tung-p'ing-fu, all by land; the other from Peking to Yang-ts'un, by land; from Yang-ts'un, via Ch'ang-lu and Tung-kuan, to Tê-chou, by boat; and again from Tê-chou to Tung-p'ing-fu and Chi-ning, by land. The Tê-chou to Chi-ning section is thus common to both. As Polo's itinerary mentions Cho-chou («Giogiu») and Ho-chien-fu («Cacanfu») and goes on to name afterwards Tung-p'ing-fu («Tundinfu» = «\*Tunpinfu»), it must have passed through Tê-chou.

The Commissioners of 1276, travelling from south to north, left Hsin-chou (= «Singiu matu», Chi-ning), passed through Tung-p'ing-fu, Kao-t'ang, and P'ing-yüan, and arrived at 陵州 Ling-chou, on the bank of the 渭河 Wei-ho (a mistake for 衛河 Wei-ho), where, leaving the carts, they embarked on boats for Tung-kuan and Ch'ang-lu (*TP*, 1915, 400). Quite evidently, the course of the Wei-ho which they followed from «Ling-chou» to the north is practically identical

with what has now become the portion of the Grand Canal until it meets with the lower Hu-t'o-ho. MOULE in 1915 (*TP*, 1915, 400) thought that the Ling-chou of the itinerary of 1276 was the modern Ling-hsien; this is not quite correct. The modern Ling-hsien is the ancient Tê-chou; and the modern Tê-chou is the ancient Ling-chou, afterwards Ling-hsien; the exchange of names dates only from the Ming dynasty (see for instance *Ti-ming ta tz'ü-tien*, 791, 867). So the Ling-chou of the Imperial envoys of 1276 is in principle the modern Tê-chou (now Tê-hsien), on the banks of the former Wei-ho and now of the Grand Canal (it may nevertheless be that the Wei-ho flowed then a little more to the east than it does now, and that the Ling-chou of the Mongol period was somewhere between the present Tê-hsien and the present Ling-hsien). The Ling-chou of the Mongol dynasty must be Polo's «Ciangli».

I had reached this conclusion before I found what I believe to be the true explanation of Polo's toponym. Under the Sung and Chin and at the beginning of the Mongol dynasty, Ling-chou (= modern Tê-chou, Tê-hsien) had always been called the *hsien* of 將陵 Chiang-ling (Tsiang-ling); it became Ling-chou only in 1253, reverted to a *hsien* in 1265, but was promoted again to a *chou* in 1266. The envoys of 1276 used the new administrative name, but the name of the *hsien*, in use for centuries, did not die out immediately from popular usage, and it is the one Polo heard. «Ciangli» is Chiang-ling; the «Ciangli» of the mss. must stand for \*Ciangli = \*Cianglin, the fall of the final *-n* being due perhaps to the attraction of «Cianglu».

«\*Cianglin» and «Cianglu» are examples, in Polo, of notations in *-ng*, and not simply in *-n*, of Chinese finals in *-ng*; others are «Cingsan» and «Scieng». We have many similar cases in Rašidu-'d-Dīn when the Persian historian has to deal with terms which had not taken a «spoken» form in Mongolian — and Persian — speaking circles (see «Pianfu»), or when he wants to be quite accurate (see «Quinsai»). CHARIGNON, who declares it necessary to divide the names into «Cian-gli» and «Cian-glu» (*Ch*, III, 5), nevertheless maintains the usual division a little further on (*Ch*, III, 8). And we have only to accept here Polo's forms at their face value, with *-ng* for Ch. *-ng*.

One difficulty remains. In this note, I have not taken into account the city of «Cianglu» mentioned by Polo as lying between «Cacanfu» and «Ciangli»; for a discussion of the point, see «Cianglu».

## 150. CIANGLU

<i>cagnul</i> VB	L <sup>1</sup> , VL, R, S	<i>cyanglu</i> FB, P
<i>chiuangu</i> , <i>cinuangu</i> , <i>zinuan-</i>	<i>cianglu</i> Z	<i>cyangula</i> G
<i>glu</i> VA	<i>cinaglu</i> Fr	<i>cynamguy</i> FB <sup>2</sup>
<i>ciaglu</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>cionglu</i> , <i>ciunglu</i> LT	<i>zanglo</i> V
<i>cianglu</i> F, Ft, FA, LT, Z, L,	<i>ciuglu</i> TA <sup>1</sup>	

From the days of MARSDEN and MURRAY, the place has been identified with Ts'ang-chou, on the Grand Canal. PAUTHIER, however, pointed to the name of 長蘆 Ch'ang-lu, not far from



Ts'ang-chou, and a little to the south of Ch'ing-hsien, also on the Grand Canal, but on its western side. YULE (*Y*, I, 133) approved of PAUTHIER's suggestion; CORDIER added that, according to PLAYFAIR, Ch'ang-lu had been the name of Ts'ang-chou during the T'ang and Chin dynasties; hence the equivalence of «Cianglu» with Ts'ang-chou also in *RR*, 417, and *B*<sup>1</sup>, 441. CHARIGNON, looking absurdly for «Cianglu» in Shun-tê-fu in southern Ho-pei, has seen Ch'ang-lu in «Ciangli», which is in fact the modern Tê-chou (Tê-hsien of the Republican geography).

The equivalence of «Cianglu» and Ch'ang-lu is not open to doubt; the notation of Ch. *-ng* by *-ng* and not *-n*, although exceptional in Polo, has a counterpart in «\*Ciangli» = Chiang-lin. But a greater precision is here necessary. A *hsien* of Ch'ang-lu existed from the Northern Chou to the beginning of the Sung; its seat corresponded to the modern Ts'ang-chou (now Ts'ang-hsien), on the Grand Canal; but this is probably not Polo's «Cianglu». The name in Polo applies to the 鎮 *chên* of Ch'ang-lu, more to the north, and 70 *li* south of the present Ch'ing-hsien («17 *li* is a slip in *Pa*, 438). When the Commissioners of 1276 reach Ch'ang-lu-chên, they note (*TP*, 1915, 401) that the natives call it «Hsiao-Yen-ching», «Little Peking», as it is a prosperous place, producing salt, and the seat of a Salt Commissioner. Polo says in the same way that salt is produced in immense quantities in «Cianglu» and in the surrounding district; and he then describes the process of its manufacture. Ch'ang-lu (but not Ts'ang-chou) was in the past the residence of the Director-General of Canal Transport, and he had under his jurisdiction 24 offices of the gabelle, of which 12 were on the Ts'ang-chou territory and in Shan-tung; when, later, the Directorate of Canal Transport was transferred to Tientsin, it went on using the name of Ch'ang-lu, in spite of its new location (cf. *Ta-Ch'ing i-t'ung chih*, 17, 1 a, 4 b, 5 a; *Ti-ming ta tz'ü-tien*, 561; and, above all, the paper by 陳雋如 CH'ËN Chien-ju, 長蘆都轉考 *Ch'ang-lu tu-chuan k'ao* [«Researches on the Directorate General of Ch'ang-lu»], in 禹貢 *Yü kung*, iv, 12 [February 1936], 7-9).

So I take it for granted that Polo's «Cacanfu» is Ho-chien-fu, that his «Cianglu» is Ch'ang-lu-chên, and his «Ciangli» is Chiang-ling, now Tê-hsien. But there is here a serious difficulty. Ho-chien-fu is in a direct line with and on the main road between Cho-chou and Tê-hsien, but Ch'ang-lu-chên (and even Ts'ang-chou) is almost due east of Ho-chien-fu and quite out of the way. It might perhaps be supposed that Polo, with the Lady «Cocacin» and the Persian envoys, having reached Ho-chien-fu from Cho-chou, turned to the east to reach the Wei-ho at Ch'ang-lu and travelled by boat, as the Commissioners of 1276 had done in the opposite direction, from Ch'ang-lu to Tê-hsien. But that does not seem very likely. Officials, travelling by postal relays, took the one road or the other; they would not combine both. From Tê-hsien, the Commissioners of 1276 had gone up by boat to Ch'ang-lu-chên, but continued on, by boat too, until they reached Yang-ts'un (between Tientsin and Ho-hsi-wu), to arrive finally at Peking from the south-east. I propose the following explanation. Polo went more than once along the road from the Peking region to Yang-chou or east of Yang-chou on the Yang-tzŭ, as for instance when he was appointed to some office at Yang-chou, or when he was sent to the Indies. Once at least, he started from Cho-chou, and went on direct by Ho-chien-fu, Tê-hsien, Tung-p'ing-fu; this was very likely the case the last time, when he accompanied the Lady «Cocacin» and her escort. But on another occasion he had followed the eastern road, partly by water, which had been the one taken in 1276

by the Imperial Commissioners in charge of the Sung princesses. Both roads converged at Tê-hsien (« Ciangli »), but Ch'ang-lu was on the eastern, Ho-chien-fu on the western one. Both places had struck him as worthy of mention, particularly Ch'ang-lu; and although the actual itinerary which he describes followed the western road only, *i. e.* via Ho-chien-fu, he inserts here a digression, so as to give an account of Ch'ang-lu and its salt production. It was Rustichello, or perhaps some later copyist, who included the place in the main itinerary. This is what certainly happened in the chapters concerning the traveller's journey from Champa to Sumatra.

## 151. CIANSCIAN

*ciafia* TA<sup>1</sup>  
*ciancian* FA, FB  
*ciangiari* VL  
*ciansan* F  
*cianscian* F, L

*ciansiam* LT  
*ciasan*(?) TA<sup>3</sup>  
*cinagnan*, *cinoian* VA  
*cyangiam* P; G

*çansan* Z  
*zansian*, *zansui* V  
*zengian* R  
*ziaxia* VB

I agree with CHARIGNON (*Ch*, III, 100-101) that this is 常山 Ch'ang-shan, and not 江山 Chiang-shan as had been proposed by others. Chiang-shan (< Kiang-šan) would not be satisfactory on account of *ci-* which represents *č*, not *k*; moreover, it is Ch'ang-shan, not Chiang-shan, which was on Polo's track.

The name of the *hsien* of Ch'ang-shan in Chê-chiang dates from the T'ang dynasty; the Sung changed it to 新安 Hsin-an, but the Yüan dynasty adopted again Ch'ang-shan (*YS*, 62, 4 a), which has remained in use ever since.

## 152. CIARCIAN

*chiarcham* VL  
*ciarchan* VB, S  
*ciarchiam* LT  
*ciarciam* TA<sup>1</sup>, TA<sup>3</sup>  
*ciarcian* F, Z, VA, L, R

*cyarchia* G  
*cyarciam* P  
*giarchan* VB  
*narciam* FA<sup>t</sup>  
*siarciam* FA

*siarcian* FB  
*siarrian* FB<sup>t</sup>  
*siartian* FB<sup>r</sup>  
*zarzian*, *zerzian*, *ziarzian* V

Phonetically = Čärčän. It is the modern Charchan, Cherchen (Čärčän), between Kerya and the Lop-nör.

The name appears for the first time c. A. D. 800 in the Tibetan transcriptions Čar-čhen and Čer-čen (cf. *JRAS*, 1928, 557, 565), and has replaced the native indianized form « Calmadana »,

Ch. 且末 Chü-mo (or Chieh-mo); cf. STEIN, *Serindia*, 323, 470. I cannot dismiss the idea that it may be connected with the ancient name of the Shan-shan kingdom in the Lop region, with a new application.

The next mention of Čärčän occurs in 1076 in Kāšyari's جرجان Čürčän, which has puzzled BROCKELMANN (p. 243), but which being «in the direction of China», and marked on the map after Kāšyar, Yarkänd and Ḥotan towards the east, is surely to be read جرجان Čärčän, as has already been recognized by HERRMANN (*Imago Mundi*, 1935, 28).

In Polo's time, Čärčän is mentioned in YS : in 1282, a postal relay was established at 關里輝 Shê-li-hui (read Shê-li-ch'an [纒]; YS, 12, 3 b), and again at 關鄴 Shê-ch'an in 1286 (YS, 14, 1 a); in 1287, a military colony was established at Shê-ch'an and 1,000 men were soon thereafter sent there from Kan-su (YS, 14, 7 b, 9 a). Although the Chinese transcriptions would suggest Järčän rather than Čärčän, the names of the postal relays named alongside do not leave any doubt as to its identification.

The next mentions of Čärčän are to be found in the *Ta'riḥ-i-Rašīdī* of the 16th cent., where the name is once written Čärčän, and once miswritten «Jurjān» (transl. ELIAS and ROSS, 52, 406; cf. STEIN, *Serindia*, 300). Čärčän appears also as 扯力昌 Ch'ê-li-ch'ang (\*Čärčäng; for the final -ng, see «Badascian») in a Ming itinerary (cf. *China Review*, v, 233). It is a curious coincidence that the miswritten form «Jurjān» in the *Ta'riḥ-i-Rašīdī* of 1547 should be identical, in Arabic letters, with the miswritten «Čürčän» of Kāšyari.

### 153. CIBAI and CABAN

*abati e chaban V*  
*cibai ou cibani F*

*unus cibay alter caban Z*  
*cimbay LT*

*cybai et cyban FA*  
*zibai e ziban VB*

BENEDETTO has adopted the reading of F, according to which Polo names only one of the two princes of the lineage of «Ciagatai», he or his copyist hesitating between Cibai and Ciban. No identification has been proposed, if we except the Jibi-tämür (?), of doubtful parentage, put forward in *Br*, II, 35, which is impossible for various reasons. We must on the contrary admit with Z, V, etc. that Polo names the two princes, and adopt for the second name the reading Caban of Z and V. According to Rašīdu-'d-Dīn (*Bl*, II, 176, 502, 536-539, 609), the longdrawn conflict between Qaidu and Qubilai began in the region of Qara-ḥočo (see «Carachoço»), where lived the prince Ajigi (cf. *Bl*, II, 164), son of Büri (himself a grandson of Čayatai), and the princes Čübäi (or Jübäi) and Qaban (*Bl*, II, 176), sons of Aluyu (another grandson of Čayatai; for the date of this event see «Caidu»). If Polo had only given one name, we might have hesitated between Ajigi and Qaban for the second one, but there can be no doubt that, by Cibai and Caban, the two brothers Čübäi and Qaban are meant; Qaban was the elder, but Čübäi played a more important part. The form of the name of Čübäi is established by a Sino-Uighur unpublished inscription of 1326 which gives the following genealogy : Čayatai, Baidar, Aluyu, Čübäi (or Jübäi), Nom-qulī, Nom-daš. In YS there are many mentions of Čübäi under the

forms 出伯 Ch'u-po (Čübäi), 朮白 Shu-po (Jübäi), 朮伯 Shu-po (Jübäi; all these *po* are used in the Mongol period with the pronunciation *pai* and transcribe regularly *bai* and *bäi*); most of the quotations relating to Čübäi have been collected by T'U Chi, 42, 3 a-5 a; 148, 60 a-b. Čübäi receives a seal in 1283, is promoted prince of 威武 Wei-wu and 西寧 Hsi-ning (in Kan-su) in 1304, bears already in 1307 the new title of prince of 𠵹 Pin (in Shàn-hsi) and must have died shortly after 1311. As to Qaban, his name is transcribed 合班 Ha-pan in YS (for instance 12, 7 a; 15, 8 a; 19, 1 a; 133, 4 b), for dates ranging from 1282 to 1296.

If Čübäi and Qaban have not been identified earlier, it was mainly because of misstatements in the genealogical tables of YS and misreadings in our editions of Persian historians. The YS, 107, 5b (as a little earlier the *Cho-kêng lu*, 1, 6 b), has transferred Čübäi to the house of Hülägü, and made him a son of Kharbanda. T'U Chi has seen the chronological impossibility of such a lineage, but leaving Čübäi in the house of Hülägü, has wrongly supposed that he was a son of Mongka-tämür. On the other hand, instead of Čübäi and Qaban, HAMMER has given «Dschobai» and «Kajan» (or Kijan») in *H*<sup>1</sup> I, 271, 275; II, 423, 460, 469, with a note saying that his ms. of Rašidu-'d-Dīn gives once «Dschanli» (Dschanti) in II, 420, 423) and another time «Dschobai», and that Waśśāf writes «Dschoban»; but in his translation of Waśśāf (*H*<sup>2</sup>, 146, 148), he writes «Dschoba» and «Kian», with a note to the effect that Rašid wrote «Dschauli». BLOCHET (*Bl*, II, 176, 502, 536-539, 609) has always adopted چوتی (or چوتای) Čütäi and قیان Qiyān instead of چوبای Čübäi and قبان Qaban; in the Appendix only, 32-33, he mentions that a ms. of Waśśāf gives چوبا and Qaban, but even then thinks that the first name should represent Čoban, while it is Jübä or Čübä (= Čübäi). These misreadings have done fresh mischief in Chinese texts; they have been retranscribed in Chinese by HUNG Chün (9, 8 b), and have passed from him to T'U Chi who, while placing Čübäi in the house of Hülägü, gives (148, 42 a and b) 出班 Ch'u-pan (Čuban) and 基類 Chi-yen (Kiyān) as the names of Aluyu's sons.

Some time after this note was written, I found that the true identification of «Cibai» and «Caban» had already been suggested by BARTHOLD in MINAEV's *Marco Polo*, 311, where it has remained unnoticed. In spite of this, BARTHOLD himself went on speaking later of «Čüba» and «Ḳayān» (instead of Ḳabān) in *El*, s. v. «Bukhārā».

The origin of the name Čübäi (Jübäi) is not clear; Qaban is Turkish and means «boar»; it was already listed as a proper name by Kāšyārī (BROCKELMANN, 246).

## 154. CIELSTAN

*chielstam* LT  
*chielstan* VB  
*cielscam* FA  
*cielstam* VA

*cielstan* F, FB, TA<sup>1</sup>, TA<sup>3</sup>, L  
*ciestam* VL  
*ciestan* S

*ciliesta* P  
*suolistan* Z, R  
*zilostan* V

This is شولستان Šūlistān; «Suolistan» of Z and R may represent an original «\*Sciolistan», more in agreement with the name and with Polo's usage than «Cielstan». For another possible

case of *ci* = *š*, see «Ciandu». The «Sulistan» of *RR*, 435, and of *B*<sup>1</sup>, 447, is a compromise. Polo gives «Cielstan» as one of the eight «kingdoms» of Persia. Cf. *Y*, I, 85; *LS*, 245, 262; and the notices «Shül» and «Shūlistān» by MINORSKY in *ET*. Fra Mauro, copying Polo, writes «Celstan» (HALLBERG, 148-149).

There is on the Chinese map of c. 1330 and in the corresponding list of *YS*, 63, 16*b*, a name, 設刺子 *Shê-la-tzū*, which BRETSCHNEIDER (*Br*, II, 127) believed to be Šūlistān (his opinion is adopted in MINORSKY's notice); I think it is more probably Šīrāz (see «Çiraç»). Next to it, however, there is another name, 泄刺失 *Hsieh-la-shih*, in which BRETSCHNEIDER saw Šīrāz, and which becomes available for Šūlistān. But the transcription would be most unsatisfactory. We should expect either simply a transcription of Šül, or a complete transcription of Šūlistān. I have no certain solution to propose. Sāraḥs (Serakhs) is too far away in the north, and moreover appears as Sa-la-ha-hsi, Saraḥs. Perhaps we ought to read in both texts 泄刺夫 *Hsieh-la-fu*, Sīrāf. No solution is offered in *T'u Chi*, 160, 24*a*.

## 155. CIN

*chuiği, chuuichan* V

*ci* F

*im* FA, TA<sup>3</sup>

*cin* F, L, TA<sup>1</sup>, TA<sup>3</sup>, Z; R

*cino, zino* VB

*cym* FA

*cyn* FB, P

*cui* L, VA

*çin* Z

*çiri, ziri* LT

The name occurs in Polo only in connection with the «sea of Cin», our China Sea, «Cin» being, according to the traveller, the name given to «Mangi» (*q. v.*) by the «islanders» of those parts. Polo evidently refers to the Persian form جین *Čin*. BENEDETTO's hypothesis (*B*<sup>1</sup>, 441) that «Cin» should probably be pronounced «Sin» on account of the Arabic «aš-Šin» cannot be retained. HALLBERG (pp. 125, 148), under «Cataia» and «Cin», refers the reader to an «appendix» which I believe has never been published.

Classical antiquity knew China and the Chinese under two names, Σῆρες (Lat. Seres), and \*Θῖν (?; Θῖνα, Θῖναι) or Sinai. Both have been the subject of long controversies, summed up in YULE, *Hobson-Jobson*<sup>2</sup>, 196-198, and *Cathay* (*Y*<sup>1</sup> I, 1-28). I agree with VON GUTSCHMID (*Kleine Schriften*, III, 606) that the formal distinction between «Seres» and «Sinai» is peculiar to Ptolemy and his school.

I shall not enter into a detailed examination of the name «Seres», but some statements will be of use for the further discussion of \*Θῖν. The occurrence of «Seres» in Ctesias and Onesicritus (in Strabo) is extremely doubtful (*Y*<sup>1</sup>, I, 14), but the name was familiar to authors of the 1st cent. B.C. (Virgil, Horace, etc.). Σηρικὰ, «silk textiles», are said to be mentioned at the end of the 4th cent. B.C. by Nearchus (in Strabo, xv, 1, 20), and σηρικόν is hardly to be separated from

«Sēres» (HERRMANN, «Seres» in PAULY-WISSOWA; *Das Land der Seide*, 25); but it does not seem certain that the use of *σηρικόν* is not here due to Strabo himself. In the 2nd cent. A.D., Pausanias (VII, 26, 6) speaks of silk as produced «by a small animal which the Greeks call *σίρρ*, although the Seres themselves give it another name». KLAPROTH's suggestion (in 1822) that *σίρρ* renders the Ch. 絲 *ssü* (\**si*), «silk», especially «silkthread», has generally been accepted, and HERRMANN (*Das Land der Seide*, 26) considers it is «so evident that no doubt ought to exist any longer on the point». Yet there are certain difficulties. LOKOTSCH (*Etymol. Wörterbuch*, No. 1878) derives from Ch. *ssü* both Med. Lat. *sēta* (> It. *seta*, Fr. *soie*, Germ. *Seide*), and, with the addition of the 兒 *êrh* suffix of Northern Chinese (*ssü-êrh*), «Sēres» (> Lat. *sericus*, Fr. Engl. *serge*, Engl. *silk*, Russ. *šëlk*). I do not for one moment believe that the Ch. *ssü* could have developed (through what channels?) into a Med. Lat. *sēta*, «silk», which must merely be the outcome, with a change of meaning, of Lat. *saeta* > *sēta*, «bristle», «coarse hair» (cf. Fr. «*soie de porc*», hog-bristles). On the other hand, although the use of 兒 *êrh* as a suffix is fairly early, going back at least to the 9th cent. (cf. LAUFER, *Sino-Iranica*, 538), *êrh* is an ancient \**ńzię*, the initial of which is transcribed *ž*- in mediaeval foreign scripts; *ssü-êrh* ought never to have been adduced to explain *σίρρ*. More important are the Corean form *sir* of *ssü*, and Mong. *širkäg* (< \**sirkäg* > Kalm. *širkæg* and *širgæg*), Manchu *sirge*, «raw silk», «silk thread». I do not know of any form similar to *širkäg* or *sirge* in mediaeval texts or vocabularies, but that does not prevent the words from being possibly ancient. LAUFER (*loc. cit.*, 538-539) was, in my opinion, mistaken when he denied a connection between Cor. *sir* and Ch. *ssü*. Cor. *sir* stands to Ch. *ssü* (\**si*) in the same relation in which Cor. *mar* does to Ch. 馬 *ma* (\**ma*), «horse». Although the Ch. \**ma* shows no final consonant c. A.D. 600, and must not have had any for some centuries before that date, it is extremely probable that the *-r* of Cor. *mar* is etymological, and that the word is fundamentally connected with Mong. *morin*, «horse». So both *ssü* and *ma* may originally have ended with an *-r*, which was dropped in the first centuries of our era; other words, such as 師 *shih* and 酒 *chiu*, seem to be in the same case. But even then the connection between *ssü* and *σίρρ* is far from established. Pausanias gives *σίρρ* as the Greek name of the silkworm, different from the Chinese name. As a matter of fact, the Chinese name is 蠶 *ts'an* (\**dz'am*), and there is no apparent reason why the silkworm should have been known in Greek by the Chinese name of the silk thread. Another difficulty is that *σίρρ* occurs only in the 2nd cent. A.D., two centuries at least after the appearance of the derivative forms «Sēres» and «*σηρικόν*» in Latin and Greek texts. Lastly, there is no other Greek or Latin word which can be traced directly to a Chinese original. Even if it be alleged that the case is exceptional, silk being in ancient times the Chinese product *par excellence*, the word would not have reached the Greeks directly, and we are at a loss to understand how it could have passed and been so well preserved phonetically. This explains why LAUFER rejected the derivation of *σίρρ* from Ch. *ssü*. But while he thought «*σίρρ*» and «Seres» to be of Iranian origin and connected them with Pers. *sāräh*, «breadth of white silk» (> Ar. *saraq*, «silk», «white silk») in *Sino-Iranica*, 539 (cf. also Y<sup>1</sup>, I, 20), he has given up this theory in the index of the same work (p. 612): there he says that he thought he had meanwhile found what he believed to be the «correct derivation» of the name Sēres. However, he does not seem to have ever published it.

There are other grounds for doubt. The Greek and Latin *s-* can represent not only the *s-* of an Oriental original, but also *š-* and even *ç-*. As a matter of fact, the Syriac word for «silk» is *šerāyā*, and a Syriac work of the 3rd cent. A.D. mentions the Sēres as Šērāyē, an ethnical form derived from \*Šēr (cf. *Mo*, 23). If we add that a theory has been started that the name «Sēres» originally referred to the inhabitants of Chinese Turkestan (HERRMANN, *Das Land der Seide*, 27), that the ancient name of Kāšyar was perhaps \*Šarag (see «Cascar»), that «Sarag» was the ancient name of Lo-yang among people of Central Asia and that Pers. *sārāh* (< Ar. *saraq*) and Mong. \**sirkäg*, Manchu *sirge* bears a disquieting resemblance to *σηρικόν*, it will appear that the problem of the names *σηρικόν*, Sēres and *σίρ* must remain in abeyance until it has been studied afresh in great detail.

The earliest mention of the second name, which is the one connected with Polo's «Cin», occurs in the form \*Θῖν(?) at the end of the 1st cent. A.D. in the *Periplus of the Erythraean Sea* (cf. HERRMANN, *Das Land der Seide*, 37; the views expressed there by HERRMANN are different from the ones he maintained in his article «Thinae» in PAULY-WISSOWA). This country lies at the extreme north of the (Indian) Sea, beyond Chrysē. There is in the interior a great city called Θῖνα (read \*Θῖν?) whence the various products called *σηρικόν* — wool, thread, and linen — are carried by land *via* Bactria to Barygaza (= Broach), and also across the Ganges to Limyrikē (coast of Malabar). I feel much hesitation in giving \*Θῖν as the form intended by the author of the *Periplus*; it does not appear in the index of the last editor, H. FRISK (Göteborg, 1927, p. 126), where we find only Θῖνα. All mentions of the name occur in §§ 64 and 65. The first mention, at the beginning of § 64, ... εἰς (Θῖνός) τινα τόπον, is due to a correction of K. MÜLLER; HERRMANN, *Das Land der Seide*, 37, would favour a bolder emendation εἰς Θῖν τῆν χώραν. Although MÜLLER's text has often been accepted (it is the one quoted in YULE, *Hobson-Jobson*<sup>2</sup>, 197), FRISK retains the original reading εἰς τινα τόπον, without comment; I shall do the same, since nothing can be said of the original form of the name on the basis of the correction. The other mentions are: 1. (§ 64) : πόλις μεσόγειος μεγίστη, λεφομένη Θῖνα, ἀφ' ἧς ...; 2. *Eis δε τὴν Θῖνα ταύτην* ...; 3. (§ 65) ... ἐπὶ τὴν συνορίαν τῆς Θῖνός ...; 4. ... τῆς συνορίας αὐτῶν καὶ τῶν ἀπὸ τῆς Θῖνός ... CÆDÈS (*Textes grecs et latins relatifs à l'Extrême-Orient*, 24) followed MÜLLER and corrected the first Θῖνα to Θῖναι (occurring in Ptolemy, VII, 3, 6, etc., and Martianus of Heraclia, I, 16); here again, FRISK retains the original reading. Half a century ago, VON GUTSCHMID, relying on the three other passages in which he saw the genitive and accusative of a name Θῖν, had corrected in the first case Θῖνα, ἀφ' ἧς to Θῖν, ἀφ' ἧς (*Kleine Schriften*, III, 604), which seemed at first sight reasonable, since a nominative Θῖνα cannot become Θῖνός in the genitive. But, according to FRISK, Θῖνα is an accusative employed as a nominative; in view of the genitive Θῖνός, the name is an -ν word, «the nominative of which, \*Θῖς, has been replaced by the accusative». FRISK adds that there was in Egypt a town of the same name, only mentioned too in the oblique cases (except once Θῖς in Stephen of Byzantium; also called Θοινίς, Θίνις, it was the well-known head-city of the Thinite name; cf. «Thinis» in PAULY-WISSOWA). He believes that, for the Chinese city as well as for the Egyptian one, the name (read «the form of the name»?) is due to popular etymology. Quite independently, my colleague BENVENISTE has suggested to me that the declension gen. Θῖνός, acc. Θῖνα, almost too regular for a foreign name which had

never been really alive in Greek, was due to the analogy of  $\Theta\acute{\iota}\varsigma$ , «sand-heap», «down», gen.  $\Theta\iota\upsilon\beta\omicron\varsigma$ , acc.  $\Theta\iota\upsilon\alpha$ . The name of the Egyptian town may also be responsible, to a certain extent, for the declension of the name in the *Periplus*. But, at the same time, I cannot accept FRISK's hypothesis of a nominative  $*\Theta\acute{\iota}\varsigma$  (<  $*\Theta\iota\upsilon\text{-s}$ ) for the name of the Chinese town. Either the name was  $\Theta\iota\upsilon\alpha$  in the nominative (with a final  $\text{-}\alpha$  which was not necessarily etymological), and the declension is irregular and due to analogy; or the nominative was  $*\Theta\iota\upsilon$ , with a regular declension due perhaps to the attraction of that of  $\Theta\acute{\iota}\varsigma$ .

In the following century, Ptolemy speaks of the  $\Sigma\iota\upsilon\alpha\iota$  and of their sea-port  $\text{Καρτίγαρα}$  as being south of the  $\text{Σηρικηή}$ . A name  $\text{Σχίβη}$  in Vettius Valens (c. 161-180) may possibly refer to China, although the form cannot easily be accounted for (cf. *TP*, 1912, 733). Cosmas Indicopleustes (middle of the 6th cent.) refers three times to  $\text{Τζιβίστα}$ ; the name is more completely given as  $\text{Τζιβίσταυ}$  in an astrological text (*ibid.*).

The view has long prevailed that «Sêres» was the name used by those who had heard of China by land, and  $*\Theta\iota\upsilon$ , «Sinai»,  $\text{Τζιβίσταυ}$  by those who had heard of it by sea. Such was still the opinion of YULE (*Y<sup>1</sup>*, I, 1) and LAUFER (*TP*, 1912, 725), and it has been repeated in 1936 by HENNIG (*Terrae incognitae*, I, 169 sq.). As the Sanskrit and Malay name of China is «Cīna» («Čīna»), attempts have been made to explain the name as a Malayan term, for the etymology of which extravagant hypotheses, like RICHTHOFEN's 日南 «Jih-nan» or TERRIEN DE LACOUPERIE's 滇 Tien, were proffered; I need not repeat the refutation I published in *BEFEO*, IV, 143-149. Evidently in connection with the «Malayan» hypothesis, YULE thought it «remarkable» that «the name of *China* is used in the Japanese maps» (*Y<sup>1</sup>*, I, 2). But the Malay name «Čīna» is merely, like so many others, borrowed from the Sanskrit. As to the «Shina» (not «China») of Japanese maps, it is not a survival of a «Malayan» form which had reached Japan independently, but represents the modern Japanese pronunciation of the Chinese Buddhist transcription Chih-na of the Skr. Cīna.

The whole case rests on a curious misunderstanding. It is true that the author of the *Periplus*, and Cosmas after him, had heard of China by sea, but only inasmuch as they had reached India by sea. They did not go farther east, and the names they use are those they had heard in India, and probably from Iranians in the north-western part of the coasts of India.

The originals of the names are not doubtful:  $*\Theta\iota\upsilon$  and «Sinai» render forms connected with Skr. Cīna;  $\text{Τζιβίστα}$  (more correctly  $\text{Τζιβίσταυ}$ ) has long been equated with Skr. Cīnasthāna. YULE (*Y<sup>1</sup>*, I, 11) was of opinion that the names must have reached the Greek world «through people of Arabian speech», as the Arabs, lacking the sound č, made Cīna (Čīna) «into *Sin*, and perhaps sometimes into *Thin*». This is certainly an error. The various transcriptions with  $\theta$ -,  $s$ -,  $\tau\zeta$ - are not due to an Arabic intermediary, but represent so many attempts to render the č which did not exist in Greek: it was in the same way that the king Candragupta had become in Greek Sandrakottos.

But, if the connection of  $*\Theta\iota\upsilon$  and  $\text{Τζιβίσταυ}$  with Cīna and Cīnasthāna is not open to doubt, I am not certain that they are the direct representatives of Sanskrit forms. If  $\theta$ - renders č,  $*\Theta\iota\upsilon$  is really  $*\check{C}\acute{\iota}\nu$ , not Cīna (Čīna). As will be seen further on, there is a language in which the name of China occurs at an early date without the final vowel; this is Middle Persian.



Even if we start from  $\Theta\acute{\iota}\nu\alpha$ , the final  $-\alpha$  may be due to the Greek author, although this can hardly be proved. Still more characteristic is  $\text{T}\zeta\omega\lambda\sigma\tau\alpha\nu = \check{\text{C}}\text{inistan}$ . Apart from the fact that Iran.  $-\text{st}\acute{\text{a}}n$  is much more common in names of countries than Skr.  $-\text{sth}\acute{\text{a}}na$ , there is no Indian dialect which has had an  $-i-$  as the second vowel of  $\text{C}\acute{\text{i}}n\text{asth}\acute{\text{a}}na$ . But  $\text{T}\zeta\omega\lambda\sigma\tau\alpha\nu = \check{\text{C}}\text{inist}\acute{\text{a}}n$  exactly covers the  $\check{\text{C}}\text{inist}\acute{\text{a}}n$  of modern Persian. So I hold the  $*\Theta\acute{\iota}\nu$  of the *Periplus* to be probably, and the  $\text{T}\zeta\omega\lambda\sigma\tau\alpha$  ( $\text{T}\zeta\omega\lambda\sigma\tau\alpha\nu$ ) of Cosmas to be certainly Iranian, and not Indian.

What then is the etymology of «Cina», «Čin», «China»? Apart from the above-mentioned «Jih-nan» and «Tien», we see in NAVARRETE, *Tratedos*, I, 1, that ALENI, in a Chinese work, had explained «China» by «land of the silk», and that others had thought of 指南 *chih-nan*, «South-pointer», «compass», or of 請請 *ch'ing-ch'ing*, «please» (in LUCENA); LAGLÈS suggested 人 *jên*, «man» (*Lettre écrite de Lintz*, 38); Mgr. GENTILI, 錢 *ch'ien*, «money» (*Memorie d'un missionario domenicano*, I, 6). Leaving these absurdities out of consideration, the only explanation which commends itself to me is that which has all along been attributed to MARTINI, but which had in fact been proposed more than half a century earlier, viz. in 1584, by RICCI (cf. TACCHI-VENTURI, II, 38). According to it, «China» represents 秦 *Ch'in* ( $*\text{Dz}'\check{\text{i}}\check{\text{e}}n$ ), the name of the great feudal state of western China the sovereign of which, *Ch'in* Shih-huang-ti, ultimately suppressed the Chinese feudal system, unified the country into an Empire and founded the *Ch'in* dynasty (221-206 B. C.). In BEFEO, IV, 148-149, I have shown that Buddhist authors of the 3rd-5th cents. were still conscious of the identity of *Cina* and *Ch'in*, and that it was also implied by the traditional explanation given by early Chinese authors for the name *Ta-Ch'in*, «Great *Ch'in*», of the Mediterranean Orient. LAUFER has since drawn attention (*TP*, 1912, 720-721) to a text in which a Tibetan author of the 18th cent. gives at great length the same etymology of «Cina» from «*Ch'in*». I may add that it is also to be found in Hsüan-tsang. When Śīlāditya asked him about *Mahācina*, «Great China», the pilgrim replied that «Cina was the dynastic title of former kings» (前王之國號; cf. JULIEN, *Mémoires*, I, 255); this can only refer to the *Ch'in* dynasty.

But we may go further. Chinese texts dating from just before or after the beginning of our era show that the Chinese were then known among non-Chinese people of Central Asia as 秦人 *Ch'in-jên*, «men of *Ch'in*». HERRMANN (*Das Land der Seide*, 40) says that a passage of DE GROOT commenting on such a text has not received the attention it deserved. As a matter of fact, long before DE GROOT, I had translated three such passages in papers expressly devoted to the name of «China» (*TP*, 1912, 736-741; 1913, 427-428). A fourth text, the inscription of Liu P'ing-kuo (A. D. 158), where Chinese of the region of Kučā are called «men of *Ch'in*», has been added to the list by AUROUSSEAU (*BEFEO*, XIII, VII, 35-36). I wish to add, what had escaped both HERRMANN and myself, that as early as 1880 VON GUTSCHMID, opposing VON RICHTHOFEN's «Malayan» theory, had already quoted from DE GUIGNES one of the passages referring to the «men of *Ch'in*» in Central Asia and drawn from it the perfectly correct conclusion that the name «Cina» must have reached India by land, and not by sea.

That «Cina», at least when meaning «China», renders «*Ch'in*» has finally been conceded by LAUFER (*Sino-Iranica*, 568-570) and is now fully endorsed by FRANKE (*Geschichte des chines-Reiches*, III, 101-102) and by HERRMANN (*Das Land der Seide*, 38-40).

Another difficulty, however, remains to be solved. The Cina are mentioned in the *Laws of Manu* and in the *Mahābhārata*, and these have long been supposed to be older than the days of Ch'in Shih-huang-ti. I do not think that such an objection can have many supporters now. More important is the mention, in the *Arthaśāstra* or *Kauṭīliya* attributed to Kauṭīliya, of «silk and Chinese ribbons (?) made in the Cina country» (*Kauṣeyam cīnapaṭṭāśca cīnabhūmijāh*; ought we not to read *cītrapatṭāśca*, «variegated silks», «brocades»? ). Kauṭīliya lived c. 300 B. C., and this would carry the name in India to a date earlier than Ch'in Shih-huang-ti. LAUFER stuck to the end to this early date for the *Arthaśāstra* (*TP*, 1912, 719; *Sino-Iranica*, 569), and so does HERRMANN (*loc. cit.* 38). In such a case, the name would have spread to Central Asia on account of the importance of the western state of Ch'in, and prior to Ch'in Shih-huang-ti's accession to the throne of China. There is nothing impossible in such an assumption, but I do not think it very likely. Moreover, it is useless, since, like S. LÉVI and FINOT, I am convinced that either the *Arthaśāstra* is of a much later date than was thought by JACOBI, or that it has been very much interpolated.

Another problem is to decide whether Cina always meant China, or whether it was originally the designation of Himalayan tribes and has been extended to China only when the name of the «men of Ch'in» reached India. The latter view was held by RICHTHOFEN, who believed that the Cina of the Indian epos were the «Šina» Dards; YULE expressed himself likewise in *Encycl. Britannica*<sup>11</sup>, VI, 188, and also LAUFER in *TP*, 1912, 723. From the last sentence of LÉVI's paper in *BEFEO*, v, 305, it can be inferred that he too, while translating the Ch'in and the Chên-tan (on which cf. *infra*) of Chinese Buddhist texts as meaning «China», because they represented the «Cina» of Sanskrit originals, doubted in 1905 the correctness of the equivalence and probably thought then of Himalayan tribes; but he never published the paper in which he intended to discuss this point. On the whole, I am not certain that such a hypothesis should be necessary. When Sanskrit texts use «Cina» in a loose manner for people to the north and of the north-west of India, we must not forget that China, at the end of the 2nd cent. B. C., had sent expeditions across Chinese Turkestan, and in the following century and again in the 1st and 2nd cents. A. D. became the dominant power there. Although there was a direct road from early days from China to the Ganges *via* Yün-nan and Burma, it was mainly by the passes of the North-West that India was brought into contact with the Chinese, either as the result of trade or diplomacy. Provisionally, I feel inclined rather to suppose that the «Cina» of Sanskrit texts represents the Chinese in principle and from the beginning.

The same holds good for Iran; unfortunately Pahlavi texts are often of doubtful reading and of uncertain date. The «Sēn» of the *Bundahišn* (xv, 29) is equated in the text with «Čīnistān» (cf. WEST, *Pahlavi Texts*, I, 59; better «Čīnistān»), but it may be, in this late text, under the influence of Ar. «Sīn» < Čīn. If it actually represents the Avestic form «Sāini» or «Sainu» of the *Yašt*, XIII, 143, 144, it is doubtful whether it should be identified with «China», either as to the location or even as to the name. «Čīnistān» (read «Čīnistān») occurs a second time, as a country lying beyond «Türkistān», in *Bundahišn*, XXIX, 13 (WEST, *ibid.*, I, 120). An adjectival form «Sēnik» in the *Šāyast-nē-šāyast*, VI, 7 (WEST, *ibid.*, I, 296; J. C. TAVADIA, *Šāyast-nē-šāyast*, Hamburg, 1930, 97-98) probably does not refer to China. HÜBSCHMANN

(*Armen. Grammatik*, I, 49), in his list of Armenian forms borrowed from Iranian, gives Arm. « Čen-kh », China, the Chinese; « Čenastan », China; « Čenbakur », the Emperor (see « Facfur ») of China; « čenik » (adj.), Chinese. According to HÜBSCHMANN, the corresponding Pahlavī forms are « Čēn », « Čēnastān » (WEST's « Čīnistān »), \*čēnik; Pers. « Čin », « Čīnistān », « čīnī ». In view of Cosmas's Τζινίστα[ν], I have little doubt that a pronunciation « Čīnistān » existed in Pahlavī, not at the early date of the Armenian borrowings, but at any rate in the 6th cent., and prior to the redaction of the *Bundahišn*. The same name occurs in a Sogdian document probably of the end of the 2nd cent.; it is written « Čynstn », which may be \*Čīnastan or \*Čēnastan, or perhaps \*Čīnstan (cf. *TP*, 1913, 428; 1931, 458). The « Čīnstān » of the Nestorian tablet of 781 renders the same form (cf. HAVRET, *Stèle chrétienne*, III, Syriac part, 1; *Mo*, 35). In the same monument, the ethnical name is « Čīnāyē » (HAVRET, 2; *Mo*, 48), based on \*Čīn < Čin (cf. also *Mo*, 75).

The Persian چين « Čin » passed to the Arabs as صين « Šīn ». In other words, the Arabs, having no č, rendered it, as usual, with ś; but this is not a sufficient reason to read ś as if the Arabs had pronounced it č, as has been arbitrarily done by FERRAND throughout his last publications. When an Arab author wished to render the Persian « Čin » and not the Arabic « Šīn », he transcribed it, for lack of a better equivalent, as شين « Šīn » (cf. *Fe*, 269); he would not have done so if the Arabic ś had sounded č. Moreover, Syriac writers have followed the same course. While their early borrowed forms render the Iranian č with ç (« Čin », etc.) as the Arabs did with ś, all the č of Turkish and Mongolian words are transcribed ś by Bar Hebraeus. Arabic form « Šīn » is used by Kāšyārī. But this does not imply, as BARTHOLD thought, that « Šīn » had been adopted by the Turks in the west (*12 Vorlesungen*, 97 : Kāšyārī knew that the term was not Turkish, and the reason why he preferred the Ar. « Šīn » to the Pers. « Čin » is simply that he was writing his *Diwān* in Arabic. A few years before Kāšyārī completed the *Diwān*, the Persian form « Čin » is actually found in Turkish in the *Qutadyu bilig* (Radlov, III, 2120).

The translators of Buddhist texts into Chinese, when finding « Cina » in the original, sometimes rendered it etymologically as « Ch'in », at other times translated it with some term meaning « China », or lastly transcribed it phonetically. The ordinary transcriptions are 脂那 Chih-na (for instance in 587; cf. *BEFEO*, IV, 574; v, 294; the 指那 Chih-na of LAUFER, in *TP*, 1912, 722, seems to be merely copied from EITEL, *Handbook*<sup>2</sup>, 176, where it is a slip or a misprint, unfortunately repeated by SOOTHILL and HODOUS, 152); 支那 Chih-na (for instance in 683; cf. *BEFEO*, IV, 575); 至那 Chih-na in Hsüan-tsang. Hsi-lin's *Hsü i-ch'ieh ching yin-i*, ch. 2 (Meiji *Tripit.* of Tōkyō, 爲, VIII, 5 a) gives a form 眞那 Chên-na, which seems to be corrupt for 眞丹 Chên-tan (on which cf. *infra*). ODA Tokunō, 750, adds 斯那 Ssü-na and 脂難 Chih-nan, for which I can find no authority.

But, apart from this mechanical transcription of « Cina », another occurs in the forms 震旦 Chên-tan (\*T'šjēn-tân; in the *Lou-i'an ching* [\**Lokasthāna sūtra*?] translated in 265-316 [according to 露, VIII, 47 a; I cannot trace the passage in the *sūtra* itself]; in a *sūtra* translated in 317-322 [成, VI, 49 a; cf. *BEFEO*, v, 304-305]; in the *Avataṃsaka*, ch. 45 [天, III, 22 b]; also in 雨, v, 107 b, and in *Hsin T'ang shu*, 221A, 11 a), 振旦 Chên-tan (\*T'šjēn-tân; cf. 爲, VI, 13 b, 73 b) and 眞丹 Chên-tan (\*T'šjēn-tân; cf. in the translation of an *Āgama*, 宿, VIII, 44 b, and in

*Sung shu*, 97, 4a). European scholars have all along been agreed that Chên-tan represents «Cinasthāna», but I must remark, (i) that the transcription is not satisfactory since we should expect at least \*Chên-t'an (with \*-t'ân; not the *chên-t'an* or *chan-t'an* with ancient \*-d'ân of *BEFEO*, III, 253); (ii) that no Chinese commentary ever speaks of «Cinasthāna», either with reference to Chên-tan or in any other case; (iii) that no case is known where Chên-tan actually renders «Cinasthāna» of a Sanskrit original. Sanskrit texts speak of «Cīna», or of «Cīnadeśa», «kingdom of China» (cf. BAGCHI, *Deux lexiques sanskrit-chinois*, 76, 295; *BSOS*, IX, 522, 523), or of «Cīnabhūmi», «land of China» (for instance in the *Arthasāstra* and in *JA*, 1915, I, 51; the two 秦地 Ch'in-ti, «land of Ch'in», of *BEFEO*, IV, 149, probably also translate «Cīnabhūmi», not «Cīnadeśa» or «Cīnasthāna») «Cīnasthāna» is found nowhere.

Nor is it a form which is to be readily expected. Names of countries formed with *-sthāna* are known in Sanskrit, but mainly in the north-west, *i. e.* in regions which had submitted to Iranian influence and in which they were used in imitation of the Iranian *-stāna* > *-stān* (the curious «Indrasthāna» of Hsüan-ying, ch. 18, in 爲, VI, 73b, given as the original name of India, may reflect the double contamination of «Indu» and «Hindustān»; for «Indudeśa», India, cf. BAGCHI, *loc. cit.*, 76, 295). The only instance of «Cīnasthāna» which I can trace at present is the *cinasthanade* of a Kharoṣṭhī tablet from Chinese Turkestan (cf. RAPSON, *Kharoṣṭhī Inscriptions*, p. 12, No. 35). Yet, if I do not believe that the «Chên-tan» of Chinese Buddhist texts actually translate a «Cīnasthāna» of the Sanskrit originals, I have no doubt that they represent, etymologically, a form connected with it. But that form, in my opinion, instead of being the Sanskrit term «Cīnasthāna», is an Iranian form of the type of Sogdian «Čynstn». If we read this last name \*Činstan as I suggested above, the interconsonantic *-s-* is likely to be omitted in the transcription and we have \*Čintan, which is exactly the original pre-supposed by Chên-tan. Of course, the original of Chên-tan need not necessarily be Sogdian, but may be of a type similar to the Sogdian \*Činstan. The names ending in *-stana* of the region of Khotan, and the very coexistence of Khotan and Gostana (see «Cotan») are perhaps to be considered as a case parallel to that of \*Činstan and Chên-tan (\*Čintan). It is well known that Khotan played an important part in the earliest spread of Buddhism to China. Whatever the true original of Chên-tan may be, it could not have been Skr. Cīnasthāna, and it was only because Chên-tan had become current in early Chinese Buddhism that its use was retained as a possible rendering of «Cīna», even after the accurate transcription of the Chih-na type had been adopted. The various Chên-tan forms were no longer understood; one of them was interpreted in connection with cinnabar (丹 *tan*); another was explained as referring to China's position in the East, where the sun rose (旦 *tan*; this occurs already in ch. 5 of Fa-lin's *Pien-chêng lun*, completed in 626; 露, VIII, 47a). Such fanciful hypotheses are on a level with the opinion of those who, losing sight of the Chinese origin of «Cīna», interpreted it as *cīvara*, «garment» because civilized China had «waistband and cap», or as *cintana*, «thought», because the Chinese were such deep thinkers (爲, X, 5a, 121a).

A last word on the subject of Chên-tan : even if the term comes from Khotan, and is not derived from a Sogdian original, it has left no trace in our Khotanese (the so-called «Śāka») texts. *Sthāna* occurs in them in the Prakrit form *thāna* (cf. KONOW, *Saka Studies*, 185).

As to the name of China, we find «Čiŋga» and «Čimġga» (< \*Cinaka), «Chinese» (KONOW, *ibid.*, 130; LEUMANN, *Das Nordarische... Lehrgedicht*, 3rd fasc., 421), and «Čaiga» (< Cina), «China» (BSOS, IX, 522-523).

Alongside of «Cina», Chinese texts of the T'ang period give 摩訶支那 Mo-ho-chih-na (\*Muâ-χâ-t'šię-nâ; Hsi-lin in 爲, VIII, 5a; in a letter of 795, 天, VI, 79b [cf. BEFEO, v, 254]), 摩訶至那 Mo-ho-chih-na (\*Muâ-χâ-t'šię-nâ; Hsüan-tsang, in JULIEN, *Vie*, 91; *Mémoires*, I, 255; II, 79; also in 結, III, 92b, and 致, IV, 76b [cf. BAGCHI, *Le canon bouddhique en Chine*, II, 552]), 莫訶支那 Mo-ho-chih-na (\*Mâk-χâ-t'šię-nâ; I-ching in CHAVANNES, *Religieux éminents*, 56; 致, VII, 96a), and twice 摩訶震旦 Mo-ho-chên-tan (\*Muâ-χâ-t'šię-nân; in *Hsin T'ang shu*, 221A, 11a [cf. Y<sup>1</sup>, I, 68; *Hobson-Jobson*<sup>2</sup>, 197]). All these forms render «Mahācina», «Great China», even the last one where «Chên-tan» is a survival, and used as a mere equivalent of «Cina». This is of course an honorific form for «China», but it is more than that. In the *Rāmāyaṇa*, the «Cina» are mentioned before the «Aparacina», both preceding the Tukhāra; the same names, in the same order, occur in the *Saddharmasmṛtyupasthāna*. The Tibetan translation of the latter text says that the rGya-nag, «Black Broad» (the usual Tibetan name of China) extends over 1,000 *yojana*, and the «Other Black Broad» (rGya-nag-gžan) over 200 *yojana*. The Chinese translation speaks only of 漢 Han, i.e. «China», but with a description which shows that, for the translator, «Han», or «China proper», was the «Aparacina», and that with all its dependencies it constituted the «Cina» of 1000 *yojana* (cf. LÉVI, in *JA*, 1918, I, 49, 122-123, 126-127). A text quoted by PÉRI (BEFEO, XVII, II, 42) distinguishes in the same way a 小支那 Hsiao Chih-na, «Small Cina», and a 大支那 Ta Chih-na, «Great Cina», but without telling us any more about the value of the two names. In a Sanskrit list of A.D. 1128, «Cina» and «Mahācina» follow each other among countries producing silk and other cloths (*Hobson-Jobson*<sup>2</sup>, 197). We have more precise information in I-ching, who says that «Chih-na (Cina) is Kuang-chou (Canton); Mo-ho-chih-na (Mahācina) is the capital (Ch'ang-an, Hsi-an-fu)» (致, VII, 96a: 支那即廣州也莫訶支那即京師也; cf. CHAVANNES, *Religieux éminents*, 56). Thirty years after I-ching (in 730 in fact) a similar notice is to be found in the *Hsü ku-chin i-ching t'u-chi* (結, III, 93b): «The kingdom of Yin-tu (Indu, India) commonly call Kuang-fu (Canton) 'Chih-na' (Cina), and give to the Imperial capital (Hsi-an-fu) the name Mo-ho-chih-na (Mahācina)» (印度國俗呼廣府爲支那名帝京爲摩訶支那). This note has passed into the *Sung kao-sêng chuan* (致, IV, 76b; cf. BAGCHI, *Le canon bouddhique en Chine*, II, 551-552).

I-ching's note, misplaced in CHAVANNES's translation, is in fact given in connection with a ruined «Cina» temple of the Ganges, traditionally founded, in the 4th cent., for twenty Chinese priests who had come to India *via* Yün-nan and Burma (CHAVANNES, *Religieux éminents*, 82-83). In the *Hsü ku-chin i-ching t'u-chi* and the *Sung kao-sêng chuan*, «Chih-na», with the note on the name, is mentioned in the biography of an Indian monk who really landed at Canton. It is of course out of the question that all mentions of «Cina» should in principle be referred to Canton, and the twenty priests of I-ching's narrative had not even passed through Canton on their way from China to India. But we may admit that, in the 7th-8th cents., there was a natural tendency to speak of the capital in the north as «Mahācina», and to understand «Cina»

as a designation of Southern China and more particularly of Canton. The reason may be that «Mahācīna» was more or less an honorific epithet, not conflicting with the name which foreigners of Central Asia then usually gave to China and which was no longer a form derived from Ch'in, but was the more recent «Taβyač», of northern origin (see «Catai»). On the other hand, in the south, sailors from Arab, Persian, Indian and Malay countries continued to refer to China as «Śin», «Čin», «Cīna» (Čīna). This explains how the name «Čin» became in a way a synonym of Canton. The note of I-ching and of the two subsequent works already foretells the state of affairs which was later reflected in Polo's «Sea of Cin».

But the question is not so simple. Persian sources often speak of جين و ماجين «Čin and Māčīn», Arabic ones more rarely of «Śin and Māśīn» (cf. Kāšyari's map). VON GUTSCHMID (*Kleine Schriften*, III, 605) was of opinion that «Čin and Māčīn» was a term created in Mussulman times as a «pendant» to Gog and Magog (see «Gog») and that «Māčīn» could have had hardly anything to do with «Mahācīna». This is surely an error. The intermediary form is provided by Al-Bīrūnī's book on India (c. 1030), where mention is made of «Mahāčīn», located north of the mountains where the Ganges takes its rise (SACHAU, *Alberuni's India*, I, 207). Rašīdu'd-Dīn was conscious of the derivation when he wrote: «In the language of the Indians, southern China is called 'Mahāčīn', i. e. 'Great China', from which name 'Māčīn' was formed» (cf. QUATREMÈRE, *Hist. des Mongols*, XCII-XCIII; also LXXXVII for Bānākātī's *rifacimento*; Šādiq Iśfahānī, in *Fe*, 560). In the *Aīn-i Akbarī*, completed in 1595, mention is made of Ĥitā (or Ĥatā, China; see «Catai»), «which is also called Mahāčīn, commonly pronounced Māčīn» (*Fe*, 552). There is perhaps, however, that much of truth in VON GUTSCHMID's theory that the reduction from «Mahāčīn» to «Māčīn» may have been favoured by the influence of «Magog», and that the vague but popular «Gog and Magog» is to some extent responsible for the frequent recurrence and the loose treatment of the parallel couple «Čin and Māčīn».

This loose treatment, however, is also and I think mainly due first to the various applications of the name «Čin» and secondly to the confusion caused in geographical nomenclature when China became divided into a Northern and a Southern China, governed by sovereigns of different races.

We have seen that the name derived from «Ch'in» had reached India and Iran *via* Chinese Turkestan, which, moreover, was at various times entirely under Chinese rule. It is therefore no surprise that Chinese Turkestan should have been more or less included in the foreign notion of «Cīna» or «Čin». What is more remarkable is that, when it became independent, its rulers, paying an unconscious tribute to the great Far Eastern civilization, still clung to the Chinese name. In the 11th cent. North China had come to be known as Ĥitai (see «Catai»), but the more ancient names were still in use, «Čin» in Persian, «Śin» in Arabic, «Taβyač» in Turkish: so the Qarakhanid sovereigns of western Chinese Turkestan took the Turkish title of «Taβyač khan», replaced on their coins by the Arabic «malik aś-Śin»; both mean «King of China» («Ṭamyāč khan», a secondary form of «Taβyač khan», is the title of the *fayfūr*, or Emperor of China, in Al-Bīrūnī; cf. Y<sup>1</sup>, I, 33).

For Kāšyari, «Ĥitai» was North China (then ruled by the Qītai of Ch'i-tan), and «Taβyač» all the rest of China under Sung rule; the latter was also called «Māśīn». As to «Śin», it is in

Kāšyari a very comprehensive term, the value of which had to be specified by some epithet : either « Upper Šin », which was China proper (sometimes used for « Ĥitai », sometimes also for « Taβyač » or « Māšin »), or « Lower Šin », which was that part of Chinese Turkestan which was under the rule of the Qarakhanids (cf. BARTHOLD, *12 Vorlesungen*, 97-98; BROCKELMANN, 250-251). HERRMANN was mistaken when he tried to identify Kāšyari's « Lower Šin » with Burma (cf. *TP*, 1936, 362). I think, moreover, that the terms « upper » and « lower » are to be understood in reference to people who took their bearings to the east, as is the case in Kāšyari's map, so that « upper » and « lower » would normally mean « east » and « west » respectively. Al-Baiṭār speaks of the rhubarb found in northern China, « that is to say in Turkestan, which the Persians call \*Šin Māšin » (LECLERC, in *Not. et Extr.*, XXV, I, No. 1018; *Fe*, 269; in both the spelling has been iranized as « Čin Māčín », which makes meaningless what follows).

The names « Čin » and « Taβyač » spread still farther over the map of Asia. In 1246, the Nestorian Simeon Rabban-ata handed over to André de Longjumeau for transmission to the Pope a *libellus* which he had brought *de pectore Orientis, scilicet de terra Sin*, and there is no doubt that « Sin » here means Northern Mongolia (cf. *Pe*, 32, 41, 53). A few years later, the author of the *Ṭabaqāt-i Nāširi* speaks of « the haughty Moyol infidels of Čin », *i. e.* of Mongolia (RAVERTY, 1288). Chinghiz-khan was first heard of in Egypt as *malik aš-Šin*, « king of China » (cf. BLOCHET, *Hist. d'Égypte de Makrizi*, 563, in the extracts from the *History of the Patriarchs of Alexandria*). For Stephen Orbelian, Qara-qorum, the Mongol capital, lay « in the country of Čin and Māčín » (BROSSET, *Hist. de la Siounie*, I, 224; cf. also *Hist. de la Géorgie*, I, 485; PATKANOV, *Istoriya Mongolov*, I, 68-70). An-Nāsawī, the biographer of Jalālu-'d-Dīn Māngūberti, gives the name « Ṭamyāč », other form of « Taβyač », to the capital of the Jučen, *i. e.* Peking (cf. *Y*<sup>1</sup>, I, 33; HOUDAS, *Hist. du sultan Djelad ed-Din*, 8, 9, 58); but, in the *Ṭabaqāt-i Nāširi*, Chinghiz-khan rose up « in the kingdoms of Čin and Ṭamyāč », and the « country of Ṭamyāč » means the « region of Qara-qorum » (RAVERTY, 935, 1291). In the 17th cent., the khans of Crimea proclaimed themselves « Great Emperors of the Tāt and Tawyāč [or 'Tawyāj'] ». The Tāt are the non-Turkish tribes; but this use of « Tawyāč » has puzzled scholars (cf. MINORSKY, in *EI*, s. v. « Tāt »). I consider it merely as the last outcome of the undue extension to the Turks and Mongols which the name Taβyač, « China » and « Chinese » (although etymologically the designation of a forgotten Altaic nation), had developed in the Middle Ages.

A similar undue extension of the name Čin, but at an earlier date, accounts for the would-be « Chinese » origin of the princes Orbelian and Čanbakurian-Orbelian (< Čenbakur, = *fayfūr* of Čin) of Armenian history, who must have been of Turkish descent (cf. BROSSET, *Hist. de la Siounie*, II, 181).

The Polian use of « Cin » is of course different. For Polo, « Cin » is the name used in the South by foreigners for « Mangi », *i. e.* South China, and the « sea of Cin » is the sea off the coast of « Mangi ». This is corroborated by Mussulman sources already at an early date. Ḥuwārizmī (first half of the 9th cent.) mentions the *bahr aš-Šin* « sea of Šin », as the most remote part of the Indian Ocean, and the same information is given in later works like the Persian *Ḥudūd al-'Ālam* (« Čin ») or the geography of Abū-'l-Fidā (cf. *Fe*, 591; *Mi*, 52, 179; REINAUD, *Géographie d'Aboulféda*, II, I, 25). Hethum mentions the « wealthy province Sim [read Sin], which is

between the kingdom of Cathay and the kingdom of Inde» and in which «fine diamonds» are found (*Hist. des Croisades*, Arm., II, 123, 263).

But our best informant is once more Rašidu-'d-Dīn. According to him, the country south of Hītai (= of North China) is called by the Hindus «Mahāčīn», hence «Māčīn», in Persian, but «Manzi» by the Chinese and «Nangias» by the Mongols (see «Mangi»). To judge from Bānākātī's account, Rašid seems also to have said that «Čīn» was the Hindu name for Hītai (cf. QUATREMÈRE, *Hist. des Mongols*, LXXXVI-LXXXVII, XCI-XCIII). For «Māčīn» = «Manzi», cf. also *Bl*, II, 370-371. At other times, Rašid uses «Čīn and Mačīn» as a mere synonym of «Nangias», i. e. South China (cf. *Ber*, I, 146).

But Rašid also speaks of the «capital of Māčīn» as of a city which is neither Hīngsai (see «Quinsai»), nor Zaitūn (see «Čaiton»), and YULE has already surmised that the place referred to must be Canton (cf. *Y*<sup>1</sup>, III, 115; cf. also LE STRANGE, *Nuhzat al-Qulūb*, transl., 250, and the somewhat conflicting account on p. 254). According to YULE (*Y*<sup>1</sup>, II, 180), both Al-Bīrūnī and Rašidu-'d-Dīn «distinctly apply the name Mahacin to a city, no doubt Canton». I do not think that this information should be traced as far back as to Al-Bīrūnī; it occurs in a chapter in which Rašid freely culled from Al-Bīrūnī, but an additional section of which cannot be earlier than the Mongol period. It is in this latter part that we find the following very interesting passage (ELLIOT, *History of India*, I<sup>1</sup>, 45-46; I<sup>2</sup>, 71-72): «Beyond [Champa; see 'Ciamba'] is Hainam (see 'Cheynam'), subject also to the Qāān. Beyond that is Māhāčīn, then the harbour of Zaitūn, on the shore of the China Sea...». Instead of «Māhāčīn», an Arabic ms. gives *الصين الاعلي*, also meaning «Čīn the Great». YULE was certainly right when he said that Rašid's «Māhāčīn» in this passage was Canton.

Canton is first mentioned in Arabic texts of the 9th cent. as خانفر Hānfū, which renders 廣府 Kuang-fu, a popular short form of Kuang-chou [州]-fu. I stated this in 1904 in *BEFEO*, IV, 215. To the examples of «Kuang-fu» I had then adduced, I can now add 結, III, 93 b, quoted above; *T'ang liu tien*, 20, 8 ro; and, at a much later date, SAINSON, *Hist. particulière du Nan-Tchao*, 45. But the identification was forgotten in the Mongol period, and Canton came to be known under new names. I concur with ELLIOT (*History of India*, I<sup>2</sup>, 71) who identified with Canton Idrīsī's صينية الصين Šīniyatu-'š-Šīn, «China of China» (JAUBERT, I, 194), although Idrīsī also repeats elsewhere (*ibid.*, I, 84) the old information on Hānfū without suspecting that both names refer to the same place. Al-Baiṭār, speaking of the Persian «\*Šīn Māšīn» (properly «Čīn ū Māčīn») says that it is somehow equivalent to an Arabic form «Šīn aš-Šīn», «China of China» (*Fe*, 269). Although this is not etymologically correct and although Al-Baiṭār wrongly refers «Čīn and Māčīn» to «Turkestan», his text shows that a correspondence was felt between the two forms. We must then take into consideration that Māhāčīn (= Mahāčīn, Māčīn) is used by Rašid as a name of Canton, and that «Šīniyatu-'š-Šīn» is practically the same form as «Šīn aš-Šīn». The identification is already all the more probable when Idrīsī speaks of «Šīniyatu-'š-Šīn» as being a city of unequalled greatness, which was situated at the extremity of the empire, and which was visited by a great number of traders from various parts of India.

This is made certain by a passage in which Ibn Baṭṭūṭah, two centuries after Idrīsī, speaks of «Šīn aš-Šīn» as being the same place as صين كلان Šīn-kālān (DEFRÉMERY, IV, 92). Ibn Baṭṭūṭah



again mentions «*Šin aš-Šin*» in iv, 254, and «*Šin-kälän*» in iv, 271-272 (cf. *Fe*, 455, 673). Now, «*Šin-kälän*» is the arabized form of Pers. Čin-kälän, «*Great China*», thus synonymous with «*Mahāčin*», and there is not the slightest doubt that «*Čin-kälän*» is Canton. The name occurs before Ibn Baṭṭūṭah in Rašidu'-d-Dīn and in Waśśāf. Rašid's passage begins as follows (*Bl*, II, 493) : «*The ninth province (šing; see 'Scieng')* is that of كونگی Kōngi (?) which the Tāzik call Čin-kälän . . .»; the Tāzik (misread «*Tāzi*» in *Fe*, 673) are the Persians. The name I have read «*Kōngi*» with the Vienna ms. is uncertain; other readings are «*Lūmkāli*», «*Lūtkāli*», «*Lūtāqali*», «*Kūilkī*» (adopted by BLOCHET). We should expect the name of Kuang-chou-fu, either as such or under the abbreviated form Kuang-fu. In Rašid's transcription, Kuang-fu would become كونفو \*Kōnfū or كونكفو \*Kōngfū. The reading لوتقالي Lūtāqālī of L has a ق *q* which may easily be corrupt for ف *f*. On the whole I feel inclined to correct the name to \*Kōngfū. This would dispose, however, of the very doubtful hypothesis I have suggested for «*Choncha*» (*q. v.*).

Waśśāf's passage has suffered at the hands of the translators. In the text (*Ha*<sup>2</sup>, Pers. text, 43) it is said that there are in China 400 great towns, the smallest of which is of greater extent than Bagdad or Šīrāz; among them are «*Lūngin-fu (Lung-hsing-fu), Zāitūn and Čin-kälän*». D'OHSSON (II, 418) misread the first name as «*Kenkan-fou*». As to HAMMER (*Ha*<sup>2</sup>, transl., 43), he jumbled up the first two names into «*Lonkin Ferwezetium*» and simply omitted «*Čin-kälän*».

This name of Canton is also mentioned by Western travellers. It is the «*Censcala*» of Odoric (*Wy*, 458; var. «*Cescalán*», «*Censcolán*»; read «*Cencalan*» or «*Cincalan*»). I have little doubt that the «*Cincalan*» of the Catalan Map comes from Odoric (cf. CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 35). Shortly after Odoric, Marignolli, in enumerating cities of «*Manzi, formerly Cyn*», speaks of «*Cynkalan, i. e. Great India, since kalan is 'great'*» (*Wy*, 543). Marignolli, for whom Southern China («*Manzi*») is the first of the three Indies, opposes «*Cynkalan*», which is «*Great India*», to «*Cynkali*» of «*Mynibar*». «*Mynibar*» (Malabar; see «*Melibar*») is Marignolli's second India, and his «*Cynkali*» has in fact nothing to do with «*Čin*»; nor is it easy to see what Oriental word he is referring to when he says that «*kali*» means «*small*» (cf. *Y*<sup>1</sup>, III, 249; *Hobson-Jobson*<sup>2</sup>, 828-829). All these texts provide overwhelming evidence that, in the Mongol period, the name «*Great China*» specifically referred to Canton, a reversion to the use which obtained in I-ching's time.

The intricacies involved in the name «*Čin*» are not brought to a close, however, with «*Čin-kälän*». The case of the sort of brazil-wood called *verzino sieni* or *seni* by Pegolotti is still obscure (see «*Brazil*»). Čin and Mačín («*Chin et Machin*») are a designation of Southern China in an unpublished section of the *Libellus de notitia orbis*, completed in 1402 (cf. A. KERN in *Arch. Frat. Praed.*, VIII [1938], 89). The double term «*Čin and Māčín*» occurs c. 1470 in Nikitin (*Y*<sup>1</sup>, I, 151). Josafa Barbaro, giving c. 1480 information collected in the Crimea in 1436, speaks of «*the Cini and Macini, part of Cataio*» and of «*all those of the Cini and Macini and of Cataio*» (RAMUSIO [1559], II, 106); he also says that «*Cini and Macini are two very great provinces inhabited by idolators*» (cf. *Y*<sup>1</sup>, I, 269). In 1503, Syrian bishops were ordered to go «*to the land of the Indians and the islands of the seas which are between Dabag and Sin and Masin*» (*Y*<sup>1</sup>, I, 127; S. GIAMIL, *Genuinae Relationes*, Rome, 1902, 8 vo, 558-600).

A century later, GOUVEA speaks of a bishop of «Masina» mentioned in an old document (*Mo*, 15).

All these are stereotyped or vague mentions, which create no serious difficulty. The case is different with Nicolò de' Conti. In his account as written down in Latin by Poggio, Conti (LONGHENA, 140-146) speaks of Ava in Burma, adding that the province is called «Macinum» (but without an alternative name «Mangi», despite HALLBERG, 334) and abounds in elephants. He then goes on to give a notice of «Cathaium» and its two principal cities «Cambaleschia» (see «Cambaluc», Peking), and «Nemptai» (certainly Nan-t'ai = our Nanking; see «Namgin»); the whole of the section on «Cathaium» (Cathay) seems to transmit hearsay information. YULE has drawn from the text the natural inference that for Conti «Mačîn» was located in Indo-China, adding that Fra Mauro followed Conti in this and that the *Aīn-i Akbarī*, if he remembered rightly, applied the name «Māčîn» to Pegu (*Y*<sup>1</sup>, I, 151).

This calls for comment. As is well known, Fra Mauro certainly obtained from Conti himself much information which is not to be found in Poggio's text (see «Caragian» and *Y*<sup>1</sup>, I, 176-177). Even when the names occur in both sources, they are more correct in Fra Mauro. In the present case, Fra Mauro's Map gives not only «Macin» (not the Latin «Macinum» of Poggio) in a region which seems to be Indo-China in fact, but also a «provincia Macin» on the border of Yün-nan, a «provincia Bangala nel Macin» already in «India seconda», and a «provincia Macin» almost in «India prima». This disposes, by the way, of LONGHENA's hypothesis (p. 96) that «Macinum» might be Fra Mauro's «Mangi»: there is no «Mangi» in Fra Mauro, but «Mango» (cf. «Mangon» in V), and quite apart from «Macin» (I do not think it necessary to refute LONGHENA's other assertion, p. 140, that «the name Macinum is given by the Chinese to northern Indo-China»!). As to the *Aīn-i Akbarī*, YULE was right in remembering that there was something strange in it about Pegu, but his remarkable memory was for once at fault as to what it was. In the *Aīn-i Akbarī*, «Mahāčîn, which is commonly pronounced Māčîn» is given as another name of Hītā, that is to say of China herself. The text speaks later of Arakan with the port of Chittagong, and goes on to say: «Near that tribe is Pegu, which is also called Čîn; in certain ancient accounts, it is mentioned as the capital of Čîn» (*Fe*, 551-552). So it is not «Māčîn», but on the contrary «Čîn», which is equated with Pegu in the *Aīn-i Akbarī*, so that Conti's Indo-Chinese «Macin» remains isolated.

I do not think that we ought to lay too much stress on these two passages. Conti's interesting account is crammed with inconsistencies. As to the *Aīn-i Akbarī*, it is a composite text, an uncritical patchwork of all sorts of data; some error must underlie the statement that ancient accounts make Pegu the capital of «Čîn». I would suggest the following explanation. During the Liao and Chin dynasties and during the first part of the Yüan, China had been divided into two parts, for which there were separate names, Hītai in the north, Čîn and Māčîn in the south. With the unification of China under the Mongols, the name Hītai gained ground and ultimately superseded that of Čîn and Māčîn in the Mussulman world. Conti's Indo-Chinese «Macin» and the Peguan «Čîn» of the *Aīn-i Akbarī* would be the outcome of the erroneous, but natural tendency to find a location for names which still lingered in popular memory, though they had already served and lost their purpose. I see no ground for supposing, with YULE (*Y*<sup>1</sup>,

I, 151; II, 177), HALLBERG (p. 335) and RICCI-ROSS (*RR*, 426), that a confusion took place in the East between «Manzi» and «Māčīn», nor to believe with BENEDETTO (*B*<sup>1</sup>, 441) that in some works «Čin» is rather the designation of «Indo-China, the Malay Peninsula and Insulind». But it may be that «Čin», without ever specifically referring to Indo-China, was sometimes used in a vague and loose manner when speaking of products that came to the West from «further India» and the Far East. This would account for the «diamonds» of Čin mentioned by Hethum, if any weight can be attached to this information. It may perhaps also explain the Persian name of the cinnamon, *dār-čīnī* (> Ar. *dār-šīnī*), «Chinese-wood», although South China is not excluded in this connection (cf. FERRAND, in *JA*, 1920, II, 37; LAUFER, *Sino-Iranica*, 541; *Mi*, 125, 375).

Fra Mauro's Map calls for one more remark. It mentions «Cin» several times; «Cin over India terça», «provincia Çouça in el Cin», «provincia Mihen in el Cin», «provincia Tebet nel Cin». The names of the «provinces» seem to be due to Polo, but Polo does not then speak of «Cin». It would be curious if we had here a superposition of notions taken from Polo with information given by Conti. Moreover, the different «Macin» in the three Indies are puzzling too.

When, towards 1500, the Portuguese came to hear of China, they correctly transcribed as «China», in Portuguese spelling, the form «Čina» used by the Malays. This Portuguese spelling has been retained in English and in German, but pronounced in ways that are no longer true to the original.

Since the 17th cent., «China» has also become in English another name for «porcelain» (see «Porcelain»). Something similar occurred in Persian, where the adjectival form *čīnī* means not only «Chinese», but «porcelain» (cf. also *Hobson-Jobson*<sup>2</sup>, 198). A similar use of *čīn* and *čīnī* exists in Osm. Turkish, although the Osmanli also uses a form *farfuru* (> Russ. *farfor*) altered from *fayfūrī*, the Persian adjectival form of *fayfūr*, «the Son of Heaven» (see «Facfur»). In the paragraph on China of the *Libellus de notitia orbis*, a transcript of which I owe to the kindness of Father R. LOENERTZ, we are told of porcelain vases that *in illa lingua persica dicuntur chim* (read *dicuntur chini*).

## 156. CINCHIM

*chimchim* P  
*chinchim, chinchin* VA  
*chinchis* VL  
*chingin* FA

*chinguy* FB  
*cinchin* F  
*cinghi* TA<sup>3</sup>  
*cinghy* TA<sup>1</sup>

*cingis* R  
*gyngym* LT  
*zinchin* V

眞金 Chên-chin, lit. «True Gold», which c. 1300 was still pronounced \*Čin-kim, was the second son of Qubilai. Later Mongol tradition continued to refer to him as Čingim (cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 119). Rašidu-'d-Din (*Bl*, II, 354-355, 582) writes جیم کیم Jim-kim (or Jim-

gim?); Waśśāf (*Ha*<sup>2</sup>, 45, 46), جمکین Ĵimkin (or Ĵimgin?); so it seems that, in Persian usage, a metathesis had changed \*Ĵingim to Ĵimgin.

As to Polo's «Cinchim», I must dissent from the latest editors, CHARIGNON (*Ch*, II, 41), RICCI-ROSS (*RR*, 417) and BENEDETTO (*B*<sup>1</sup>, 441). Polo says (in F) that «Cinchin» was so called «por le amor dou buen Cinchin Kan». As «Cinchin Kan» means certainly here Chinghiz-khan, the three editors have introduced into the text, as Chên-chin's name, «Cinghis» instead of «Cinchin» (=«Cinchim»); ROSS explains at some length in his Introduction (*RR*, XIV-XV) that otherwise the sentence makes no sense. I think we can prove, on the contrary, that «Cinchim» is correct.

It is perfectly true that there is no connection between the names of Chên-chin and Chinghiz, and also that the mss. which give the present sentence write the two names identically. But it is no less true that all editors agree to read, in other chapters where Chinghiz-khan is meant, his correct name «Cinghis» (F), «Cingis» (R), «Cinghyscan» (Z). In the present chapter, RAMUSIO writes «Cingis» instead of «Cinchim», but without anything in the way of «por le amor dou buen Cinchin Kan» or of the still more developed sentence of the Court French *rifacimento* followed by PAUTHIER. All other texts give here forms akin to «Cinchin», and such is also the case in the four other passages where Qubilai's second son is named by Polo. Now, there is no doubt that «Cinchim», which sounds Činkim, is a very fair representation of that son's true name. If Polo had spoken of Chên-chin (Čin-kim) as «Cinghis», wrongly of course, how is it conceivable that copyists should have systematically altered that well-known name of «Cinghis» in such a way as to give by chance the correct name of Chên-chin (Čin-kim)? On the other hand, it is evident that Polo knew the two names of Chinghiz-khan and of Chên-chin (PAUTHIER's remarks in *Pa*, 263, that personal names were known only after death are absurd). Must we admit that he found a certain phonetic analogy between the two names and so, without confounding them, connected one with the other? That is the conclusion we must arrive at if the whole sentence is genuine. But I see things differently. While the obscure name of «Cinchim» could not contaminate the well-known name of Chinghiz, the reverse is not true. Let us suppose that Polo had simply said that Qubilai's eldest son was called «Cinchim»; readers and copyists would have been struck by the similarity of that name, whether it was written «Cinchim» or «Cinchin», and of that of «Cinghis», and would have imagined a connection. A first one inserts «por le amor dou buen Cinchin Kan», without noticing that elsewhere he always ends Chinghiz-khan's name with -s; the Court French reviser adds «le premier seigneur des Tatars». But what I believe to be the original text — that is without anything explanatory after «Cinchim»'s name — is still found in ancient recensions, for instance in LT, and also in RAMUSIO. The interesting point is that RAMUSIO, who must not have had in his manuscripts the explanatory sentence about «Cinchim»'s name, altered that name everywhere to «Cingis»; I think he provides us with a striking example of the contagious attraction of the name of Chinghiz-khan. My conclusion is that Polo is not responsible either for the error or for the absurdity which recent editors have put on his shoulders, and that, in any case, we should not correct «Cinchim» to «Cinchis».

One point remains. I have said that the pronunciation of the name, about 1300, was \*Čin-kim, still attested in Mongolian, and indirectly borne out by the Persian transcriptions.

Polo must also have used a form with *m*, either «Cinchim» as in Chinese, or «Cimchin» with the Persian metathesis; that is why I have adopted «Cinchim». The problem is always that of divergent interpretations by copyists of an *-l̄*, which could be read as *-in* or as *-im*. The mss. do not vouchsafe any definite conclusion.

The name given to Qubilai's eldest son is purely Chinese, and Rašidu-'d-Dīn must have thought so to divide it into its two elements. As Chên-chin was born in 1243, it is somewhat surprising to see Qubilai give him a Chinese name. At such a date, when the supreme power was in Ögödäi's branch, Qubilai could not have foreseen the accession of his own branch, nor the early death of Mongka, nor the later circumstances which, from a nomad of Upper Mongolia, turned him into an Emperor in China. But Qubilai was always attracted by what was foreign to his native land, and so his eldest son was named Dorji, which is Tibetan rDo-rje, *vajra*, «diamond». Moreover, a Buddhist priest of the Lin-chi school, born in Shàn-hsi in 1202, 海雲 Hai-yün, had probably come into touch with one of Chinghiz-khan's sons already in 1214. At any rate he found real favour with the first Mongol Emperors, and seems to have been responsible for the name given to Qubilai's eldest son (cf. H. KUNISHITA, in *Tōyō gakuhō*. XI, 547-577; XII, 89-124, 245-249).

Chên-chin was nominated Heir-Apparent in 1273; he died on January 5, 1286; his biography is in *YS*, 115, 2*a-4 b*, and, with additional matter and with notes, in *T'u Chi*, 76, 1*a-4 a*.

The same name, Chên-chin, was borne in the middle of the 14th cent. by a son of the Emperor Shun-ti who died when he was one year old (*YS*, 114, 4*b*).

## 157. CINGHIANFU

*chinghianfu* FB  
*chynghiamfu* FBt  
*cianghianfu* Ft, Z  
*cigianfu* P<sup>s</sup>  
*cigiansu*, *gigiansu* Pr  
*cinghiau* TA<sup>1</sup>, TA<sup>3</sup>

*cinghianfu* F, Fr, L, Z; R  
*cinghiansu* LT  
*cinghinanfu* F  
*cinghyanfu* Z  
*cingiafu*, *çingiafu* VB  
*cingianfu* Lr, VA

*cinginanfu* FA<sup>t</sup>  
*cygianfu*, *syianfu* P  
*cymgiansu*, *cyngiasu* G  
*giginafu* V  
*pingianfu* VL

Ch. 鎮江府 Chên-chiang-fu, on the southern bank of the Yang-tzü. All particulars about Chên-chiang and the ancient Christians there have been collected and studied by MOULE and L. GILES in *TP*, 1915, 627-686; cf. also *Mo*, 145-165, and see here «Marsarchis».

For Odoric's «Mençu», etc. (*Wy*, 470), which has been sometimes associated tentatively with Chên-chiang (cf. HALLBERG, 350; *TP*, 1915, 412; *Pe*, LII; *Wy*, 470), I rather side with those who see in it Ming-chou, the ancient name of Ning-po; but I must add that Odoric's data as to the position of «Mençu» can hardly be reconciled with such an identification.

## 158. CINGHIS

Pages.		Pages.		Pages.
281	Birth of Chinghiz-khan.....	303	Chinghiz-khan and Ong-khan's daughter.....	309
288	The clot of blood.....	304	Chinghiz-khan as King David..	330
289	The name Tämüjin.....	305	The date of Chinghiz-khan's death.....	353
291	The title of Ča'ut-quri.....		The place of Chinghiz-khan's death.....	
295	The enthronement of 1206....		The tomb of Chinghiz-khan....	
296	The title «Chinghiz-khan»....		The other Imperial tombs of the Mongol dynasty.....	

*chimchim, chimchis, cimchim, cimchis* P  
*chinchis* VA, VI.  
*chincho* VL  
*chingin* FA, FBr, *t*  
*chingins* FA, FAt, FB, FBr, *t*  
*chinguis* FAt  
*chinguy, singuras* FB  
*cinchim, cinchym, cinghim, cinghym* LT

*cinchin* F, Fr, *t*, L  
*cinchinikanis, cinchinkan, cinchnkan, cinchnkanc, cinchini* L  
*cinchins* F, L  
*cinchis, cinghins, cingin, cingins* F  
*cinghis* F, L, TA<sup>1</sup>, TA<sup>3</sup>  
*cinghi, cinghy, cinghys, cinghi* TA<sup>1</sup>

*cingims, cinguins* FA  
*cingis* R  
*cingiscan, cinghyscan* Z  
*cingius, cymgius, cyngius* G  
*zenzi, zenzin, zezino, zinzin, zizino* VB  
*zesiachan, zischan, zischi chan* V

The Mongolian form of the name is «Činggis», in Chinese transcription 成吉思 Ch'êng-chi-ssü, in Tibetan Čhiŋ-gis, and Čāngiz in the dialect of the Moghols of Afghanistan (RAMSTEDT, *Mogholica*, 25). The Persian transcriptions are چنگیز Čingiz (in Güyük's letter to Innocent IV, in Juwainī, in Waśśāf), چنگیز Čingiz in Rašidu-'d-Dīn (in BEREZIN's edition; cf. also *Bl*, II, 1; the spelling چنگکیز Činggiz adopted in BLOCHER's edition seems to have less authority). For lack of a č, the Arabs transcribed it «Jingiz» (for instance, an-Nāsāwī); the Syriac chroniclers, «Šingiz». The Armenian transcription is «Čang'z», which accounts for «Changuis» and «Canguis» in Hethum (*Hist. des Croisades*, Arm., II, 148 sq., 284 sq.). Ricoldo da Montecroce's «Camiustan» is probably to be read «Canguiscan» (not «Cangiuscan» as in ZARNCKE, *Der Priester Johannes*, II, 103, nor even «Canjus can» as in *Y*, I, 247). San Antonino's «Chingiscan rectius Tamgius Can» is based on a misreading of the type proposed by YULE and ZARNCKE. The same may be said of Chaucer's «Cambuscan». Both Plan Carpine (*Wy*, 52-53, 58, 84) and Rubrouck (*Wy*, 186, 222, 223) give «Chingis» (with *ch* = č). Marignolli has «Cingwis» (*Wy*, 543). We find Τζιγκίς in George Pachymeres (*Corpus Script. hist. byzant.*, Bonn, 1835, I, 347).

The meaning of «Činggis» will be discussed further on.

**BIRTH OF CHINGHIZ-KHAN.** — The early history of Chinghiz-khan is full of uncertainties and contradictions, beginning at his very birth. Two points, however, seem securely established: that the future Chinghiz-khan was the son of Yästügai-ba'atur and Hö'alün-äkä (> Ö'lün-äkä; a name formed with the feminine suffix *-lün*, and not to be explained as *a'ulän*, «cloud», despite ERDMANN,

*Temudschin*, 567), and that he was born at Däli'ün-boldaq, a place on the right bank of the Onon which is said to be still known under that name (cf. VLADIMIROV, *Čingis-khan*, 18; ERDMANN, *Temudschin*, 572, relying on an unidentified report of the Nerčinsk merchant YURINSKIĪ, says it lies seven versts above the island Yäkä-aral, and three versts from the guard station [?] Kočuev; ABRAMOV of Nerčinsk, quoted from the *Severn. Pčela*, 1854, No. 258, in PALLADIUS, *Trudy členov Rossijskoj dukhovnoj Missii*, IV, 175-176, and BANZAROV, quoted in *Ber*, II, 232, locate it « on the right bank of the Onon, somewhat above the Čindant fort, opposite the village Yäkä-aral, about lat. 50° and long. 132° [= east of Greenwich 114°] »; the coordinates given in WOLFF, *Gesch. der Mongolen*, 33 [lat. 40° 45'; long. 127°], are impossible). But the date of his birth is a very moot problem.

Various years have been given : 1163 by DE GUIGNES, 1161 by MAILLA, 1162 by GAUBIL, 1155 by all those who follow Rašidu-'d-Dīn.

According to DE GUIGNES (*Hist. gén. des Huns*, III, 10), Chinghiz-khan was born in « 1163 », « A. H. 559 », a « pig » year. This date can be rejected at once. The year 559 of the hegira begins on November 30, 1163, but it is not a « pig » year; the nearest « pig » years are 1155 and 1167; « 559 » is certainly the result of a wrong reading for « 549 » which is given in most Mussulman sources and corresponds to 1155 (KLAPROTH's assertion that it is « 549 » which is a corruption for « 559 » [*Asia Polyglotta*, 256] is not acceptable). The same may be said of the impossible « A. H. 599 », *i. e. A. D. 1202*, in Bānākātī (cf. ERDMANN, *Temudschin*, 574).

The date of 1161 occurs in MAILLA (IX, 2, 8), and also in the Mongol chronicle *Altan tobči*, completed at the beginning of the 17th cent., where Chinghiz-khan is said to have been born « in the year of the snake » and to have died in the « pig » year at the age of sixty-seven, *i. e.* sixty-six in European reckoning (cf. GOMBOEV ed., in *Trudy Vost. Otd. IRAO*, VI, 130, 146). MAILLA (and his editor DESHOUTERAYES, who revised the translation and the notes) had no direct authority for the year of his birth, and calculated it back from the statement in the continuation of the *Kang mu* that Chinghiz was « sixty-six » years old when he died in 1127. But the years must be counted in Chinese fashion, a child being regarded as being one year old at the moment of his birth, so that the date to be deduced from the Chinese text is « 1162 ». The same mistake lies at the basis of the « year of the snake », *i. e.* 1161, in the *Altan tobči*, where this date is immediately followed by the statement that Chinghiz-khan was forty-five (forty-four for us) when he raised the « white standard with nine pennants » « in the tiger-bing year », *i. e.* *ping-yin*, 1206. There can be no doubt that the source of the *Altan tobči* meant that Chinghiz was born in 1162.

This date of 1162 has long been supposed to be the only one to be found in Chinese sources. It is the one to be deduced from the *YS*, I, 9 b, where Chinghiz is said to have been sixty-six years old (sixty-five for us) at the time of his death in 1227. The same information occurs before the fall of the Mongol dynasty in the *Cho-kêng lu* (I, 11a), a work completed in 1366. An earlier text seems to bear testimony to the same tradition; it is the *Shêng-wu ch'in-chêng lu*, compiled or, more probably, translated from the Mongolian in the second half of the 13th cent. (cf. *TP*, 1929, 170-171), and incorporated between 1350 and 1366 in T'ao Tsung-i's *Shuo fu*. The *Shêng-wu ch'in-chêng lu* (WANG Kuo-wei's edition, 43 a), after relating Ong-khan's death (see « Unc ») which probably took place in 1203, before passing on to events expressly dated *chia-tzū* (1204), adds that Chinghiz-

khan was then « forty-two years old » (forty-one for us); the year of his birth would thus be 1162. The date « 1162 », given in the Chinese official dynastic histories, was adopted by « Sanang Setsen », who completed his chronicle in 1662 (SCHMIDT, *Gesch. der Ost-Mongolen*, 63, 376). In Tibetan works like the *Deb-ter mgon-po* (« Blue book »), the year of the birth of Chinghiz is given as « water-tiger », *i. e.* *jên-yin* (A. D. 1182), but Sum-pa Hūtuḥtu (cf. VASIL'EV, in ZVOIRAO, IV, 375) insisted that this was an error for the « water-horse » year (A. D. 1162). « 1182 » is of course impossible, and the errors seems to be due to a misreading of *stag*, « tiger », for *rta*, « horse ». 'ĪIGs-MED NAM-MKHA gives « water-horse », *i. e.* 1162 (miscalculated as « 1161 » in HUTH, *Gesch. des Buddhismus*, II, 14, a work in which all the dates are systematically too early by one year). GAUBIL (*Hist. de Gentschiscan*, 2) correctly gave the date « 1162 » of the Chinese official historiography, and D'OHSSON (*Oh*, I, 35, 38), impressed by the agreement of the Chinese texts with « Sanang Setsen », decided in their favour as against the data he found in Mussulman sources. WOLFF (*Gesch. der Mongolen*, 34) and HOWORTH (I, 50) both shared D'OHSSON's opinion.

D'OHSSON was, however, soon opposed by HAMMER (*Ha*<sup>3</sup>, 56) and ERDMANN (*Temudschin*, 572-574). HAMMER, quoting Mirḥōnd, said that Chinghiz-khan was born on the 20th *ḡū-'l-qa'dah*, A. H. 549, = January 26, 1155 (the equivalence « January 27, 1154 » in RAVERTY, *Ṭabaḡāt-i-Nāṣiri*, 938, is pretentiously and egregiously wrong), a « pig » year, and that both Ĵuwainī and Rašīdu-'d-Dīn gave the same year and day. There is in fact nothing of the kind in Ĵuwainī, who is silent not only as to the month and day of the birth of Chinghiz-khan, but even as to the year. As to Rašīd, he does not say that Chinghiz-khan was born on a day corresponding to January 26, 1155; nor could he be expected to do so, for the following reasons : first, since he says that « the month, day and hour of the birth of Chinghiz-khan are not known . . . » (cf. the long passage translated in ERDMANN, *Temudschin*, 572-573; and the analogous one, though widely divergent in details, of *Oh*, I, 38; neither is an accurate rendering of the original text, which is given in *Ber*, II, Pers. text, 139; transl. 85-86). Secondly, the year A. H. 549 corresponds to March 18, 1154-March 6, 1155, while the intended « pig » year corresponds to February 4, 1155-January 23, 1156 (in the Chinese calendar; the « Uighur » calendar followed by Rašīd might differ by one or two days). In other words, A. H. 549 is not really a « pig » year. But *ḡū-'l-qa'dah* is the eleventh month of the Arabic year, and the first day of the « pig » year, February 4, 1155, corresponds to the 29th *ḡū-'l-qa'dah* of A. H. 549. That is why Rašīd says more than once that the « pig » year in which Chinghiz-khan was born began in *ḡū-'l-qa'dah* of A. H. 549 (cf. *Ber*, II, 85, 86, 88, 89; III, 102). I have no doubt that it is the indication of this beginning of the year in *ḡū-'l-qa'dah*, coupled with a wrong indication of the « 20th » instead of the « 29th », which is responsible for the day and month in Mirḥōnd (*Rawḡatu-'s-Safā*, Lucknow ed., v, 10), or rather, as my friend M. M. QAZWINI kindly tells me, in Mirḥōnd's source, the *Ta'riḡ-i Guzidā* of Ḥamdullāh Mustawfī (*Gibb Memorial* ed. 572). The same may be said of the sources which speak of the eighth of *ḡū-'l-qa'dah* of A. H. 549 (KLAPROTH, *Asia Polyglotta*, 256, where the « 13th » of January must be corrected to « 14th »; WOLFF, *Geschichte der Mongolen*, 33, where « 27. Januar » for the twentieth of the Arabic month is a slip for « 26. Januar »). The « 26 » of *ḡū-'l-qa'dah* in DESMAISONS, *Hist. des Mogols*, II, 73, is a misprint for « 20 ». The birth of Chinghiz-khan « in the winter of the pig year », according to Bānākāti (ERDMANN, *Temudschin*, 575) is likewise without authority. Nobody knew the month or day in the Mongol period, and it does



not even seem that there was then a conventional date to commemorate the birth of the founder of the dynasty.

There is, however, an early Chinese work, written in the lifetime of Chinghiz-khan, which gives for his birth a year different from the one adopted in the *YS*; it is the *Mêng-Ta pei-lu*, which is generally attributed to the Chinese general 孟珙 *Mêng Hung*, but the author of which must be 趙珙 *Chao Hung* (cf. *TP*, 1929, 166). *Chao Hung*, a Sung official, visited the Mongol vice-regent Muqali at Peking in 1221 while Chinghiz-khan was fighting in the west against the Mussulmans. He says (WANG Kuo-wei ed., 2*b*): « The present Emperor Ch'êng-chi-ssü (Čingis) was born in *chia-hsü* (1154). In their customs (= of the Mongols), there is absolutely no sexagenary cycle (*kêng-chia*); now I have written this (= *chia-hsü*) after investigating what they say, and in order to get an easier understanding of their age. In their customs, they count a year every time the grass grows green ». The *Mêng-Ta pei-lu* has long been known in VASIL'EV'S translation (in *Trudy VOIRAO*, iv, 216-235), and seemed to give strong support to Rašid's date of 1155, instead of the 1162 of the *YS*; for that reason, BARTHOLD was always in favour of 1155 and VLADIMIROV (*Čingis-khan*, 18) says that Chinghiz-khan was born « c. 1155 ».

Modern Chinese scholars have gone further. HUNG Čhün (iB, 69-71), knowing the date, 1154, of the *Mêng-Ta pei-lu* and the date, 1155, of Rašidu-'d-Dīn, called attention to a text of 楊維禎 *Yang Wei-chêng* (1296-1370) which is preserved in the *Cho-kêng lu* (3, 1*a-9 b*). In or about 1343, while the *Sung shih* was being compiled in compliance with an Imperial edict, *Yang Wei-chêng* submitted to the Emperor a memorial establishing the fact that the Mongol Emperors did not perpetuate the Liao and the Chin dynasties, but were, by the mandate of Heaven and the consent of the people, the rightful heirs to the Sung dynasty. In the course of his argument, we find the following passage: « Moreover, I observe that the ancestor of the Sung was born in *ting-hai* and founded the Empire (建國 *chien-kuo*) in *kêng-shên*, and that our T'ai-tsu (= Chinghiz-khan) was born and founded the Empire in the same years; that the Sung crossed the [Yang-tzŭ-]chiang in *chia-hsü* and pacified Chiang-nan in *i-hai* and *ping-tzŭ*, and that the years in which our Imperial army crossed the [Yang-tzŭ-]chiang and pacified Chiang-nan are the same. The correspondence of the Heavenly numbers is not accidental; their [common] dependency on the Celestial spirit is not fortuitous ». Now, it is true that *Chao K'uang-yin* (T'ai-tsu of the Sung) was born in a *ting-hai* year (927) and founded his dynasty in a *kêng-shên* year (960); that his armies crossed the Yang-tzŭ in *chia-hsü* (974) and subdued the « Chiang-nan » dynasty (= Li Yü of the Southern T'ang) in *i-hai* (975); also that Qubilai's armies crossed the Yang-tzŭ in *chia-hsü* (1274), and subdued the Sung in *i-hai* (1275) and *ping-tzŭ* (1276). But the references to Chinghiz-khan are not so clear, since his birth in *ting-hai* would suppose him to be born in 1167, and the foundation of his Empire in *kêng-shên* would suppose that the Mongol Empire was founded in 1200, both dates being in apparent contradiction with those given elsewhere in Chinese as well as in Mussulman sources.

On the last two items, HUNG Čhün's remarks are to the following effect. For the foundation of the Empire in *kêng-shên* (1200), instead of the commonly accepted A. D. 1206, a sentence occurring in Chinghiz-khan's summons to the Taoist monk Ch'iu Ch'u-chi in 1219 may be adduced. The text says (*Cho-kêng lu*, 10, 3*b*): « In the course of seven years I completed the great task; within the six directions (*i. e.* the four cardinal points, plus the zenith and the nadir) all had been

unified ». This sentence, puzzling as it is, has been translated without comment by BRETSCHNEIDER (*Br*, I, 38) and by CHAVANNES (*TP*, 1908, 300). HUNG Chün thought that the « unification » was reached when the Mongol chieftain, heretofore called Tämüjin, took the title of « Chinghiz-khan » in 1206. If we count seven years back from 1206 in the Chinese fashion, viz. with both final terms included, the result is 1200, *i. e.* a *kêng-shên* year. HUNG Chün thus supposes that the Empire was « founded » in that year, which is that of the victory of Tämüjin and Ong-khan over the Taiçi'ut, though the « unification » only took place when Tämüjin proclaimed himself « Chinghiz-khan » in 1206. The inference is possible, but not binding. Astrologers and certain chroniclers used to make Chinghiz-khan's reign begin at Ong-khan's defeat and death in 1203, so that he could be said to be born, to have begun his reign and to have died in « pig » years (cf. Rašid in *Ber*, II, 147; III, 110-111). According to the Buddhist chronicle *Fo-tsu li-tai t'ung-tsai*, which was completed in 1344 (ch. 32; Nanjiō, No. 1637; Meiji *Tripit.* of Tōkyō, 致, XI, 36 a-b), Tämüjin took the title of « Chinghiz-khan » in 1206, but it was only in 1211 that he created (*chien*) for his Empire (*kuo*) the official designation of « Ta Mêng-ku », « Great Mongol » (cf. Plan Carpine's « Yeka mongal », Yäkä-Monyol, of same meaning, in *Wy*, 52). The words used are the same as in Yang Wei-chêng's *chien-kuo*, but they represent a different tradition since 1211 is a *hsin-wei*, not a *kêng-shên* year. It may be that Yang Wei-chêng gave a special and undue significance to the events of 1200, in order to make them fit into a forced parallel.

For Chinghiz-khan's birth in *ting-hai*, a year which in principle corresponds to 1167, HUNG Chün devised the following explanation. Diviners and astrologers use in their calculations only the duodenary « branches », not the denary « stems »; in other words, we ought to take into account for Chinghiz-khan only the duodenary *hai*, meaning that Chinghiz was born in a « pig » year like Sung T'ai-tsu. But it was only Sung T'ai-tsu's birth-year which actually was a *ting-hai* year; the comparison was valid, although Chinghiz-khan was born in a *i-hai* year, *i. e.* 1155. Consequently, Yang Wei-chêng, while apparently speaking of 1167, confirmed in fact the Mussulman information which gave 1155 as the birth-year of Chinghiz-khan.

This argument, proffered as a hypothesis by HUNG Chün, appeared so decisive to T'ü Chi and K'o Shao-min that both, abandoning the traditional Chinese date, which was equivalent to 1162, came to state that Chinghiz-khan was born in 1155 (T'ü Chi, 2, 1 a; *Ch*, I, 172; K'o Shao-min, *Hsin Yüan shih k'ao-chêng*, 2, 1 b; 3, 11 a; WANG Kuo-wei, *Kuan-t'ang chi-lin*, 14, 21 b, however, stuck to the date 1162). I am afraid that they have given to HUNG Chün's argument too ready an acceptance. I agree with them that Yang Wei-chêng, submitting a memorial to the Emperor, could not run counter to what was the official tradition of the time, and could not have said that Chinghiz-khan was born in a *ting-hai* year if it had then been admitted that the true date was a *jên-wu* year. On the other hand, Yang Wei-chêng, in his eagerness to find parallel dates, deviated, at least slightly, from the absolute historical truth in another case. When he says that Sung T'ai-tsu pacified Chiang-nan in *i-hai* (975) and *ping-tzū* (976), that was because the submission of the Sung to Qubilai took place in *ping-tzū* (1276); but Li Yü of the Southern T'ang had already tendered his allegiance to the Sung at the end of 975, and 976 can only be adduced inasmuch as, at the beginning of that year, Li Yü presented himself in audience at the Court of Sung T'ai-tsu and was granted a title. This is, however, merely a biassed interpretation of the facts, not an actual misstatement. The case of the

birth in *ting-hai* is quite different. In all the other cases, that of *ting-hai* for Sung T'ai-tsu as well as the double *chia-hsü*, *i-hai* and *ping-tzū* for both Sung T'ai-tsu and Chinghiz-khan, Yang Wei-chêng's parallels must be taken with the full value of both the elements which determine the cyclical position of the year. The same should hold good in the case of the birth of Chinghiz-khan, and the only significance I can see in Yang Wei-chêng's parallel is that he actually believed that Chinghiz-khan was born in the *ting-hai* year, *i. e.* in 1167.

I think, moreover, that we can trace the same tradition in other Chinese works more or less contemporary with Yang Wei-chêng's memorial. In WANG Kuo-wei's edition (64 *b*) of the *Shêng-wu ch'in-chêng lu*, it is stated that in 1226, Chinghiz-khan was sixty-five years old (counted in the Chinese fashion, *i. e.* sixty-four for us), which is in agreement with the later official date of his birth in 1162. But this is the result of a correction: all the ancient texts of the work say he was then «sixty years old» (時上年六十矣), which would imply that he was born in 1167. Of course, the correction seems at first sight justified by the fact that, just before speaking of 1204 and so, apparently, referring to 1203, the same work speaks of Chinghiz as being then «forty-two years old» (forty-one for us), which would date his birth in 1162. I shall not dwell on the discrepancy with Rašidu'd-Dîn, according to whom the events recorded in the *Shêng-wu ch'in-chêng lu* under 1203 appear in the «rat» year, which began on February 3, 1204. More stress should be laid, I believe, on the manner in which the age is indicated under 1203. That the age of the Emperor should be mentioned when he starts for his last campaign (in the course of which he died) need cause no surprise. But the situation is different when, in 1203, after relating other events which seem to have taken place in the same year, and before passing on to *chia-tzū* (1204), the texts begins the account of the attempt made by Tayang-khan of the Naiman to make the Öngüt sovereign join in a league against Chinghiz-khan with the unusual mention «when the age of the Emperor was forty-two» (上春秋四十二時). I think that, except perhaps for the last word, we might have to deal here with an interpolation (there is at least one other interpolation in the *Shêng-wu ch'in-chêng lu* as we have it now; cf. WANG Kuo-wei, 58 *b*, and my paper *Sur un passage du «Cheng-wou ts'in-tcheng lou»*, in *Ts'ai Yüan P'ei Anniversary Volume*, 937). This is the only explanation I can think of if the original text under the year 1226 really says that the Emperor was then «sixty years old». The opinion is literally confirmed by the *Li-tai fo-tsu t'ung-tsai* (ch. 32; *ibid.* 37 *a*), which, under 1226, and exactly in the same words as the original *Shêng-wu ch'in-chêng lu*, says that the Emperor was then «sixty years old» (上年六十矣). Now it is out of the question that a corrupt reading which might in the Ming period have crept into the *Shêng-wu ch'in-chêng lu* should have contaminated at a still later date the Buddhist chronicle of 1344, or that our ancient texts of the *Shêng-wu ch'in-chêng lu* should have been altered on the authority of the Buddhist chronicler. That the compiler of the *Li-tai fo-tsu t'ung-tsai* knew the *Shêng-wu ch'in-chêng lu* is established by the fact that his entry under 1206 (大會于斡難河建九游之白旗共上尊號曰成吉思皇帝) is a *verbatim* quotation from the latter work (46 *b*). The only admissible conclusion is that the chronicler of 1344 used a copy of the *Shêng-wu ch'in-chêng lu* which already attributed sixty (Chinese) years to the age of Chinghiz-khan in 1226, or in other words which implied that 1167 was the year of his birth. This was certainly the ancient tradition, of which we have yet another mention in Yang Wei-chêng's memorial.

That this tradition of the first half of the 14th cent. should be correct is, however, quite a different matter. We do not know the reasons which, in the third quarter of that century, and even before the fall of the dynasty, turned the scales in favour of the date of 1162. One point may, however, engage our attention. The early Mongols had no sexagenary cycle, but, despite the *Mêng-Ta pei-lu*, they most probably knew the duodenary animal cycle, which was popular at so early a date among the Altaïc tribes. Before the adoption of a Chinese *nien-hao* by Qubilai in 1260, the edicts of the Mongol Emperors are regularly dated by the years of the animal cycle. Right or wrong, the contemporaries of Chinghiz-khan believed, and probably he himself believed, that he was born in a « pig » year. Rašid's information equated that « pig » year to 1155, and this is corroborated to a certain extent by the passage in the *Mêng-Ta pei-lu* which gives 1154 as the year of the birth. Both sources are earlier than the ones which, c. 1330-1340, give that « pig » year as 1167. There is, in one way or the other, an error of a whole duodenary cycle, and we have no direct proof in favour of either date. Against 1167, it may be argued that Ögödäi, third son of Chinghiz-khan by the same mother, was born in 1186, so that Jöçi and Čayatai ought to have been born, at the latest, in 1184 and 1185, respectively. Chinghiz ought in such a case to have married not later than 1183, when he was sixteen. There is nothing unlikely in that fact, however, in a country where early marriages were the rule. On the other hand, if Chinghiz-khan was born in 1155, there is a long gap in the accounts of his early life, a gap which has always puzzled me. Some years ago, recalling that Chinghiz-khan had established his power over a great part of the Mongol tribes towards the end of the 12th cent., and after his victory over the Märkit, and still starting from the two traditional dates of 1155 or 1162 for his birth, I remarked (*La Haute Asie*, 27) : « Since Chinghiz-khan was then either about to attain or had passed the age of forty, one sees at once to what an extent our information on that part of his life is fragmentary. The Mongols used to marry early, and the campaign against the Märkit took place before the birth of Börtä's eldest son. A long time must have elapsed between this campaign and the proclamation of Tämüjin as khan, twenty years perhaps, of which we know nothing. » The adoption of 1167 as the date of the birth of Chinghiz-khan would not overcome the difficulty, since the date of Ögödäi's birth seems to be firmly established as 1186, and so the campaign against the Märkit, anterior to Jöçi's birth, could not have taken place later than 1184. But the margin of difference is diminished. If Chinghiz was born in 1155, the date of the campaign against the Märkit, anterior to the birth of his eldest son, ought to be put back by about ten years more. Such an assumption, however, could not be reconciled with the statements which make Jöçi no more than forty when he died early in 1227 (cf. BARTHOLD, *Turkestan*<sup>2</sup>, 458). I am far from being positive about the date 1167, because of the earlier texts which speak of 1154-1155, but 1167 is perhaps more consistent with Chinghiz-khan's later life, and for that reason commends itself to the attention and the criticism of future historians.

In the manuscript of Ulän-Bātor (the Uрга of our maps) which has preserved, admits much additional matter, part of the original Mongol text of the *Secret History*, a precise date is given for the birth of Chinghiz-khan, « on the sixteenth of the first month of spring of the 'black horse' year, which was the day of the red full moon, at noon » ; in the Chinese calendar this would correspond to February 1, 1162. The day chosen has a mythical significance as in the case of other

exceptional events; there can be no doubt that the original *Secret History* had no date, and that the whole sentence is a late interpolation.

CHARIGNON says (*Ch*, I, 171) that RAMUSIO gives 1162 as the date of the birth of Chinghiz-khan, but no text of Polo, in RAMUSIO or elsewhere, says anything about his birth. The date of « 1162 » in RAMUSIO is that of Chinghiz-khan's accession to the sovereignty, corresponding to « 1187 » in most mss, « 1287 » in a few others, and « 1172 » in V (« 1172 » for R in Vol. I, 162, n. 3, is a slip). I shall speak of this further on.

THE CLOT OF BLOOD. — According to the *Secret History* (§ 59), Chinghiz-khan was born with a clot of blood (*nödün* > modern Mong. *nöjin*, *nöji*, Kalm. *nödžig*) in the shape of a knuckle-bone (*ši'a*, *ši'ai*, class. Mong. *šiyai*, Kalm. *šayā*) in his right hand; this clot of blood is also mentioned in *Shêng-wu ch'in-chêng lu*, I b, in *YS*, I, 2 a, and in Rašidu-'d-Dîn (*Ber*, II, 87). Buddhist works of the beginning of our era like the *Aśokāvadāna* (cf. PRZYLUKI, *La légende de l'Empereur Açoka*, 400-401) and the *Tsa a-han ching* (ch. 25; Meiji *Tripit.* of Tōkyō, 辰, III, 45 a) have a prophecy about the cruel kings who will destroy the Buddhist Law. The fourth one they say will be a king of Kauśambī, called Mahāsenā. Mahāsenā will have a son who will be born clad in armour, and holding blood in his hand. Five hundred chiefs of families will at that same moment have five hundred sons, who will all be born clad in armour and holding blood in their hands. On that day, a great rain of blood will fall. The diviners will foretell that Mahāsenā's son will reign over the whole world, but that, unfortunately, his victims will be many. The clot of blood in the hand of Chinghiz-khan is interpreted in almost the same terms on the birth of the child in Abū-'l-Ghāzī (cf. DESMAISONS, *Hist. des Mogols*, II, 73). The coincidence is the more striking when we remember that Chinghiz-khan, by the very title he assumed, pretended to be a universal monarch and that later texts of the Mongol period unreservedly call him by the corresponding Indian title of *cakravartin*. Yet, there is no trace of Lamaism, or of Buddhism in general, in the *Secret History*, entirely pervaded as it is with the shamanistic spirit. The probable explanation is that the detail of the clot of blood of the early Buddhist texts was or became an element of folklore which spread among the shamanistic tribes of Upper Asia, and of which we ought to be able to discover other traces.

ERDMANN (*Temudschin*, 255), citing DUBEUX, *La Perse*, 230, says that, in the Iranian tradition, Rustam, like Chinghiz-khan, was born with blood in his hand. The case is not so simple, and I had to turn to my colleague E. BENVENISTE to get a more precise explanation of the facts. The passage relating to Rustam's miraculous birth varies in the different redactions of the *Šāh-nāmāh*. In the editions due to MOHL and to VULLERS, nothing is said of blood in Rustam's hands. But, in the so-called Calcutta text (MACAN ed., I, 163, vv. 11-12), we read: « The hair of his head was all red, and his face like blood. Like the bright sun, he made his appearance. He was born from his mother with both hands full of blood (*dō dast-aš pur az hūn zə-mādar bazād*). Nobody had ever seen such a child. » This mention of the hands full of blood does not occur in the traditional accounts of Rustam, nor has it been commented upon by modern interpreters. It is not easy to decide whether it belongs to Firdausi's original text, or is an interpolation prompted by the description given in the preceding lines. But, even if we admit that it is an interpolation,

the idea of blood in the hands of the new-born hero seems to me to be hardly separable from the parallel cases of Mahāsena's son and of Chinghiz-khan.

THE NAME TÄMÜJIN. — Chinghiz-khan's real name, that is to say, the one he received at the time of his birth, was Tämüjin (鐵木真 T'ieh-mu-chên in most sources; 忒沒真 T'ê-mu-chên in the *Mêng-Ta pei-lu*, 2 a, and the *Hei-Ta shih-liao*, 1 a). The early Mongol tradition was that he was given this name because his father had then just defeated a Tatar chief called Tämüjin; this is in agreement with the Mongol habit of giving to new-born children a name connected either with a fresh event or with the first object or person which attracted the mother's attention after the child was born. The name of the Tatar chief is simply 鐵木真 T'ieh-mu-chên in *YS*, 1, 2 a; but it is more fully given as Tämüjin-ügä in the *Secret History* (§ 59), Tämüjin-ökä in the *Shêng-wu ch'in-chêng lu* (WANG Kuo-wei ed., 1 b); Mongolian writing does not distinguish between *ügä* and *ökä*, and the alternation merely shows that the transcribers of one of our sources, and perhaps of both, had no tradition as to the proper pronunciation to guide him. The same uncertainty prevails with Rašidu'd-Dîn's *نورجین اوکه* Tämüjin-ügä (or-ökä; *Ber*, II, 64, 86, 87). *Ökä* could be connected with the word which occurs in the adjectival form *Ögädäi* or *Ökädäi*; *ügä* reminds one of an ancient title — and perhaps two — in the inscriptions and historical texts of the ancient Turks and Tatars. I am not prepared to choose between these two forms.

Opinions have been at variance on the name «Tämüjin». Rubrouck gives «Demugin» (var. «Temigu»; read «Temugin»; *Wy*, 307), and explains it as «sonitus ferri»; he adds the explanation: «Ipsi vocant Chingis sonitum ferri, quia faber fuit». This statement roused SCHMIDT's indignation, who reviled Rubrouck for having dared to say that Chinghiz-khan had been a blacksmith (*Gesch. der Ost-Mongolen*, 376). According to SCHMIDT, «Tämüjin» has no known meaning in Mongolian, and was mistaken by RUBROUCK's interpreters for Mong. *tämürči* or *tämürčin*, «blacksmith». In the first edition of his *Histoire des Mongols*, D'OHSSON had said that «Tämüjin» meant «warrior», which was rightly denied by SCHMIDT. The second edition gives a different explanation (*Oh*, I, 36), because D'OHSSON had in the meantime become acquainted with BIČURIN's *Istoriya pervykh četyrëkh khanov iz doma Čingisova*, where «Tämüjin», on the authority of the Ch'ien-lung Commissioners, is explained as meaning «best iron». D'OHSSON explained that the various texts which spoke of Chinghiz-khan as of a former blacksmith had confused «*tämüjin*», «best iron», and «*tämürči*», «blacksmith». This conclusion of D'OHSSON has been slavishly repeated by BROSSET (*Hist. de la Géorgie*, 488), ROCKHILL (*Rubruck*, 249) and VAN DEN WYNGAERT (*Wy*, 307). It is, however, valueless. The interpretation «best iron» of the Ch'ien-lung Commissioners (*Yüan shih yü-chieh*, 1, 2 a) is a desperate attempt to explain «Tämüjin» from the fanciful meaning of an imaginary word.

Rašidu'd-Dîn, whose information is here, as in many other cases, directly derived from Mongol sources, gives the name in the correct Mongol form *نورجین* Tämüjin (*Ber*, I, 177; II, 54, etc.). Such is also the form found in subsequent writers who depended on Rāšid. But the case is different with the earlier Mussulman sources. An-Nāsāwī says that Chinghiz-khan was «of the tribe of the *نورجی* Tämürji» (or Tämürči; HOUDAS, *Hist. du Sultan Djelal ed-Din*, 8). In the *Tabaqāt-i Nāsiri*, *نورجی* Tämürči becomes Chinghiz-khan's father (RAVERTY, 935). The editor of

Juwainī (I, 26, 28, 253) has adopted تَمْرُجِين Tāmūrčīn, while granting the possibility of تَمْرُجِين Tāmūrčīn. Of course, a confusion between *č* and *ç* is easy in Arabic script; I think, however, that Tāmūrčīn is really the form originally adopted by Juwainī. My opinion rests to some extent on the fact that Juwainī generally gives the Turkish forms of Mongols names, like An-Nāsāwī and the *Ṭabaqāt-i Nāsiri*, and the latter two works give Tāmūrčī or Tāmūrčī, not \*Tāmūrčī. But there is more. Bar Hebraeus's information regarding the Mongols is copied wholesale from Juwainī. His Arabic chronicle would seem to favour «Tāmūrčīn», since such is the form found in POCOCKE's edition (*Historia Dynastiarum*, text, 427, 428; transl., 280, 281), though this may be due to the easy misreading of *ç*, *ū* for *ç*, *r* in Arabic. There is no such ambiguity in Syriac script, however, and Bar Hebraeus, in his Syriac chronicle, writes the name as Tāmūršīn (*i.e.* Tāmūrčīn; BRUNS ed., text, 438; transl., 449). That such was the form which reached the West is confirmed by Georgian texts, where the name occurs as «Themurčī» (BROSSET, *Hist. de la Géorgie*, 488).

But *tāmūrčī* in Turkish, *tāmūrčī* or *tāmūrčīn* in Mongolian is the *nomen agentis* derived from *tāmūr*, «iron», and means «blacksmith» (RAVERTY is mistaken, p. 936, when he says that *tāmūrčī* means «iron-like», and not «blacksmith»). So we need not be surprised when Rubrouck says that Chinghiz-khan had been a smith. The same tradition is found in Hethum (*Hist. des Croisades*, Arm., II, 148, 284), in Nuwairī (cf. *Oh*, I, 36; I cannot trace the original passage), in Pachymeres (Bonn ed., I, 345-346), and in Ibn-Baṭṭūṭah (III, 22). From TIMKOVSKIĪ's travels, it would even seem that some similar belief still lingered in Eastern Mongolia at the beginning of the last century (cf. TIMKOVSKI, *Voyage à Péking*, I, 179; SCHMIDT's critic, in *Gesch. der Ost-Mongolen*, 376, is prejudiced).

But if the Mongols themselves had a tradition, however unfounded, that Chinghiz-khan had been a smith, that would imply that the Turkish form, Tāmūrčī, was not an erroneous equivalence and that the same meaning attached to «Tāmūrčīn» among the Mongols themselves. This I believe to have been the case. The modern Mongol word for «smith» is *tāmūrčī*, but I know of no example of the word in mediaeval texts. There is, however, a case quite similar and perfectly clear. The modern Mongol word for a cowherd is *ükärčī*, the *nomen agentis* of *ükär*, «ox». But the mediaeval form, attested in Mongol texts, and, as a proper name, in Chinese and Persian texts as well as in Polo, is *hügäčī*, *hükäčīn*, with the fall of the final *-r* of *hükär* > *ükär* (see «Cogacin»). I even suspect that the final *-r* was dropped with other suffixes than *-čī* (*-ji*, *-čīn*, *-jin*). The name of Chinghiz-khan's sister «Tämülün» (*Secret History*, §§ 60, 79, 99) seems to have been formed with *tämür*, plus the feminine suffix *-lün*; and «Tämüdär» (*ibid.* § 278) probably represents *tämür*, plus the suffix *-dar*, *-där*.

From HAENISCH in *TP*, 1913, 21, 113, it would seem that Tämūrčīn survived as a proper name until the 17th cent. But the name restored by HAENISCH as «Bügen Temüčīn» is uncertain; there are other possible explanations (\*Bugente Mušan, etc.).

According to the *Secret History* (§§ 60, 61, 68), Chinghiz-khan was nine years old when his father Yäsügäi was poisoned by the Tatars; but he was thirteen according to Rašidu-'d-Dīn (*Ber*, II, 89, 90). All sorts of dates have been proffered for Yäsügäi's death by adding either nine or thirteen to the different dates given for his son's birth: they range from 1175 in WOLFF (p. 34)

and HOWORTH (I, 48) to c. 1165 in VLADIMIROV (p. 23). One point is clear : the years of the son's age are counted in Chinese fashion, that is to say, we have to add to the year of his birth not nine or thirteen, but eight or twelve, respectively. This is almost self-evident, but it is established beyond doubt by Rašid (*Ber*, II, 89; III, 102-103, 111), who, dating Chinghiz-khan's birth in a « pig » year (1155), makes him an orphan at the age of « thirteen » also in a « pig » year, consequently twelve years later (1167). Too many complete cycles divide the main periods of Chinghiz-khan's life. The *Secret History*, our earliest source, is free from such combinations and therefore the age of « nine » years (eight actual years) which it gives is more likely to be true. If we add eight to 1155, Yäsügäi must have died in 1163. But such an early date is difficult to reconcile with the subsequent history of Chinghiz-khan. I am tempted rather to start from 1167 for Chinghiz-khan's birth, and to suppose that Yäsügäi died in 1175. It is quite by accident that this date falls in with the one suggested by WOLFF, who started from entirely different premises, which are, moreover, warped with a miscalculation.

THE TITLE OF ČA'UT-QURI. — At a date which was in a « tiger » year (1194) according to Raši (*Ber*, II, 104; BARTHOLD, in *EI*, s. v. « Čingiz-khān »), but which is more likely to have been 1196 (cf. NAKA Michiyo, *Chingisu-kan jitsuroku*, 129; WANG Kuo-wei, *Kuan-t'ang chi-lin*, 14, 23 a; T'u Chi, 11, 14 a), Tämüjin joined with the sovereign of the Kerait, To'oril (< Turk. Toyru), in an action against the Tatars together with the armies of the Chin. As a reward, the Chin general gave to To'oril the Chinese title of 王 *wang* (> Turk. and Mong. Ong), from which To'oril was henceforward known as « Ong-khan » (see « Uncan »), and to Tämüjin a title which has often been misread and, I think, misinterpreted. The problem of this title is fairly intricate, and must be studied in some detail.

GAUBIL (*Hist. de Gentschiscan*, 4) merely states that Tämüjin received « a high office in the army ». BIČURIN (*Istoriya pervykh četerëkh khanov*, 39), translating from the Supplement to the *T'ung-chien kang-mu*, says the title given to Tämüjin was « Ča-u-tu-lu », meaning « commander-in-chief against rebels ». The *Yüan-shih lei-pien* (I, 1 b-2 a), quoting from the *Shêng-wu ch'in-chêng lu*, gives likewise 察兀秃魯 *Ch'a-wu-t'u-lu*, adding that it is the equivalent of 招討使 *chao-chi-shih*. In PI Yüan's *Hsü tzü-chih t'ung-chien* (chap. 156, s. a. 1202), the title is given as having been originally written 察兀圖魯 *ch'a-wu-t'u-lu*, changed by the Ch'ien-lung Commissioners to 察袞圖魯 *ch'a-kun-t'u-lu*, with the Chinese equivalent 招討使 *chao-t'ao-shih*. This points to a text of the *Shêng-wu ch'in-chêng lu* which had *ch'a-wu-t'u-lu*, although all our manuscripts of the latter work only give 察兀忽魯 *ch'a-wu-hu-lu*, with a note saying that it was a title equivalent to the 移計使 *i-chi-shih* of the Chin (WANG Kuo-wei ed., 13 a; but the ms. of the ancient *Shuo fu*, ch. 55, published by the Commercial Press, which WANG Kuo-wei did not know, gives as the Chinese equivalent *chao-t'ao-shih* [with a wrong reading 今 *chin* instead of 金 *Chin*]; the Fu Tsêng-hsiang ms., which was also not known to WANG, has 移計金使 *i-chi Chin shih*, which is absurd; HUNG Čhüing, I A, 28, and NAKA, *Chingisu-kan jitsuroku*, 132, say that *ch'a-wu-hu-lu* occurs in YS, but I cannot find it there. I am afraid that it was a slip of one author repeated by another). The curious fact is that, contrary to what will soon be seen to be the reading of the *Secret History* itself, the Ulän-Bātor ms., in the passage



corresponding to § 134 of the *Secret History*, gives *čay-un törö* (or *ötürü*), which is very close to the reading *ch'a-wu-t'u-lu* and still more to the *ch'a-kun-t'u-lu* of the Ch'ien-lung Commissioners.

The Chinese abridged version of the *Secret History*, the only one which PALLADIUS knew when he published his translation, gives (§ 134) 札兀忽里 *cha-wu-hu-li*, which would seem to render *ja'uquri*. PALLADIUS (*Trudy členov...*, IV, 66, 194) read it as «*jauhuri*», which is still retained by VLADIMIROV (*Čingis-khan*, 50). There can be no doubt, however, that this restitution is not correct. The complete text of the *Secret History*, in a phonetic transcription of the Mongolian original, gives once (§ 134) *cha-wu-t'i*[惕]-*hu-li*, and once (§ 179) *ch'a*[察]-*wu-t'i-hu-li* (the *t'i* being both times written smaller and a little to the right of the column), which would suppose *\*ja'utquri* and *\*ča'utquri*, respectively.

That the *-t-* is original is proved by Rašidu-'d-Din, where the title occurs twice, as in the *Secret History* (*Ber*, II, 104, 140). It has been misread «*Odjaout-kodi*», «*Djaout-kodi*», «*Tschavat-kodi*» (cf. ERDMANN, *Temudschin*, 585), «*Dschaukturi*» (WOLFF, *Gesch. der Mongolen*, 39), «*Dschaut-ikuri*» (ERDMANN, *ibid.*, 267), «*Jaut-ikuri*» (HOWORTH, I, 54). The correct form had, however, been given long ago by D'OHSSON (*Oh*, I, 47) and HAMMER (*Ha*<sup>3</sup>, 61), and is confirmed by BEREZIN's mss : it is جارت قوری *Jāūt-qūrī* (or *Čāūt-qūrī*), which is identical with the transcriptions of the complete *Secret History*.

An occlusive consonant at the end of a syllable, transcribed with a smaller character, is frequently omitted by less accurate copyists, as is the case in the *ch'a-wu-hu-li* of the abridged version of the *Secret History*. On the other hand, *-i* and *-u*, at the end of a word, are easily confused in Mongolian writing. There would thus be nothing surprising in finding *ch'a-wu-hu-lu* for *\*ča'utquri* in the *Shêng-wu ch'in-chêng lu* if we did not have the puzzling *ch'a-wu-t'u-lu* of the Supplement to the *Kang-mu* and of the *Yüan-shih lei-pien*, going back to one or several lost mss. of the *Shêng-wu ch'in-chêng lu*, and the *čay-un törö* (or *ötürü*) of the Ulân-Bātor ms. based on the original Mongol text of the *Secret History*. The form of the very faulty Ulân-Bātor ms. seems to be the result of an absurd interpretation «*rule of the moment*». That the *-törö* of this ms. should be the outcome of [*ča'u*]*t-quri*, the *-t* being mistaken for the first letter of the second element of the title, the *-q-* omitted and the final *-i* misread as *-u* (*-ü*, *-o*, *-ö*) would be possible in itself, and it may also be possible that the final *-i* was miswritten or misread *-u* in the Mongolian manuscript from which the *Shêng-wu ch'in-chêng lu* was translated. Yet a difficulty remains, since we have also the *ch'a-wu-t'u-lu* derived from one or several lost mss. of the *Shêng-wu ch'in-chêng lu* and because, as will be seen further on, a form *\*-quru* may be etymologically as correct as *quri*. *Ch'a-wu-t'u-lu* cannot be due to a misreading of a manuscript in Mongolian, since the known mss. have, correctly, *ch'a-wu-hu-lu* and since there is no trace of two independent translations of the work from the Mongolian. On the other hand, a mere coincidence which would make a corrupt reading of Mongolian origin in one work fall in with a corrupt reading of Chinese origin in another is hardly acceptable. I have no satisfactory solution to proffer. As to the hesitation between *j-* and *č-*, which is reflected in the different transcriptions of the *Secret History* in §§ 134 and 179, it must be remembered that Mongolian manuscripts rarely distinguish the two letters, and that the translators and transcribers of the 14th cent. had no longer a living tradition to guide them as to the pronun-

ciation of the title. The Persian gives no help, since Rašidu-'d-Dīn uses چ J with both the values of č and j.

The meaning of *ja'ut-quri* or *ča'ut-quri* is not clear either. There can be no serious doubt that the equivalence intended in the note of the *Shêng-wu ch'in-chêng lu* is 招討使 *chao-t'ao-shih* (corrupt in the *Yuän-shih lei-pien*, and in all the mss. of the *Shêng-wu ch'in-chêng lu* except the one included in the ancient *Shuo fu* re-edited by the Commercial Press). It is the *chao-t'ao-shih* whom BICURIN had in view when he spoke of a « commander-in-chief against rebels ». Under the Chin dynasty, the *chao-t'ao-shih* were high officials of the first degree of the third rank; there were three, one for the north-east, one for the north-west, one for the south-west; their task was to « bring » (*chao*) and cherish those who submitted to the dynasty and to punish (*t'ao*) and seize those who rebelled against it (*Chin shih*, 57, 10 a). Yet, it seems certain that the note of the *Shêng-wu ch'in-chêng lu* is a mistaken one, added by translators who no longer knew what *ja'ut-quri* meant, and were perhaps guided by the phonetic resemblance in the first syllable of both titles (if it were not for the *čay-un törö* of the Ulān-Bātor ms., one might even think that the corrupt *ch'a-wu-t'u-lu* was due to the influence of *t'ao* in *chao-t'ao-shih*). But in the *Secret History* (§ 134), the Chin general, after granting to Tämüjin, in the name of the Chin Emperor, the title of *ja'ut-quri*, adds that the Emperor may himself promote him later to the higher rank of *jao-tao* (= *chao-t'ao[-shih]*). So there can be no doubt that the *ja'ut-quri* was different from the *chao-t'ao-shih*, and ranked below him (for a mention in Rašid of a *chao-t'ao-shih* whose title was misread in *Ber*, III, 17, cf. my paper in *The Ts'ai Yüan P'ei Anniversary Volume*, 934).

Rašidu-'d-Dīn does not throw much light on the point. In BEREZIN'S translation (*Ber*, II, 104), he explains *ja'ut-quri* as meaning « powerful prince » in « Chinese » (*bä zibān-i Hittäyī*), the Persian words actually used being (*Ber*, II, Pers. text, 169) امیر معظم *amīr-i mu'azzam* « great emir » (the امیر بزرگ *amīr-i buzurg* of *Temudschin*, 585, seems to be an arbitrary invention of ERDMANN). By « Chinese », we must here understand the Jučen language of the Chin ruling over North China. But Rašid wrote at a time when the real meaning of *ja'ut-quri* was forgotten, and, as a minister of a Chinghiz-khanid dynasty, he was prone to magnify the value of the title granted to its great ancestor.

Another solution has been proposed by modern Japanese and Chinese scholars. NAKA (*Chingisu-kan jitsuroku*, 132) says that *ja'ut* is the plural of *ja'un*, « hundred », and that *quri* is the Mongol root meaning « to assemble », « to gather together »; the whole term would thus mean « chief of a hundred families ». This explanation is clearly impossible for the second part, since a noun cannot be formed of a bare verbal root. T'u Chi (2, 13-14), while also explaining *ja'ut* as « hundred », adduced a passage of the *Chin shih* (55, 1 a), according to which 忽魯 *hu-lu* was the title of a chief placed at the head of several clans. His conclusion, identical with that of NAKA, was that *ja'ut-quri*, or *ch'a-wu-hu-lu* as in the *Shêng-wu ch'in-chêng lu*, meant « chief of a hundred men » (百夫長 *pai-fu chang*). This Chinese equivalent of *ch'a-wu-hu-lu* is also given, without comment, by WANG Kuo-wei (13 a).

It is true that the plural *ja'ut* of *ja'un*, now obsolete, occurs nearly twenty times in the *Secret History*, whenever it speaks of several hundred (this plural is also used as a tribal name; the

«Saghad» of SCHMIDT, 189, 191, 195, are Ja'ut). But *hu-lu* is more perplexing. The same text of the *Chin shih* from which T'U Chi cited a sentence says that *hu-lu* meant 總師 *tsung-shuai*, «commander-in-chief» (the word is not recorded in our Jučen vocabularies, where *hu-lu*, with the same Chinese characters, only renders a word meaning «a ring», «a [finger-]ring»; cf. GRUBE, *Die Sprache und Schrift der Jučen*, No. 548). But the text adds that all the titles mentioned in the passage adduced, including those with *hu-lu*, were suppressed before 1149. Another difficulty is that *ja'ut*, «hundreds», which is Mongolian, would be combined, according to T'U Chi's hypothesis, with a second element which would be Jučen. To make matters still more intricate, the *Chin shih* adds that, after the suppression of the other Jučen titles, one was retained for the officials who were in charge of the people at the frontier, and that was 禿里 *t'u-li* (on which cf. also *Chin shih*, 57, 10 b), which bears a certain resemblance to the *t'u-lu* of *ch'a-wu-t'u-lu* and *čay-un törö*.

Yet I think that T'U Chi was fundamentally right. To explain the juxtaposition of a Mongol *ja'ut* and a Jučen *hu-lu*, we have only to suppose that the title *hu-lu* was inherited by the Chin from the Liao who were Mongols; and the *Chin shih* says in so many words that the Chin, in those titles, followed the Liao. The suppression of most of them before 1149 may not have been strictly enforced. The real Jučen title for a «chief of a hundred» (百戶 *pai-hu*) was quite different, to wit 謀克 *mou-k'o* (*Chin shih*, 57, 10 a) or 毛克 *mao-k'o* (WANG Kuo-wei's edition of the *Mêng-Ta pei-lu*, 10 a), \**mükä* or \**mökä*. The alternation of *hu-lu*, \**quru*, and *hu-li*, *quri*, is not without other examples. At the time of Chinghiz-khan's birth, the texts mention, alongside of the Tatar chief Tämüjin-ügä, another chief of the same tribe whom the *Secret History* (§ 59) calls Qori-buqa, but whose name is given as Quru-buqa in both the *Shêng-wu ch'in-chêng lu* (1 b) and Rašidu-'d-Dîn (*Ber*, II, 86). In another case, Chinghiz-khan's relative Taiču is called Taiču-kiru (probably a misreading of \**Taiču-quru*) in the *Shêng-wu ch'in-chêng lu* (33 a), but Taiču-quri in the corresponding passage of Rašidu-'d-Dîn (*Ber*, II, 136); here, *quri* (or \**quru*) is clearly an epithet, probably a title. The Qulbari-quri of the *Secret History*, § 177, simply called Qulbari in § 152, provides another instance similar to that of Taiču and Taiču-quri.

This title *quri* can also be traced, I think, in the name of the prince of the Öngüt (see «Unc») who took sides with Chinghiz-khan. This name is given as Alaquš-tigin-quri in Rašidu-'d-Dîn (*Ber*, I, 115), Alaquš-digit-quri in the *Secret History* (§§ 182, 190, 202), Alaqus-tiki-qori in the *Shêng-wu ch'in-chêng lu* (43 a), Alaqus in YS (I, 5 b), Ala'us-tigi-quri in Prince Georges's funerary tablet composed by Yen Fu c. 1305 (*Mo*, 235, and Chinese text opposite p. 236); hence the same form in the 柏林寺 Po-lin-ssü inscription of 1355 and in YS, 118, 4 b («Alaquš-tägin-quli» in TP, 1914, 631, is a *lapsus* of mine, responsible for the same restoration in *Mo*, 235). Rašid says (*Ber*, I, 115) that Alaquš is the name (*nām*), and that Tigin-quri is an epithet or agnomen (*laqab*); on the other hand he seldom uses Alaquš alone (once in *Ber*, I, 116), and nearly always speaks of Alaquš-tigin (*Ber*, I, 109, 115, 116; II, 2; III, 111, 137). There can be no doubt that the name itself is Turkish; Turk. *ala-quš*, «motley bird», is a name of the magpie (Alaquš's grandsons Kün-buqa and Ai-buqa bore Turkish names too; the «Algus» of SÆKI, *Tōhō gakuhō* of Tōkyō, No. 9 [1939], 82, is not receivable). The second element seems to be *tegin* > *tigin*, plur. *tigit*, the old princely title which the Turk of the Orkhon probably inherited in the 6th cent. from their Avar predecessors. It is a Turkish form too, since *ti* or *di* had already become in mediaeval Mongolian *či* and *ji* respectively

(cf. Turk. *ot-tegin*, اوتكين Otägin = Ot-tägin in Jüwaini, > Mong. *ot-čigin*). The alternations in the transcriptions are due to the fact that there was no longer a tradition as to the pronunciation, and *-q-* or *-ʔ-*, *š* or *s*, *t* or *d*, *k* or *g*, *o* or *u* are not distinguished in Mongolian script. The *Secret History* suggests that *-tiki-* and *-tigi-* may stand for *-tiki[t]-* and *-tigi[t]-*, but a fall of the final *-n* of *tigin* is also possible in Mongolian manuscripts. Whether we should take the whole name to stand for Alaquš, who was known as Alaquš-tigin, «Prince Alaquš» (in Turkish), and had the additional title of *quri*, or as Alaquš, who was a *tigit-quri*, «chief of princes» (there is also a plural in *ja'ut-quri*), the fact remains that *quri* was a title, which I hold to be identical with the *quri* of *ja'ut-quri*. In *La Haute Asie* (p. 27), I have accepted the explanation of *ja'ut-quri* as «centurion», «head of a century», and I still believe that it has a fair chance of being etymologically correct (a connection of *ja'ut* with the mysterious 𐰉 troops of the Liao and the Chin, if we read the character as \**chao*, though not impossible, is improbable; on these troops, cf. *TP*, 1929, 128-129; *Chin shih*, 57, 10 b). But it may be that, taking the term at its face value, I have underrated what it represented. Of course, the titles given to chiefs of non-Jučen tribes remained much inferior to the real Chinese titles of the Jučen officials; not only a *ja'ut-quri*, but even a *wang*, «king», like Ong-khan, ranked low in the Liao and Jučen hierarchy. We must take into account, however, that we find *quri* as the title of the chief of the Öngüt, who were a numerous tribe, long in the service of the Chin, and of great importance to them because they guarded the frontier. Though all *quri* need not have been of the same importance, Tämüjin, «chief of hundreds [? not only 'of a century']» may, after all, have already been more than an insignificant local leader in 1196.

THE ENTHRONEMENT OF 1206. — Polo says that Chinghiz-khan was chosen king of the «Tartars» in 1187. RAMUSIO alone gives «1162» instead, and PENZER (*Pe*, 188) says that the latter date «agrees with the Chinese annals»; but the Chinese annals give 1162 as the date of Chinghiz-khan's birth, not of his election as king. Most of Polo's dates are unreliable; one may doubt, moreover, Polo's ability to equate a more or less remote Chinese, Uighur, or Persian date to a precise year of the Western calendar. In the present case, Polo has very naturally been supposed to refer to Chinghiz-khan's final proclamation as overlord of Mongolia and potential universal monarch, which must have taken place in 1206. But the *Secret History* suggests an earlier proclamation as king of the Mongols. «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 71), the Sum-pa Үтүхтү (cf. VASIL'EV, in *ZVOIRAO*, IV, 375) and 'JIGS-MED NAM-MKHA (cf. HIRTH, *Gesch. des Buddhismus*, II, 15) agree in saying that in 1189 Chinghiz-khan was hailed as king of the Mongols («Sog» or «Bete»). According to T'U CHI (3, 1 a), the same date is given in the *Secret History*. As a matter of fact, the *Secret History* (§ 141) merely speaks of a «hen» year, which would agree indeed with 1189, but with 1201 as well, and the latter date is much more probable (NAKA, *Chingisu-kan jitsuroku*, 143, adopts 1201). The texts which give 1189 have confused two duodenary cycles. The *Li-tai fo-tsu t'ung-t'sai* (致, XI, 30 a) makes Chinghiz begin his campaigns in 1191, we do not know on what authority. I leave aside the texts of «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongalen*, 83-85) and of the Sum-pa Үтүхтү (VASIL'EV, *ibid.* 376) according to which Chinghiz-khan became «Emperor of China» in 1194: both texts seem to refer to the final

enthronement in 1206, and the date of 1194 seems also to be due to an error of a complete duodecary cycle.

The final enthronement calls for one more remark. We always date it in 1206, because the year *ping-yin*, a «tiger» year, roughly corresponds to 1206, but it runs into 1207. The *Cho-kêng lu*, however, adds (I, 11 a) that it was in the 12th month, which, in the Chinese calendar, corresponds to December 31, 1206–January 29, 1207. The enthronement ought thus to be dated in January 1207. But the statement of the *Cho-kêng lu*, although accepted by T'u Chi (3, 1 a), is not above suspicion. In 1206, the Mongols did not follow the Chinese calendar, nor probably even the Uighur calendar, which is slightly divergent from the Chinese; this is the first reason of uncertainty. Another is that the *Cho-kêng lu*, a very sound source for the later period of the Mongol dynasty, has for Chinghiz-khan's birth the probably wrong date of 1162, and may depend here too upon untrustworthy information. If we add that climatic reasons did not favour the holding of important gatherings in Mongolia during the winter, and that the later important diets for which we have precise dates actually took place in other seasons of the year, it seems an almost necessary conclusion that the «twelfth month» of the *Cho-kên lu* cannot be retained. According to Rašid (*Ber*, III, 8), the diet of 1206 was convened in the beginning of spring; but this is not binding either.

THE TITLE «CHINGHIZ-KHAN». — Whether adopted at the end of the 12th cent., or in 1203, or only in 1206, the title «Činggis khan» has been explained in many different ways. One point seems very plausible. If Chinghiz-khan chose a new title instead of appropriating the old Mongol title of *gür-khan*, «universal khan», already used by the Qara-Ĥitai and the Kerait (see «Catai»), it must have been because an opposing Mongol diet had lessened its value in his eyes by granting it to his rival Jumuqa (cf. *Ber*, III, 110, 112; *Oh*, I, 98-99). The Ch'ien-lung Commissioners did not attempt an etymology of «Činggis» (*Yüan shih yü-chieh*, I, 2 b). GAUBIL (*Hist. de Gentchiscan*, 12) considered that «Činggis» had no meaning in Mongolian, and SCHMIDT (*Gesch. der Ost-Mongolen*, 379) repeats this and BANZAROV agrees with him (*Čěrnaya véra*, 77).

Chao Hung, our earliest source, says: «Some say that Ch'êng-chi-ssü (Činggis) renders the two characters 天賜 *t'ien-tz'ü*» (*Mêng-Ta pei-lu*, 3 a). In VASIL'EV's translation (*Trudy VOIRAO*, IV, 218), this is translated: «Some say that Čingis is a corruption of the double term *t'ien-tz'ü*». Though not literal, VASIL'EV's interpretation may be correct; what I have translated as «renders» is 譯語 *i-yü*, which may mean both a translation or a phonetic transcription. It is more probable, however, that Chao Hung thought «Ch'êng-chi-ssü» «translated» (and not «transcribed») *t'ien-tz'ü* (this text was already known to VISDELOU; cf. his *Supplement* to D'HERBELOT, 150). At any rate, it is disconcerting to find that, to the last, VASIL'EV should have stuck (cf. *ZVOIRAO*, IV, 379) to Chao Hung's etymology, which is clearly fantastic. *T'ien-tz'ü* means «granted by Heaven», and, according to Chao Hung (12 b), the term occurred at the beginning of the Mongol tablets of authority (*p'ai-tz'ü*), before the name of «the Emperor Ch'êng-chi-ssü». It is the translation of *tängri-yin öggüksän*, «given by Heaven», which is still prefixed to the name of Tämüjin in «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 63). Chao Hung's information about the tablets has been confirmed by the recent discovery, in Jehol province, of a gold tablet bearing the very formula given in the *Mêng-Ta pei-lu* (cf. HANEDA, in *Memoirs... of the Toyo Bunko*, No. 8, 85-91). It

is, moreover, the mediaeval expression of a belief which goes back to the early Hsiung-nu. So «Činggis» has nothing to do with *t'ien-tz'ü*. The same may be said of VASIL'EV's alternative solution (*ibid.* iv, 379) that «Činggis» may be a transcription of *t'ien-tz'ü*, «Son of Heaven»; the mediaeval transcriptions from Mongolian or into Mongolian are much too accurate to admit of such an unaccountable corruption. I shall not stop to discuss the «Chingsze, *i. e.* perfect warrior» of DOUGLAS, *The life of Jenghis Khan*, 54.

Rašid says three times (*Ber*, I, 159; III, 8, 112) that «Čingiz» is the plural of *čing*, which in Mongolian means «firm» (مستحکم), or «powerful and strong» (قوی و سخت), so that the name is tantamount to «king of kings» (*pādšāh-i pādšāhān*) or «emperor of emperors» (*šahinšāh*). This plural was, according to Rašid (*Ber*, I, 159) a plural *majestatis* applied to Chinghiz-khan himself; D'OHSSON's «khan of the powerful ones» (*Oh*, I, 99) is not in accordance with the text. The tradition passed on to Abū'l-Ghāzī (DESMAISONS, text, 81; transl., 88) who says, in Turkish, that «Čingiz» is the plural of *čing*, and that the latter word means «great» (*uluγ*) and «firm» (*qatı*). SCHMIDT objected (*Gesch. der Ost-Mongolen*, 379) that *čing* was an adverb, meaning «solidly», «immoveably», which could have no plural, and BANZAROV concurred with him. But in vain: ERDMANN, relying on the consensus of Mussulman writers, maintained that they could not have erred, and derived from their explanations the very title of his book, «*Temudschin der Unerschütterliche*» (cf. his long note, pp. 599-609). SCHMIDT's argument was not of the soundest. *Čing* often is an adverb, but it can also be used as an adjective (the cognate *činya* is only an adjective), and plurals of adjectives are known in ancient Mongolian (for instance *yākās* in the *Secret History*, §§ 230, 271). A much stronger objection would have been to point out that, if *čing* had a plural, it could only have been \**čingut*, not *činggis*. It seems clear that Rašid's informants had no longer a tradition to guide them to the meaning and origin of «Činggis», and that they imagined an etymology which afterwards met with undue success. Their ignorance is shown by the parallel case of *gürhan*, always explained by Rašid in the passage where he comments on «Čingiz». According to him, *gür* means «firm» (مستحکم), or «powerful and great» (قوی و معظم). The word *gür* existed, however, and still exists in Mongolian, and the translators of the *Secret History* have correctly rendered it «universal» (see «Catai»). Except for the sake of completeness, I would not mention BLOCHET's explanation of «Činggis han» (*Moufazzal*, 532-533) as a «purely Mongolian» title «Činkkiz qayan», «Emperor of the brave men», -z being the mark of the plural. Neither a word \**činkki*, nor a plural in -z exist in Mongolian.

BANZAROV (*Čermaya Véra*, 78) proposed to see in «Činggis» the old Hsiung-nu title transcribed in Chinese as 單于 *shan-yü*, the ancient sound of which, according to BIČURIN, was «čen-yü». This was for the time a clever suggestion, which ERDMANN's supercilious refutation (*Temudschin*, 607-608) hardly affects. But BANZAROV was misinformed about the ancient pronunciation of the Chinese: *shan-yü* is an ancient \**šän-jü* (? from a more archaic \**šän-gü*), and I do not believe that the ancient sonant initial of the Hsiung-nu original could give an initial č- in Mongolian (I leave out the pronunciation *tan-yü*, \**tän-jü*, adopted by DE GROOT, which I consider to be erroneous). On the other hand, we should not lay too much stress on the following point, the only reasonable one among the seven raised by ERDMANN, that if «Činggis» was the mediaeval form of the term transcribed *shan-yü* in ancient times, it would form a title by itself, which would

not require, as «Činggis» does, the addition of «khan». We know in fact of cases like Ong-khan, Buiruq-khan, Tayang-khan, in which the first element is itself a title. I agree, however, that «Činggis» is probably an epithet, or name, not a title.

Perhaps, though not actually in Chinghiz-khan's lifetime, but soon after his death, a whole network of legends had been woven around his accession to the throne and the conditions in which he had adopted his world-famed title. Yet, the *Secret History*, completed only thirteen years after the death of the conqueror, and despite its epic character, is still very sober in the present case. It merely states that Tämüjin was proclaimed «Činggis-qa'an» (read «Činggis-khan»; §§ 123, 202); the *shaman* Kököčü Tüb-tängri plays no part in the ceremony, and only comes in at some length later on, when he is about to meet his tragic end. The *Shêng-wu ch'in-chêng lu* and the *Yüan shih* keep the same reserve. But Juwaini (I, 28), in the middle of the 13th cent., had already heard that Tüb-tängri (misread «But-tängri»), by the will of Heaven, had given to Tämüjin the title of «Čingiz-khan». Rašidu'd-Din expands on Kököčü Tüb-tängri's intervention and arrogance (*Ber*, I, 158-160); he also relates that Kököčü was believed to ride up to Heaven on a white horse.

Later Mongol chronicles tell another tale. In «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 71), a five-coloured bird similar to a lark came down three successive mornings on a square stone in front of the tent of Tämüjin, calling «Činggis, Činggis»; and Chinghiz-khan was named from the cry of a bird. The text of 'JIGS-MED NAM-MKHA (HIRTH, *Gesch. des Buddhismus*, II, 15-16) is an almost word for word translation of this passage. According to SCHMIDT (*ibid.* 379) and BANZAROV (*Černaya Véra*, 76), the tradition about this bird also occurs in Chinese sources, but this is a mistake. GAUBIL (*Hist. de Gentchiscan*, 12) and VISDELOU (*Supplement to D'HERBELOT*, 150), who mention it, state definitely that they heard it, orally, from some Mongols, and DE GUIGNES (*Hist. gén. des Huns*, III, 22), who speaks of Chinese sources for the legend of the bird, seems merely to have misunderstood GAUBIL.

Similar tales had spread abroad, however, at an early date. The Armenian monk Malakia, who lived in the 13th cent., says that an angel, who had taken the appearance of a golden-winged eagle, appeared among the Mongols, summoned their leader «Čangz» (Chinghiz-khan) and delivered to him, in their own language, the text of his laws, the *yasaq*. This being done, the angel called the leader «kaan», so that thenceforward the latter called himself «Čangz-kaan» (PATKANOV, *Istoriya Mongolov inoka Magakii*, 4). On the other hand, Malakia's countryman Hethum relates how the old smith «Cangius» (= Chinghiz-khan) had a vision of an armed knight, riding a white horse, who ordered him in the name of God to free the Mongols and to become their king (*Hist. des Croisades*, Arm., II, 148, 284). One of the legends must be connected with the later Mongolian story told by «Sanang Setsen», while the other recalls Kököčü and the white horse on which he used to ride up to Heaven, in the legend transmitted by Rašidu'd-Din.

This atmosphere of legend and supernatural intervention must be taken into account in discussing the last two explanations of «Činggis» which we have to examine. One must be fairly ancient, since it was already mentioned by GIBBON (BURY ed., VII, 3), but I have not traced its origin; this is the one which connects «Činggis» with Turk. *tängiz*, Osm. *deñiz*, «sea», «ocean». ERDMANN alludes to it as unworthy of refutation (*Temudschin*, 600). Yet, it was again thought of, simultaneously and independently, by RAMSTEDT (*Mogholica*, 25) and by me (cf. *Pe*, 23). In a

paper which I cannot trace for the present, VLADIMIROV remarked that such a derivation was improbable, since both « Činggis » and *tänggis*, « sea », occurred in the *Secret History* (§ 1). I do not think that this objection is of great moment. Double forms occur in the *Secret History*; for instance « Idil » and « Äjil », discussed further on. Moreover, if we are right, both « Činggis » and *tänggis* were foreign words in Mongolian; *tänggis* entered the legend of Mongol origins, closely connected with the ancient legend of the early Turks, at an unknown date, but there is no reason to date this contemporaneously with that at which « Činggis » became in certain religious or political circles the accepted term it must have been to have been adopted by Chinghiz-khan. If the two forms came to the Mongols at different periods and through different channels, and moreover acquired different uses, we need not be surprised to find both occurring in the same work.

Mong. *tänggis*, « sea », is an ἀπαξ λεγόμενον in the *Secret History*, and is of rare occurrence elsewhere. KOWALEWSKI does not give it. Late Sino-Mongolian lexicography knew it, however (cf. KLAPROTH, *Asia Polyglotta*, 263), and it is found in GOLSTUNSKIĪ. In the Mongol period, it occurs in YS, 63, 16 a, as *t'ien-chi-ssü*, *tängis*, in the name of the Caspian (cf. JA, 1920, I, 166). The word is current in Kalmuk, and pronounced *tengis* (RAMSTEDT, *Kalm. Wörterbuch*, 392). *Tänggis* is merely borrowed from the Turkish *tängiz* (*täniz*), but is written according to the rules of Mongolian spelling, where Turk. -ng- (-ñ-) is rendered as -ngg- (-ñg-), and where -z becomes -s since there is no z in Mongolian.

The case of Turk. *tängiz* > Mong. Činggis would be somewhat different. The treatment of the final -z > -s is the same, but the initial č- implies a mongolization. In Mongolian, all ancient \*ti- became či- at an early date, for instance in the pronoun of the second person čī < \*ti. I have quoted above the case of Turk. *tegin*, *tägin* (< proto-Mong. \*tegin) > *tigin* > Mong. čigin (in *otčigin*). This process of palatalization went on in Mongolian even after the 13th-14th cents. We still find in the *Secret History* (§ 262) « Idil » alongside of « Äjil » (§ 274) as a name of the Volga (« Äjil », for « Äjil », in the Ulän-Bätor ms.; the 也 只 里 Yeh-chih-li River [ho] of YS, 121, 2 b, and the A-chih-li Lake [hai-tzü] of the map of the *Yuän shih lei-pien* may represent other originals). Cf. Turk. Ätil in Kāšyari (not « İtil » as in BROCKELMANN, 244), Čay. Ätil and Idil (Edil), Kirgh. Edil, Tel. Ädäl, Kaz. Idil (RADLOV, I, 842, 850, 857, 1509; *Proben*, II, transl., p. XII), Ethil in Plan Carpine (*Wy*, 136), Etilia and Ethilia in Rubrouck (*Wy*, 195, 205, 210, 216, 223, 315), Edil in the Catalan Map (BUCHON, in *Not. et Extr.*, XIV, II, 131); Mong. Äjil (KOWALEWSKI, 227), New Mong. Ijil (VLADIMIROV, *Doklady Ak. Nauk*, 1929, 289), Kalm. Ijil, Idžl (VLADIMIROV, *ibid.*; RAMSTEDT, *Kalm. Wörterbuch*, 205). The same obtains in the case of the Irtysh, called in the *Secret History* Ärdiš (§§ 207, 264) or Ärdis (§ 198; the latter form is a misreading of the transcribers; the ancient Mongolian script did not distinguish -s and -š), Yeh-êrh-ti-shih (Ärdiš) in YS, I, s. a. 1206 and 1208; 122, 1 b; Yeh-li-ti-shih in YS, 22, 1 a; Yeh-êrh-ti-shih in the *Shêng-wu ch'in-chêng lu* (47 a); cf. Ärtiš in Kāšyari (misread « Artiš » in BROCKELMANN, 240); Ertič in Kirakos (PATKANOV, *Istoriya Mongolov*, II, 82). But the Mongol name is Ärcis in « Sanang Setsen » (cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 211, 412), or more probably Ärjis as in the Chinese translation of that work (*Mêng-ku yüan-liu chien-chêng*, 6, 22 b).

A derivation Turk. *tängiz* > Mong. Činggis would require either the existence of an intermediary form \*tingiz, or the palatalization t- > č- before vowels other than i. Both are possible.



Even in Turkish, and for words which do not belong to the « palatal » class, we find in the late Uighur redaction of the legend of Oyuz-khan (? Uyuz-khan) such forms as *čang* (< *tang*), *čašqarun* (< *tašqarun*), *čaptılar* (< *taptılar*); cf. *TP*, 1930, 270, 342, 347. In the middle of the 13th cent., the Mongolian name of Chinghiz-khan's eldest son Jöči was known in Turkish circles as Töši, written توشی Töši in Juwainī, and « Tossuc » and « Tossuccan » (= \*Tossu-can) in Plan Carpine (*Wy*, 58, 65). A similar direct passage Turk. *tängiz* > Mong. Čingis is far from impossible.

But an intermediary form \**tingiz* is so much the more acceptable since a first -i- vowel is attested in some Turkish dialects. In Turkish the word is *tängiz* in Kāšyari (BROCKELMANN, 203), « *tengiz* » (= *tengiz*) in the *Codex Cumanicus* (KUUN ed., 38, 88), Čay. تينگيز *tengiz* (or *tingiz*?; not *tängiz* as in RADLOV), late Uigh., Kar., Turkī *tängiz*, Kirgh. *teniz*, Tel., Kmd, Alt. *tānis*, yuzz. دینگیز *dingiz* (*Ber*, I, 6), Krm. *dāniz*, Osm. دنگيز *dāniz*, Kaz. *diniz* (RADLOV, III, 1045, 1055, 1661, 1667, 1756). An uncertainty in the nature of the first vowel is moreover traceable in the very name Činggis. The Armenians wrote it « Čangz » = \*Čang<sup>l</sup>z, which explains Hethum's « Changuis » and « Canguis ». A first *a* vowel occurs also in Ricoldo da Montecroce's « Camiustan » (< \*Canguiscan), and in the cognate corrupt forms of San Antonino and Chaucer. Even now, the Moghols of Afghanistan pronounce it « Čāngiz xān » (RAMSTEDT, *Mogholica*, 25).

The transcriptions of « Činggis » in Persian, Arabic, Turkish, and Armenian are also noteworthy on account of the final -z, when these languages possess both *s* and *z*; there is on the contrary no *z* in the Mongol language. Now, when transcribing Mongol words ending in -s, the -s has been retained in Persian, Armenian, etc., for instance, in *qaraunas* (see « Caraunas »), in « Nangias » (see « Mangi »), in *kāriyās* (the name of the place where horses were kept under guard outside the camp); why should the case be different with « Činggis »? On the other hand, if we suppose that « Činggis » was borrowed from a palatalized form of *tängiz*, the -z of the transcriptions Čingiz, Čang<sup>l</sup>z would be traceable to the original foreign word. It may even be that at first the Mongols used this borrowed word with a final -z. Although there was no *z* in Mongolian, the Mongols could pronounce it, and the letter *z* existed in the Uighur script which the Mongols made their own. At the end of a word, later Mongol usage wrote for -s the Uighur -s or the Uighur -z, indifferently; but this need not have been the original practice when they first wrote their language in Uighur script. Now, the only document in the Mongol language which is an original dating as far back as Chinghiz-khan's lifetime is the so-called « stone of Chinghiz-khan » preserved at Leningrad, which was engraved c. 1225. It begins with the name of Chinghiz-khan, but the final letter of the first word is the final Uighur -z, not -s. Although it may have already been used by the Mongols in the value of -s, I do not dismiss the possibility that the form intended was actually « \*Činggis » (in the archaic Mongol dialect of the Moghols of Afghanistan, « Čāngiz » has a final -z). Whatever the case may be, the script itself may have helped in the change from \*Činggis (< \*Tinggis) to Činggis. No attention need be paid, however, to the occurrence of would-be Turkish word *čingiz*, « great », « powerful », in RADLOV's dictionary (III, 2117). This word is to be found only in Chinghiz-khan's name, and by oversight it was taken over from Abū'l-Ghāzī, where this Mongolian, not Turkish, form is followed by the explanation given by RADLOV.

By a remarkable coincidence, Ibn Baṭṭūṭah, when speaking of Chinghiz-khan, always calls him تانگيز Tängiz (III, 22-27, 40, 57, 86; IV, 258, 300). I have heard the opinion that this was due

to the absence of *č* in Arabic, but the argument is unsound. Ibn Baṭṭūṭah might have resorted to the old Arabic device of transcribing *č* with *š*, as he does in the inherited spelling *Šin* of the name of China (see « Cin »). Above all, he might have done in this case what he did in all other cases, that is to say, write *j* instead and in value of *č*. For instance, we find in his text جرخ *Jarḫ* for the Persian *Čarḫ* (III, 88), جَوَطَرِي *Jautāri* for the Hindi *čaudhari* (III, 388, and cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. « chowdry »), جَرَكَس *Järkäs* for the Turko-Persian *Čärkäs* (II, 448; see « Čic »), سَرَايُوق *Sarājūq* for the Turkish *Saraičiq*. If he did not do the same for the Mongolian « Činggis », it must have been either because he heard the name pronounced « Tängiz », or because he or his informants connected it with the Turk. *tängiz*, « sea » (YULE's tentative explanation connecting it with Ar. *tānkīs*, « upside down » [Y, I, 247] cannot be seriously considered).

« Tängiz » occurs in real or legendary Altaic onomastic at an early date. I leave aside the name of Attila's son Δεγγιζίχ, which MARQUART thought was derived from *tengiz*, *deñiz* (*Die Chronologie der alttürk. Inschriften*, 109; *Izv. R. Arkh. Inst. v Konstantinople*, xv [1911], 26). Although the name strongly recalls the Turkish word for « sea », and still more the « tengizich » (= \**tengizik*), « auster », « southern wind », of the *Codex Cumanicus* (Kuun, 181; left unexplained by RADLOV, *Das türk. Sprachmaterial des Codex Comanicus*, 127, but perhaps a derivative form of *tängiz*), the initial *d-* of Δεγγιζίχ is hard to explain, since all the ancient forms of *tängiz* begin with *t-*; Osmanli or Kazan forms with *d-* are out of the question at such an early date. But, in the legend of Uighur origin, Oyuz-khan's youngest son was Tengiz-[? Tingiz-]khan (Abū'l-Ghāzi, DESMAISONS's transl., 27; cf. also RAVERTY, 881) or Dengiz-[? Dingiz-]khan (*Ber*, I, 6). Whatever the origin of « Činggis », there is no doubt that Chinghiz-khan, when taking this title, intended that he should be considered sovereign of the universe. The ocean, supposed to surround the earth, provided a convenient term for it. We know that Chinghiz-khan's successors held the same view. The real Mongol word for « ocean », « sea », is *dalai*, corresponding to Turk. *talui*. In the *Secret History* (§ 280), Ögödäi, Chinghiz's third son and successor, is spoken of as *dalai-in qa'an*, « oceanic Emperor ». When Güyük, Ögödäi's successor, writes to Pope Innocent IV in 1246, the Turkish preamble of his Persian letter gives him the title of *talui-nung han*, and his Mongol seal that of *dalai-in qan*, both having the same meaning as in the case of Ögödäi (*Pe*, 22-24). One century later, the 'Phags-pa text of the Chü-yung-kuan inscriptions (l. 17) speaks of the Mongol Emperor as *talayi-yin e'än ulus-un qa'an*, « master of the ocean, Emperor of the Nation ». On account of this last formula, I now believe that Stephen Orbelian had in view the same title when he spoke of Mongka as « master of the earth and the sea ». It is well known that *rgya-mcho*, « ocean », was used in the same way in Tibetan, and that the same image lies at the basis of the Mongol-Tibetan title of the Dalai-lama, « Oceanic religious lord », i. e. « Universal ».

So I think it probable that « Činggis » is derived from the Turk. *tängiz*, « sea », « ocean ». Such is not, however, the opinion expressed by VLADIMIROV in his *Čingis-khan* (p. 45). While seeing in « Činggis » the old title *šan-yü* of the Hsiung-nu sovereigns, BANZAROV had drawn attention to a god « Ḥajir Činggis tängri », mentioned in a Mongolian shamanist text (*Černaya Véra*, 78). Since Mongol tradition makes the shaman Kōkōčü play an important part in the attribution of the name « Činggis » to Chinghiz-khan, BARTHOLD supposed that this was « the name of one of the

spirits to whom the shamans paid reverence » (*ZVOIRAO*, x, 116), and he mentioned BANZAROV'S «*Ḥajir Činggis tängri*». This is also the view taken by VLADIMIROV. I do not feel inclined to accept it without some qualification. Of course, it is difficult to discuss a name occurring in unknown conditions in a manuscript of unknown date and origin. But it may be a prejudiced view to give to *Kököü*, on the authority of Rašidu-'d-Din, an importance in the ceremony of the enthronement which is not countenanced by any other source. At any rate, the very name of the shamanist god seems to betray a non-Mongolian origin. *Tängri*, «Heaven» and «God», is common to both Turkish and Mongolian; but *ḥajir* < *qajir* is the form taken by the Turkish *qadir*, «powerful», «terrible», when it was borrowed from Turkish into Mongolian (cf. *TP*, 1930, 53). Even if there be a connection with the title adopted by Chinghiz-khan, we may explain «*Ḥajir Činggis tängri*» as being originally the name of a Turkish shamanist god, \**Qadir Tängiz tängri*, «the Powerful God Ocean». The etymology of «*Činggis*» would remain the same.

In Mongolian, Chinghiz-khan is never called simply «*Činggis*», but always «*Činggis-ḥan*» or «*Činggis-qa'an*». In F, despite some exceptions, «Cinghis can» is generally given in the text, but «Cinghis kaan» in the table of contents and in the titles of chapters; on the other hand, «kaan» occurs even in the text when Polo speaks of Qubilai. YULE adopted «Chinghis Kaan» throughout his edition; the present one gives «Cinghis Kan». Without being too positive, it looks as though Polo had distinguished «can» or «kan» = *ḥan*, «khan», and «kaan» = *qa'an*; the forms «Cinghis kaan» in the table of contents and in the titles of chapters would be the result of a mistaken normalization. Whatever the case may be with Polo himself, the distinction actually existed; the relation and the respective values of *ḥan* and *qa'an* will be discussed under «Kaan». Among mediaeval travellers, Plan Carpine and Rubrouck give to Chinghiz-khan the title of «chan» = khan; Marignolli alone speaks of «Cingwis caam» (*Wy*, 543), which probably renders «*Činggis-qa'an*». Rašidu-'d-Din devotes chapters to «*Čingiz-ḥan*», «*Ögötai-ḥan*», «*Ĵöči-ḥan*», «*Čayatai-ḥan*», «*Tului-ḥan*», «*Güyük-ḥan*», and «*Mongka-ḥan*», but to «*Qubilai-qaan*». The first to take the title of *qa'an* was *Ögötai*, and this as a sort of personal epithet, which was even at a later date a sufficient designation of him as *qa'an-ḥan* (see «Kaan»). It was only Qubilai who took on the title of the great *qa'an* as a mere epithet, and was spoken of as *Qubilai-qa'an*. It may be objected that the *Secret History* always speaks of «*Činggis-qa'an*», and even applies the title of *qa'an* to the sovereigns of the first short-lived Mongol Empire which preceded that of Chinghiz-khan, «*Qabul-qa'an*», «*Qutula-qa'an*». All this must be due to later tradition, when *qa'an* had replaced *ḥan* in Mongol usage, and is ascribable either to the compilers of the *Secret History* in 1240, or more probably to later copyists. We have irrefutable proof, however, that Chinghiz-khan did not take the title of *qa'an*. The so-called «stone of Chinghiz-khan» preserved in Leningrad begins with the words *Činggis qan-i* (or *Činggiz qan-i*?; cf. *supra*), a popular genitive of «*Činggis-qan*»; *qan* is the regular notation of *ḥan* in Uighur script, because this, like the later Uighur-Mongolian script, had no special letter for *ḥ*. This was still the case in *Güyük*'s time. We have already seen that, in his letter of 1246 addressed to Innocent IV, he styles himself in Turkish *talut-nung ḥan*, and in his Mongol seal *dalai-in qanu* (genitive), i. e. both times *ḥan*, not *qa'an*. When Malakia, in the 13th cent., speaks of «*Čang'z-yayan*» = *Činggis-qa'an*, it was already the result of the same change of habit which altered *Činggis-qan* to

Činggis-qa'an in the present text of the *Secret History* (cf. PATKANOV, *Istoriya Mongolov inoki Magakit*, 4, 63).

For the « white standard with nine pennants », which was raised by Chinghiz-khan in 1206, and which was not, as is often said, made of yak or horse tails, see « Tuc ».

CHINGHIZ-KHAN AND ONG-KHAN'S DAUGHTER. — According to Polo, war broke out between Chinghiz-khan and Ong-khan in 1200, because the latter scorned the former's request for the hand of his daughter. YULE (*Y*, I, 239), while agreeing that such a marriage never took place, adduces Oriental and Western authorities which state that Ong-khan's daughter was taken over by Chinghiz. One of these authorities may be rejected at once : PÉTIS DE LA CROIX is here untrustworthy, since in many instances, and most probably in the present one, his statements are tacitly based not on Oriental sources, but on Polo himself. Two writers remain, Vincent de Beauvais and Bar-Hebraeus (Abū-'l Faraġ). In Vincent de Beauvais's account (*Speculum historiale*, xxx, ch. 69 and 70), the surviving daughter of King David, *i. e.* Prester John (= Ong-khan), became the wife of Chinghiz-khan and, according to a tradition (*ut dicitur*), bore him sons; it was because of her that the Nestorian monk Rabban-ata found favour with Chinghiz. Vincent de Beauvais's source is surely the lost *Historia Tartarorum* which had been written, probably in 1248, by Simon de Saint-Quentin, one of the members of the mission of Ascelin; Rabban-ata, whom we know from various authorities, died almost certainly in 1247 (cf. *Pe*, 43, 80, 134). A similar story, which was not known to YULE, was told by André de Longjumeau : according to him, Chinghiz-khan had married the daughter of Prester John (= Ong-khan), and she bore him a son, the then reigning Emperor (*i. e.* Güyük; cf. *Pe*, 56-57). In the text of Bar-Hebraeus, Chinghiz-khan sees in a dream a monk in black garb, and his Christian wife, Ong-khan's daughter, calls in the bishop Mār Denha, who tells Chinghiz that he must have seen a Christian saint; thenceforward, Chinghiz showed his benevolence to the Christians (POCOCKE, *Historia Dynastiarum*, 285-286). Polo's statement, to which I have referred above, that, after Ong-khan's defeat and death, Chinghiz took his daughter to wife, is only found in VB and in RAMUSIO, who seems here to be summarizing VB (cf. Vol. I, 166, 498). In fact, Chinghiz did not marry the daughter, but a niece of Ong-khan, Ibaka-bāki (*Secret History*, §§ 186, 208; this form is more correct than the « Abika » of *Y*, I, 243; cf. also *Ber*, I, 100; II, 80, 108), daughter of Ĵagambu (< rGya-sgam-po), whom moreover he handed over afterwards to Ĵürčädäi (= Kätäi-noyan), and Güyük was a grandson, not a son, of Chinghiz-khan. Two other daughters of Ĵagambu were given to two sons of Chinghiz, Ĵöči and Tului, but none to Ögödäi, Güyük's father. The tales then current in the East may, however, account to some extent for Polo's story.

There is still another origin which must be considered. The marriages of Chinghiz-khan and his sons with women of Ong-khan's family all took place after the latter's death. But when he was still, outwardly at least, on good terms with Chinghiz, a union had been projected between his daughter Čaur-bāki and Chinghiz-khan's eldest son Ĵöči, and between Chinghiz-khan's daughter Qoĵin-bāki (or Qo'aĵin-bāki) and Ong-khan's grandson Tusaqa (cf. *Secret History*, § 165; *Shêng-wu ch'in-chêng lu*, 27 b; *Ber*, I, 101; II, 78; the names as I give them here are more correct than those of *Y*, I, 239). But owing to Ong-khan's ill-will, the project fell through, and, as Rašid says,

« on that account, Chinghiz-khan felt uneasy in his heart ». This event seems to have taken place in 1202. It does not seem open to doubt that it is a distorted echo of it which appears in Polo's texts, and for once the date there given, « 1200 », is not so far from the truth.

CHINGHIZ-KHAN AS KING DAVID. — Chinghiz-khan was first heard of in Europe, though not under his true name, in 1221, when an Arabic report on his advance against the Mussulmans reached Damietta, where it was translated into Latin. This document, fraught with a very rich though often corrupt nomenclature, has been studied in great detail by ZARNCKE in the second instalment of his monograph *Der Priester Johannes*, and it is to be regretted that CORDIER, while mentioning ZARNCKE in a final note, should not have used his work to correct and supplement YULE's most valuable note on Prester John in *Y*, I, 231-237. This « charta » of 1221, as ZARNCKE calls it, has come down to us in three redactions. In the first one, the longest, Chinghiz-khan appears as King David, the younger son of King Israel, son himself of King « Sarchisi », son of King John, son of King Bulgaboga (var. Bulchabot, Bidgaboga), a believer in Christ Jesus. The second redaction speaks merely of King David, a servant of the Lord; the third one, of the Christian King David, son of Prester John of India. Though such a genealogy of Chinghiz-Khan is out of the question, the names are not fantastic : David, « Sarchisi » (Sargis, Sergius, mistaken by ZARNCKE, II, 31, for a non-Christian Turkish name) and John are good Christian names; a Seljuk prince was called Israel; « Bulgaboga » seems to be a regular Turkish name, \*Bilgä-buqa (not a Mongolian name, as is said in ZARNCKE, II, 31). The arrival of the document at Damietta in 1221 finds a striking confirmation in an Arabic source which ZARNCKE did not know, the *History of the Patriarchs of Alexandria*. Passages from this work relating to the siege of Damietta by the Franks have been translated by BLOCHET as an appendix to his translation of the section of Maqrīzī's *History of Egypt (Sulūk)* relating to the time of the Crusades before the Mamluk Sultans. Under the year 937 of the Martyrs (= A. D. 1221), a paragraph begins as follows (BLOCHET, *Histoire d'Égypte*, 563) : « That year, the news was received that a sovereign of the east who was called « king of China » (*malik-as-Sîn*), and who had in his service a great number of Turks from the country of Hītā (see « Catai ») and from Qīpčaq, had vanquished the Hwārizmšāh, King of Persia... » Mussulman and Armenian writers often spoke of Chinghiz-khan as having come from « China »; this is the result of the undue extension which the name Čin or Sîn had received in the Middle Ages (see « Cin »). It is certainly the same report which is alluded to in the *History of the Patriarchs of Alexandria*, the Latin versions of which were published by ZARNCKE. Two reasons may have contributed to their calling Chinghiz-khan a Christian king. It was a natural tendency to see a Christian in the sovereign who had destroyed the Mussulman Empire of Hwārizm, and the more so since a tradition had been current for almost a century that a great Nestorian king and priest called John reigned far away, somewhere in « India » (see « Prester John »). The second reason is that there were actually many Nestorians in Central Asia, and the report which came to Damietta in 1221 may well have been composed in Nestorian circles to impress the Mussulmans of Syria and Egypt and to make them relax their antagonistic attitude towards the Nestorian communities. Similar reports were circulated in Armenia. Kirakos, speaking of the sudden appearance of the Mongols in Armenia in 1220, says (BROSSET, *Deux historiens arméniens*, 100) :

« A false rumour spread that they were Magi or Christians, that they worked miracles, and that they had come to avenge the tyranny of the Mussulmans against the Christians. It was also said that they had a tent in the shape of a church, and a miraculous cross, in front of which a bushel of barley was laid; the whole army would take from this to feed their horses, without the corn growing less; and when all had made an end of taking of the corn, the [full] bushel was still there. » Polo attributes the favour shown by Chinghiz-khan to the Christians to the success they had predicted for him against Ong-khan by their divination with twigs (Vol. I, 166). The truth is that he showed them no special favour and that the exemptions he granted their clergy from taxes and villain service, which are recalled in subsequent edicts, were the same as for the ministers of other creeds.

THE DATE OF CHINGHIZ-KHAN'S DEATH. — Though with a much lesser margin of uncertainty, the true date of the death of Chinghiz-khan is not much easier to establish than the date of his birth. Polo's assertion that Chinghiz survived Ong-khan's defeat (A. D. 1203) only by six years is of course a glaring error. According to YULE (*Y*, I, 245), Chinghiz-khan died on August 18, 1227; most historians give the same date. The question, however, is not so simple, and BARTHOLD (*EI*, s. v. « Čingiz-khān ») contented himself by saying that the death took place in the first half of *ramadān* A. H. 624 = August 1227, the sources disagreeing as to the day. VLADIMIROV (*Cingiz-khan*, 150) is no more definite than BARTHOLD.

The date of August 18, 1227, corresponds to the one given for the death of the conqueror by Ĵuwainī, « on the fourth of *ramadān* 624 » (I, 144<sup>b</sup>), and was copied from him by Bar Hebraeus (*Historia Dynastiarum*, POCOCCKE transl., 305). In his *Hist. de Gentchiscan* (p. 51), GAUBIL also says that Chinghiz-khan died after giving, on August 18, 1227, his last instructions to his sons and generals. But, in a note of the following page, GAUBIL adds that, according to a certain history of the Mongols, Chinghiz fell ill on August 18, but died only seven days later. I do not know of which abridged text GAUBIL made use in the first passage. The information given in his subsequent note is the only one which is found in the *YS* (I, 9 b), where we read : « In the 22nd year, which was *ting-hai* (1227)..., in the autumn, the seventh month, on the day *jên-wu* (August 18, 1227), [the Emperor] fell ill (不豫 *pu-yü*); on the day *chi-ch'ou* (August 25, 1227), he died... » Neither the *Secret History* nor the *Shêng-wu ch'in-chêng lu* says anything about Chinghiz-khan's death; this was certainly because of some sort of taboo, and not because, as alleged in WANG Kuo-wei's edition of the latter work (64 b), there is a lacuna in our mss. Consequently, the only date to be found in Chinese sources for the death of Chinghiz-khan is August 25, 1227, one week later than the date given by Ĵuwainī. The day *chi-ch'ou* was the twelfth of the month, so that there is an intrinsic contradiction in CORDIER's statement (*Hist. gén. de la Chine*, II, 222) that Chinghiz-khan died « on the twelfth of the seventh month (August 18, 1227) ». GROUSSET (*L'Empire des steppes*, 309) is mistaken when he says that the date of August 18, 1227, is given in the *YS*.

But Rašīdu-'d-Dīn has a different story, which he relates and which occurs in several places in the course of his biography of Chinghiz-khan :

a. (*Ber*, II, Pers. text, 140; transl., 85-86) : « Serious astrologers (*munaffimān*) have written down the date of his death, ... and their report is that he died in the *qāqā yīl* (= Mong. *yaqai*,

« pig », and Turk. *ytl*, Mong. *jil*, « year [of the cycle] », which is the year of the pig, in the middle of the month of autumn (*dar miyānah-i mäh-i pāyiz*), on the 15th day (*pānzdäh rōz*).» D'OHSSON (*Oh*, I, 38), whose text, if it be a literal translation, is taken from a different passage which I cannot trace, speaks too of « the 15th of the month of autumn of the pig year ». ERDMANN (*Temudschin*, 573) is in closer agreement with BEREZIN's version, except that he speaks of « spring » instead of « autumn », by a slip probably. None of the three translators gives an equivalence in the European calendar, perhaps because they could not understand what Rašid meant in the present passage. There are of course three autumn months, and we ought to have been told which of the three is referred to. Moreover, what would be the use of the pleonastic mention, first of the middle of the month, then of the fifteenth day? Rašid gives the dates of the animal cycle according to the Uighur calendar, and we know that there were some discrepancies between that calendar and the Chinese one. But since both use lunar months, there could be no great difference — one or two days at the utmost — in the number attached to any day of a given month : the full moon was practically full at the same moment for the Uighurs as well as the Chinese. If the first month of autumn were meant, the fifteenth day would be August 28, 1227, in Chinese reckoning. But, in my opinion, the natural translation is to read, without any change of letters, *dar miyānah mäh-i pāyiz*, « in the middle month of autumn ». The fifteenth of the middle month of autumn, in the Chinese calendar, was September 26, 1227 (this seems to be the solution adopted by HUNG Chün, I A, 45, when he says, I do not know on what authority, that Western writers make Chinghiz-khan die on the fifteenth day of the eighth month). This is certainly too late, but we shall see hereafter that the apparent difficulty may give a clue for the explanation of another passage hitherto misunderstood.

b. (*Ber*, III, Pers. text, 149; transl., 99) : « On the 15th day of the month of autumn of the pig year, corresponding to *ramadān* of the year 624, [the Emperor] departed from the perishable world... » Here there is no *miyānah*, « middle », nor is the day of the month of *ramadān* indicated. *Ramadān* A. H. 624 corresponds to August 15-September 14, 1227; no precision is possible, except that the fifteenth day of the Chinese month falling in *ramadān* A. H. 624 would of course be again August 28, 1227.

c. (*Ber*, III, Pers. text, 152-153; transl., 101-102) : « Among the Mongols it is well established and known that Chinghiz-khan lived seventy-two Turkish years. He was born in the *qāqā yil*, which is the year of the pig, and he died too in the *qāqā yil* in the country of Tangut (= Tangut). They brought his coffin to his *ordo* (*ordōhāi*, in the plural) on the fifteenth day of the month of *šawwāl* of that year, which corresponds to the fourth of *ramadān* of the year 624 of the Hegira, and [then] made known the catastrophe. » This translation of BEREZIN is absurd. The fifteenth of *šawwāl* would correspond to September 29, 1227, and the fourth of *ramadān* to August 18, 1227. But how could a day of a month in a certain calendar correspond, in the same year, to another day of another month in the same calendar? This is tantamount to saying that September 29, 1227, is the same as August 18, 1227! Obviously, the text is corrupt. Moreover, BEREZIN's Persian text gives « the fourteenth (چهارده) of *ramadān* », without any variant, while his translation gives « the fourth (چهارم) of *ramadān* ». Instead of شهر شوال, « month of *šawwāl* », adopted in the text and the translation, BEREZIN mentions in a note the variant شهر خون, « month of *hūn* » in one manuscript.

Before BEREZIN, this same passage had been edited and translated by ERDMANN (*Temudschin*, 573-574). He too speaks of the «fourth of *ramadān*»; but instead of the absurd «*šawwāl*», says «on the fifteenth of the *خون آی* *hūn ai* of that year».

In the Paris mss. (Mīrzā Muḥammad QAZWĪNĪ was kind enough to collate them for me), three contain the corresponding passage; they read as follows :

Suppl. persan 209, 154 r° : «... on the fifteenth of *شون آی* *šūn ai* of that year, corresponding to the fourth of *ramadān* of the year 624 of the Hegira...».

Suppl. persan 1113, 121 v° : «... on the [lacuna] of *سُون آی* *sūn ai* of that year, corresponding to the fourteenth of *ramadān* of the year 624 of the Hegira...».

Anc. fonds persan 68, 121 r° : «... in *سُون آی* *sūn ai* of that year, corresponding to the fourteenth of *ramadān* of the year 624 of the Hegira...».

Before discussing these readings, I must quote a last passage :

*d.* (*Ber*, III, Pers. text, 117; transl., 119) : «In ... *سُون* *sūn* (var. *شمردن*) of this (*این*) said year of the pig, corresponding to the fourteenth of *ramadān* of the year 624, they brought his coffin to the *ordo* (*ordōhā*), and [then] made known the catastrophe about him...»

This passage, according to QAZWĪNĪ, occurs in two of the Paris mss. in the following form :

Suppl. persan 209, 160 r° : «... in [lacuna] of *سُون* of this (*این*) said year of the pig, corresponding to the fourteenth of *ramadān* of the year 624», etc.

Suppl. persan 1113, 124 r° : «... in [lacuna] of *šūn ai* of the said year of the pig, corresponding to the fourteenth of *ramadān* of the year 624», etc.

We are now in a position to restore the correct text. It is clear that instead of *hūn ai*, or *sūn ai*, or *sūn* alone, we should read *شون آی* *šūn ai* everywhere; but *آی ai*, which was not understood by the copyists, was altered by them to *این in*, «this», in the two texts of *d.* out of three, although *in*, «this», was pleonastic with the «said» year.

There can be no doubt either as to the meaning of *šūn ai*; it must be vocalized *šūn ai*. *Ai* means «month» in Turkish, and Rašīd is merely quoting a month of the Uighur calendar. *Šūn ai* occurs in the Sino-Uighur Vocabulary of the Bureau of Translators of the Ming dynasty; it is the name of the intercalary month, formed with *šūn* < Ch. 閏 *jun*, «intercalary [month]», and Turk. *ai*, «month» (cf. RADLOV, IV, 1109).

There is, however, a difficulty. The year 1227 was embolismic, but, in the Chinese calendar, the intercalary month of 1227 followed the fifth month, and the fifteenth would correspond to June 30, 1227, which is clearly unacceptable.

But here intervenes the fact that the Uighur calendar did not always coincide with the Chinese. Although the phases of the moon were of course the same in both countries, it may be that the Uighurs placed the intercalary month of 1227, not after the fifth month, but after the seventh. In such a case, not having an intercalary month after the fifth, the Chinese intercalary fifth month would be their sixth month, the Chinese sixth month would be their seventh, and the Chinese seventh month would be their intercalary month. The fifteenth of the Chinese seventh month would thus correspond to August 28, 1227, and this is exactly the date corresponding to the fourteenth of *ramadān* A. H. 624. So I have no doubt that the fourteenth of *ramadān* is the date actually given by Rašīd, in agreement with most mss. in one case, and with all the mss. in the



other. If in the first case, we have some readings giving « fourth » of *ramadān*, it must be either because in Persian « fourth » and « fourteenth » differ only by one letter, or because some « intelligent » copyist was influenced by its equivalence to the « fourth » of *ramadān* given for the date of Chinghiz-khan's death in Ĵuwaīnī. Lastly, we may thus account, to a certain extent, for our first quotation, in which Rašīd speaks of the « middle » month of autumn. If the Uighurs placed their intercalary month after the seventh month of 1227 and not after the fifth, their seventh month, in other words their autumn of that year, began one month earlier than the autumn of the Chinese calendar, and their intercalary month was the second month of the autumn. This may be the reason why, in the first passage, Rašīd, avoiding the uncouth foreign term *šūn ai*, spoke of the intercalary month as being the « middle » month of the autumn, although, including the intercalary month, that autumn actually had four months (the opinions of 北村三郎 KITAMURA Saburō and of 佐久間信恭 SAKUMA Nobugasu, quoted in T'U Chi, 3, 31 b, are both worthless, because they combine dates given in different calendars).

So we are confronted with three different dates : the fourth of *ramadān*, August 18, 1227, and the day *chi-ch'ou* of the seventh month, August 25, 1227, expressly given as the date of the death of Chinghiz-khan by Ĵuwaīnī and by the *Yüan shih*, respectively; and the fourteenth of *ramadān*, August 28, 1227, which is, according to Rašīdu-'d-Dīn, the date on which Chinghiz-khan's coffin reached his *ordo* and on which the news of his death was announced. But are we to believe that the third date is fundamentally independent of the other two ? A difference of three days (August 25 and August 28) is the utmost possible limit of divergence between the Uighur and the Chinese calendars, the months of both being based on the phases of the moon; but there are similar cases for other dates expressed in the Uighur calendar and given by Rašīd with the equivalence in the Mussulman calendar, sometimes with the indication of the day of the week which provides an additional means of ascertaining the intended day. Let us take the case of a date originally given in the Uighur calendar, or in the Chinese calendar, as being such a day of such a month. When translating it from one language into the other, the translator might quite possibly retain the date of the month as it was given in the original, so that, if there were a divergence of one, two or three days between the two calendars, the date, though expressed with the same numbers, would actually differ by one, two or three days. This may at first seem to account for the difference between August 25, 1227, given by the Chinese and August 28, 1227, given by Rašīdu-'d-Dīn. I am afraid, however, that such a hypothesis can hardly be accepted in the present case, since, as we have seen, the fifteenth of the Uighur [seventh] intercalary month of 1227 corresponds to the fifteenth day of the seventh Chinese month of the same year; that is to say, there is here no divergence between the two calendars in the numbering of the days of the month. I see no solution if the two dates, August 25, 1227, of the Chinese and August 28, 1227, of Rašīd, refer to the same event. As to Ĵuwaīnī's date, August 18, 1227, it is irreconcilable with either. But the suspicion naturally arises whether the « fourth » of *ramadān*, in Ĵuwaīnī, though read as such by Bar Hebraeus as early as the end of the 13th cent., may not have been a mistake for « fourteenth », be it a slip of the author or the misreading of a very early copyist, of the same order as that which occurred later in some of Rašīd's manuscripts.

If on the other hand we accept that Ĵuwaīnī and the *Yüan shih* give for Chinghiz-khan's

death two dates between which we cannot decide, and that Rašid's date, being that of the arrival of the coffin at the *ordo*, has nothing to do with either, the whole difficulty remains, since three days (August 25-August 28) or even ten days (August 18-August 28) seem to be too short for the carriage of the remains of the deceased sovereign from the supposed place of death to the *ordo* in Mongolia. But that is another very intricate problem.

Before passing on from the statements relating to the date of the death, I may add, however, that the *Altan tobči* also says that Chinghiz-khan died in 1227 (read «*ding-pig*» instead of «*bing-pig*», which cannot exist; cf. *infra*), «the fifteenth of the month» (GOMBOEV ed., 40<sup>1</sup>; the day of the month has been accidentally omitted in the translation, 146). Since the month itself is not mentioned, we may have there merely the result of a belief that all important events should take place when the moon is full (generally just after the full moon, on the sixteenth day, in the *Secret History*). The coincidence with Rašid is, however, striking. At any rate, it would tend to suggest that even in the case of Rašid, the date of the «fifteenth» day of the intercalary month may be of religious, and not historical origin.

THE PLACE OF CHINGHIZ-KHAN'S DEATH. — According to *YS* (I, 9 a-b), Chinghiz-khan, returning from his six years' campaign against the Mussulmans in the West, arrived at his 行宮 *hsing-kung* (in Chinese «temporary Imperial residence», but here «royal encampment», = Mong. *ordu* > *ordo*) of Mongolia in the first month of 1225. The *ordo* is said in the *Secret History*, § 264, to have been that of the «Black Forest of the Tula», which was probably outside the south-eastern angle of the southern bend of that river. According to Rašidu-'d-Dīn, Chinghiz remained for some time at \*Buqa-sučiqu before reaching his *ordo* (*Ber*, III, 94; Pers. text, 141). This may be the doubtful name \*Buqa-[s]učiqai (? «[Place where] the bull was frightened») which occurs on the famous «stone of Chinghiz-khan» preserved in the Asiatic Museum of Leningrad. Yäsünggä (= Yäsüngä) is said to have won a shooting contest when the Mongol leaders had assembled there on Chinghiz-khan's return from the Mohammedan countries (cf. BANZAROV, *Černaya véra*, 92-93; I. KLUKIN [KLYUKIN], *Drevneišaya mongol'skaya nadpis'*, in *Trudy gosudarst. Dal'nevostočn. Universiteta*, Series VI, No. 5, 26 ss., with a different decipherment of this passage). But whatever reading we may adopt, it is difficult to account for the region where the «stone of Chinghiz-khan» was found, presumably *in situ*: the stone was discovered in the basin of the Argun, *i. e.* far to the east of the track which Chinghiz followed on his way back from the Ili region to his *ordo*. I have no solution to proffer for this question, which former inquirers have ignored.

In the first month of 1226, Chinghiz-khan decided personally to lead an expedition against the Hsi-Hsia. «In the second month (February 28-March 29), he took the city of 黑水 Hei-shui («Black Water»; = Qara-ḥoto; see «Eçina»), together with several others. In the summer, he 'escaped the heat' (避暑) at the 渾垂山 Hun-ch'ui-shan and seized the *chou* of Kan (= Kan-chou) and Su (= Su-chou). (Hun-ch'ui Mountain; possibly \*Qunčui Mountain, «Mountain of the Princess», is unidentified; its location north of Su-chou in *Hsi-Hsia shu-shih*, 42, 8 b, or south of Liang-chou in *T'u Chi*, 3, 30 b, is mere guesswork; TING Ch'ien's attempt [*Yüan pi-shih ti-li k'ao-chêng*, 14, 2 b] to change Hun-ch'ui to \*Ch'ui-hun is a failure; CHARIGNON's 渾楚山 Hun-ch'u-shan [*Ch*

1, 189] simply does not exist; it is merely Ch'ien-lung's «reformed» transcription of Hun-ch'ui-shan). In the autumn, he captured the districts (*hsien*) of 朔羅 Shuo-lo (Ch'o-lo; cf. *TP*, 1935, 161) and 河羅 Ho-lo [belonging] to 西涼府 Hsi-liang-fu (= Liang-chou), and then, crossing the sandy desert (沙陀 *sha-t'o*), arrived at the 九渡 Chiu-tu («Nine crossing») of the Huang-ho (probably E. S. E. of Liang-chou)... In the winter, in the eleventh month, on [the day] *kêng-shên* (November 29, 1226), the Emperor attacked 靈州 Ling-chou (not far from the right bank of the Huang-ho, to the south-east of Ning-hsia). The [Hsi-]Hsia sent 崑名令公 Wei-ming *ling-kung* to the rescue [of the city]. On [the day] *ping-yin* (December 5), the Emperor crossed the [Yellow] River, attacked the [Hsi-]Hsia army and defeated it. On [the day] *ting-ch'ou* (December 16), he took up his quarters at 鹽州川 Yen-chou-ch'uan (= the modern 花馬池 Hua-ma-ch'ih, to the south-east of Ling-chou; not «Yen-ch'uan-chou» as in *Ch*, 1, 189, whose correction merely to «Yen-ch'uan» and its identification are erroneous; cf. T'u Chi, 3, 31 *a*)... In the 22nd year, which was *ting-hai*, in the spring (January 19-April 17, 1227), the Emperor left troops to attack the royal city of the [Hsi-]Hsia (*i. e.* 中興府 Chung-hsing-fu, now Ning-hsia; the information of the *Chin shih*, 17, 3 *a*, that Chung-hsing-fu was «pacified» [平 *p'ing*], *i. e.* conquered, towards the end of 1226 rests on a false report which reached the Chin court on December 11, 1226), and, leading his [other] troops, himself crossed the [Huang-]ho and attacked 積石州 Chi-shih-chou (unidentified; it must have lain north of the Huang-ho). In the second month (February 18-March 18), he conquered 臨洮府 Lin-t'ao-fu (= the *hsien* of 狄道 Ti-tao of the Manchu dynasty, now the *hsien* of Lin-t'ao, to the south of Lan-chou). In the third month (March 19-April 17), he captured the two *chou* of 洮河 T'ao-ho and of 西寧 Hsi-ning (read «the three [三 instead of 二] *chou* of T'ao[-chou], Ho[-chou] and Hsi-ning» cf. *YS*, 121, 1 *b*, and 122, 5 *a*; T'u Chi's more important correction, 3, 31 *a*, is not necessary; T'ao-chou and Ho-chou lay south, and Hsi-ning north of the Huang-ho)... In the fourth month (April 18-May 16), the Emperor halted at 龍德 Lung-tê (read 隆德 Lung-tê, to the west of P'ing-liang-fu) and carried among others the *chou* of 德順 Tê-shun (now Ching-ning, west of Lung-tê; cf. also *YS*, 121, 1 *b*; 122, 5 *a*; the *Chin shih*, 17, 3 *a*, says «third month»)... In the fifth intercalary month (June 16-July 14), he «escaped the heat» on the 六盤山 Liu-p'an-shan. In the sixth month (July 15-August 13), the Chin sent 完顏合周 Wan-yen Ho-chou and 奧屯阿虎 Ao-t'un A-hu to ask for peace (this is the time of their arrival at Chinghiz-khan's encampment; a very curious passage in the *Chin shih*, 112, 3 *a*, shows that, during the stay of the Chin envoys, the Mongol leaders, *showing maps* [以輿地圖指示之], questioned Ao-t'un A-hu as to the number of troops stationed between them and Shang-chou, to the south-east of Hsi-an-fu; the Mongols actually captured Shang-chou in the beginning of 1228 [cf. *Chin shih*, 112, 4 *a*])... In this month, the chief (主 *chu*, a depreciatory term for sovereign) of the [Hsi-]Hsia, 李睨 Li Hsien, made his submission. The Emperor halted at the 西江 Hsi-chiang («Western River») of the district (*hsien*) of 清水 Ch'ing-shui (= the present 牛頭河 Niu-t'ou-ho, according to *YANAI*, 57; but cf. below). In the autumn, in the seventh month, on the day *jên-wu* (August 18, 1227), he fell ill; on the day *chi-ch'ou* (August 25, 1227), he died at the *hsing-kung* of Ha-lao-t'u of the Sa-li-ch'uan («Sa-li Valley»; 薩里川哈老徒之行宮).»

In the biography of Čaγān (*YS*, 120, 1 *a-b*), the capture of Su-chou is mentioned before that of Kan-chou, and this is probably correct, since Su-chou was the first place to be reached by

an army coming down from Qara-ḥoto along the Etsin-yol (but, perhaps on account of the traditional order « Kan and Su » which gave the name of the province of « Kan-su », Rašidu'd-Dīn and the *Cho-kêng-lu* [22, 7 b] give the sequence « Kan-chou and Su-chou » like the *YS*). It is from Su-chou that Chinghiz-khan must have gone on to Kan-chou and Liang-chou. From Liang-chou, Chinghiz had to traverse the southern part of the sandy desert to reach the Huang-ho, which he crossed in order to attack Ling-chou. He must have crossed the Huang-ho again, this time not very far from Ning-hsia, so as to engage on the western side of the river the Hsi-Hsia troops sent to the rescue of Ling-chou (see « Calacian »). But, without then attempting to take the Hsi-Hsia capital (Ning-hsia), he retired a second time to the eastern bank, and took up his winter quarters south-east of Ling-chou. When he moved again, he left to his generals the conduct of the siege of Ning-hsia, and proceeded south-west to conquer the south-western part of Kan-su, north and south of the Huang-ho. He then moved again to the south-east, and « escaped the heat » at the Liu-p'an-shan, with the further intention, as it seems, to encroach on Sung territory to the south-east, so as to turn the position of the strongholds which had made a direct attack difficult to launch against the Chin in Shan-hsi.

Čayān's biography, drawn from some private documents in which the part he played was probably unduly extolled, goes on in the following terms : « [The army] advanced to the attack of Ling-chou. The [Hsi-]Hsia people came, ten myriads in number, to its rescue. The Emperor personally fought against them, and defeated them. He returned, and halted at the Liu-p'an[-shan]. The [Hsi-]Hsia chief (= sovereign) made a resolute defence at 中興 Chung-hsing (= Ning-hsia). The Emperor sent Čayān to enter the city and warn [the inhabitants] of the weal or woe [which their conduct would bring them]. The people then decided to submit. At that moment, the Emperor died. The generals seized the [Hsi-]Hsia chief and killed him. Moreover, they decided to put every one of [the inhabitants of] Chung-hsing to the sword. Čayān strongly remonstrated and prevented them [from doing so]. In haste he entered [the city], and having placated them, he gathered together what remained of the inhabitants. » In the notice devoted to A-shu-lu (\*Ajul ?; *YS*, 123, 2 b), it is said that the capture of the Hsi-Hsia sovereign was made by \*Ajul, and that the Hsi-Hsia sovereign was put to death by Ögödäi; both these statements seem to be unfounded.

The Liu-p'an-shan (« Liu-p'an Mountain ») in Kan-su is well known. It stands 20 *li* east of the *hsien* of Lung-tê, and 70 *li* south of the *hsien* of Ku-yüan, on the main road from Lan-chou to Hsi-an-fu. It is said that it was called Liu-p'an because of its six (*liu*) windings (*p'an*), to the abruptness and picturesqueness of which I can testify from personal knowledge. Another tradition is that the road was in ancient times called 絡盤道 Lo-p'an-tao, the « Road of successive windings ». The place has always been of great strategic importance, and also a favourite hot weather resort. During the reign of Mongka, Qubilai, on his way back from Yün-nan, spent at the Liu-p'an-shan the fifth month of 1254 (*YS*, 4, 2 a). In 1258, during the fourth month, Mongka himself stopped there when on his way to Ssü-ch'uan, and in the seventh month left there his impedimenta 輜重 *tzü-chung*, Mong. *oyruq* (cf. Rašid's parallel texts in *Bl*, II, 325-326; in BLOCHER's note, *tzü-chung* has been wrongly taken for the name of an individual). In 1296, military colonies consisting of 10,000 men in all were established from the Liu-p'an-shan to the Yellow

River (YS, 19, 1 b). In 1272, Qubilai's grandson Mangala (see «Mangalai») was made prince of An-hsi with a palace at Hsi-an-fu, and received in 1273 the cumulative appanage of prince of 秦 Ch'in with a palace in the territory of 原州 Yüan-chou (the present *hsien* of Ku-yüan), which was on that occasion promoted to 開成府 K'ai-ch'êng-fu (its seat being 40 *li* south-east of Ku-yüan). In 1278, the son of Hoqu (see «Cotan»), a prince who had revolted, was captured at the Liu-p'an-shan (YS, 121, 6 a). On Mangala's death in 1280, his son Ananda (= Ānanda) succeeded him in both appanages. But in 1287, while Ananda retained the position of prince of An-hsi, that of prince of Ch'in was given to his younger brother Altan-buqa, who resided at K'ai-ch'êng, and was in charge of the defence of the Liu-p'an-shan (cf. YS, 60, 3 a; 107, 9 a; 108, 1 b). From a letter written in 1318, it seems that Altan-buqa was then still alive; it may have been because of his death that K'ai-ch'êng-fu was degraded to K'ai-ch'êng-chou in 1323. A summer resort, the 清暑樓 Ch'ing-shu-lou, created by the prince of An-hsi under the Yüan, still existed in modern times at the Liu-p'an-shan (cf. *Ta-Ch'ing i-t'ung chih*, 201, 2 b); I cannot say whether this prince was Mangala or Ananda. Altan-buqa had married \*Ärä'öl, who was baptized as Sarah, and who was the second daughter of the Christian king of the Öngüt Ai-buqa and sister of Polo's King George (see «George»); she must have died in 1314, at the latest. At the beginning of the present century, Pognon saw at Diarbekir in Syria a Syriac gospel, written in 1298 for \*Ärä'öl, probably in the region of the Liu-p'an-shan (cf. *TP*, 1914, 635, to which I have added the above information hitherto unpublished). At the beginning of the Ming dynasty, several battles were fought against Mongol troops at the Liu-p'an-shan. In the *Chin shih* (123, 5 b), we read that «In the fourth *chêng-ta* year, in the spring (January 19-April 17, 1227), the 'great' troops (= the Mongol army) came (from the) west and decided to spend the summer at Lung-tê.» This also refers to the Liu-p'an-shan, located 20 *li* east of the *hsien* of Lung-tê.

The *hsien* of Ch'ing-shui, formerly belonging to Ch'in-chou and now to Wei-ch'uan-tao, is far to the south of the Liu-p'an-shan, on a small affluent of the Wei River. This tributary, called the Ch'ing-shui, is formed of two branches, and, if the text of the YS is to be trusted, the «Western River» (Hsi-chiang) must be the one to the west, or, more strictly speaking, to the north-west; it does not cross the district city of Ch'ing-shui. This «Western River» was also known as 秦水 Ch'in-shui, and was itself formed by the conjunction of several streams, one of which passed through the 秦川 Ch'in-ch'uan, or «Ch'in Valley» (*Ta-Ch'ing i-t'ung chih*, 210, 4 a). But it is a mistake of T'u Chi (3, 31 b) to adduce, by way of confirming the text relating Chinghiz-khan's stay at the Hsi-chiang in 1227, a passage in the biography of the Öngüt \*Anjur (or \*Aljur), according to which «[the imperial progress] made a halt at Ch'in-ch'uan» (駐蹕秦川); the actual text (YS, 121, 4 a) says that, in the course of the campaign of 1227, \*Anjur «halted his troops at Ch'in-chou» (駐兵秦州), and this text is followed by T'u Chi himself elsewhere (47, 5 b). On the other hand, 高寶銓 Kao Pao-ch'üan, in his 元秘史李注補正 *Yüan pi-shih Li chu pu-chêng* (14, 2-3; cf. also 15, 2 b), after copying the passage of the *pên-chi* of YS on the death of Chinghiz-khan, maintains that Ch'ing-shui here means the 清水河 Ch'ing-shui-ho, or Ch'ing-shui River, which takes its rise in the territory of Ku-yüan, runs to the north-west and flows finally into the Huang-ho about long. 130° 20'. This would also be the Ch'ing-shui mentioned in Čayān's biography for the time of Ögödäi, a region in which there

was a place called 荅蘭荅八 Ta-lan-ta-pa. KAO's identification cannot be retained. He was led to adopt it by «Sanang Setsen» who says that Chinghiz-khan died at Ling-chou, and the course of Ch'ing-shui-ho stretches between the Liu-p'an-shan and Ling-chou. But the text of the YS speaks not of the Ch'ing-shui-ho, but of the «district» (*hsien*) of Ch'ing-shui, which, beyond all doubt, was then situated, as it is now, on the Ch'ing-shui River which flows into the Wei. As to the Ch'ing-shui of the biography of Čayān, the identification is more difficult. The Ta-lan-ta-pa mentioned within its territory is a Mongolian place-name, very probably Dalan-dabā (<Dalan-daba'a), «Seventy Passes». It is exceptional to find a Mongolian place-name in a territory which goes by a Chinese designation; when this occurs, the place is generally found to be in China rather than in Mongolia. Yet, the natural inference is that Čayān, after the campaign to the «south of the [Huang-]ho» with Ögödäi, returned to Mongolia. At any rate he «returned north», which precludes the possibility that Ch'ing-shui might here mean either the district of Ch'ing-shui or the Ch'ing-shui-ho, both of which are south of the Huang-ho. Ta-lan-ta-pa, on the other hand, is hardly to be distinguished from the «region» (地) of Ta-lan-ta-pa (龍), where Ögödäi was in the fifth month of 1234 (YS, 2, 2 a), from the «region» of Ta-lan-ta-pa[八]-ssü[思] (Ta-lan-ta-pa-ssü in *Shêng-wu ch'in-chêng lu*, 69 a) of 八里里 Pa-li-li, where he was in the autumn of the same year (YS, 2, 2 b), and from the «region» of Ta-lan-ta-pa-ssü where Güyük was elected Great Khan in 1245 (YS, 2, 3 a). It is also, probably, the دالان تابان Dālān-tābān of *Ber*, I, 70, and certainly the طالان ديس Ṭūlān-dābās misread as «Ṭālān-dābsāng» in *Bl*, II, 41, and in «Ṭālān-dābān», *ibid.*, Appendix, 25-26 (the only difficulty is that we should expect *daba'an* [*Secret History*, § 146], even when it occurs as *daba'a* [*ibid.*, §§ 199, 251, 254], to form its plural as *daba'at* [as *ibid.*, § 247], not as \**daba'as* > \**dabās*; but there are many such irregularities). Now, it is impossible to believe, with Kao Pao-ch'üan, that the \*Dalan-Dabā or \*Dalan-Dabās to which Ögödäi resorted in 1234 and where Güyük was elected in 1245 lay in Kan-su; it was certainly in Mongolia, and I think that such was the case also for the \*Dalan-Dabā of Ch'ing-shui mentioned in Čayān's biography. Consequently, there must have been in Mongolia a Ch'ing-shui, of which we know nothing. I have discussed the name at some length because I shall have to revert to it further on.

The text of the *Yüan shih*, however precise it may seem to be as to the date and the place of the death of Chinghiz-khan, does not represent the only tradition of this event to be found in Chinese texts. The *Cho-kêng lu*, written in 1366, *i. e.* before the fall of the Mongol dynasty, says (I, 11 a) that Chinghiz-khan died in the Sa-li Valley, and the date it gives is identical with that of the *Yüan-shih*. But GAUBIL (*Hist. de Gentchiscan*, 51) as well as the *Tz'ü yüan* and the *Chung-kuo ti-ming ta tz'ü-tien* (*s. v.* «Liu-p'an-shan») state that Chinghiz-khan died at the Liu-p'an-shan. In his 元史新編 *Yüan shih hsien pien* (2, 13 b), 魏源 WEI Yüan (1794-1856) changes the text of the YS to «he died in the *hsing-kung* of the Sa-li Valley of the Liu-p'an-shan». Now, it is of course impossible that GAUBIL, WEI Yüan and the modern Chinese works of reference should reflect the opinion of Rašidu-'d-Dīn (which will be discussed later). Even in the YS, though with less precision, the passage of Čayān's biography translated above might suggest that Chinghiz-khan died at the Liu-p'an-shan. T'ü Chi (3, 31 b) considers that Chinghiz-khan died after he had left the Liu-p'an-shan for the south, when trying to turn the Chin positions by passing on to Sung territory. He adduces, however, but rejects, the following passage which he says occurs in the

*pên-chi* of the Emperor Li-tsung in the *Sung-shih*, under the third *pao-ch'ing* year (A. D. 1227) : « [In the third *pao-ch'ing* year,] . . . the chief (主 *chu*) of the Mêng-ku (= Mongols) T'ieh-mu-chên (Tämüjin) died (殂 *ts'u*) at the Liu-p'an-shan. » But in fact there is no such text in the *Sung shih*, under the year 1227 (41, 3 a-b). Nor should we expect to find it there, at least in such terms. Both the *Chin shih* and the *Sung shih*, compiled under the Yüan dynasty, always speak retrospectively of the Mongols of Chinghiz-khan as « Ta-Yüan », « Great Yüan », never as « Mêng-ku », and give to Chinghiz-khan himself the title of *huang-ti* or *ti*, « Emperor », not merely of *chu*, « chief »; nor is the word *ts'u*, « to die », the term to be used for an « imperial » death. As a matter of fact, both the *Chin shih* (under the fourth *chêng-ta* year, A. D. 1227; 17, 3) and the *Sung shih* deliberately ignore the death of Chinghiz-khan, which in fact is alluded to merely in an indirect way by the *Chin shih* when we are told that, at the beginning of 1228, an envoy was sent to the « Great Yüan » to « express condolence ». But although the source is wrongly indicated by T'ü Chi, the passage he quotes was certainly not invented by him. Either it is a Chinese rendering of Rašid's statements by an earlier modern scholar, or, as the case certainly is with one of GAUBIL's accounts of Chinghiz-khan's death, it is to be found in some historical work of early Ming date. GAUBIL's direct or indirect source, which must be the same as that of WEI Yüan as well as that of the *Tz'ü yüan* and of the *Chung-kuo ti-ming ta tz'ü-tien* can be identified with almost absolute certainty : it is the « continuation » of the *T'ung-chien kang-mu* composed by 陳經 Ch'ên Ching, in 24 chapters, under the title of 通鑑續編 *T'ung-chien hsü-pien* : there Chinghiz-khan is made to die at the Liu-p'an-shan. Ch'ên Ching lived towards the end of the Yüan and saw the beginnings of the Ming dynasty. Though often said to be a Ming writer, the first edition of his *T'ung-chien hsü-pien* was engraved under the Yüan, in 1361 in fact (cf. *Ssü-k'u ch'üan-shu*, 47, 49-51; Mo Yu-chih's *Lü-t'ing chih-chien ch'uan-pên shu-mu*, 4, 17 a). Ch'ên Ching is not always to be trusted; his work, however, is earlier than both the *Cho-kêng lu* and the *Yüan-shih* and sometimes gives more correct names than the official history. On the other hand, writing under the Mongol dynasty, he certainly could not refer to Chinghiz-khan as « Tämüjin, the chief of the Mêng-ku », not at least in the edition of 1361, and so cannot be the author of the passage erroneously quoted by T'ü Chi as taken from the *Sung shih*. Unfortunately the *T'ung-chien hsü-pien* is a rare book (not only in the original edition of 1361, but even in the Ming editions, one of which was published in 1562), and I can only quote the passage on the death of Chinghiz-khan from NAKA's *Chingisu-kan jitsuroku*, 579. From the *T'ung-chien hsü-pien*, the tradition passed into the *Hsü t'ung-chien kang-mu* compiled in 1476 by 商輅 Shang Lu, where we read (19, 13; cf. *Pa*, 183) that « Tämüjin of the Mêng-ku died at the Liu-p'an-shan ». I have no doubt that this is the text which T'ü Chi erroneously quoted as from the *Sung shih*.

The *Shêng-wu ch'in-chêng lu* (64 b) merely says that in the autumn of 1225, Chinghiz-khan started again with his army to march against the Hsi-Hsia, reached their country in the spring of 1226, in the course of a year conquered all their cities, and, in 1227, « destroyed their kingdom and returned » (滅其國以還). The next sentence relates to what occurred after Chinghiz-khan had gone to Heaven.

The second Chinese tradition, which makes Chinghiz die at the Liu-p'an-shan, falls in remarkably well with part of Rašidu-'d-Dîn's account. According to Rašid (*Ber*, III, 94-99, 118-

119), Chinghiz moved towards «Hašin» (Mong. Qašin < Ch. 河西 Ho-hsi) or «Tangqut» (*i.e.* Hsi-Hsia) in the autumn of 1225 (instead of the beginning of 1226 as in YS). Once in the Hsi-Hsia country, he captured the cities of قامجو Qamjū (Kan-chou), سجو S.jū (Su-chou), قاجو Qājū (Ho-chou) and ارقی Urūqāi, besieged the city درمکای Dörmägäi (Ling-chou; misread «Dersekai» in *Ber*) and burnt it. In this sentence S.jū must be Su-chou, but the name is taken from some written text, not from the current form of the name in Central Asia, which was then \*Sukčü; see «Succiu». Urūqāi is اراقی Urāqāi of *Ber*, III, 12 (misread «Iraqai»); it is the 兀刺海 Wu-la-hai of YS, I, 6 b, and 60, 13 a, the 斡羅孩 Wo-lo-hai (\*Oroqai) of *Shêng-wu ch'in-chêng lu*, 47 a, the Uraqai of the *Secret History*, § 267, perhaps the 兀納刺 Wu-na-la of YS, 129, 7 a (this is also the 兀納 Wu-na of the inscription of the family temple of Li Hêng written by Yao Sui, as given in the *Yüan wên lei*, 21, 1 b; in the edition of the literary works of Yao Sui, or *Mu-an chi*, recovered from the *Yung-lo ta-tien*, but «corrected» by the Ch'ien-lung Commissioners, the name has been replaced by *mou-mou*, «so-and-so»). Although not identified, it must have been in Kan-su proper, north of the Huang-ho; the identifications in 李文田 Li Wên-t'ien's commentary of the *Secret History*, 14, 7 (north of Ku-peï-k'ou in Ho-peï!), and in KAO Pao-ch'üan [*loc. cit.*, 14, 2 a] or T'ü Chi (3, 7 a, «Alaq-ola», three *li* west of the *hsien* of Shan-tan and 30 *li* east of Kan-chou, on account of phonetic analogy!) are valueless. The Hsi-Hsia sovereign sallied forth from his city of Irqai, which the Mongols call Eriqāya (see «Egrigaya» = Ning-hsia), with fifty *tümän* («myriads»; see «Toman») of men, but was defeated; more than half his men were killed, and he retired back into the city. Taking no more account of him, Chinghiz-khan went to the south. In the beginning of spring of the dog year (1226), he arrived at اوتقون طالان قودوق Utqun-Talan-Quduq (or اويقون اوتقون Uiqun-Talan-Quduq; cf. ERDMANN, *Temudschin*, 639; BEREZIN's and D'OHSSON's [*Oh*, I, 379] reading «Ongon-Talan-Quduq» is improbable, since Rašid's habit, in such a case, would be to write «\*Ongqun»; «Talan-Quduq» means «Seventy Wells» [not «Well of the Steppe» as in *Oh*, I, 379]; with the rejection of «Ongon», the identification proposed by T'ü Chi, 3, 30 b, and by K'ö Shao-wên [cf. *Ch*, I, 189], geographically untenable, lose even the appearance of phonetic analogy; the place in fact remains unidentified). There he had a dream, which foretold that his end was near. Two of his sons, Ögödäi and Tolui, were with the army; he gave them his instructions, and sent them back to Mongolia. Moving himself to the south, he arrived at the ليون شان Liū-bān-šān (also ليون شان Liū-bān-šān in *Bl*, II, 326-327; read «Liū-pān-šān» = Liu-p'an-shan; the «Leung-Shan» in *Y*, I, 245, which YULE took from ERDMANN, *Temudschin*, 443, 640, and seems to have identified with the 隴山 Lung-shan, is a misreading of Liū-bān-šān), which is at the meeting-point of the three territories of the Jürčä (= Chin), the «Nangias» (= Sung) and the «Tangqut» (= Hsi-Hsia; although the Sung boundary actually ran some distance south of the Liu-p'an-shan, this is substantially correct and shows that the Persian historian had heard of the importance of the site). Envoys from the Chin arrived there with presents (this is confirmed by YS, I, 9 b, translated above, and by *Chin shih*, 17, 3 a). The Hsi-Hsia sovereign also made his submission, but Chinghiz-khan, making an excuse of illness, did not grant him audience, and left him under the guard of Tolunčärbi. The Emperor's disease, however, was getting worse every day. His last instructions were to conceal the news of his death from the Hsi-Hsia people (of Ning-hsia) until all of them had come out of the city as had been agreed upon, and then to massacre them. The date in the «pig»



year (1227) given for his death by Rašid, in *ramadān* 624 (août 1227) has been discussed above. After his death, the Mongol leaders, killing all those they met on the way so that the news should not spread, carried Chinghiz-khan's coffin back to the *ordo* in Mongolia. His death was then announced, and the wailing began. Nothing in the text suggests that Chinghiz-khan, after he had arrived at the Liu-p'an-shan, should have left it before he died. In the account of Mongka's reign, Rašid recalls that Chinghiz fell ill (*rān'vār*) and died at the Liu-p'an-shan (*Bl*, II, 325). The account in Abū-'l-Ghāzī (DESMAISONS, 141-142) is but a distorted iteration of that in Rašid. It will be noticed that Rašid agrees with the *Shêng-wu ch'in-chêng lu* in making Chinghiz-khan start against the Hsi-Hsia in the autumn of 1225, and not in the spring of 1226 as in the *YS*.

The account in the *Secret History* (§§ 265-268) is not clear. It makes Chinghiz move against Tangut (= Hsi-Hsia) in the autumn of the « dog » year (1226). In the winter, Chinghiz was hunting wild horses (*qulan*) at Arbuqa when his affrighted horse Josotu-boro (« Reddish-Grey ») threw him and he was gravely injured. He then went to Čo'orqat, and the next morning his wife told the princes and the nobles that he had had a high temperature (*qala'un*) in the night. The princes and nobles wished to retreat, and return when the fever had subsided. But Chinghiz, to whom this was reported, said that in such a case the Hsi-Hsia people would think that he was afraid of them and that he wanted to nurse himself at Čo'orqat. Envoys were dispatched to reproach the Hsi-Hsia sovereign with his past attitude; but they brought back a fierce answer, which enraged Chinghiz-khan. Feverish as he was, « I should rather die » he said, and, marching at the head of his troops, he won a great victory over the Hsi-Hsia commander Aša-gambu in the Alašan region (see « Caraunas »). Chinghiz-khan spent the [next] summer (1227) at Časutu (the « Snowy »), 雪山 Hsüeh-shan (« Snowy mountains ») in the Chinese translation. Leaving the Časutu, he went on to Uraqai (cf. above), and passing Uraqai, laid siege to Dörmägäi (= Ling-chou). At this juncture, the Hsi-Hsia sovereign came to render homage and offer presents. Chinghiz made him wait three days at the entrance of his tent, and then ordered Tolun-čärbi to kill him (Tolun is the would-be « Koang-luen » of *Ch*, I, 203). « In the pig year (1227), Chinghiz-khan went to Heaven. »

It seems clear that the compilers of the *Secret History* have somehow gone astray in relating the succession of these events, and the situation has been made worse by the commentators. The mention of the « pig » year (1227) comes only in the last sentence, whereas the seasons indicated in the text would suppose that the stay at the Časutu should have been dated 1227. On the other hand, the passage relating to Uraqai and Dörmägäi surely belongs to 1226. Aša-gambu plays in the *Secret History* the same part as Wei-ming *ling-kung* in the *YS*; both may be in fact different names of the same man; *gambu* (< Tib. *sgam-po*) is a title, like Ch. *ling-kung* (> Mong. *lingqu*, *lingqun*, *lingqum*); Aša may be his personal name; Wei-ming is a family name. Unfortunately, the place-names are mostly unidentifiable. LI Wên-t'ien had the strange idea (14, 1-2) that the compilers of the *Secret History* had by mistake dated in 1226, during the campaign against the Hsi-Hsia, an equestrian accident which we know from the *Hsi-yu chi* to have occurred in 1223 in the West, when Chinghiz-khan was shooting a boar (cf *Br*, I, 97) : consequently, Arbuqa is identified with Almalıq, Čo'orqat with Samarkand and the « Snowy mountains » with the Hindukush (!). K'o Shao-wên (*Hsin Yüan-shih k'ao-chêng*, 3, 11 a) concurs with LI Wên-t'ien. According to 施元杰 SHIH Yüan-chieh and KAO Pao-ch'üan (*loc. cit.* 14, 1-2),

Arbuqa is phonetically the same as \*Arbutan (a mountain to the north-west of the *hsien* of P'ing-lo, outside the great bend of the Yellow River; cf. also POPOV, *Mên-gu yu-mu czi*, 49, 313), and Čo'orqat the same as Čoqor-moritu («Having a pie-bald horse», also a place of the Ordos; cf. POPOV, 51). But there is no phonetic resemblance between the names; moreover, Chinghiz travelled from Mongolia to the Etsin-yol, not by crossing the great bend of the Huang-ho. The fall from his horse at Arbuqa referred to in the *Secret History* led to the same counsels as the dream at \*Utqun-Talan-quduq in Rašidu-'d-Din; but there is no other connection between the names or the facts. Čo'orqat (= Čo'oryat) is the plural of Mong. *čó'oryan* and means «The Locks» (*i. e.* Key-locks); it may have been applied figuratively to some strong position, and may actually refer to the same place as the \*Qunčui Mountain of the YS. For the Časutu (or Hsüeh-shan, Snowy Mountain) Kao Pao-ch'üan says that there is a Hsüeh-shan to the south of Kan-chou (cf. *Ta-Ch'ing i-t'ung chih*, 205, 1 b); but this is a vague designation, and the maps of the *Ta-Ch'ing i-t'ung chih* (197, 1 a; 201, 1 a) show to the west of the Ch'ing-shui-ho two other «Hsüeh-shan» which are not otherwise referred to in the text. In the *Secret History*, Chinghiz-khan's stay at the Časutu is mentioned after the great battle with the Hsi-Hsia army and before the passage of Uraqai and the attack on Dörmägäi (Ling-chou). If I am right in supposing that the attack on Ling-chou ought to have been referred to before the mention of the summer resort at Časutu, it may well be that Časutu, in the present case, is but a Mongolian designation of the Liu-p'an-shan.

In «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 101-107), Chinghiz-khan besieged Türmägäi (= Dörmägäi, Ling-chou). During the siege, he and the Hsi-Hsia sovereign transformed themselves into various animals, a folklore theme of the Magali type. Chinghiz finally killed the Hsi-Hsia sovereign and took hold of his wife Kürbäljin-yoa, but was mortally wounded by her at night, and died at Dörmägäi. The date given, August 25, 1227, like so many in «Sanang Setsen», seems to be borrowed from Chinese sources connected with the *Yüan shih*.

Half a century before «Sanang Setsen», the same folkloristic elements are found in the *Altan tobči*, but with further historical data of some interest. There we read (GOMBOEV transl., in *Trudy VOIRAO*, VI, 144): «Having reduced the Tangut people to submission, killed Šudurγu-khan (the Hsi-Hsia sovereign), sacked the city of Türmägäi, and taken the queen Gürbäljin-yoa, the Emperor spent the summer of this campaign in the place [called] Luban-ḥan ('Luuban-ḥan' in the Mongolian text, p. 375). Later, he fell ill at Türmägäi. . . [and] died in the year *bing-pig*, at the age of sixty-seven, on the fifteenth of the month.» There are here evident contradictions: a «*bing-pig*» year, *i. e.* 丙亥 *ping-hai*, is impossible in the sexagenary cycle; but we may suppose this to be a clerical error for «*ding-pig*», *i. e.* 丁亥 *ding-hai* (1227). On the other hand, there can be no doubt that the Hsi-Hsia sovereign was not killed before Chinghiz-khan «escaped the heat» in the Liu-p'an-shan. Yet, it is noteworthy that the tradition of the summer station at the Liu-p'an-shan has been preserved in a text which is relatively free from late Chinese influence, and where the name of the mountain has been mongolized (*šan* becoming *ḥan*, from phonetic analogy coupled with the Mongol habit of calling the mountains *ḥan*, «khan», «sovereign»).

We are now in a position to examine the different opinions which have been expressed on the «*hsing-kung* (*ordo*) of Ha-lao-t'u in the Sa-li Valley». A few modern scholars have merely retained the text of the YS and taken it at its face value, declaring Chinghiz-khan to have died on

the territory of the *hsien* of Ch'ing-shui. We have seen that others had decided in favour of the Liu-p'an-shan, so that it is there that we should have to look for the *hsing-kung*. Something may be said in favour of this solution, which requires a minimum of emendation in the text of the *YS*, and is in agreement with Rašidu-'d-Dīn. Although *hsing-kung* literally means « moving palace », and is in Chinese the term for buildings prepared for the temporary residence of the Emperor when he was out of his capital, in the Mongol period it is invariably the equivalent of the Turk. *ordu*, Mong. *ordu* > *ordo*, and *ordo* is above all the designation of a « royal encampment » of a semi-sedentary character, particularly of the place where the women stay while the men are engaged in far-away service. It is improbable that there should have been an *ordo* at Ch'ing-shui, but there might have been one on the Liu-p'an-shan. Yet even this is open to doubt. When, in 1258, Mongka left his impedimenta, the *oyruq* of mediaeval Mongol texts, at the Liu-p'an-shan, the Chinese account speaks of his « baggage », not of this *hsing-kung* or *ordo*; the *ordo* remained in Mongolia.

Of modern scholars, HUNG Chün (I B, 44-47) maintained that Ha-lao-t'u must be the same name as 哈柳圖 Ha-liu-t'u (the name of one of the streams which unite with the 金河 Chin-ho to form the 無定河 Wu-ting-ho, flowing from west to east, south of Yü-lin in the Ordos country). The Mongolian name of the Chin-ho (« Gold River ») being Šira-usu (or Šara-usu, « Yellow River »; cf. POPOV, *Mên-gu yu-mu czi*, 321), HUNG Chün sees in it the « Sa-li Valley » of the *Yüan shih*. According to him, Chinghiz-khan died in this part of the Ordos which gave rise to the later Mongol traditions about his tomb being in the Ordos, although the remains of the conqueror had actually been carried to Northern Mongolia. But Ha-liu-t'u represents a Mongolian name Qali'utu, « Place with otters » (*qali'un*), and surely is not the same as Ha-lao-t'u. Nor it is possible to connect the name of the « Sa-li Valley » with Mong. *šira*, *šara*, « yellow », as will be shown further on. HUNG Chün's identifications, based only on erroneous phonetic analogy, cannot be sustained.

Another theory, which has been adopted by T'U Chi (2, 9 b; 3, 31 b-33 b) and by K'o Shao-wên (*Hsin Yüan shih*, small ed., 3, 17 a), and which from K'o has passed into *Ch*, I, 190, is that Chinghiz-khan died at Ling-chou (« Dörmägäi »), and that his coffin was carried thence to the « Sa-li Valley » in Northern Mongolia. Probably under the influence of « Sanang Setsen », HAENISCH thought that Ha-lao-t'u, being given in the *YS* as the place where Chinghiz died, must be in the neighbourhood of Ling-chou (cf. *TP*, 1935, 164). But the death at Ling-chou, as indicated in « Sanang Setsen » (and, I may add, in the *Altan tobči* half a century earlier), is a late tradition, coming among many others devoid of historical value, and we need not attach any importance to it. The location of the « Ha-lao-t'u *ordo* of the Sa-li Valley » in Mongolia makes a better case.

As far as I am aware, this location was first indicated by NAKA (*loc. cit.* 578), who referred to the « Palace map » (內府輿圖 *Nei-fu yü-t'u*), in which there are a 嚙老台嶺 Ko-lao-t'ai-ling (« Ko-lao-t'ai Pass », or « Ko-lao-t'ai Range »), a Ko-lao-t'ai River (*ho*) and a Ko-lao-t'ai Lake (泊 *po*). To these T'U Chi added the « Qariltu na'ur » or Qariltu Lake of the *Secret History*, § 136, so that in his text (3, 32 b), Chinghiz-khan's coffin was carried to « the *hsing-kung* of Qariltu-na'ur »; WANG Kuo-wei (*Shêng-wu ch'in-chêng lu*, 13 b) follows in his wake and says that the Qariltu Lake is the same as the Ko-lao-t'ai Lake. NAKA (134), knowing better, did not connect the two names

which clearly have nothing in common. But the identification of Ha-lao-t'u with Ko-lao-t'ai is more tempting, since both names are clearly adjectives, and the adjectival endings *-tu* and *-tai* are freely interchangeable in Mongolian. BIČURIN (*Istoriya pervykh četerëkh khanov*, 137) had said that Chinghiz-khan died *v Kharatuskom putevom dvorcé*. This is the origin of the camp of « Caratouski » in *Oh*, I, 378, copied in *Pa*, 183, and as « Karatouski? » by CHAVANNES in *TP*, 1905, and it even led CHAVANNES to read the name as Ha-lao-t'u-chih [之] instead of Ha-lao-t'u (an error repeated by CORDIER, *Hist. gén. de la Chine*, II, 222). But « Kharatuskii » is merely a Russian adjectival form derived from *Haratu* = Ha-la-t'u, the Ch'ien-lung's « reformed » spelling of Ha-lao-t'u, and, like all these « reformed » spellings, it must be abandoned. Ha-lao-t'u may represent \*Qara'utu, \*Qala'utu, \*γara'utu, \*γala'utu even \*Hara'utu (> \*Ara'utu) or \*Hala'utu (> \*Ala'utu); Ko-lao-t'ai represents in principle \*γarautai, or \*γalautai but may also be \*Qarautai or Qalautai. Three at least of these forms are actually known or would be correct in Mongolian: *qara'utai*, « blackish » (*Secret History*, 6, 100, 244, and see « Caraunas »), \**qala'utai*, « feverish », \**γala'utai*, « place with wild geese ». For reasons to be given further on, I incline to the last interpretation (cf. also *TP*, 1935, 165).

While we know the *hsing-kung* of Ha-lao-t'u only from the *YS*, we have almost too many mentions of the « Valley of Sa-li », and they have given rise to the theory that there were at least two « Valleys of Sa-li » in Mongolia and a third one north of Peking (cf. T'u Chi, 3, 33 a; *Shêng-wu ch'in-chêng lu*, WANG Kuo-wei's commentary, 3 a); a fourth one should be added if the « Sa-li Valley » where the *YS* makes Chinghiz-khan die were to be located in Kan-su.

In the case of Kan-su, one point must first be made clear. 撒里 Sa-li may in principle render the Turk. *sarıy*, « yellow ». This is the case with the 撒里畏吾 Sa-li Wei-wu of *YS*, 121, 1 b, the 駸里畏吾 Sa-li Wei-wu of *YS*, 122, 5 a, who are the *Sarıy Uiyur*, or « Yellow Uighur », of south-western Kan-su (now known under the mongolized name of Šara-Yoyur). In other cases, Sa-li renders the Turk. and Mong. *Sali* in names of individuals (cf. *YS*, 3, 2 b [cf. *Ber*, I, 62-63]; 130, 6 a; 195, 2 b). But there is no reason why there should be a Turkish name of a place in central Kan-su. In the 13th cent., a foreign name could only have been given to this Chinese part of the province by the Mongols and in Mongolian; moreover, Sa-li must be Mongolian, since it appears together with Ha-lao-t'u, the Mongolian character of which is beyond dispute.

The would-be homonymous place to the north of Peking must be left out of account. It is the one called Šira-kä'är, « Yellow Steppe », in the *Secret History* (§§ 67, 252); the Chinese name, corrupt in the *Shêng-wu ch'in-chêng lu* (54 b), was probably 黃甸 Huang-tien (> 王甸 > 壬甸), of identical meaning. *Kä'är*, classical Mong. *kä'ärä*, « steppe », also occurs in the names of the Jä'ärän-kä'är, « Antelope Steppe », transcribed in the *YS* in various ways (cf. WANG Hui-tsu<sup>2</sup>, 49, 7 a), and of the Bu'ura-kä'är, « Male camel Steppe » (< Turk. *buyra*; *Secret History*, §§ 105, 109, 152). But the place name in Mongolia corresponding to the « Sa-li Valley » of the Chinese texts is always written Sa'ari-kä'är in the *Secret History* (§§ 128, 161, 177, 193, 197, 250), whereas the word for « yellow » invariably appears there as *šira* (§§ 12, 200, 246, 274; > mod. Mong. *šara*). The meaning of *sa'ari* is not open to doubt, and the two words should be strictly distinguished (CHARIGNON [*Ch*, I, 197] repeats the erroneous explanation of *sa'ari* as « yellow »). The Mong. *sa'ari*

(< Turk. *sayrt*) > Kalm. *sār'*, *sār'*, Manchu *sarin*, means «loins». It is generally used as the designation of the hide of the croup of a horse or donkey; hence our «shagreen» (see «Camut»); but, in the only case where it occurs in the *Secret History* (§ 140), it refers to the loins of a man. The word *sā'ūji*, which also means «loins», is sometimes used in the figurative sense of a rounded «hill» (cf. *infra*). I think that in the same manner, Sa'ari-kä'är, lit. «Loin Steppe», actually means «Hilly Steppe». In Chinese texts, the place name is rendered 薩里河 Sa-li-ho, «Sali River» (YS, I, 2 a, 3 b; *Shêng-wu ch'in-chêng lu*, 3 a, 20 a), 薩里川 Sa-li-ch'uan, «Sa-li Valley» (*Shêng-wu ch'in-chêng lu*, 18 a), 撒里怯兒 Sa-li-ch'ieh-êrh (YS, 31, 4 a; 金幼孜 Chin Yu-tzü's 北征後錄 *Pei-chêng hou-lu*, *Ku-chin shuo-hai* ed., 4 b). The Persian transcription is ساریکهر Sāri-kähār (*Ber*, II, 92, 115, 118; III, 107). BIČURIN'S «Saligol» (*loc. cit.* 137), reflected in the «Sali Kol» of *Oh*, I, 378, the «Sari gool» of *Pa*, 183, and the «Sari gol» of *TP*, 1905, 3, does not exist.

It may be, although I am inclined to doubt it, that there were two Sa'ari-kä'är, one in the upper basin of the Kerulen, the other more to the west. At any rate, we are only concerned here with the first one, lying in the region where Chinghiz-khan first asserted himself as a great leader.

It cannot be doubted that this Sa'ari-kä'är was located in the upper basin of the Kerulen. Chinghiz-khan's traditional birthplace (*Secret History*, § 59) was at «Däli'ün-boldağ of the Onan (> Onon)», near the source of the latter river; on the other hand, the Burqan-qaldun Mountain, where both the Onon and the Kerulen take their rise, plays a great part in the history of Chinghiz-khan. As mention is made in the *Shêng-wu ch'in-chêng lu* (20 a) of Ong-khan's stay at «the Pu-lu-ku Cliff of the Sa-li River» (薩里河不魯古崖), WANG Kuo-wei (3 a) was of the opinion that Pu-lu-ku was another transcription of Burqan, and that, consequently, the Sa'ari-kä'är should be located in the neighbourhood of the Burqan-qaldun. But there is in fact no connection between the two names; the «Pu-lu-ku Cliff» is not the Burqan-qaldun, but the Bürgi-ärgi, «Bürgi Cliff», of the *Secret History*, §§ 96, 177, the exact location of which is unknown (it lay in the neighbourhood of the source of the Kerulen; but the *Secret History*, safer than the much briefer *Shêng-wu ch'in-chêng lu*, does not connect it with Sa'ari-kä'är).

We have, however, other means of ascertaining the location of Sa'ari-kä'är. In 1328, while waiting for the arrival of his elder brother 和失刺 Ho-shih-la (忽世朮 Hu-shih-la in the *Yüan tien chang*; \*Kusala; «Ko-sa-la» in HIRTH, *Gesch. des Buddhismus*, II, 36-37, 166; canonized as Ming-tsung), who was in Mongolia, 圖帖睦爾 T'u-t'ieh-mu-êrh (Tuγ-Tämür; «T'og-t'e-mur» in HIRTH, *ibid.*; cf. *TP*, 1930, 57-61; not «Tup Timur» as in GILES, *Biogr. Dict.*, Nos 663 and 2110; canonized as Wên-tsung) had provisionally ascended the throne in Northern China. \*Kusala, alias Ming-tsung, was himself enthroned north of Qara-Qorum on February 27, 1329. On March 31, he halted in the region (地) of 潔堅察罕 Chieh-chien-ch'a-han, \*Gägän-čayän, the «Brilliant White» (over 70 li north of Qara-Qorum; cf. YS, 58, 18 a) and a few days later decided to proceed to Shang-tu; the YS (31, 2 a-4 b) gives the stages of his progress to the east (cf. also *Hsi-yü shui tao chi chiao-pu*, *Ch'ên-fêng-ko ts'ung-shu* ed., 10-11; T'u Chi, 3, 33 a). «On May 29, he halted (次 *tz'ü*) in the region (地) of 朶伯真 To-pai-chên (Dörbäljin, a common name for quadrangular enclosures). . . . On June 1, he halted «east of the Wo-êrh-han-mu» (斡耳罕木東; the text is here certainly corrupt; T'u Chi [3, 33 a; 14, 4 b] tacitly corrected it to

« Wo-êrh-han shui [水] tung », « east of the Orkhon River », which is quite possible; but another no less natural correction would be « Wo-êrh-han-mu-lien [連] », he halted at the Orhan-mürän, *i. e.* at the Orkhon). . . On June 4, he halted in the region of 必忒怯秃 Pi-t'ê-ch'ieh-t'u (\*Bitäkätü, « Wen-necked? »). . . On June 13, he halted at 探秃兒海 T'an-t'u-êrh-hai (\*Tamturqai; cf. *tamturaqai*, « broken »). . . On June 16, he halted at 秃忽刺 T'u-hu-la (mechanically transcribed from a Mongolian document, instead of the correct pronunciation T'u-wu-la, Tu'ula in the *Secret History*, §§ 96, 104, 115, 164, 177, the Tula [Tūla] River). . . On June 20, he halted east of the T'u-hu-la River (河 *ho*; *i. e.* east of the Tula). . . On June 22, he halted in the region of 斡羅斡秃 Wo-lo-wo-t'u (\*Oro'otu). . . On June 23, he halted in the region of 不魯通 Pu-lu-t'ung (\*Burutung). . . On June 25, he halted in the region of 忽刺火失温 Hu-la-huo-shih-wên (\*Qulan-qoši'un, « Wild-horse Muzzle », or perhaps \*Hulān-qoši'un, « Red Promontory »; certainly the Hu-lan[蘭]-hu[忽]-shih-wên of the *T'ai-tsung shih-lu*, 92, 10 *b*, and of the *Ming shih*, 7, 1 *a*, and the miswritten Chi[急, instead of 忽]-lan-hu-shih-wên of the *Pei-chêng hou-lu*, 5 *a*, and of the ancient map of the *Yüan-shih lei-pien*). . . On June 28, he halted in the region of 坤都也不刺 K'un-tu-yeh-pou-la (\*Kündüi-bulaq, « Hollow Spring »). . . On July 1, he halted in the region of 撒里 Sali (\*Sāri < \*Sa'ari). . . On July 8, he halted in the region of 兀納八 Wu-na-pa (\*Unaba, « He fell »). . . On July 10, he halted in the region of 闊朶 K'o-to (\*Ködö, perhaps \*Ködö < \*Ködö'ä, « Barren ground »). . . On July 12, he halted in the region of 撒里怯兒 Sa-li-ch'ieh-êrh (\*Sāri-kār < Sa'ari-kä'är). . . On July 18, he halted at 哈里温 Ha-li-wên (\*Qali'un, « Otter », but perhaps misread for \*Ha-la-wên, \*γala'un, the substantive of which Ha-lao-t'u is the adjectival form). . . On July 19, he halted at 闊朶傑阿刺倫 K'o-to-chieh-a-la-lun (\*Ködögä-aralun; this is the « Ködä'ä-aral [« Barren Island »] of the Kälürän [= Kerulen] » of the *Secret History*, § 282, also written Ködä'ü-aral [§ 269] and K'ödö'ä-aral [§ 136]; the translators of the Mongolian document into Chinese have erroneously added to the name the -un ending of the genitive; the name occurs in the *Altan tobči* [GOMBOEV's ed., 41<sup>6</sup>, 42<sup>11</sup>, 43<sup>5</sup>, 44<sup>10</sup>], where « Ködö'ä-Arlan » and « Ködö'ä-Aralan » are misreadings of Ködö'ä-aral-a, « at Ködö'ä-aral » [the translations, pp. 147, 148, 150, are erroneous], and also in « Sanang Setsen » [SCHMIDT, *Gesch. der Ost-Mongolen*, 70<sup>6</sup>, 106<sup>26</sup>], where, through corrupt readings « Ködä'ä-arulada-yin » and « Ködä'ä-arulad-un », Chinghiz-khan becomes in the translation « qa'an of the Arulad » [the Chinese translation too (*Mêng-ku yüan-liu chien-chêng*, 3, 10 *b*, and 4, 7 *b*) did not recognize the proper name and merely speaks of « northern waste », 北郊 *pei-chiao*, and of « region », 地方 *ti-fang*, but at least does not give the absurd « Arulad »]; cf. also WANG Kuo-wei in *Shêng-wu ch'in-chêng lu*, 13 *b*-14 *a*, and 40 *a* [but two names, which have in fact nothing to do with Ködö'ä-aral, need not be taken into consideration : one is 只忒忽盧 Chih-t'ê-hu-lu, which corresponds to the Qit-qūluyāt-älät of *Ber*, II, 142, itself altered from \*Jit-qūluyāt-älät, « Sands of \*Jit-qūluyāt »; the other is the 鉞鐵鐸胡蘭 O-t'ieh-ku-hu-lan Mountain where Ögödäi died in 1241, *YS*, 2, 3 *b*, the 月帖古忽蘭 Yüeh-t'ieh-ku-hu-lan where Mongka went in 1252, *YS*, 3, 2 *a*, and which is the Ötägü-qulan of the *Altan tobči*, text, 43<sup>1</sup>, misread « Utuku-ḥulana » in GOMBOEV's translation, 148]). . . On July 22, he halted in the region of 哈兒哈納秃 Ha-êrh-ha-na-t'u (Qaryanatu [or Qarayanatu], « Place with Acacia-trees »). . . On July 24, he halted in the region of 忽秃 Hu-t'u (\*Qutuq?, « Bliss », perhaps misread for \*Quduq, « the Well »). . . On

August 4, he halted in the region of 孛羅火你 Po-lo-huo-ni (\*Boro-qoni, «Grey Sheep») . . . On August 15, he halted in the region of 不羅察罕 Pu-lo-ch'a-han (\*Boro-čayān?, «Grey-white»; it seems to be the same as Po[孛]lo-ch'a-han, at which Wên-tsung halted on September 1, after Ming-tsung's death) . . . On August 18, he halted in the region of 小只 Hsiao-chih (\*Sä'üji, «Loins»; used as place name with the meaning of «hill»; cf. RAMSTEDT, *Kalm. Wörterbuch*, 342 t, s. v. *sıldži*; in TIMKOVSKI, *Voyage à Pékin*, there is a Sä'üji-yin obō [I, 222], a mountain Sä'üji [II, 427-428], and a station of Sä'üji [II, 390-391]; this may be the one of our itinerary) . . . On August 25, he halted in the region of 王忽察都 Wang-hu-ch'a-tu (\*Ongyučatu, \*Ongyočatu, «The Place with boats [or with troughs]»; Ongyučatu or Ongyočatu occurs several times in YS; cf. WANG Hui-tsu<sup>2</sup>, 49, 8 b; YANAI [641, 643] locates it north of Kalgan, outside the Great Wall) . . . On August 26, Tuy-Tämür came to Ongyočatu and rendered homage to his elder brother Ming-tsung, who offered him a banquet. But on August 30, Ming-tsung «died by violence» (暴崩; cf. YS, 31, 4 b; 32, 5 b).

In this long series of place names, only a few give as yet a clue to the identification of Sa'ari-kä'är, but this clue is fairly clear. Ming-tsung, starting from a place north of Qara-qorum, i. e. north of the present Erdeni-dzō Monastery of the Orkhon basin, reached the Tula River and followed its course for four days; at the end of the fourth day, he halted east of it. This cannot be far from the «Ulan-daba» of our maps (at the eastern end of the southern bend of the Tula). The next guide-post is Ködä'ü-aral. Its location on the Kerulen is mentioned not only in the *Secret History*, but also in the YS (2, 1 a) : in 1229, Ögödäi was enthroned at a diet held in the region of 怯綠連河曲雕阿蘭之地 «in the region of Ch'ü-tiao-a-lan (\*Küdäü-aral) of the Ch'ieh-lü-lien River (Kälürän = Kerulen)». The text adds that the enthronement took place at 庫鐵烏阿刺里 K'u-t'ieh-wu-a-la-li (\*Kütäü-aral), which is a needless duplication of the former name due to the hasty compilers of the YS. On the other hand, Ch'ien-lung's Commissioners misunderstood in the first passage the word 曲 *ch'ü* as meaning «bend», so that the diet has been supposed to have been held at «Tiao-a-lan» on the «bend» (*ch'ü*) of the Kerulen River; this fictitious «bend of the river» occurs more than once in later Chinese researches (and in *Ch*, I, 201; cf. also *Shêng-wu ch'in-chêng lu*, WANG Kuo-wei's note, 65 a), and of course cannot be taken into account for the identification of the place. It is a fact, however, that, to the east of the Tula, the Kerulen, which in its upper course runs south-west, makes a great curve to the south, then to the south-east, and finally follows a north-north-eastern course. In YANAI's opinion (594, 672), Ködä'ü-aral was an island at the confluence of the Sängkür (or Sänggür) and the Kerulen. The Sängkür (probably the Sänggür of the *Secret History*, §§ 89, 93, 94, 96, 122; now called Cenkir [c = ts]; cf. POZDNEEV, *Mongoliya*, II, 470) is an intermittent stream which runs from north to south inside the bend of the Kerulen and flows into the Kerulen at the southernmost point of the curve of the latter (cf. on it POPOV, *Mên-gu yu-mu czi*, 400). There are several «sand islands» (沙洲 *sha-chou*) in the upper course of the Kerulen (cf. POPOV, *ibid.*, 399, 400); moreover, *aral* may mean not only an «island», but a whole region determined by the confluence of two rivers. However the case may be, the very direction of Ming-tsung's progress towards Shang-tu requires that he should have reached the Kerulen at about the middle of its curve, and fairly to the west of the confluence of the Sängkür and the Kerulen. Consequently \*Oro'otu, \*Burutung, \*Qulan-

goši'un, \*Kündüi-bulaq, Sa-li, \*Unaba, Ködö, Sa'ari-kä'är and \*Quli'un must all stretch in a line north-west to south-east, from the eastern end of the southern bend of the Tula to the southern bend of the Kerulen. If Ködä'ü-aral was at the confluence of the Sängkür and the Kerulen, \*Qali'un should probably be the place where Ming-tsung reached the Kärülän, and Sa'ari-kä'är should be the last stage before reaching the latter river. QUATREMÈRE's attempted identification (*Hist. des Mongols*, 117) of Sa'ari-kä'är with the «Saritei» stream mentioned as in the basin of the Onon by PALLAS is out of the question.

BRETSCHNEIDER (*Br*, I, 157-158) remarked that the ancient map of Mongolia reproduced at the beginning of the *Yüan-shih lei-pien* showed Sa-li-ch'ieh-êrh (Säri-kār = Sa'ari-kä'är) south of the Onon. This is true, but a map in which both the Ongin and the Tula are shown as flowing into the Onon is not to be trusted. From the names given, it is clear that the map is derived from information connected with Yung-lo's Mongolian campaign of 1414, and the accounts of this campaign provide more valuable data.

The name of Sa'ari-kä'är does not occur in the *Ming shih*, either in the *pên-chi* (7, 1 a) or in the chapter on the Mongols (328, 1 a). In MAILLA's *Histoire générale de la Chine* (x, 174) we read, however, that, in the sixth month of 1415 (to be corrected to «1414»; all the dates given in this part of MAILLA's translation have to be advanced by one year), the Imperial army reached «Salihor». According to QUATREMÈRE (*loc. cit.*, 117), GAUBIL, in a manuscript note, wrote «Salikor» or «Salikoure». The subsequent remark of GAUBIL that «koure» means «a place where there are several pools or springs, a place surrounded by water», leaves no doubt that the word he meant was *kä'är*, *kä'ärä* (this note of GAUBIL, the original of which I cannot trace, must belong to the manuscripts from which CORDIER reproduced a memoir on the location of Qara-qorum and the history of the Western Liao in *TP*, IV, 33-80). But «Salihor» and «Salikor» are bad transcriptions of «\*Salikar», Sa'ari-kä'är.

Of this we are assured not only by the Säri-kār of the map of the *Yüan-shih lei-pien*, but by the account of Yung-lo's campaign of 1414 written by Chin Yu-tzü under the title of 北征後錄 *Pei-chêng hou lu*. There we read (3 b-5 a; cf. NAKA, *Chingisu-kan jitsuroku*, 122) that, on June 10, 1414, Yung-lo halted at the 飲馬河 Yin-ma-ho (Yin-ma-ho, «the River where they water the horses», was the name given to the Kerulen by Yung-lo in 1410 [cf. Chin Yu-tzü's account of the campaign of 1410, entitled 北征錄 *Pei-chêng lu*, *Ku-chin shuo-hai* ed., 20 a]; it has not survived), and remained there five days, without making much progress. On June 16, the Emperor, starting from north of the Yin-ma-ho and crossing it five times, halted at the 三峯山 San-fêng-shan («Three-Peak Mountain»; it is also mentioned on the map of the *Yüan-shih lei-pien*), west of the Yin-ma-ho. On June 17, he halted at the 清流港 Ch'ing-liu-chiang («Pure-flowing Lagoon»; the name is also given in the *T'ai-tsung shih-lu*, 92, 10 a) of the Yin-ma-ho. On June 18, the Yin-ma-ho was at first followed for 25 li, and, after proceeding further in the afternoon, the camp was pitched at the 崇山塢 Ch'ung-shan-wu («Entrenchment of the Revered Mountain), or place without water. On June 19, in the afternoon, a mountain defile (山峽 *shan-hsia*), several tens of li in length, was reached, and, in the evening, the Emperor «halted at the 雙泉海 Shuang-ch'üan-hai («Twin-Spring Sea [=Lake]», also marked on the map of the *Yüan-shih lei-pien*), which is Sa-li-ch'ieh-êrh (Säri-kār < Sa'ari-kä'är; the same information occurs



in *T'ai-tsung shih-lu*, 92, 10 a). It is the place at which T'ai-tsu of the Yuän (= Chinghiz-khan) made himself famous (發迹). In former times a palace (*kung-tien*) and an altar for sacrifice (郊壇 *chiao-t'an*) were built. Every year [Chinghiz-khan] spent the summer at this place. Mountains and streams surround it; it extends over several tens of *li*. In front (前 *ch'ien*) [of the buildings?; i. e. south of them], there are two lakes (海子 *hai-tzü*), one of salt water, one of fresh water. Ten *li* to the south-west, there is a lake formed by the water of a spring (泉水海子二處). In the mountains to the north-west there is the 三關口 San-kuan-k'ou («Three-Pass Entrance»), by which [the place] communicates with the Yin-ma-ho and the 土刺河 T'u-la-ho («Tūla River»); it is a place of incessant passage for the Barbarians (= the Mongols)... The fourth day [of the sixth month] (June 20) was spent in front of the Shuang-ch'üan-hai... On the fifth day (June 21), at noon, [the Emperor] left Shuang-ch'üan-hai and in the evening arrived at the north-western 三峽口 San-hsia-k'ou («Three-Defile Entrance», also marked on the map of the *Yüan-shih lei-pien*; it seems to be identical with the San-kuan-k'ou above), which is 康哈里該 K'ang-ha-li-kai (\*Qangyärqai; more correctly transcribed K'ang-ha-li-hai [孩] in *T'ai-tsung shih-lu*, 92, 10 a, and in *Ming shih*, 7, 1 a; probably identical with the Qangqarqan [= Qangyarqan] of the *Secret History*, § 193; it seems to be the «Bogöltuin-amà» of POZDNEEV, *Mongoliya*, II, 444); there was no water (there)... On the sixth day (June 22), [the Emperor] halted at 蒼崖峽 Ts'ang-yai-hsia («Blue-green Cliff Pass»; also marked on the map of the *Yüan-shih lei-pien*). On the seventh day (June 23), he halted at Chi-lan-hu-shih-wên (\*Qulan-qoši'un; cf. above)... There the Mongols were defeated in a pitched battle and were pursued by night as far as the Tūla. On June 26, Yung-lo halted at 廻流甸 Hui-liu-tien («Back-flowing Steppe»; also marked on the map of the *Yüan-shih lei-pien*; it must have been close to the Tūla, near «Ulan-daba»). On June 27, Yung-lo, starting from Hui-liu-tien, retraced his steps, marched out of the San-hsia-k'ou and in the evening encamped at Shuang-ch'üan-hai. On June 28, he was at 平山鎮 P'ing-shan-chên («Garrison of the Flat Mountain»); on June 29, at 清源峽 Ch'ing-yüan-hsia («Pure Spring Defile») of the Yin-ma-ho; on June 30, at 平川洲 P'ing-ch'üan-chou («Flat Valley Island») of the Yin-ma-ho; on July 1, at 青楊灣 Ch'ing-yang-wan («Poplar bend») of the Yin-ma-ho; on July 2, at San-fêng-shan of the Yin-ma-ho.

Here again, although most names still defy our attempts at identification, the region referred to in Chin Yu-tzü's detailed account is not a matter of doubt: it is the well-watered stretch of land between the Tula and the Kerulen. To the south-eastern angle of the southern bend of the Tula was Jō-modo (< Ja'un-modun, «Hundred Trees»), the place where K'ang-hsi defeated Galdan in 1696 (cf. the description in DU HALDE, IV, 413-414), which seems to be the «Qara-tün of the Tula», in Chinese 黑林 Hei-lin, «Black Forest», mentioned in Chinese texts (cf. *Secret History*, § 96, 104, 115, 164, 177, 264, and POPOV, *Mên-gu yu-mu czi*, 348). It was one of Chinghiz-khan's favourite resorts, the one to which he returned in the spring of 1225 after his six years' campaign against the Mussulmans. But Chinghiz-khan also had a semi-sedentary camp at Sa'ari-kä'är in the vicinity of the Kerulen. There is here a difficulty, however. In the account of Ming-tsung's journey, Sa'ari-kä'är is the fifth stage after \*Qulan-qoši'un, while Yung-lo, returning from Hui-liu-tien, passes the San-hsia-k'ou, does not stop at \*Qulan-qoši'un and is back at Shuang-ch'üan-hai = Sa'ari-kä'är, all in one day. It looks as though it were the Sa-li, \*Sa'ari, of Ming-tsung's

itinerary, and not its Sa'ari-kä'är, which would correspond to the Sa'ari-kä'är of Chin Yu-tzü's account. I have no satisfactory solution to proffer. It may be that Qara-tün was a designation of the woody region immediately east of the Tula, and Sa'ari-kä'är a comprehensive term referring to the whole of the watery steppe west of the Kerulen; in Ming-tsung's itinerary, both Sa-li and Sa-li-ch'ieh-êrh would be Sa'ari-kä'är, with a more or less arbitrary specification not corresponding to the use of Sa'ari-kä'är by Chin Yu-tzü. But such a hypothesis is far from meeting all possible objections.

Whatever the case may be, it can hardly be doubted that Chin Yu-tzü, who was on the spot, knew what he was writing about when he said that Shuang-ch'üan-hai was the Sa'ari-kä'är of Chinghiz-khan and gave a description of it. Moreover, we are in a position to identify the «Twin Spring Lake» and the lake to the south of it. The southern lake is probably the «Kouen omo» of D'ANVILLE's *Nouvel Atlas de la Chine* («Tartarie chinoise», seventh sheet; *omo* is the Manchu word for «lake»), the 衰泊 Kun-po, «Kun Lake», of the so-called Wu-ch'ang Map. It is also the 衰模 Kun-mo of the *Mêng-ku yu-mu chi* (9, 24 b; cf. POPOV, *Mên-gu yu-mu czi*, 400), in which the second part of the Manchu *omo* has erroneously become part of the name in Chinese. Another 滾泊 Kun-po in inner Mongolia (misread as «Gombo» by POPOV, 288) is said to be called in Mongolian GÜN-nŏr, meaning «Deep Lake»; such is surely also the meaning of the name of the Kun-po west of the Kerulen. This GÜN-nŏr also west of the Kerulen is the 軍腦兒 Chün-nao-êrh to which Mongka repaired for the autumn in 1253 and again in 1257 (YS, 3, 2 b, 3 b; 72, 3 a) and the 君腦兒 Chün-nao-êrh where Mongka issued in the autumn of 1255 an edict for the suppression of certain Taoist books (cf. TP, 1904, 380; *Tōyō gakuō*, XII, 103; YANAI, 388-389, 676; WALEY, *Travels of an Alchemist*, 31 [but read «GÜN-nŏr», not «Kun-nor»]). The 軍腦兒 Chün-nao-êrh of YS, 100, 2 a, may be different. I see no reason to identify with the GÜN-nŏr west of the Kerulen, as YANAI does, the 口瀾腦兒 K'ou-wên-nao-êrh of YS, 15, 3 a (\*Käwün-nŏr?; the same *k'ou-wên*, the restoration of which is uncertain, occurs in the name of a prince K'ou-wên-buqa in YS, 2, 2 b, 3 a, s. a. 1235 and 1237; it is also the name of a Mongol musical air mentioned in the *Cho-kêng lu*, 28, 8 a). There is still less ground to believe, with YANAI, that the 顛顛腦兒 K'o-k'o-nao-êrh (Kökö-nŏr, «Blue Lake») of YS, 3, 3 a, and 72, 1 b, and Rašidu'd-Din's ګوکا ناوور Kökä-nawūr (*Oh*, II, 195; *Bl*, 241) are but other names of the GÜN-nŏr (BLOCHET's identification of this Kökä-nawūr with «the famous 青海 [Ch'ing-hai, «Blue Sea»] of the Chinese in the extreme west of Mongolia», i. e. with the Kökö-nŏr of our maps, is absurd, and moreover the only well-known Kökö-nŏr lies west of Kan-su, not in Western Mongolia; as to YANAI's correction of D'OHSSON's ګوشه ناوور Kūšä-nawur [*Oh*, II, 85; *Bl*, II, 49] to Kökä-nawur, it is arbitrary). I may add that the Kökö-na'ur (> Kökö-nŏr) is mentioned in the *Secret History*, §§ 89, 122 (cf. also *Ta-Ming i-t'ung chih*, 90, 27 b) in connection with the Sänggür, and so is not to be looked for to the west of the Kerulen.

The southern lake being the GÜN-nŏr, we can also identify the Shuang-ch'üan-hai or «Twin Spring Lake». North of the «Kouen omo», D'ANVILLE's map shows a «Calotey Omo», «Calotey Lake», which is the 噶老台泊 Ko-lao-t'ai-po, «Ko-lao-t'ai Lake», of the Wu-ch'ang map. NAKA (*Chingisu-kan jitsuroku*, 122) was, I think, the first to connect this Ko-lao-t'ai Lake with Chin Yu-tzü's Shuang-ch'üan-hai; YANAI (389, 672) followed him; I have no doubt they are right. Ko-

lao-t'ai has been restored by POPOV as «Galtai» (*Mên-gu yu-mu czi*, 401) and as «Kalotai» by WALEY (*Travels of an Alchemist*, 31); both are only partly right. In the system adopted by the transcribers of c. 1700, a Mongolian *qa-* (WALEY's «ka-») is rendered 喀 *k'a*, and the regular value of 噶 *ko* is *ya-* (POPOV's «ga-»); on the other hand, the vowels of *lao* cannot be slurred and require an original *-lau-* (*-la'u-* > *-lō-*, *-lū-*); the name is thus to be read *yalautai* > *yalōtai*, *yalūtai* («Place with Geese», an adjectival form derived from *yalā'un*, «goose»; \**yalautu* would be identical). I may add that the name is «Kalotou Nor» (= *yalōtu-nōr*) in the «Carte générale de la Tartarie Chinoise» of DU HALDE, t. IV. GERBILLON, who camped there on August 1, 1698, describes the place and calls it «Kaloutou Nor» (= *yalūtu-nōr*; DU HALDE, IV, 411-412). Another *yalautu-nōr* is known in the region of the Ulyui River (south-east of the ᠬᠠᠯᠬᠠ River); cf. POPOV, 284 (where the name of the Ulyui is misread «ᠬᠣᠯᠬᠣ»). A third one is outside the north-western angle of the bend of the Huang-ho (D'ANVILLE's «Kalotou omo»). The form is confirmed by POZDNEEV, who speaks of «the steppe of Gün-Galūtai» (*Mongoliya*, II, 443, 444), evidently the «steppe» of the Gün-nōr and the *yalautai-nōr*. It is much to be regretted that POZDNEEV, when he passed the site, had no suspicion of its historical importance.

The question is then to decide whether the «Ha-lao-t'u *hsing-kung* of the Sa-li Valley» of the YS is to be located in or near the district of Ch'eng-shui in Kan-su, or in the Sa'ari-kä'är west of the Kerulen, where there is still a *yalautai*, at the very place where, according to Chin Yu-tzü, Chinghiz-khan «made himself famous». In other words, did Chinghiz-khan die in Kan-su or in Mongolia?

One of WANG Kuo-wei's notes (*Shêng-wu ch'in-chêng lu*, 3 a) would seem to imply that he believed Chinghiz-khan had died at the Sa'ari-kä'är in Mongolia, but this may be due merely to a too concise redaction. BRETSCHNEIDER is more precise (*Br*, I, 157-158). According to him, the «Sa-li Valley» of the YS can only be the Sa'ari-kä'är of Mongolia, and he adds that the *Secret History* and the *Shêng-wu ch'in-chêng lu* «both agree in stating that, after subduing the Tangut empire, Chinghiz returned home, and then died». This is not quite accurate. It is true that the *Shêng-wu ch'in-chêng lu* (64 b) has the sentence: «In *ting-hai* (1227), [the Emperor] destroyed their (*i. e.* the Hsi-Hsia) kingdom and went back» (丁亥滅其國以還; this may be the source of a similar statement in the *Hsi-Hsia shu-shih*, 42, 14 a). But the somewhat awkward wording and the fact that the work avoids speaking of the death itself do not permit us to attach much weight to this statement. As to the *Secret History* (§ 268), BRETSCHNEIDER relied on PALLADIUS's translation (*Trudy členov Rossiiskoi dukhovnoi missii*, IV, 152), based on the Chinese abridged version; but it is not in the least supported by the original Mongolian text. The 回來 *hui-lai*, «returned», of the Chinese version, must not be understood as «returned [to Mongolia]», but represents the *nökö'ätä... iräju*, «came... a second time» of the Mongolian, that is to say Chinghiz «came» for a «second» campaign against Tangut. The death in Kan-su, implied in the account of the YS, and confirmed by its near location at the Liu-p'an-shan in the *T'ung-chien hsü-pien* and in Rašidu'd-Dīn and even, to a certain extent, by the late Mongol chronicles which speak of Ling-chou, is moreover supported by an independent text which was first adduced by YANAI (p. 56). In the biography of Sa-ha-lien (\*Saḥaliyan, «The Black») in *Chin shih*, 111, 4 b, there is the following passage: «In the fourth [*chêng-ta*] year (1227), after the Great Yüan had

destroyed Hsi-Hsia, they moved their troops towards Shàn-hsi». In the fourth month, on the day *ping-shên* (May 3), the Chin Emperor called a meeting of his principal officials to make a decision on the conditions in the west (*i. e.* in Shàn-hsi). The Emperor said: «I have already issued an order to [Wan-yen] Ha-ta (there is nothing about this in Wan-yen Ha-ta's biography; he had been recalled from Shàn-hsi in the second month [February 18-March 18]; cf. *Chin shih*, 112, 2 b) to engage in a decisive battle with all his strength». Most of the officials were in favour of reaching a peaceful settlement with the Mongols; Sa-ha-lien was the only one to oppose it, and with considerable energy (more details are given in another biography, *Chin shih*, 109, 7 a, where, however, the deliberation is said to have taken place in the third month [March 19-April 17], and where Sa-ha-lien is not alone in his stand for a decisive battle; the *pên-chi*, 17, 3 a, give no information). «In the eighth month (September 12-October 11), the Court received the report of [what had happened at] Ch'ing-shui, and the authorities were ordered to stop levying men for garrisoning the cities and repairing their walls; all the taxes and duties for the requirements of the army which were not urgent were suspended.» What had happened at Ch'ing-shui was evidently the death of Chinghiz-khan, on account of which the Chin Court thought they could relax their efforts for the defence of the country.

The conclusion we reach is that Chinghiz-khan certainly died south of the Liu-p'an-shan, within the jurisdiction of the *hsien* of Ch'ing-shui. But should the «Ha-lao-t'u *hsing-kung* of the Sa-li Valley» be located there? PAUTHIER (*Pa*, 183) said that «Sari-gool» was merely the Mongol translation of Ch'ing-shui, «Pure water». We have seen above that no text mentions a «Sari-gool»; moreover, I know of no Altaic word «*sari*» meaning «pure». The Ch'ien-lung Commissioners have explained Sa-li as Sali, meaning 地弩 *ti-nu*, lit. «earth-crossbow» (*Yüan-shih yü-chieh*, 4, 1 b). In Mongolian, *sali* (< Tib. *ša-li* < Skr. *śāli*) occurs in the sense of «rice», but I am at a loss to understand what is meant by *ti-nu*. In any case, the restoration is of course arbitrary. I leave out of consideration BLOCHET's statement that, according to Maqrizī, Chinghiz-khan died at صر بانيق *Śarū-bāliq* (= *Sarī-baliq*, «Yellow City»), which would seem to offer another instance of this «Sa-li» occurring in the name of the «Sa-li Valley». But the Egyptian historian could have had no independent information as to the place where Chinghiz died, and I strongly suspect «*Śarū-bāliq*» to be a tendencious misreading of a corrupt form of «*Liū-pān*» or «*Liū-pān-šan*» (cf. *TP*, 1935, 166). In principle, the existence of some elements of Mongolian nomenclature in Kan-su should not be discarded off-hand, since we know of other uses of it there as well as elsewhere in China, *e. g.* «Qara-ḡang», «Čayan-ḡang», «Čitkör», «Yäkä-busä» in Yün-nan (see «Caragian»), «Čayan-balyasun» in Ho-pei (see «Achbaluch»); in Kan-su, we find a \*Qara-ḡaḡar between the Alašan and the Huang-ho (see «Calacian»), and, south of the Liu-p'an-shan, the name misread as بيسون قهلقه *Bisun-qahalqa* in *Bl*, II, 326-327, and wrongly identified with 米倉 *Mi-ts'ang*, but which certainly is بيسون قهلقه *Yēsün-qāhālqā* = *Yesün-qa'alya*, «The Nine Passes» (cf., for the name, *Yäsün-qa'alyatu hota* of SCHMIDT, 289, «Nine-gates City», which is but another name of Peking); a 撒都兒川 *Sa-tu-êhr-ch'uan*, which seems to be a \*Sadur Valley, and a 須滅都河 *Hsü-mieh-tu-ho*, \**Sümädü* (or \**Sümätü*) River (? «River with Temples») are still mentioned under the Ming, north-west of the Liu-p'an-shan, in the 天下名勝志 *T'ien-hsia ming-shêng chih* (section of the *Shàn-hsi ming-shêng chih*, 7, 9 a).

But the fact remains that *hsing-kung*, in the Mongol period, was the Chinese translation of *ordo*, «royal [semi-sedentary] encampment»; that the four *ordos* (the Mongolian plural of *ordo*) of Chinghiz were in Mongolia; that the chief of them, perhaps that of *yalautai* (NAKA, followed by YANAI, 672, thought that the «Great Ordo» of Chinghiz-khan was that of Kōdā'ū-aral on the Kerulen; he may be right, but this also leaves us in Northern Mongolia), is meant when, in the beginning of 1233, Ögödäi repaired to the *hsing-kung* of T'ai-tsu (= Chinghiz-khan, YS, 2, 2 a), when again, in the sixth month of 1257, Mongka went to the *hsing-kung* of T'ai-tsu and offered a sacrifice to his «standard and drum» (YS, 3, 3 b), and when lastly the Emperor T'ai-ting, on ascending the throne on October 4, 1323, issued an edict of amnesty «in the Great Ordos of Chinghiz-khan» (YS, 29, 1 b : 於成吉思皇帝的大斡耳朵; another passage in the text shows that CHAVANNES was right [TP, 1905, 39], when he translated the *wo-êrh-to*, *ordo*, in this passage as being in the plural); finally, that it would be an extraordinary coincidence if Chinghiz-khan had died at a «Ha-lao-t'u *hsing-kung* of the Sa-li Valley» in Kan-su, while one of his four Mongolian *ordos* was at *yalautai* (= *yalautu*) of Sa'ari-kä'är in Mongolia.

For these reasons, I agree with NAKA and YANAI that it is the name of the latter which appears in the account of the YS. This entails of course a correction in the text. Chinghiz-khan died at the «western river» within the jurisdiction of the *hsien* of Ch'ing-shui in Kan-su, but immediately, his coffin was carried to the *yalautu-ordo* of Sa'ari-kä'är. Something is missing here, but the error must not be put to the account of the compilers of the YS; it must have occurred in their primary source, the *shih-lu*, or «true annals», since the *Cho-kêng lu*, completed three years before the compilers of the YS set to work, already says (I, 11 a) that Chinghiz-khan died «in the Sa-li Valley». On the other hand, it will be remembered that there is a discrepancy between the date of Chinghiz-khan's death as given by Jūwainī, August 18, 1227, and that of the YS, August 25, 1227. Does the correction in regard to the place of his death in the text of the YS affect the question of the dates it gives? In other words, would it be possible that August 18 should be the date of his death near Ch'ing-shui and August 25 the date on which, the coffin having reached Sa'ari-kä'är, his death was announced? This is in fact not possible. The distance between the two places is so great that, whatever speed the procession might have made, it could not have been covered in seven days. There is every probability then that the YS is right here, and that Chinghiz-khan, having fallen ill on August 18, actually died on August 25, 1227.

According to Polo, Chinghiz «died of an arrow-shot in the knee at the siege of Caagiu» (*q. v.*: HOWORTH, I, 103, attributes to Bar Hebraeus too the story of the arrow-shot; but this is a mistake; Bar Hebraeus, in both his chronicles [POCOCKE, II, 304; BRUNS, II, 498], says that Chinghiz-khan died of malaria). YULE (Y, I, 245) quotes GAUBIL in support of a wound from an arrow-shot at the siege of Ta-t'ung in 1212. The original source is YS, I, 7 a : in 1212, Chinghiz «attacked the Western Capital (西京 Hsi-king, the «Segin» of Rubrouck, WY, 237, 578, = Ta-t'ung). The Emperor was hit by a stray arrow, whereupon the siege was raised.» But he survived the siege a full fifteen years, and, if this wound were the origin of Polo's statement, the traveller was misinformed as to its consequences. Plan Carpine's statement that Chinghiz-khan was killed by lightning (WY, 65) may be a distorted echo of the superstitious terror which thunder created among the Mongols. No attention need be paid to the late Mongolian legends of poisoning, which are tinged

with popular magic. The most likely cause of the death of Chinghiz-khan is the one given thirteen years later by the *Secret History*, a serious fall from his horse J̄osotu-boro.

Polo says that, when the Great Khans were being carried to the mountain where they used to be buried, the escort used to kill every living soul they met, saying : « Go serve your lord in the other world ». Twenty thousand men would have thus been killed when Mongka was brought back from Ssü-ch'uan to Mongolia. The Mongols slew the best horses of the deceased Great Khans for the same purpose (Vol. I, 167-168). According to Rašidu-'d-Dīn (*Ber*, III, 29, 119) all were killed who were met on the road when Chinghiz-khan's coffin was carried back from Tangut to Mongolia, so that the news of the Great Khan's decease should not spread before it was announced even after the body had reached the *ordo*. YULE (*Y*, I, 250) has also adduced a text of Rašid, quoted by D'OHSSON, on the sacrifice of forty girls and of horses. Several rites and beliefs have become more or less mixed up in these accounts. It is very likely that the Mongol leaders, as related by Rašid, wished to keep the news of the death secret as long as they had not returned to their native lands; and to achieve that purpose, they would not have hesitated to slaughter any number of people whose misfortune it had been to appear in their way. But there was also a fear that people meeting the procession might exert a baleful influence on the after-life of the deceased. When Gūyūk died, all the roads were occupied, and orders were issued that nobody was to go out; similar rules were observed at the funerals of Hülägü (QUATREMÈRE, *Hist. des Mongols*, 416; PÉTIS DE LA CROIX's text quoted in *Pa*, 188, and *Ch*, I, 208-209, is of no value, being probably based on Polo himself). On the other hand, sacrifices of human beings and of animals by the grave of the deceased are a well-known ancient Altaic custom, and, in the case of the Manchu emperors, it was not suppressed until the end of the 17th or the beginning of the 18th century. It obtained also among the Chinese. In a note added by CORDIER (*Y*, I, 251), it is said, on DE GROOT's authority, that the most ancient case of human sacrifice recorded in Chinese history occurred in 677 B.C. (read « 678 B.C. »), and the theory has long been prevalent among Sinologists that this was an innovation due to Altaic influence. But the excavations at An-yang have established that human as well as animal sacrifices were extensively practised by the Shang dynasty in the middle of the second millennium B.C. As to the girls sacrificed to the manes of Chinghiz-khan, this barbarous rite was not exercised at the time of his funeral, which is probably to be dated in the last months of 1227, but two years later in September 1229 in fact and after the accession of Ögödäi to the throne. When the festivities following his enthronement had come to an end, Ögödäi ordered that, « in agreement with the ancient *yāsāq* (= code), the custom and the tradition, offerings of food should be made, three days in succession, to the soul of Chinghiz-khan, that forty beautiful girls should be selected from the families and the progeny of the generals who had been the common companions of [Chinghiz-khan], that they should be dressed in gorgeous attire and adorned with precious stones and jewels, and, together with excellent horses, they should be sent to join the soul of [Chinghiz-khan]» (*Bl*, 17). According to DE GROOT (*Religious System of China*, II, 437-438), at every funeral of a Mongol Emperor, « a white-powdered skin (a woman?) was buried along with the dead », but this rests on a mispunctuation and a misconstruction. The sentence does not refer to the grave, but to the coffin into which were « placed for use after death » (𠄎) *hsün*) two gold bottles, one cup, etc.; as

to the «white-powdered skin» (白粉皮 *po-fên p'i*), it refers merely to the «white-tanned skin» of which the boots, the outer warm boots and the belt-depending bowl used for the mortuary costume of the defunct Emperor were made.

THE TOMB OF CHINGHIZ-KHAN. — Opinions concerning the place where Chinghiz-khan was buried are no less divergent than those about the dates of his birth and death.

GAUBIL (p. 54) and MAILLA (IX, 128) state that Chinghiz was buried «in the cave («caverne») of Kinien», and this has been repeated by CORDIER (*Hist. gén. de la Chine*, II, 223); it is doubly misleading. The actual text (YS, I, 9 b) says that Chinghiz was buried at the 起輦谷 Ch'i-lien-ku. Although the second character is often now read *nien* (POPOV too, 313, transcribes «Cinyan» = Ch'i-nien), it occurs in mediaeval transcriptions only with its correct reading *lien*; as to *ku*, it is not a «cave», but a «valley [in the mountain]», in principle, a dry one, as opposed to 川 *ch'uan*, a wellwatered «valley». All the Emperors of the Yüan dynasty, except Mongka, are expressly stated in the YS, and before it in the *Cho-kêng lu* (I, 11-14), to have been buried in the Ch'i-lien-ku. Even for the last Emperor, who was expelled from China in 1368, and who, according to the YS, died in 1370 at Ying-ch'ang-fu (see «Barscol»), we are told (YS, 47, 6 a) that «his coffin was taken north to be buried». In several cases (YS, 26, 8 a; 28, 7 a; 31, 4 b; 36, 4 a; 37, 2 b), the mention of the burial at the Ch'i-lien-ku is followed by the words 從諸帝陵 *ts'ung chu-ti ling*, «after the funerary mounds of the [other] Emperors». This would seem to imply that *tumuli* were erected over the tombs; but it is probable that Chinese writers here used merely the ordinary term for a Chinese Imperial tomb, without any reference to its nature.

Where did the Ch'i-lien-ku or Ch'i-lien Valley lie? The *Yüan-shih lei-pien* alone (I, 10 a), we do not know on what evidence, says that it lay «north of the Desert». As to the name, if Ch'i-lien be a transcription, the possible originals would be \*Kilän, \*Kiläl, \*Kirän, \*Kiräl (the other «K'i-lien» mentioned in Y, I, 248, is 祁連 Ch'i-lien, and, as PALLADIUS says, lay in quite a different region; on the confusion made at a late date between the two, cf. *infra*, p. 361-362). PALLADIUS suggested that Ch'i-lien was an abbreviated transcription of the name of the Kerulen (cf. Y, I, 248; *Ch*, I, 195), and this opinion has been expressed independently, and with greater confidence, by YANAI (p. 751) and in 沈曾植 *Shên Ts'êng-chih's* notes on the Chinese version of «Sanang Setsen» (蒙古源流箋證 *Mêng-ku yüan-liu chien-chêng*, 4, 7 a). I felt inclined to adopt the same view in *TP*, 1935, 167. But, after reconsidering the case, I have come to a different conclusion.

The name of the Kerulen is now pronounced *Herülün* in Mongolian, and the modern Chinese transcription is 喀魯倫 *K'a-lu-lun* or, more correctly, 克魯倫 *K'o-lu-lun*. In the Mongol period, despite Rubrouck's «Kerule» in «Onankerule» (= Onon and Kerulen; *W*y, 208 [where, and also on p. 29, «Orkhon» is a slip for «Onon»], 243, 268), the name was *Kälürän*, attested by both the *Secret History* and *Rašidu-'d-Din*; «Kulurum» is still found in WITSEN'S *Nord-en Oost-Tartarye* (1705 ed., 279). In Chinese, the river is designated either by the regular transcriptions of *Kälürän*, 怯綠連 *Ch'ieh-lü-lien*, 怯綠憐 *Ch'ieh-lü-lien*, 怯呂連 *Ch'ieh-lü-lien*, 怯魯連 *Ch'ieh-lu-lien* (ch. WANG Hui-tsu<sup>2</sup>, 49, 2 a), 翕陸連 *Hsi-lu-lien* (cf. WANG Kuo-wei's edition of Ch'iu Ch'u-chi's *Hsi-yu chi*, 2, 13 a-b; *Br*, I, 54), or they render a name of unknown

origin, \*Lügü or \*Lüngü, 臚胸 Lu-chü in the *Liao shih*, 龍駒 Lung-chü and 龍居 Lung-chü in the *Chin shih*, 臚胸 in YS, I, s. a. 1216 (Nan-chien ed.; this is a form taken from the *Ch'ien-Han shu*, where it does not refer to the Kerulen; cf. DE GROOT, *Die Hunnen*, 154), 龍居 Lung-chü in YS, 29, 1 b, 臚胸 Lu-chü, 閭居 Lü-chü, 驢駒 Lu-chü, 陸局 Lu-chü and perhaps 瀘溝 Lu-kou in other works of the Mongol and early Ming periods (cf. WANG Kuo-wei, *ibid.*, and TP, 1935, 166). The 俱輪 Chü-lun is not the Kerulen, as was stated by DE GROOT (*loc. cit.* 182), but the Külün Lake (misread «Beī-lun» [= Pei-lun] by POPOV, *loc. cit.* 400). There is no ground for supposing an abnormal transcription of the name Kerulen as Ch'i-lien, which would only have been used in reference to the Imperial tombs. Moreover, a particular mountain gorge is not likely to have been called by the name of a great river, flowing for the greater part of its course across the steppes.

But the main point is that «Ch'i-lien» need not be a transcription at all, and, as HAENISCH suggested (*Asia Major*, IX, 549-550; cf. TP, 1935, 167), may mean «the Valley where the Car is lifted». As a matter of fact, I am unable to trace any example of 起 *ch'i*, «to raise», being used in a transcription (except perhaps in the somewhat doubtful title of a Mongolian musical air in the *Cho-kêng lu*, 28, 8 a). As to 輦 *lien*, it occurs in transcriptions, but in its true meaning it signifies a kind of chair on wheels (a sort of bath chair, as Giles says); it is mainly used as a designation of the Imperial chariot, though it can also be applied to the Imperial hearse. The Chinese translation of «Sanang Setsen» (4, 6 b) says that, after the death of Chinghiz-khan, «his coffin was raised on a *lien*» (以輦奉柩). The Mongolian term, here translated *lien* in Chinese, is *qasaq tärgän*, or simply *qasaq*, both in the *Altan tobči* (GOMBOEV's ed., 40<sup>1</sup> [where *qas tärgän* is a faulty reading], 40<sup>2</sup>, 42<sup>3</sup>, 42<sup>6</sup>, 47<sup>11</sup>, 48<sup>1</sup>) and in «Sanang Setsen» (SCHMIDT, *Gesch. der Ost-Mongolen*, 106<sup>1</sup>, 106<sup>7</sup>, 108<sup>9</sup>, 132<sup>9</sup>). *Tärgän* is the ordinary Mongolian word for «car»; *qasaq* or *qasaq tärgän*, now obsolete, is explained in the dictionaries as «a light two-wheeled waggon». In Mongolian, where there is no *z*, *Qasaq* is, in principle, the name of the Qazaq (> Russ. Kazak, Kozak, «Cossack»), *i. e.* of the Kirghiz. I have no doubt that RAMSTEDT was right when he proposed (*Kalm. Wörterbuch*, 171<sup>2</sup>) to explain *qasaq tärgän* as a term originally meaning «Qazaq chariot». In Osmanli, *qazaq* has become the name of a sort of «sledge» for the transport of heavy weights (RADLOV, II, 366). The Sino-Mongolian Vocabulary of c. 1600 published by POZDNEEV (*Lekcii po istorii mongol'skoj literatury*, III, 37) gives only one word for «car», 哈撒 *ha-sa*, *i. e.* *qasaq*. In a further chapter of «Sanang Setsen», the Chinese translation (5, 1 a, corresponding to SCHMIDT, 133) merely renders *qasaq* as 車 *ch'é*, «car [in general]». But I doubt whether the *qasaq tärgän* was originally a «light» car, as is said in the dictionaries. The *Secret History* (§ 64), in an alliterative passage of epic character, speaks of girls seated in *qasaq tärgän*, who were to become the wives of Emperors; and the Chinese translation, done in the 14th cent., renders *qasaq tärgän* as 大車 *ta-ch'é*, «big car». The Mongols had their ordinary cars or *tärgän*. Since the text on the death of Chinghiz-khan speaks of a *qasaq tärgän*, it must have been a great chariot, a state car, which the Chinese translators of the 18th cent. were right in translating as *lien*, «the Emperor's chariot». I may add that, if RAMSTEDT and I are right in explaining *qasaq* as Turk. Qazaq, the appearance of the word in the *Secret History*, which was completed in 1240, antedates the earliest mention of this name in



other sources by almost two centuries. At a later period, the Imperial hearse was made of white felt, with a curtain of blue and green brocade (*nasīj* > *našīš*; see «Nac and Nascici»); *nasīj* was also laid on the coffin. In front of the procession, a Mongol witch, wearing a new dress, rode on horseback, leading another horse, with a saddle ornamented with gold and a bridle and a halter of *nasīj*, which was called «the Horse of the Golden Soul» (cf. the *altan bā'ä*, «golden body», and *altan kä'ür*, «golden corpse», used in reference to Chinghiz-khan in SCHMIDT, *Gesch. der Ost-Mongolen*, 108<sup>13-15</sup>; YS, 77, 8 a-b; PALLADIUS, in *Trudy členov...*, IV, 251-252; DE GROOT's version, *Relig. system of China*, II, 438, is not accurate). I think that the name Ch'i-lien-ku is not a transcription, but means the «Valley» (*ku*) where this «Imperial hearse» (*lien*) was «raised» (*ch'i*) for burial.

Chinghiz-khan was buried after his coffin had been brought back to Mongolia. According to «Sanang Setsen» (SCHMIDT, 109), the Mongols, «in despair of being able to get the golden body (*i. e.* the corpse) [out of the car]», «erected» for it «an eternal tomb». This may perhaps be construed as a misunderstood tradition that, in agreement with the old Altaic habit, Chinghiz-khan was buried with the car. The interpretation may find some support in a text of Plan Carpine which will be quoted farther on. But it may also be that the reference is to a rite in which the witch and the other attendants performed incantations to bring the dead Emperor back to life. The expression I have translated as «eternal tomb» is rendered in the Chinese translation 長陵 *ch'ang-ling*, «long-lasting [Imperial funerary] mound» (not «great tomb» as in *Ch*, I, 208); the Mongolian original, *möngkä kör* (or *möngkä kür*) is puzzling. It also occurs in the *Altan tobči* (42<sup>a</sup>), where GOMBOEV (p. 147) misunderstood it, taking *kür* for the Mongolian word which means «all»; SCHMIDT (p. 109) correctly translates it as «tomb». But *kör*, «tomb», is not a true Mongolian word. KOWALEWSKI does not list it; it is given by GOL'STUNSKIĪ (who reads it *kür*) as meaning «corpse», «tomb», but only in reference to the present passage, and clearly as a guess. RAMSTEDT (*Kalm. Wörterbuch*, 250<sup>1</sup>) mentions in Kalmuk \**kür* with the double meaning of «corpse» and «tomb». *Kür*, «corpse», is of course Mong. *kä'ür*, «corpse», and in the present passage, both the *Altan tobči* and «Sanang Setsen» distinguish the «corpse» of Chinghiz, *kä'ür*, and his «tomb», *kör*. The confusion between the two words seems, however, to have been made in Mongolian at an early date, since *kä'ür* occurs in the unpublished Sino-Mongolian inscription of 1362 with the meaning of «ancestral grave-yard» (先塋 *hsien-ying*). The word *kör* is evidently the same as Uiy., Coman and Osm. *kör* («*kór*»), Kar. *gör*, Kaz. *gür*, all meaning «tomb», and probably Yak. *kürüö*; so the word covers a wide field in Turkish dialects. At the same time, Turk. *kör*, *gör* cannot be separated from Pers. گور *gör*, «tomb» (cf. the well-known «Gur Esnir» of Samarkand). On the other hand, Pers. *gör* is generally considered as borrowed from the Arabic قبر *qabr*, «tomb», which has itself passed into Kaz. and Kirghiz Turkish as *qabır* and *qabir* (VULLERS, II, 1043; HORN, *Grundriss der neupers. Etymologie*, p. 210; RADLOV, II, 450, 453, 1248; Sassanian Persia «exposed» the dead instead of burying them; this would account for the borrowing of the Arabic word when Persia was converted to Islam). It seems as though the Mongols had no word of their own for a real «tomb», with something which marked it above ground. The *Altan tobči* and «Sanang Setsen» use a foreign word, which may well convey a wrong idea of what Chinghiz-khan's tomb actually was like, and

the Chinese translation, with its *ling*, «funerary mound», makes it worse by ascribing to the Mongol Emperors the habits of the truly Chinese dynasties.

That there were no «funerary mounds» over the Mongol Imperial tombs is made practically certain by contemporary accounts.

We read in *YS*, 77, 8 b : « [For Imperial funerals], when they reached the place of the burial mound (*ling*), the earth removed to dig the pit was made into lumps which were disposed in [due] order. Once the coffin had been lowered [into the pit], [the pit] was filled and covered in the order [of the lumps]. If there was earth in excess, it was carried to other places far away.» This description implies that there was no «mound», and that the word *ling* is merely used in the text under the influence of the Imperial mounds (*ling*) of Chinese dynasties. The arrangement of the lumps put back in the same order in which they had been taken may refer to the clods of grass which were not to be damaged so that the ground should retain its original appearance after the funeral. This was important for preserving the secrecy of the tomb. The detail is confirmed by Plan Carpine for the burial of some Mongols of high rank (*Wy*, 43) : «They fill up the pit..., and place over it the grass as it was before, so that the place should be impossible to find afterwards».

The *Hei-Ta shih-liao* is an account of the Chinese envoy, P'êng Ta-ya, who went to Mongolia probably in 1232. He says (24 a) : «The tombs (墓 *mu*) of [the Mongols] have no mound (塚 *ch'ung*); they are trodden over by horses so as to appear as the even [ordinary] ground. At the tomb (*mu*) of T'ê-mu-chên (Tämüjin, Chinghiz-khan), arrows have been stuck [into the ground] so as to make a fence (垣 *yüan*) — more than 30 *li* wide —. Horsemen patrol it so as to guard it.» To this, another envoy, Hsü T'ing who went to Mongolia in 1235-1236, added the following note : «I, [Hsü] T'ing, have seen the tomb of T'ê-mu-chên (Chinghiz-khan). It is on the side of the 濶溝河 Lu-kou-ho (Lu-kou River); mountains and rivers surround it. It is reported that T'ê-mu-chên was born there and that for that reason, on his death he was buried there; I do not know if it be true or not.»

PALLADIUS stated more than once (first in *Trudy členov rossitkoï dukhovnoï missii v Pekiné*, IV, 252) that the burial rites of the Mongol Emperors are described in the *Cho-kêng lu*, and the statement has passed into *Y*, I, 248, *Ch*, I, 195, and even into FÊNG Ch'êng-chün's abridged translation of CHARIGNON's book (I, 242). But this must be a slip of PALLADIUS : there is nothing of the kind in the *Cho-kêng lu*.

Of some importance is another work also alluded to by PALLADIUS, the 草木子 *Ts'ao-mu tzü* of 葉子奇 Yeh Tzü-chi; the author's preface is dated in 1378 (cf. *Ssü-k'u...*, 122, 18 b; so it is not quite correct to call Yeh Tzü-chi a «Yüan» author as was done by PALLADIUS and repeated by YULE and CHARIGNON; the title *Ts'ao-mu tzü*, «The Philosopher of Plants and Trees», has become «The Book of Plants, Trees and Seeds» in DE GROOT, *Relig. system of China*, II, 438). On the making of Imperial coffins during the Yüan dynasty, the information given by Yeh Tzü-chi is very similar to that of *YS*, 77, 7 a, and of the second-hand source translated by DE GROOT, II, 437. The text goes on as follows (cf. NAKA, *Chingisu-kan jitsuroku*, 581; T'U Chi, 3, 34 a; the text in DE GROOT is a *rifacimento*) : «[The coffin] is sent due north to the burial ground, where it is deeply interred. The national custom

is not to raise funerary mounds (墳隴 *fên-lung*). When the burial is over, 10,000 horsemen trample the ground so as to make it even. A young camel is killed over it. A thousand horsemen guard [the tomb]. The next year, in the spring, when the grass has grown, they move the tents and go away. One sees only flat ground all over [the surface], and nobody knows [where the tomb is]. When [the Mongols] wish to offer sacrifices [to it], they take as their guide the mother of the [young] camel they had killed and from watching the place where she stops and wails, they know the place of the burial. Consequently, after a long time, even the descendants (*lit.* the sons and grandsons) cannot have any knowledge of it.» Yeh Tzū-chi was, it is true, a well-read Chinese; he was born in Chê-chiang under the Mongol dynasty, but never attained any high position at Court, nor does he seem ever to have visited Mongolia. His account is not free from popular legend, like, for example, the story of the young camel. But the whole story could not have been imagined if in fact there had been «mounds» over the Mongol Imperial tombs.

Jourdain de Severac had heard another tale. According to him (CORDIER, *Les Merveilles de l'Asie*, 92, 121) : «When the Emperor dies, he is carried, together with an immense treasure, by several men to a certain place, where they lay down the body and flee at the utmost speed, as if the Devil pursued them. There are other people ready who immediately seize the corpse and proceed in the same way to another place; and so on, until they reach the place where it is to be buried. And this they do so that the place should not be found, and consequently so that nobody might steal the treasures.» A baseless story, but one which testifies to the secrecy of the Mongol Imperial tombs.

D'OHSSON (*Oh*, I, 384-385) and HOWORTH (I, 105) have quoted from Maundeville a text on the funeral of the Great Khans which this pseudo-traveller copied, in fact, from Plan Carpine. In the original, it does not refer to the Great Khans, but it is of sufficient interest, however, to be translated here (*Wy*, 42-43; cf. ROCKHILL, *Rubruck*, 81) : «When one of them (*i. e.* the Mongols) is dead, if he be [one] of the ordinary leaders (*de minoribus*, in opposition to *quosdam majores* in the next paragraph), he is secretly buried in the steppe, wherever it pleases them. He is buried with one of his tents, [and] seated in the middle of it. They put a table in front of him, with a bowl filled with meat and a jar of mare's milk. With him are buried a mare with her foal and a horse with bit and saddle. They eat another horse, stuff its hide with straw and raise it on high on two or four poles. [All this is done] in order that [the deceased] should have in the other world a tent in which he could stay, a mare from which he could have milk and even procure more horses, and horses which he could ride. They burn for his soul the bones of the horse they have eaten. The women often assemble to burn bones for the souls of men, as we [ourselves] have seen with our own eyes and have known from others. . . They bury with him in the same way gold and silver.» This account reminds us more of what we know of Coman rites in southern Russia than of Mongol customs. It may, however, lend colour to the interpretation of «Sanang Setsen»'s text quoted above which implies a tradition that Chinghiz-khan was buried with the hearse which had brought him back from Kan-su to Mongolia. Moreover, the horse stuffed with straw and stuck on poles over the tomb of Mongol nobles is also mentioned by Vincent de Beauvais (xxx, ch. 86), who

does not seem here to draw his information from Plan Carpine, and by the Armenian Kirakos (cf. PATKANOV, *Istoriya Mongolov*, II, 47).

Rašidu'-d-Dīn's account of the burial of Chinghiz-khan must be quoted in full and compared with other passages in his work. The Persian historian says (*Ber*, III, 99-100; cf. also ERDMANN, *Temudschin*, 443-444) : « One day, Chinghiz-khan was out hunting, when he saw at one place a solitary tree. The sight of that tree pleased him, and he sat for an hour under it. Within himself he was moved [by it] and thus gave his orders : 'This place is suitable for my burial; remember it'. At the time of the wailing, people who had heard these words repeated them. The princes (*šahzādagān*) and the leaders (*umārā*, the emirs) in compliance with this order chose [that place]. It is said that in the very year of the burial, trees and grass grew beyond measure over that steppe, and now the wood is so thick that it leaves no passage and they do not know [which is] the original tree or the place of the burial, so much so that even the old keepers of the 'forbidden precinct' (*qorūqčīān*) do not find their way to it. Among Chinghiz-khan's children, this is also the burial ground of his youngest son Tolui-khan and of [the latter's] children, — Mängü-qa'an (= Mongka-qa'an), Qubilai-qa'an, Ariq-bögä, — and those of their children who remained in that land. The other children of Chinghiz-khan (I think that the nonsensical *āz* and its Russian translation *ot*, 'among', 'from', which follows this in BEREZIN should be omitted), Jöči, Čayatai, Ögödäi and their children and their kin (*ürüq*) have their burial grounds elsewhere (*i. e.* in other regions; not 'in other parts of the burial ground' as in BEREZIN's translation). The keepers (*qarūqčīān*) of this 'great forbidden precinct' (*yarūq-i buzurg*) are leaders (*umārā*) of the tribes of the *اورباگقت* Uryāngqät. In the four 'great ordo' (*ordōi-buzurg*) of Chinghiz-khan, wailing took place every day, in each *ordo*. When the news reached regions and places far and near, wives (*hatünān*) and princes flocked there within a few days from every place and wailed. Since many came from the most remote quarters, about three months [thus] elapsed, during which one after the other they came in succession and performed the mourning rites. » The story of the tree seems to belong to some popular tradition connected with one of the sacred « lone trees » so often mentioned in different parts of Asia (see « Caccia Modun » and « Lone Tree »). It reminds one also of the « leafy tree » (*saqlaqar modun*) mentioned in the *Secret History* (§ 57) at Qorqonaq-jubur of the Onon, round which, at the election of Qutula-khan, the Mongols are said to have danced with such frenzy that a waist-deep moat was formed around it.

In the section of Rašid's description of the tribes which is devoted to the « Uryāngqat of the Woods » (*Uryāngqät-i bēšä*; *Ber*, I, 92), we read : « ... In the time of Chinghiz-khan, there was, belonging to that tribe, a leader (*emir*), who was one of the chiliarchs of the left hand, called *اورداجی* Ūdäči (probably \*Ūdäči < Ä'üdäči, *lit.* « Door-keeper », but here used as a personal name; 'Ouradji' [= Uraji] is a misreading in *Not. et Extr.*, XIII, I, 275). After him (*i. e.* after the death of Chinghiz-khan), it was decided that he (= \*Ūdäči), with his chiliarchy (*hüzärü*), should keep guard at the 'great forbidden precinct' (*yorūq-i buzurg*) of Chinghiz-khan which is at the place [called] Bürqān-qäldün, and none of the [men of this chiliarchy] should [have to] enter the (Imperial) guard (*کزدک* *käzik*; see 'Quesitan'; but the proper reading is *چردک* *čerik*, 'the army', as in the parallel passages below; these men were exempt from military service

because of their duties at the tomb). Tului-khan and his descendants — excepting Qubilai-qa'an — (the text is here in contradiction with the one translated above; I shall discuss the point later), Mānggü-qa'an (= Mongka-qa'an) and their (*sic*) descendants (*uruq*), were all buried in that 'forbidden precinct'. The 'forbidden precinct' of the rest of the descent of Chinghiz-khan is in other places.»

In the section of the Uryāngqät proper, Rašid repeats that they are not the same as the «Uryāngqät of the Woods», and nevertheless speaks again of the latter (*Ber*, I, 144-145) : «In the time of Chinghiz-khan, there was, belonging to the tribe of the Uryāngqät of the Woods, a chiliarch who was a leader (*emir*) of the left hand, called Ūdāči (\*Ūdäči). After the death of Chinghiz-khan, his children [decided] that [\*Ūdäči], with his chiliarchy, should guard the 'forbidden precinct' of the great *yōsūn* (غروق یوسون نزرک) of Chinghiz-khan, which is at a place called Būrqañ-qāldūn. These [men] do not have to join the army (*čerik*). Until now, [their office] has been perpetuated [by edicts] and they are attached to that same *yōsūn*. Among the children of Chinghiz-khan, the great *yōsūn* of Tului-khan, of Mānggü-qa'an (= Mongka-qa'an), and of the children of Qubilai-qa'an and his descendants (*uruq*) have all been deposited in the said place. People maintain that Chinghiz-khan once arrived at that place. An exceptionally verdant tree had grown on the steppe. He was extremely pleased by the fresh and flourishing appearance of the tree. He sat for an hour under that tree, and felt moved within himself; in that condition he said to the leaders and to the 'great : 'It must be that this will be my last [resting] place'. After he died, since they had heard such words from him, his great 'forbidden precinct' (*yorūq-i buzurg*) was fixed at that place, [and] under that tree. It is said that, in the same year, this steppe, on account of the numerous trees that grew up [there], became a great wood, so that it became impossible to recognize the original tree, and nobody [now] knows which it is. The great *yōsūn* of the other children [of Chinghiz-khan] are in another place. The descent (*uruq*) of this \*Ūdäči continues through hereditary slaves (I read اوتوگو بوغول \*ötügü boyol, or \*ötägü boyol, instead of BEREZIN's «*utalu boyol*» [cf. also *Ber*, I, 276; ERDMANN, *Temudschin*, 193]; whatever the correct form may be, it is certainly the same term as the one read *öngü-boyol* in *Ber*, I, 33 [and 227], 58, and II, 11), since they do not give away girls [to other tribes] nor do they take any [from them].»

In the account of the Mongol army, there is again the following text (*Ber*, III, 141) : «Chiliarchy (*hāzarā*) of Ūdāči (\*Ūdäči). He was from the tribe of the *هوین اوربانکفت* Hōin-Uryāngqät. This tribe and the children of this \*Ūdäči, according to the *yāsā* and the *yōsūn*, keep guard at the great 'forbidden precinct' (*yorūq-i buzurg*) which is at the place Būrqañ-Qāldūn. They do not [have to] join the army.»

The *yāsā* is the code laid down by Chinghiz-khan, and the *yōsūn* (Mong. *yosun*) is the Mongol customary law (on the word *yosun* in Persian texts, cf. QUATREMÈRE, *Hist. des Mongols*, 35). The use of *yosun* in the third text is quite regular. So it may be too in the second text when Rašid says that the descendants of \*Ūdäči went on keeping guard at the tomb, if we translate this passage as «according to that same rule» (*yosun*). But then the «forbidden precinct of the great *yōsūn*» makes no sense, nor do the «great *yōsūn*» of Tului and the «great *yōsūn*» of the other children. I have no certain solution to suggest. It may be that *yosun*

has unduly crept into the text between *yoruq* and *buzurg* in the first case, and that, in the other two cases, *yosun* is a corrupt form of *yoruq*; such a corruption, however, is not easy to explain. Moreover, in these two cases, the wording of the text would suggest something like «bones» rather than *yoruq-i buzurg*.

Rašid's «Uryāngqāt» is the plural of Uriyangqa, Uriyangqan, Uriyangqai > Uryangqai (Uryangqai in the *Secret History*, § 9; Uryangqan, § 120; Uriyangqan in «Sanang Setsen», 86<sup>10</sup>, 190<sup>12</sup>); D'OHSSON's «Ourianguites» or «Orianguites» (*Oh*, I, 383, 425, 426) and BEREZIN's «Uryankhit» cannot be retained. Rašid distinguishes the «Uryāngqat» proper, and the «Uryāngqat of the Woods» (Uryāngqat-i bēšā in Persian; Hōin-Uryāngqat = Hoi-yin Uryāngqat > Oi-yin Uryāngqat in Mongolian; the Mongolian form again occurs in *Ber*, I, 9, 86; cf. T'u Chi, 153, 57 b, where, however, the phonetic equivalence of «Hoi-yin» and of Oirat is of course wrong). From a comparison of *Ber*, I, 9 and 90, it is clear that more than once Rašid has mixed up the two tribes which he in fact wished to distinguish. We may provisionally accept, however, that the Uryangqai in charge of Chinghiz-khan's tomb were in fact «Uryangqai of the Woods». They are Rubrouck's «Orengai», who used to ski (*Wy*, 269); the same is said by Rašid of the «Uryangqai of the Woods» (*Not. et Extr.*, XIII, I, 275; *Oh*, I, 422; *Ber*, I, 91, where *čānā* is the Mong. *čana*, «ski»). ROCKHILL (*Rubruck*, 198, followed by *Wy*, 269) said that the «Uryangqai of the Woods» were Tungus, because, like QUATREMÈRE (*Not. et Extr.*, XIII, I, 276), he identified the name Uryangqai («Urianghit») with that of the Orončo or Oročon, the «Reindeer-Breeders», which is of course impossible. Nor have they anything to do with the \*Uryangqa of Manchuria of the Ming period. The Uryangqai proper must have been Mongols, and may have been the ancestors of some of the present Uryangqai tribes of the Republic of Tuva (on the complex and often uncertain question of the origin of the modern Uryangqai, cf. GRUM-GRŽIMAĬLO, *Zapadnaya Mongoliya i Uryankhatskii Krai*, III, 22-25, and add *Mêng-ku yüan-liu chien-chêng*, 6, 9 a-b). As to the «Uryangqai of the Woods», they too were probably Mongols, although this is not certain (cf. *Ber*, I, 90, 141; and the emended version in a note to the Persian text, 186). We are not in a position to say why the «Uryangqai of the Woods» were in hereditary charge of the tomb of Chinghiz-khan. It may be worth noting, however, that the Mongol legend connects the name of Uryangqai with that of the Burqan-qaldun at an early date. According to the *Secret History* (§ 9), the father of Alan-qo'a (Alan-yo'a), Qorilartai-mārgān, resenting the fact that his territory to the south-east of the Baikal had been made into a *taboo* district where he could no longer hunt, moved to the region belonging to Burqan-bosqaqsan and Sānči-bayan, the Uryangqai lords of the Burqan-qaldun. In § 97 (cf. also § 211), the Uryangqadai (the ethnical derivative form of Uryangqai) Jarči'udai-ābügān comes from the Burqan-qaldun to bring his son Jālmā to Chinghiz-khan. So there must have been Uryangqai people at the Burqan-qaldun before the death of Chinghiz-khan. We do not know whether they were Uryangqai proper or «Uryangqai of the Woods». On the other hand, Rašidu-'d-Din may have confused the two in the case of the tomb of Chinghiz-khan, as he did in other passages. That there actually were Uryangqai at the tomb of Chinghiz-khan and that they were real Mongols seems to be confirmed by the passage in «Sanang Setsen», where Dayan-ḥan († 1543), addressing various Mongol tribes, says: «And you, Uriangqan, who have

watched over the golden treasures (*altan kümürgä*; 金穀倉庫 *chin-mu ts'ang-k'u* in the Chinese version, 6, 6 b) of the Lord (*äjän*, = Chinghiz-khan), you too are a nation (*ulus*) with a great destiny (*yäkä jiya'atu*)» (SCHMIDT, 191).

When Mongka died in Ssü-ch'uan (1259), his son Asutai (*Bl*, II, 335-336; cf. *Oh*, II, 333-334) «took the coffin of his father and brought it to his *ordos* (Pers. plur. *ordohā*). In the four *ordos* of [Mongka], the mourning rites were performed, the first day at the *ordo* of Qutuqtai-ḥatun, the second day at the *ordo* of Qutluq-ḥatun, the third day at the *ordo* of Čabui-ḥatun (?) who had served [Mongka] during his campaign [in China], and the fourth day at the *ordo* of \*Kütäni-ḥatun. Every day, the coffin was put on a table in one of the *ordos* . . . Afterwards, they carried it to the place Būrḡān-qāldūn, which they call يکه قوروق *Yäkä-qōrūq*, and buried it by the side of [those of] Chinghiz-khan and Tului-khan.»

In 1292, Qubilai's grandson Kamala was sent to the north, in command of the troops there and in charge of the «four great *ordos*» of Chinghiz-khan (*YS*, 108, 1 b; 115, 5 a). According to Rašidu'd-Dīn (*Bl*, II, 591-592), whose text contains several obscure and doubtful names, Kamala wielded considerable power over the regions of Qara-qorum, the Onon and the Kerulen, Kämkičüüt, the Selenga, Qayaliq — as far as the boundaries of the Kirghiz — and the «great 'forbidden precinct' (*γorūq-i buzug*) of Chinghiz-khan which is called Būrḡān-qāldūn». Haidar-Raḡi speaks of a temple erected by Kamala at the Būrḡān-qāldūn (QUATREMÈRE, *Hist. des Mongols*, 120).

*Yäkä-qorūq*, *lit.* «Great *qorūq*», is a slightly «turkicized» form of Mong. *Yäkä-qoriq*, «Great *qoriq*». Both Turkish *qorī-* and Mongolian *qorī-* (< *qorī-*) mean «to enclose», «to guard», and «to forbid». The technical sense of «tabooed precinct», «ground reserved for the prince», already occurs in Kāšyari for the Turk. *qorīy* (BROCKELMANN, 160). We even find in the same work (*ibid.* 161) the *nomen agentis qoruyčī*, «keeper [of a *qorīy*]», which shows a secondary *-u-* < *-ī-* vocalism in the second syllable. This secondary *-u-* vocalism, which also occurs in the *Qutaḡyū bilig*, is common in later Turkish dialects (cf. Čay. *qoruy* and *qorūq*, Osm. *qoru*). From the Turkish, the word passed into Persian, mainly as قوروق *qorūq* (but with many other spellings, قوروق, قوروق, قوروق, قوروق, قوروق, قوروق; cf. VULLERS, II, 721), so that *qorūq-i buzug* is the exact Persian equivalent of the Mong. *Yäkä-qorūq* < *Yäkä-qoriq*. On the technical use of *qorīy* > *qorūq*, cf. the long note of QUATREMÈRE in *Not. et Extr.*, XIV, 1, 65-66. Hunting and the felling of trees were prohibited in *qorīy* places. This was what Qorilartai-mārgān resented (cf. *supra*) when his territory had been made *qorīy* (in Mong. *qoriq*; *qorilaldu-*; the whole story may have developed because of the phonetic analogy between *qoriq* and the name of Qorilartai-mārgān, lord of the Qori-Tumat). In Kalmuk, *χōr'g* (< *qoriq*) now means «prohibition», and the word for a *qoriq* area is the derivative *χōf'ül* (< *qorī'ul*; cf. RAMSTEDT, *Kalm. Wörterbuch*, 193). The tomb of Chinghiz-khan, of Tului, and of part of Tului's descendants was the «Great *Qoriq*», but it was not the only one, nor was a *qoriq* always the site of a tomb. Plan Carpine had travelled past a bush which Ögödäi had consecrated to his own soul (*dimisit unum virgultum crescere pro anima sua*; *Wy*, 43). Nobody was allowed to cut any twig from it; Plan Carpine refrained from infringing the prohibition, although he badly needed a switch to whip up his horse. The bush was clearly a *qoriq* bush. Chinghiz-khan's tomb was the «Great *Qoriq*», as distinguished from other *qoriq*. BEREZIN was mistaken when he construed Rašidu'd-Dīn's statement as meaning that the tombs of

the other sons of Chinghiz-khan and of their descendants lay in other parts of the cemetery; they were in other regions, and that is what Rašid really meant to say. The fact is well known, even in the case of Tului's lineage, for his son Hülägü and Hülägü's successors were all buried in Persia. Persian texts speak more than once of the «*yorūq* (or *qōrīγ*) of Sulṭāniya» or of the «*qorūq* of Aryūn», the latter one being the place where the *ilkhān* Arγun was buried (cf. *Not. et Extr.*, XIV, 1, 65). Plan Carpine knew of only two Mongol cemeteries (*Wy*, 44), one for the Emperors and the great leaders, to which they were carried, when possible, wherever they might have died, the other for the many (*multi*) Mongols who had been killed in Hungary. Plan Carpine, who did not go farther east than Qara-qorum, speaks of the Imperial tombs only from hearsay, and unduly extends the number and the quality of their occupants. But he passed by the cemetery of the princes killed in Hungary; he even entered its precincts unwittingly, and had a narrow escape from the keepers (clearly the Turk. *qoruqčī*, Mong. *qoriqčī*; *Qoriqčī* occurs as the name of an individual in *Bl*, II, 331). RISCHE (*Johann de Plano Carpini*, Leipzig, 1930, 86) expressed the opinion that the cemetery was probably in Hungary. This is certainly an error. Plan Carpine's journey, either way, did not take him to Hungary; moreover, his account implies that the cemetery was in the hands of the Mongols, and the Mongols had vacated Hungary long before 1245-1246. The cemetery must have lain somewhere between Kiev and the Volga. On the other hand, it is clear that the Mongols would bring back from Hungary to southern Russia only the bodies of their higher leaders, probably even only of the princes. I have no information on the tombs of the members of the branch of Jöčī, but they must have been buried in the basin of the Volga. The tombs of those of the Čayatai branch are surely to be looked for in the region of the Ili and of both Turkestans, and Qaidu, a grandson of Ögödäi (see «*Caidu*») was buried in the mountains between the Ču and the Ili (cf. BARTHOLD, *12 Vorlesungen*, 186). According to Rašidu-'d-Dīn, Ögödäi's branch was not represented among the Chinghiz-khanids buried at the Yākā-qoriq; this would exclude Ögödäi and Güyük and is in contradiction with the Chinese authorities, according to which both those Emperors, like Chinghiz-khan himself, were buried in the «*Ch'i-lien Valley*». It is not clear, however, that Rašidu-'d-Dīn was entirely wrong. The *ordo* of a defunct Emperor used to be at no great distance from his tomb, and we know that after Güyük's death, his *ordo* remained in the region of the Emil (east of Lake Balkash); Rubrouck saw it there (*Wy*, 240). Moreover, the conditions which followed Güyük's death at the time of the regency of his widow, Oγul-qaimiš, in the region of the Emil make it quite probable that the body of the defunct Emperor was carried from the valley of the Urungu to the Emil and not to north-eastern Mongolia. As to Qubilai and his line, they are in one case said by Rašid to have been buried at the Yākā-qoriq, and in the two other texts above, as well as in another which will be dealt with farther on, the same historian seems to exclude them, or at least Qubilai himself, from the list. I shall revert to this point later.

In all the texts quoted above, Rašidu-'d-Dīn locates the Yākā-qoriq at the «*Bürqān-qaldūn*». The name is well known. It is the *Burqān-qaldun* of the *Secret History* (§§ 1, 5, 9, 89, 97, etc.), of the *Altan tobči* (GOMBOEV, 4<sup>8-9</sup> «*Burqān-γaldun*»; 120, misread «*Burqān-galdan*») and of «*Sanang Setsen*» (SCHMIDT, 57, 59). «*Burgin-gal-düt*», in PALLAS's translation of another late Mongol chronicle (*Sammlungen historischen Nachrichten*, 1, 21), is merely a faulty reading of the



same name. De GUIGNES (*Hist. gén. des Huns*, I, II, p. lvi) speaks of a mountain «Po-ulh-han-chan» where the Onon takes its rise, in the country of the Hsiung-nu. But the name does not go so far back. The whole paragraph is taken by DE GUIGNES from the miscellaneous list of mountains of Mongolia at the end of ch. 90 of the *Ta-Ming i-t'ung-chih*, where the name 不爾罕山 Pu-êrh-han-shan, «Burqan-Mountain», is itself taken in fact from the Chinese abridged version of the *Secret History*. The YS (134, 4 a) gives the biography of Kōkō, a Märkit, «whose clan lived in the region (地 *ti*) of 不里罕哈里敦 Pu-li-han-ha-li-tun (Burqan-qaldun)». «Burqan» can only be the usual Mongol word for Buddha (< Uiy. *burhan*). One of the lords of the Burqan-qaldun in the *Secret History* (§ 9) is called Burqan-bosqaqsan (= Burqan bosyaqsan), «Who has raised Buddha». *Qaldun* does not seem to have survived in Mongolian. The Ch'ien-lung Commissioners did not understand it, and changed Burqan-qaldun to an absurd Burqan-qurdun (lit. «Buddha-quick»; *Yüan shih yü-chieh*, 7, 14 b). NAKA (*Chingisu-kan jitsuroku*, 2) renders *qaldun* as 嶽 *yo*, «mountain», «peak», and T'U Chi (1, 3 a) as 峻嶺 *hsün-ling*, «lofty range»; I know of no authority for either interpretation. The word-for-word version of the *Secret History* merely says that Burhan-qaldun is the «name of a mountain». But, in § 164, *qaldun* occurs alone (in the plural *qaldut*), and there it is translated 崖 *yai*, «cliff». The true rendering of Burqan-qaldun must thus be «Buddha-Cliff». The *Secret History* (§ 1) expressly says of the Burqan-qaldun that it lay «at the source of the Onon» (*Onan-mürän-ü tär'i'ün-ä*). Mention is made elsewhere (§ 89) of «the Kōkō-na'ur of the Qara-Jirügän of the Sänggür River, within [Mount] Gürälgü, in front (= to the east) of the Burqan-qaldun»; the Sänggür is the modern Sängkür, which flows from north to south within the bend of the Kärülän. Ong-khan, after the victory over the Märkit at the confluence of the Orkhon and the Selenga, retired first to «Hökörtü-jubur» (? = \*Hükärtü-jubur, the «Valley [*ch'uan*, well-watered valley] where there are oxen) behind (= west of) the Burqan-qaldun, and afterwards to the «Black Forest» (on the south-eastern side of the southern bend of the Tūla). On the other hand, the Burqan-qaldun cannot be a whole range, since the Märkit, pursuing Chinghiz-khan, circled three times round it (*ibid.* §§ 102, 111). It may be that the name, being well known, was occasionally extended to a larger area, but it must specifically refer to a particular spot, a sacred «cliff» at the source of the Onon. On some maps, for instance that of ROBOROVSKIĪ's travels, the mountains at the source of the Onon are expressly called Burhan-qaldun, and this was considered by T'U Chi (1, 3 a) as decisive for its identification. As a matter of fact, the location is quite probable, but appears to be based merely on historical research. The name Burqan-qaldun seems to be unknown to modern Mongols; it does not appear in any modern work I know of — neither in the Chinese maps of the Manchu dynasty (nor in D'ANVILLE who followed them), nor in the *Mêng-ku yu-mu chi*.

In the late Mongol legend, when the car with Chinghiz-khan's coffin remained stuck in the mud at Muna (cf. *infra*), the Sünit Kälägütäi-ba'atur (or Kilügän-ba'atur) beseeched the defunct Emperor to proceed to his native land. In the course of the song, he reminds him of «Burqatu-qan, thy land and water» (*Altan tobči*, 41<sup>7</sup>, 147; «land and water», *yaĵar usun*, is the Mongolian equivalent of the Turk. *yār-suv*, which refers both to the «mother land» and to its «spirits» or deities), or of «the pasture-lands of thy great people at the fortunate Burqatu-qan» (SCHMIDT, *Gesch. der Ost-Mongolen*, 106<sup>17-18</sup>, 107; *ölyäitiü*, «fortunate», also implies divine action).

SCHMIDT (p. 379) and WANG Kuo-wei (in a note to the Chinese translation, 4, 7 b) considered that Burqatu-qan was merely another form of Burqan-qaldun; I think they were right. In modern Mongolian, *qan*, *han*, «khan», is often used as the last element in the names of mountains. The same practice probably obtained already in Middle Turkish: «Altunqan», the name of a mountain of the Uighur country in Kāšyārī (BROCKELMANN, 240) should be considered as Altun-han = Altai (see «Altai»), and the «Qadīrhan» Mountain of the Orkhon runic texts must be Qadīr-han, «Severe Khan», identical with *qadīrhan*, the title of the *qayan* in Kāšyārī (BROCKELMANN, 246; TP, 1930, 52-53). *Burqan*, *burhan*, «Buddha», occurs more than once in the orography of Central Asia; our maps show a «Burkhan Buddha Range» (a strange duplication) south-west of the Kōkō-nōr, and a «Burkhan-ōla» south of Uliasutai. «Burqatu» would be an adjectival form, «Having Buddha» (though I know no example of it). It may even be, although not a single mountain name ending with *-han*, occurs in the *Secret History*, that the form Burqatu[-qan] goes back to the 13th cent. QUATREMÈRE (*Hist. des Mongols*, 120) alludes to a passage of Rašīdu-'d-Dīn in which the Onon, the Kerulen and the Tūla are said to take their rise in a mountain called برغادو B.ryādū or برغد، B.ry.da. The first vowel is not indicated. On the other hand, \*Buryadu might be construed as an older form of Buryasutu or Buryasutai, «Having water-willows», a name of frequent occurrence in modern Mongolia (twelve places of that name are listed in POPOV, *Mên-gu yu-mu czi*, Index, p. 32; for the possibility of deriving \*Buryadu from *buryasun*, cf. \*Hičätü < hičäsün + tü [see «Caccia-modun»]). T'u Chi (75, 3 a), on another occasion, thought of a Burqatu = Buryatu as meaning «Having water-willows». A Buryači is known between Kulja and Tarbayatai (POPOV, 463). Onggin-Burhatai (misread «Ungu-burhatai» in POPOV, 73) and a stream Burhatai (POPOV, 358), which are in Mongolia, are names the forms of which are more probably derived from *buryasun* than from Burqan, Burhān. In the same way, in the modern nomenclature of Mongolia there are several Uliatai (< Uliyatai; POPOV, 284, 371 [where «Uliasutai» is a misreading]) together with the more common Uliastai and Uliasutai (cf. POPOV's index, 62<sup>2</sup>, 63<sup>1</sup>). Nevertheless, in view of the location, and also on account of the Burqatu-han of the late Mongol legend, I incline to QUATREMÈRE's view that B.ryādū is Burqatu, another name of the Burqan-qaldun.

In all the above texts, Rašīdu-'d-Dīn located Chinghiz-khan's tomb at the Burqan-qaldun; but elsewhere he gives information which seems to contradict this. According to SCHMIDT (*loc. cit.* 389), Rašīd speaks also of the tomb of Chinghiz-khan as being in the country of Qara-qorum and at «Nuda Undur» in the vicinity of the Selenga. I do not know the passage on Qara-qorum (WOLFF, *Gesch. der Mongolen*, 117, certainly copied SCHMIDT); it appears to be some vague or distorted statement, which we may surely leave out of consideration. The case of «Nuda Undur» is more puzzling. SCHMIDT corrects the reading to «Nuta Öndör», without giving any reason. The «Nuda Undur» in HOWORTH (1, 107) seems to be taken from SCHMIDT's note. SCHMIDT gives no authority for the name; WOLFF (*loc. cit.* 117) takes it for that of the whole «district», but the very form he gives, «Nuta Öndör», shows that, here again, SCHMIDT is his only source. As a matter of fact, «Nuda Undur» is, directly or indirectly, itself derived from Rašīdu-'d-Dīn, where we find the two following passages:

a. (*Bl*, II, 560-562): «After Ariq-bögä's death at Qubilai's Court, his sons were sent to his own territory (*yurt*). Ariq-bögä used to spend the summer (*yailaq*) in the Altaï (التای); such is the

true reading, not the الباس تو, tortured so as to give the 'Ulyasutai' of BLOCHET's edition) and the winter (*qīslaq*) at the اورنگه \*Urüngä (= the Urungu of our maps, the Ūrünggü of the *Secret History*, §§ 158, 177; such again seems to be the correct reading for the اورنگه of the mss., certainly not 'Uryāngqat' as in BLOCHET) and... (the name is corrupt; BLOCHET's \*Qırqız, 'Kirghiz', seems improbable here); the distance between both is a three days' route. Soryoqtani-bägi (or -bäki; the mother of Mongka, Qubilai, Hülägü and Arıq-bögä; on the name, misread by BLOCHET, cf. *TP*, 1932, 43-54) had been there (but she died in 1252, many years before Arıq-bögä). Arıq-bögä was one month and six days in the service of [Qubilai]-qa'an, and then died. They brought him to بوده اوندور Buda-ündür, which is the great 'forbidden precinct' (*γorūq-i buzurg*) of Chinghiz-khan, in the vicinity of the Selenga River ((ردخانه سلنگه)); Soryoqtani-bägi and the other princes have also been buried there, except (أى illa) Qubilai-qa'an...» In the mss., the name I have read Buda-ündür is written نوره اوندور and نوره آوردند, نوده اوندور, which explains the «Nuda-undur» mentioned by SCHMIDT.

b. (*Bl*, II, 576-577) : «Another [chief] was جانگقى كورگان Čangqī-kūrāgān ('the Imperial son-in-law Čangqi'; he must be the Imperial son-in-law 'Čanggi' often mentioned in the *YS* as 長吉 Ch'ang-chi, 昌吉 Ch'ang-chi and 章吉 Chang-chi; cf. WANG Hui-tsu<sup>1</sup>, 19, 3 b; his name occurs first in 1275; he was made a prince in 1287; for the name, cf. another Čanggi in the *Secret History*, § 277) of the Jalair (not 'Čalair' as in BLOCHET's edition), emir of a chiliarchy (*hāzārū*). This is the chiliarchy which was formerly that of an emir called اوقى Uqi, and this [Uqi], with a chiliarchy of Oirāt (*var.* Urāt), by the rule of *yarlty* (*i. e.* on account of an Imperial order) kept guard at بوده اوندور Būda-ündür (*var.* Būda-ūmūr), which is the great 'forbidden precinct' (*γorūq-i buzurg*), and the bones (*usth-wānhāt*) of the princes (*šahzādāgān*) are deposited there. When the princes who were under the orders of Nomogan rebelled (see 'Nomogan'), and the troops went with them, most of this chiliarchy joined the troops of Qaidu; [but] some remained there. Now, this chiliarchy belongs to the children of Uqi.» In view of the other passages, translated above, it is tempting to correct «Uqi» to «Ūdači» and «Oirāt» (or «Urat») to «Uryāngqät». The place intended cannot be any other than the «Great qoriq», Yäkä-qoriq, of the Burqan-qaldun. On the other hand, the readings in the present passage are in favour of the correction «Buda-ündür» in the preceding one. This was already the solution adopted by QUATREMÈRE (*Hist. des Mongols*, 118), which BLOCHET accepted. QUATREMÈRE also said that «Buda» must render Buddha, of which Burqan is the Mongolian equivalent. I feel inclined to agree with him, though partly only with BLOCHET, who explains «Buda-ündür» as «the great Buddha» (*Bl*, II, 561) : *ündür* is a geographical term, and Buda-ündür can only mean «Buddha Height», «Buddha Hill». One obscure point remains to be cleared up. QUATREMÈRE and BLOCHET found it quite natural that «Buda-ündür» should be in the vicinity of the Selenga. But the Selenga is in an entirely different region, west of the Tūla and the Orkhon. If Buda-ündür, as is practically certain, is but another name of Burqan-qaldun and of Burqatu-qan, the mention of the Selenga is a bad slip of the Persian historian.

The location of the Burqan-qaldun, and consequently of the place where Chinghiz-khan was buried, which, according to Rašidu-'d-Din, was at the source of the Onon, entails another important consequence. Several Mongol noblemen claiming descent from Chinghiz-khan told GAUBIL

(*Hist. de Gentschiscan*, 54) that the tomb of their great ancestor was at the «Han mountain», *i. e.*, according to GAUBIL, lat. 47° 54' and long. 9° 3' (west of Peking; cf. also GAUBIL's ms. notes quoted by QUATREMÈRE, 119). This is the «Han alin» («Han Mountain») of D'ANVILLE's Map («Tartarie chinoise», 7th sheet), the 漢山 Han-shan («Han Mountain») or 汗山 of the Chinese maps, facing Ulān-bātor (Urga), just south of the eastern end of the southern bend of the Tūla (cf. also TIMKOVSKI, *Voyage à Pékin*, I, 119; II, 426 sq.). I think that some misinterpretation of this location south of the Tūla is responsible for the mention found in D'ANVILLE («Tartarie chinoise», 4th sheet) that the Imperial tombs of the Mongol dynasty are supposed to be in the mountains south-west of the lake into which the Ongin flows; the same indication is found in the «Carte générale de la Tartarie chinoise» in DU HALDE, t. IV, coupled with a wrong location of Qara-Qorum on the southern bank of the Ongin, near the lake. Whatever the truth may be, it is a fact that the Pu-êrh-han-shan (= Burqan-qaldun) of the Chinese abridged version of the *Secret History* has been identified with the 汗山 «south of the Tūla» by the author of the *Mêng-ku yu-mu chi* (POPOV, 347-348). Since 汗山 means «Mountain of the Khan», such an identification appeared convincing to most scholars, *e. g.* DE GUIGNES (*Hist. gén. des Huns*, III, 74), D'OHSSON (*Oh*, I, 384), QUATREMÈRE (*Hist. des Mongols*, 120), YULE (*Y*, I, 247), HOWORTH (I, 107) and BLOCHET (II, 336). It is, however, clearly untenable. A tomb cannot at the same time be at the source of the Onon as well as on the left bank of the Tūla, opposite Ulān-bātor. The 汗山 is a sacred mountain, a *goriq*, that of Ulān-bātor, but there is not the slightest ground, either in ancient texts or in modern tradition, to locate the tomb of Chinghiz-khan in that region.

After Chinghiz-khan's death, moreover, his four main *ordos* were probably installed at a place not far from his tomb, and, although Rubrouck never visited them, he knew that they were in the region of the «Onan Kerule», *i. e.* of the Onon and the Kerulen (*Wy*, 208, 268). It must have been there that T'ai-tsung went in 1232 and Mongka in 1257 when they visited the *hsing-kung* (= *ordo*) of T'ai-tsu (= Chinghiz-khan); there too that Yāsūn Tāmūr (T'ai-ting) published his edict of amnesty in 1323 (cf. *supra*).

But Rašīdu-'d-Dīn's texts were not familiar even to modern Chinese scholars, and there is now a tradition among the Mongols that Chinghiz-khan's tomb still exists in the region of the Ordos. The *Tz'ü yüan* (*s. v.* Ch'i-lien-ku) has no hesitation in stating that the Ch'i-lien-ku is on the north side of the Altan Mountain, north-west of the Huang-ho, in the central banner of the right wing of the Ordos, and that the tomb of Chinghiz-khan is there. A protracted controversy over this question between T'u Chi and 張相文 CHANG Hsiang-wên was published in the geographical collection *Ti-hsüeh ts'ung-shu* and in the geographical review *Ti-hsüeh tsa-chih* of 1921 (cf. the list of the papers in the bibliographical appendix of YANAI, Nos. 418-425). I have had no access to the original papers, but T'u Chi's views are summed up in his book. Moreover, most of his arguments have been translated by CHARIGNON (*Ch.* I, 197-208), unfortunately with many mistakes, beginning with one which makes him call his author 武進 «Wou Tsin», since he mistook his native place Wu-chin for his name.

The starting-point of the controversy was of course the modern tradition of the Ordos, for which certain Chinese authors thought they found confirmation in «Sanang Setsen». According to the Mongol chronicler, after the death of Chinghiz which he places at Dörmägäi (= Ling-chou),

his body was put on a cart that went north. But when the procession reached Muna-yin kögübür (*Altan tobči*, 40<sup>6</sup>) or Muna-kögäbür («Sanang Setsen», 106<sup>6-7</sup>), the car stuck up to its nave in the mud and could not be moved. It was on that occasion that a Sünit leader addressed the corpse with the song of which mention has already been made. SCHMIDT (p. 107) merely rendered Mona-kögäbür «the region of the Mona», which seems inadequate; GOMBOEV (p. 147) understood *Muna-yin kögübür* as meaning «the marshy place of the Muna», which is in agreement with the Chinese translation (4, 6 b; 穆納之淖泥處, «a muddy place of the Mu-na»). KOWALEWSKI, on the strength of the present passage, tentatively read *kügübür* and rendered it «promontory»; GOL'STUNSKIĪ, also in referring to «Sanang Setsen», gave «swollen» as the meaning of *kügübür*. I think that we should read *kögübür* (*kö'übür*) and *kögäbür* (*kö'äbür*), both > *köbür*, and that this is a noun derived from *kögä-*, *kö'ä-*, «to swell», «to rise» > Kalm. *kö-* (RAMSTEDT, *Kalm. Wörterbuch*, 243<sup>1</sup>). It is not a place name, and we should probably translate it «a swollen place of the Muna» (in another passage, p. 249, SCHMIDT made of *kögäbür* a place name, «Köke-Bor»; there is no place name in the Chinese translation, 7, 10 b, but the sentence is puzzling). In an earlier passage, when Chinghiz-khan was marching from Mongolia against the Hsi-Hsia, he arrived at the Muna-ḥan, i. e. the Muna Mountain, and exclaimed (SCHMIDT, 99; I have retranslated the passage): «For a dispersed nation, this is a fine place of refuge; and for a peaceful nation, this is good pasture-land. For deer and stags it is a fine hunting place, and for an old man a good resting place.» Incidentally, it may be noticed that the ordinary texts of the Chinese version give in this passage Na-mu instead of Mu-na, and this is the origin of the pseudo-Lake Na-mu in *Ch*, I, 206; but the Palace ms. of the Chinese version correctly has Mu-na, as in the Mongol original. WANG Kuo-wei (cf. his note in the *Mêng-ku yüan-liu chien-chêng*, 4, 3 a) has called attention to the fact that this Mu-na must be the 母納山 Mu-na-shan («Mu-na Mountain») where, according to the *Ming shih* (327, 4 a; 328, 2 b), the Mongol leader A-lu-t'ai (Aruḡtai) sought refuge when he fled from Toqto-buqa, and also the «land of 母納 Mu-na of the Huang-ho» mentioned in another passage of the *Ming shih* (328, 3 b). WANG concluded that the Mu-na Mountain of «Sanang Setsen» must be in the neighbourhood of the great bend of the Yellow River. I think we can go one step farther. WOLFF (*Gesch. der Mongolen*, 112; cf. also HOWORTH, I, 101) says that the Mona-ḥan, belonging to the system of the Yin-shan, was situated at the north-western end of the great bend of the Huang-ho, on the road from the Gobi to Ning-hsia. Although this is not quite correct and WOLFF does not give any authority, he seems to have already noticed the text which, in my opinion, provides the true location. In 1697, GERBILLON went from Peking to Ning-hsia in the train of the Emperor K'ang-hsi. On May 30, following the northern part of the great bend of the Huang-ho, but south of the river, he camped «in a great meadow which», he says, «extends to the Huang-ho, opposite a mountain which is on the other side of the river and is called Mona; as the river here makes some sort of an angle, it is called *Mona hojo* (= Mona qoṣō < Mona qoṣi'un, 'Mona promontory'). There was good fodder in that meadow, but it was marshy in many places. Many Mongols are camped here and there, on account of the convenience of the fodder» (DU HALDE, IV, 378). The «Mona hojo» is marked on DU HALDE's maps. This marshy place must be the «swollen (= marshy) place of the Muna» mentioned in the account of the *Altan tobči* and of «Sanang Setsen». As to the Muna-khan, it

must be the Mona Mountain mentioned by GERBILLON on the northern bank of the Huang-ho. It was not unknown to Chinese geographers, and the name of the 木納山 Mu-na-shan occurs in the *Mêng-ku yu-mu chi*, 5, 8 a, although it has been omitted in POPOV's translation (p. 300, n. 321). Nor do I think there can be any doubt as to its present identity. It is the «Muni-ula» («Muni Mountain») of ROBOROVSKIĬ's map and of the Yü-lin-fu sheet of the German map of East China, a range which runs from west to east, just north of the Huang-ho, and west of Pao-t'ou.

It is clear that the importance given to the Muna in the *Altan tobči* and in «Sanang Setsen» has some significance in the history or at least in the legend of Chinghiz-Khan. The *Altan tobči* does not give Chinghiz's effusions on the subject of the region of the Muna-ḥan, but, after relating the episode of the car stuck in the mud in that place, it adds (p. 147) : «On a former occasion, when the Ruler passed through this place, it pleased him, and that is why the car sank up to its nave. Later false rumours spread among the people (that he was buried there) . . . » In a note, GOMBOEV adds that, even now, the belief obtains among the Mongols that a man, when passing a place, must not show that he likes it, since this would foretell that, sooner or later, he will die there. SCHMIDT was also struck by the episode, and, as he could not believe that the body of Chinghiz-khan had been carried as far as Burqan-qaldun, he expressed the opinion (p. 390) that the praise given by Chinghiz to the Muna-ḥan and the story of the car stuck in the mud at Muna were intended to express the choice made of the Muna by Chinghiz as his last resting place. In China also, this has been adduced by the upholders of the Ordos as the region where Chinghiz-khan was buried. There can be no doubt that the account more or less suggests such a conclusion, but it is a late development and, even so, full of contradictions. In 1226, Chinghiz-khan marched against the Hsi-Hsia not across the great bend of the Huang-ho, but by coming direct from Mongolia to the Etsin-yol. The mention of the Altan-ḥan at the north-western angle of the bend of the Huang-ho (SCHMIDT, 103, and 388, 389, when it is erroneously identified with the Liu-p'an-shan in Kan-su) is also due to the same tendency (see «Altai»). I have little doubt that the words put into the mouth of Chinghiz-khan on the Muna-khan that it was a good place of refuge for a dispersed nation really allude to Aruqtai's flight to the Muna Mountain, which took place in the 15th cent. Both chronicles are of the 17th cent., at a time when power among the Mongols had passed from northern Mongolia to the Ordos region. In the passages on the Muna-khan and the Muna marsh, we have merely the first state of the claims which we shall examine when they have reached their full development. But, as the commentator of the *Mêng-ku yüan-liu* remarked (4, 7 a), the very text of the song of the Sünit bard, with its constantly recurring mention of places in Mongolia like the Burqatu-qan and the Kerulen, still testifies to the belief that the goal of the procession was really Chinghiz-khan's *ordo* in northern Mongolia.

This would seem to be confirmed by the indications of the chronicles on the location of the tomb. In the *Altan tobči* (42<sup>8-9</sup>, 148), «his true corpse (*ünän kă'ür*) was buried, according to some, at Burqan-qaldana (read «Burqan-qaldun»), according to others at the Yäkä-undui which is at the back of the Altai-qan and in front of the Qadayiqan». According to «Sanang Setsen» (SCHMIDT, 109), «the golden corpse of the Ruler was buried at a place called Yäkä-ütäk, which is at the back of the Altai-qa'an and on the sunny side of the Kântäi-qa'an». The Chinese version (4, 8 b) gives «the place [called] Great Ötäk, on the shady

side of the Altai Mountain and on the sunny side of the Qadai Mountain». «Yäkä-ütäk (or ötäk)» is unknown. CHARIGNON's «Ta-o-t'ê-k'o», «Great O-t'ê-k'o» (*Ch*, I, 201), is merely taken from the Chinese version of «Sanang Setsen» (POPOV's «Da-Otok» p. 313, is equally misleading). On the other hand, there is an 鄂圖科 O-t'u-k'o (or O-t'u-[克]k'o) Mountain, \*Ötük > \*Ötök, the «Oudouc Alin» of D'ANVILLE's map, the «Êtuk» of POPOV, 352, north of the Tūla and opposite the Ḥan-ūla (it is the pseudo-«Gontou» of *Ch*, I, 200); but it is not known as «\*Yäkä Ötük», and there is nothing to show that «Sanang Setsen», whose family connections were in the Ordos region, ever thought or even knew of it (in the present state of our knowledge, it would be arbitrary to try to connect the name of the \*Ötük Mountain with that of the sacred Ötükän Mountain of the ancient Turks, on which cf. *TP*, 1929, 212-219). In view of the «Yäkä-undui» of the *Altan tobči*, I suspect that both its form and that in «Sanang Setsen» are corrupt for \*Yäkä-ündür, «Great Height», «Great Hill». In other words, it would be merely another form of the Buda-ündür of Rašidu-'d-Dīn, *i. e.* another name of the Burqan-qaldun. As the *Altan tobči* shows, the tradition of the burial at the Burqan-qaldun, attested at an early date by Rašidu-'d-Dīn, survived in Mongolia. The same was expressed by texts speaking of the \*Yäkä-ündür, but the author of the *Altan tobči*, who had the name in a corrupt form, did not recognize it, and was erroneously led to believe that it represented a different tradition.

That the \*Yäkä-ündür is identical with the Burqan-qaldun would seem also to be shown by its location north of the Altai and south of the Kāntāi. The name of the Altai had in Mongol popular usage a wider range of application than in our nomenclature. Although, strictly speaking, the Altai lay in western Mongolia, both Polo and the Mongol chroniclers agree in mentioning it in connection with the tomb of Chinghiz-khan. It seems to have been a vague designation of the range of mountains which extended south of the Tūla and the Kerulen and which, like the mountains north of Peking, were and are sometimes still known as the Ḥinggan Mountains (the 興安 Hsing-an of the Chinese; see «Altai»). The Chinese translation of «Sanang Setsen» (4, 8 b) gives here the same reading 阿勒台 A-lo-t'ai, Altai, as in SCHMIDT's text. It would be arbitrary to change it to 阿勒坦 A-lo-t'an, Altan, as is done by T'U Chi (3, 33 b), in order to connect it the more easily with the 按坦孛都罕 An-t'an-po-tu-han of *YS*, 149, 1 b (this is repeated in *Ch*, I, 204); the latter name probably renders \*Altan-botuqaṅ (= \*Altan-botuyan), «Golden-Young Camel», and has nothing to do with the name of the Altai Mountains. On the other hand, the Kāntāi Mountains are the mountains from the southern slopes of which the Tūla takes its rise; the sources of the Onon and the Kerulen are in the mountains to the east of the Kāntāi Range. I do not know the origin of the name, nor can I trace it back farther than the 17th cent. KOWALEWSKI reads the name «Gentei-ḥan», but all the Chinese transcriptions suppose an initial *k*-. The Chinese geographers of the Manchu dynasty distinguish a «Small Kāntāi», 小肯特 Hsiao K'ên-t'ê, to the north-west of the sources of the Kerulen and the Onon, from the Kāntāi Range more to the south-east. According to the *Mêng-ku yu-mu chi* (7, 22 a; this passage has been omitted by POPOV, 347), the native name of the «Small Kāntāi» is 阿即格肯特 A-chi-ko K'ên-tê; this name, written 阿集格克音特 A-chi-ko K'o-yin-t'ê in the Wu-ch'ang map, is the «Agigue Kentei» of D'ANVILLE. But it cannot be a «native», *i. e.* Mongolian name, since *ačige*, «small», is not Mongolian, but Manchu. In any case, it is the «Small Kāntāi» which is the highest range,

and which forms the line of demarcation for the rivers flowing north, north-east, south-east, and south-west. The Burqan-qaldun, as we have located it, actually lies north of the Altai, taken in its broader sense, and the tomb of Chinghiz-khan, especially if it be on the southern slopes of the Burqan-qaldun, may well be said to have been on the sunny side of the Kântäi Mountains. The conclusion would be that even the Mongol chroniclers of the 17th cent. agree, as to the site of the tomb, with the information provided at an early date by Polo and by Rašidu-'d-Din.

Yet, if I do not doubt the accuracy of the location, I am afraid that the mention of the Kântäi-qan is highly doubtful. The Kântäi-ḡan is mentioned elsewhere in «Sanang Setsen» (SCHMIDT, p. 159), but not in connection with Chinghiz-khan's tomb, and there the Chinese translation too (5, 15 b) renders the name 肯特汗山 K'en-t'ê-han-shan, «the Kântäi-ḡan Mountains». On the contrary, in the passage relating to Chinghiz-khan's tomb, the Chinese translation gives Qadai-ḡan. NAKA (*Chingisu-kan jitsuroku*, 582) has quoted SCHMIDT's edition of the Mongol text to correct Qadai-ḡan to Kântäi-ḡan, and T'u Chi (3, 33 b) concurred with him. But Qadai-ḡan, is also the reading in the *Altan tobči*. Now, the *Altan tobči*, the complete title of which is *Qad-un ündüsün quriyanggui altan tobči*, seems to be the first of the seven works used by «Sanang Setsen», that which he calls *Ḥad-un ündüsün-ü ärdäni-yin tobčiya* (SCHMIDT, 299). It cannot be an accident that we have «Qadayiqan» occurring in the *Altan tobči* as well as in the Chinese version of «Sanang Setsen». The only possible conclusion is that it is the original reading in both works (the second perhaps taking it from the first), and that «Kântäi-ḡan» in SCHMIDT's Mongolian text is a misreading, due to the influence of this second well-known name. Whether «Qadayiqan» (= Qadai-qan ?) be itself a correct ancient form is another question. The name exists in Mongolian. A postal relay «Qadayiqan» is mentioned in the *Mêng-ku yu-mu chi* (POPOV, 323), but remains unidentified; moreover, this may be merely an onomastic coincidence. My conclusion is that there is every probability of our being right if we locate Chinghiz-khan's tomb on the Burqan-qaldun, at the source of the Kerulen, but that we should not adduce, as additional proof, the pseudo-Kântäi-ḡan of SCHMIDT's edition of «Sanang Setsen», as has been done hitherto.

A «southern» theory for the tomb of Chinghiz-khan has, however, been proposed, and CORDIER, who summed it up in *Y*, I, 249-250, seems to give it pre-eminence in his *Histoire générale de la Chine*, II, 223. According to this theory, which has taken different forms, Chinghiz-khan's tomb would not be in upper Mongolia, but in the region of the Ordos.

The first Chinese exponent of such a view was 張穆 CHANG Mu, the author of the *Mêng-ku yu-mu chi* (6, 9-10; POPOV, 312-315, where the translation is somewhat abridged; the preface to CHANG Mu's work is dated 1859). CHANG Mu started from the name 伊克昭 I-k'o-chao of the «league» of the Ordos, which is the Mongolian Yäkä-jö (or Yäkä-dzö), meaning «Great Temple» (in Chinese 大廟 *ta-miao*). The 理藩院則例 *Li-fan-yüan tsê-li* («Regulations of the Court of border vassals», under the Manchu dynasty) says that «in the Yäkä-jö League, there is the tomb (園寢 *yüan-ch'in*) of Chinghiz-khan. The seven 'banners' of the Ordos have established five hundred families of *darḡat* to guard the tomb and make sacrifices...» The name Yäkä-jö, according to CHANG Mu, was given because of the tomb, which had never been in northern Mongolia. To determine its location, CHANG resorts to the «Altai» (which he changes to «Altan») and the «Qadai» of the Chinese version of «Sanang Setsen». He connects «Qadai» with an



otherwise unknown \*Ha-lu-t'ê (\*Qalut ?) Mountain, which is mentioned in 1680 and 1688 as lying six days' journey from the Urat (cf. also POPOV, 376; CHARIGNON, *Ch*, I, 205, mistakes «Qadai» for the Altai). As to the pseudo-Altan, it would be the Altan Mountain, to the north-west of and outside the great bend of the Yellow River, which in turn would be the same as the 阿爾布坦 A-êrh-pu-t'an (\*Arbutan) Mountain of the *Ta-ch'ing i-t'ung chih* (the whole of CHANG's argument has been accepted and repeated by KAO Pao-ch'üan, 14, 1-5). It is this \*Arbutan, erroneously identified by SHIH Yüan-chieh and KAO Pao-ch'üan with the Arbuqa of the *Secret History* (cf. *supra*, p. 316-317), which has become «Arbouz-ola» in *Ch*, I, 189. The latter form cannot be correct, as there is no final -z in Mongolian; *arbuz* is the Russian form of Turk. *qarpuz*, *qarbuз*, «water-melon». According to CHANG Mu, the \*Arbutan Mountain is outside the bend of the Huang-ho. Chinese and European maps have an «Arbus» Mountain, but within the bend of the river. Unfortunately I cannot find \*Arbutan on any map, nor can I account for either \*Arbutan or «Arbus» in Mongolian. But whatever may be the truth of this point, the fact remains that CHANG Mu's identification relies only on the phonetic resemblance between the name of the Altan Mountain, north-west of the Ordos, and that of the Altai in «Sanang Setsen». He would never have thought of it, had it not been for the «tomb» which was said to be honoured by the Yäkä-jö league.

According to GRUM-GRŽIMAĀLO (*Zapadnaya Mongoliya i Uryankhatskii krat*, II [1926], 64, perhaps quoting an unpublished ms. of ŽAMCARANO on «The Cult of Chinghiz in the Ordos»), «the *darhat* of the Ordos are convinced that Chinghiz was buried near the Muna Mountain, lying in the *hošün* of the Urat, north of the Huang-ho». This is merely the outcome of the legend of the Muna related by «Sanang Setsen», which I have discussed above (p. 344). The same author continues: «In the *Chronological History of the Mongols* which I [? GRUM-GRŽIMAĀLO, or ŽAMCARANO] copied in the Ordos, it is said that Chinghiz and his descendants, the Great Khans, were buried in the temple Či-nan-ḥu. The location of that temple is unknown.» Without being positive on the point, it seems to me as though «Či-nan-ḥu» were a modern Mongolian transcription of Ch'i-nien-ku = Ch'i-lien-ku, the Ch'i-lien Valley of the Chinese texts. In such a case, the information is of no value.

Hsü Sung (1781-1848) is quoted in the *Mêng-ku yu-mu czi* (POPOV, 313) as being indebted to the 德 TÊ *pei-tzŭ* of the Tümat for the following statement: «The tomb of T'ai-tsu (= Chinghiz-khan) lies in the extreme north-west, outside the territory of 榆林 Yü-lin. The place is called Čayän-ärgä; *čayän* means 'white'; *ärgä* means 'tent'.» CHANG Mu sees in *ärgä* another transcription of the Ötäk, Ötüik of «Sanang Setsen», which is of course impossible. Nor do I know any Mongolian word *ärgä*, «tent»; the word meant ought to be *örgä* < *örgü'ä*, «chieftain's tent». I suspect, however, that some misunderstanding may have here arisen, and that we should perhaps read Čayän-ärgi, «White Steep bank». A Čayän-ärgi, which I do not find on the maps, is mentioned more than once in the *Mêng-ku yu-mu czi* (6, 6a, 13b, 14a; POPOV, 49, 51) and seems to be in the required direction. For us, the information merely indicates that the Tümat prince thought that the tomb was in the Ordos.

The belief that the tomb was in the Ordos, *i. e.* within the great bend of the Huang-ho, became well known in Europe when two Belgian missionaries, DE VOS and VERLINDEN, published in the

*Missions Catholiques* of June 18, 1875 (No. 315) an account of their visit to its supposed site. Further information was furnished by PRŽEVAL'SKIĪ, POTANIN, ROCKHILL, BONIN and DE LESDAIN (cf. *Y*, I, 249-250; *Ch*, I, 196); CHARIGNON (*Ch*, I, 207-208) adds recent Sino-Mongolian statements which were made during the controversy of 1921, but to these papers I have had no access.

The name of the place is certain, and is correctly given by most travellers : it is «Ejen-ḥoro» < Ajän-ḥorā < Ajän-qoriya, «the Master's Residence» («Edjen-joro» in *Y*, I, 249, and «Edjen Djoro» in CORDIER, *Hist. gén. de la Chine*, II, 223, are misreadings erroneously attributed to POTANIN). But there are two «Ejen-ḥoro», the Yākā-ājän-ḥorā or «Great Master's Residence», and the Baya-ājän-ḥorā or «Small Master's Residence». The «Ejen-ḥoro» marked on our maps (for instance that of POTANIN or the German map of Yü-lin-fu), in the north-eastern part of the Ordos territory and south-west of the camp of the «Dzungar» Prince, as being the supposed tomb of Chinghiz-khan is in fact the Baya-ājän-ḥorā, the supposed tomb of the wife who is said to have killed Chinghiz. The Yākā-ājän-ḥorā reputed to be the tomb of Chinghiz lies much more to the south-west, not far from the encampment of the Wan Prince, and in the neighbourhood of the Čayān-nōr. Every year, relics of Chinghiz-khan are brought over there from different parts of the Ordos country, on the 21st day of the sixth moon according to POTANIN, on the 21st day of the third moon according to BONIN. The latter date is confirmed by a Mongol account (cf. *Ch*, I, 207); but we do not know why it was chosen. It may be worth noting that among the relics of Chinghiz brought on that occasion, his gun is listed! I am unable to restore most of the names given in PRŽEVAL'SKIĪ and BONIN's accounts. According to GRUM-GRŽIMAĬLO (*loc. cit.*, II, 64), the site of the Yākā-ājän-ḥorā where the relics are kept is called «Bayan-čonḥuk».

A fresh claim to the discovery of Chinghiz-khan's tomb was made in 1927 by the late P. K. KOZLOV. I know it only from the accounts which appeared in the *Berliner-Zeitung am Mittag* of October 31, 1927, and in the *China Express and Daily Telegraph* of November 3, 1927, and I hope it was not reported in the words of the Russian explorer. A direct descendant of Chinghiz-Khan is said to have revealed to KOZLOV that the tomb lay in the Gobi, «near the ruins of the dead city of Khara Khoto» (probably that of the Edzin-γol; see «Ečina»). «The remains, in a silver coffin, rest on seventy-eight crowns of princes and chieftains. . .» Among the treasures buried with the conqueror, is «a copy of the Bible written by an English monk». The whole story is a good illustration of the abysmal depth which separates credulity from credibility.

All the traditions locating Chinghiz-khan's tomb south of the Gobi, and especially those of the Ordos, may easily be proved to be late inventions; they are the result both of a confusion between the tomb itself and the *ordos* which attended to the cult of the defunct Emperor, and of the migration of these *ordos* from northern Mongolia into the great bend of the Huang-ho.

According to BARTHOLD (*Otčet o poézdké v Srednyuyu Aziyu*, 20), Rašidu-'d-Dīn speaks of stone statues (*kamennyya baby*) erected at the tombs of Chinghiz-khan and of the lineage of Tolui, in front of which sweet-smelling substances were constantly burnt. Unfortunately, I cannot discover the corresponding passage either in BEREZIN or in BLOCHET; BARTHOLD's interpretation can, however, hardly be correct. The word used (in the plural) by Rašid is *šūrat*, «portrait», which may mean a pictorial representation, or a felt or a woven figure, just as well as stone sculpture; on this point I quite agree with the objections raised by GRUM-GRŽIMAĬLO (*Zapadnaya Mongo-*

*liya*. . ., II, 61-66). Moreover, we know from many sources, and from Rašid himself, that there was no outward sign to indicate the site of the tomb. Whatever the wording of the passage may be (BARTHOLD only quotes one sentence from it), and whether the fault lay with the Persian author or with his Russian commentator, the «portraits» must have been the images which are often mentioned by mediaeval travellers and by Chinese sources; at first they hung in the tents of the *ordos*; at a later period of the Mongol dynasty, when Chinese influence became predominant, these portraits, woven with brocade, were placed in various temples in or near the capital (cf. *YS*, 33, 10 a; 75, 8 a). Both the *Altan tobči* (425, 147; the text however is not always clear) and «Sanang Setsen» (SCHMIDT, 109) speak of «eight white tents (or chambers)», *naiman čayān gār*, erected after the funeral at or near Chinghiz's tomb. This can only refer to conditions that prevailed at a later date. In 1266, the ancestral temple of the Mongol Imperial family at Peking was finally laid out by Qubilai with eight «chambers» (室 *shih*), dedicated respectively to Yäsügäi, Chinghiz, Ögödäi, Jöči, Čayatai, Tolui, Güyük, Mongka, together with the first wife of each of them. Despite the new regulations adopted by Qubilai's successors, the principle of the eight «chambers» was long maintained, although we hear of ten «chambers» in 1327; at that time, of the eight of 1266 Chinghiz and Tolui alone had «chambers», all the others having been allotted to later sovereigns (cf. *YS*, 74; also 69, 4-5, 9-10). The whole scheme is of Chinese origin. Chinghiz-khan had in Mongolia four main *ordo* (plur. *ordos*), in which his cult must have been celebrated, and some time after his death these may have been moved together to a place perhaps not far distant from his tomb. But there is no apparent reason for the existence of the «eight white tents» of the late Mongol chroniclers, except as a notion which must have originated from the eight «chambers» of the ancestral temple created by Qubilai.

These *ordos*, which Rubrouck knew to be in the region of «the Onon and the Kerulen», are the ones to which Ögödäi repaired in the beginning of 1233. In 1257, Mongka, visiting Chinghiz-khan's *ordo*, sacrificed to his «standard and drum»; this shows that relics of the conqueror were kept there. In 1292, Qubilai's grandson Kamala was put in charge of the guard of the «four great *ordos*». His son Yesün-tämür (the Emperor T'ai-ting) succeeded him there in 1302, and there also ascended the Imperial throne in 1323, since the edict of amnesty issued on that very day is dated «at the great *ordo*(s) of Chinghiz-khan». But we know from the same text that this was «at the Lung-chü River», i. e. at the Kerulen (cf. *TP*, 1905, 36-37); so the four great *ordos* must then have been in the region of the upper Kerulen. On the other hand, we have seen (p. 338) that, according to a Persian historian, Kamala had erected a temple on the Burqan-qaldun; it is very tempting to see there Chinghiz-khan's funerary chapel in his *ordos*, and perhaps the prototype of the «eight white tents (or chambers)» of later Mongol chroniclers. Chinghiz-khan's *ordo*, or in the plural *ordos*, are mentioned in the *YS* more than once after 1323 : in 1331 (*YS*, 35, 8 b : *ordos*); in 1340 (*YS*, 40, 2 b : «the great *ordos*»); in 1353 (*YS*, 43, 3 a : the prince of 寧 Ning, \*Hümägäi, goes back to «the great *ordos*»); in 1355 (*YS*, 44, 1 a : «the great *ordo*»); in 1360 (*YS*, 45, 8 a : \*Därviš, with the title of 太尉 *t'ai-wei*, has the keep of the «great *ordos*»).

SCHMIDT (p. 389) says that, according to Abū'l-Ghāzī, Chinghiz became Great Khan at «Naimankürä» («Nimān-Kèh-rè» in DESMAISONS, 87), and suggests that this may be due to a later confusion with the *čayān naiman gār*, since *naiman kürä* means in Mongolian «the eight court-

yards». But «Naimankürä» is a misreading of نمان کهر Tämân-kähär (< Tämä'an-kähär), «the Camel Steppe», mentioned in the *Secret History* (§ 190), the *Shêng-wu ch'in-chêng lu* (42 b), the *YS* (118, 2 b) and by Rašidu'd-Dīn (*Ber*, II, 147; III, 110). It has nothing to do with the *ordo* of Chinghiz-khan.

In later Mongol history, we hear more than once of nobles or princes who sought refuge, mostly in vain, at «the *ordo* of the Lord» (*äjä-n-u ordo*) or at «the white tent (or chamber)» (*čayān gār*; cf. SCHMIDT, 151, 187, 195). The word used for «to seek refuge», «to hide», is *qoryula*, *qoru'ula*-, and, in the passive form *qoryuda'uluqsan gār*, «the house where one has been made to hide». I wonder whether the root of this word may not be the same as of *qoriq*, «taboo precinct», and whether, in theory at least, a «droit d'asile» did not belong to Chinghiz-khan's tomb as well as to his *ordos*.

In the first half of the 15th cent., Toyon-taiši was for a short time a powerful leader in Mongolia; both the *Altan tobči* and «Sanang Setsen» give a curious account of his death. According to the *Altan tobči* (GOMBOEV, 75<sup>b-11</sup>, 170), Toyon-taiši, on his accession to power, decided to go and bow to the «eight white chambers» and to ascend the imperial throne. But, being drunk, he abused «the guard of the Lord» (*äjä-n-ü käšik*; see «Qesitan»; *käšik* was misunderstood by GOMBOEV), saying [of Chinghiz-khan] in a loud voice: «You may be the Holy Majesty (*sutu boqda*, in the masculine); I am the descendant (*ürä*) of Her Majesty the Qatun (*sutai qatun*).» Throwing himself against the «pavilion» (*čomčaq*), he struck it. But as he wanted to turn his horse back, blood flew from his mouth and nose, and he fell, gripping the mane of his horse. At the same time, it was noticed that fresh blood dripped from an arrow in the quiver of the Lord. In «Sanang Setsen» (SCHMIDT, 151), Toyon-taiši rides three times round the «pavilion» (*čamaqa*) of the *ordo* of the Lord and strikes it with his sword, saying: «You may be the white chamber of the body of His Majesty (*sutu*); I am Toyon, the descendant of Her Majesty (*sutai*).» The officials remonstrated with Toyon, who retorted with arrogance. Finally, however, he rendered homage to the Lord. When he turned away, the central arrow in the golden quiver trembled, and blood spurted from Toyon's mouth and nose, and he fell. On taking off his clothes, there was a wound as from an arrow between his shoulderblades, and there was blood on the arrow in the quiver. The word *čamaqa*, which I have translated «pavilion», is not known in this form, but is clearly the same as *čamqaq* (> Manchu *čamhan*) and *čamya*, which denotes a «storeyed pavilion»; SCHMIDT's «enclosure» (Umzäunung) is not satisfactory; the Chinese translation (5, 11 a) gives the same word 亭 *t'ing*, «pavilion», which it uses a few lines before to render *ordo* (it adds from another source a third parallel account, where Toyon strikes with his sword the wall surrounding the wood at the tomb). The word *čomčaq* of the *Altan tobči* is rendered «obelisk» by GOMBOEV; in an earlier chapter, SCHMIDT (p. 42<sup>2</sup>) translates it «cupola»; it seems to apply to something high and pointed (cf. RAMSTEDT, *Kalm. Wörterbuch*, 430<sup>1</sup>). The interest of the text lies in the fact that there must have been at the *ordo* of Chinghiz-khan a building, where his paraphernalia were kept (his quiver with arrows, his standard, his drum, and perhaps his portrait).

Except in the case of one sovereign who we know was put on the throne «in front of the first Empress» (*äši qatun-u ämünä*; SCHMIDT, 181) by a woman, his mother, most of the Mongol sovereigns of the 15th cent. and of the first half of the 16th were enthroned «in front of the Lord»

(*äjän-ü ämünä*; SCHMIDT, 171) or «at the white chamber», or «at the eight white chambers» (*Altan tobči*, 189, 193; «Sanang Setsen», in SCHMIDT, 193, 199).

It is in connection with the «eight white chambers» that the name of the Ordos tribe first occurs. In the same speech in which Dayan-khan addressed the Uryanqai as the keepers of the treasures of [the tomb of] Chinghiz-khan, he said to the Ordos (SCHMIDT, 191): «You Ordos, who have watched over (*qara'ula-*) the eight white chambers of the Lord, are a nation with a great destiny (*yäkä jiya'atu ulus*)». The standard-bearer (*tuqči*; see «Tuc») of the «black flag» (*qara süldä*) of the Lord was an Ordos man (SCHMIDT, 193). The name of the «Ordos» is not met with before the first half of the 16th cent., and SCHMIDT (p. 389) has already proposed the obvious explanation that this tribal name is due to the fact that the Ordos were the descendants of the people attached to the «great ordos» of Chinghiz-khan.

The history of the Ordos has still to be written; the sketch in HOWORTH (I, 399-415) is hardly satisfactory (it begins with a serious blunder, when HOWORTH maintains against SCHMIDT that *bara'un* and *jä'ün* mean «left» and «right» in Mongolian, and not «right» and «left»; moreover, the question of the *bara'un yar* and the *žä'ün yar* [ > Dzungar ] is much more intricate than HOWORTH imagined). It may be that, in the beginning of the 16th cent., the Ordos were still in northern Mongolia. But they soon migrated to the south, first towards the Kökö-nör, and finally settled within the great bend of the Huang-ho, now known as the Ordos region. No Mongol tribe lived there before the 16th cent. Under such conditions, it is clear that there can be no question of the tomb of Chinghiz-khan being in the region of the Ordos. But when the Ordos, the former keepers of the *ordos* of Chinghiz-khan and of their relics, had settled in what we now call the region of the Ordos, they brought with them their old traditions, as well as relics of a more or less ancient date, though none of these probably went back to the time of Chinghiz. It would be interesting to have the so-called relics now existing carefully examined, but they have certainly no bearing on the question either of the place where Chinghiz-khan died, or of the site of his tomb.

In 1634, at a time when the Ordos had already migrated to within the bend of the Huang-ho, a last mention of the «white chamber of the Lord» occurs in «Sanang Setsen», but the passage, which is difficult and slightly corrupt, has been misunderstood by SCHMIDT (p. 281; cf. the Chinese translation, 8, 13 a). The text does not speak, as in SCHMIDT, of a man called «Ssereng Bodomal» and of a «golden pyramid», but says that the *jaisang* (< Ch. 宰相 *tsai-hsiang*, «minister») Tsereng (< Tib. Che-riñ, «Long life») of Altan-suburyan («Golden-stupa») of the Čaḡar (< Čaqar < Iran. *čäkär*, «lifeguardsman», > Ch. 柘轔 *ché-chieh*; cf. CHAVANNES, *Documents sur les Toukiue*, 365) lodged the Tuba (= Tuwa) Taisong Hōng-taiji (< Ch. *huang-t'ai-tzü*) Tägüldär at «the white chamber of the Lord called Bodomal» (*Bodomal kämäkü äjän-ü čayan gār*). I read «Bodomal» the name of the chamber to conform with SCHMIDT and with the Chinese translator, but there can be no doubt that the word is the same as *budomal* of the dictionaries, meaning «painted», «coloured». It belongs to the same root as Mong. *budaq*, «colour», which has the same first -u- vowel in Kalmuk (cf. RAMSTEDT, *Kalm. Wörterbuch*, 57). But this first vowel is -o- in the corresponding Turkish word *boḡuy* of Kāšyari (BROCKELMANN, 39) and in modern Turkish dialects, like Turki *boyaq*, Osm. *boya*. The Chinese translator (or the Manchu translator from whose version the Chinese translation was made) must have heard the word from a Mongol

who pronounced it *bodomal*. This name given to one of the «white chambers» is significant, since it implies that «the chamber» was «painted» in a way which distinguished it from the others.

In an obscure sentence, the *Altan tobči* (42<sup>6-8</sup>, 147-148), speaking of the funerary car of Chinghiz khan which sank into the mud at the Muna marsh, on the southern bank of the great bend, seems to say that false rumours were spread amongst the people about Chinghiz being buried there, but that in fact only «the shirt he had worn (*ämüsüksän čamča*), his tent (*örgü'ä gār*), and one of his felt boots (*öriyäsün oimusun = örü'äsün oimasun*) were there buried (*ongyola-*)». The verb *ongyola-*, lit. «to devote to the *ongyon* (or spirits)», is used in the sense of «to bury». Here again, as in Plan Carpine, we find the story of the buried tent. But it is clear that the burial of these relics close to the Ordos region has no more foundation than the burial there of Chinghiz himself. The existence of such a tradition, prior to the migration of the Ordos to the south, is inconceivable.

The information given in the press in May-June 1939 about the removal of Chinghiz-khan's remains from the Ordos region to West China refers of course to the spurious silver coffin of *Yäkä-äjän-ħorä*.

THE OTHER IMPERIAL TOMBS OF THE MONGOL DYNASTY. — We may take it as practically certain that Chinghiz-khan, Tolui, Tolui's wife *Soryaqtani bägi* and his sons *Mongka* and *Ariq-bögä* were buried within the «great forbidden precinct» of the *Burqan-qaldun*. But, while the *YS* states that all the Mongols Emperors, beginning with Chinghiz himself, were taken to the *Ch'i-lien Valley*, *Rašid* is positive that *Ögödäi* and *Güyük* had their tombs elsewhere, and, in most passages, also excludes *Qubilai* (and possibly his lineage?) from the Chinghiz-khanids whose tombs lay by the side of that of their great ancestor. *BLOCHET* (*Bl*, II, 577) says most definitely that *Qubilai* and his successors were buried in China. Although he adduces no reason beyond the very text of *Rašidu-'d-Din*, the question may be raised whether the *Ch'i-lien Valley* was not merely the resting place of *Qubilai* and his successors, and whether it has not been erroneously taken by the *YS* (and before it probably by the lost *Shih lu*, since the statement already occurs in 1366 in the *Cho-kêng lu*) to be identical with that of the earlier Great Khans. As I have said above, the conditions which prevailed after *Güyük's* death make it unlikely that his body should have been taken anywhere but to his appanage of the *Emil*. If the *YS* is mistaken about *Güyük*, could it not also have been mistaken about Chinghiz-khan and *Ögödäi* (the mention of *Mongka's* burial site is omitted in the *YS*)? In such a case, *Rašid's* statement which excludes *Qubilai* from the «great forbidden precinct» would be of great weight. I do not think that we are in a position to reach a final solution as yet, but certain texts which may help further research may now be examined.

I. — Both *ERDMANN* (*Vollständ. Uebersicht*, 128, and *Temudschin*, 444) and *BEREZIN* (*Ber*, I, 92; *Pers.* 192; III, 99; *Pers.* 150) have translated *Rašid's* statements without noticing that they were contradictory. The consensus of most passages tends to show that, in *Rašid's* opinion, *Qubilai* was not buried by the side of Chinghiz. But *Rašid* could not really speak of *Qubilai's* successors, since he wrote before the death of any of them. The only information he might have had would refer to those of *Qubilai's* children (not «successors») who had died before their

father, as for instance Činkim (see «Cinchim»), but, as far as I know, Rašid says nothing about the place where Činkim was buried. Moreover, various readings make it difficult to be positive about Rašid's statement which relates to some of the other Chinghiz-khanids. The explicit exclusion of Qubilai and Hülägü from among the children of Tolui who were buried at the «great qoriq» occurs also in another passage of Rašid which is alluded to by BARTHOLD (*Otčët o poézdke*, 20), but which I have not been able to trace. Systematic collation of the readings of all the important mss. of Rašid in the different passages would be necessary to determine, if possible, whether Rašid's various statements are reconcilable. Pending this, we must, I think, admit that the Persian historian excludes from the «great qoriq», among Tolui's children, Hülägü and his descendants on the one hand, and on the other Qubilai, though not Qubilai's descendants, about the burial-sites of whom he could know nothing. It seems hard to believe, however, that, at a time and for a reign of which Chinese history has kept precise records, the YS should err in stating that Qubilai was buried by the side of Chinghiz-khan and Tolui. If there is any foundation in Rašid's exclusion of Qubilai from the «great qoriq», the following solution might be imagined. At the time of Qubilai's death, and owing to the warfare that still raged between the Mongols of China and those of outer Mongolia and Turkestan, a provisional tomb of the late Emperor might have been erected somewhere in the mountains north of Peking, and the actual burial of Qubilai by the side of Chinghiz-khan and Tolui would have taken place only at a later date, when, after Qaidu's death in 1301, the Mongol princes of Mongolia came to terms with Qubilai's successor Tämür. Most of Rašid's text would have been written before Qubilai's remains had been carried to their final resting place at the «great qoriq». But Rašid may still have heard of the transfer, and this would account for the statement at the end of his biography of Chinghiz (*Ber*, III, 99) that Qubilai-qa'an, as well as Mongka-qa'an and Ariq-bögä, were buried by the side of Chinghiz-khan and Tolui. I admit that I can find no trace in Chinese texts of the supposition that Qubilai's burial took place in two stages. But we must not forget that there was a sort of a taboo on the publication of such proceedings, and Chinese history of the Mongol period never goes beyond the bare statement that such and such an Emperor was buried in the Ch'i-lien Valley, without any precision as to the dates and conditions of the burial. As a consequence, I dismiss the otherwise possible hypothesis of a constant and grievous error in the Chinese annals which would have made the Ch'i-lien Valley the site of the tomb of Chinghiz-khan and Tolui when it was merely that of Qubilai and his successors.

II. — In PALLADIUS's *Elucidations of Marco Polo's travels in North-China* (JNCB, x [1876], 12), we find the following statement, which has been copied in *Y*, I, 248, and in *Ch*, I, 195 : «When Khubilai marched out against Prince Nayan, and reached the modern Tainor, news was received of the occupation of the khan's burial ground by the rebels. They held out there very long, which exceedingly afflicted Khubilai; and this goes to prove that the tombs could not be situated much to the West.» PALLADIUS gives the *Yüan-shih lei-pien* as the source for this statement. No comment is made on the passage in FÉNG Ch'êng-chün's Chinese translation of CHARIGNON's book (I, 242); PALLADIUS's reference is, however, very puzzling. Not a word about the khan's burial ground being taken over by the rebels occurs either in the *pên-chi* of the *Yüan-shih*

*lei-pien* or in the short notice of Nayan (cf. 30); and I have failed to discover any such passage in the rest of the work. Moreover, there is nothing of the kind in the *pên-chi* of the *YS* itself, nor in the *pên-chi* of T'U Chi or in his account of Nayan (see «Naian»). The texts relating to the other Nayan, the descendant of Bālgütāi who has sometimes been confused with Polo's Christian Prince Nayan, are silent too on the point. One must exclude, however, the possibility that PALLADIUS's account was entirely baseless; the Russian sinologist must have found the original statement in some biographical notice, either in the *Yüan-shih lei-pien* or elsewhere. But in any case, no safe conclusion can be reached from this text as to the location of the tombs of the Great Khans. What PALLADIUS calls «the modern Talnor» is not of course the «Tal-nor» west of Kobdo, but the «Dalai-nör» of our maps situated about 120 kilometres north-north-east of Dolön-nör (see «Barscol»), GERBILLON's «Taal-nor» (in DU HALDE, IV, 167-168), called 魚兒泊 Yü-êrh-po (or 魚兒樂 Yü-êrh-po), «Fish-Lake», 捕魚兒海 Pu-yü-êrh-hai, «Fish-catching Lake», and 蒼兒腦兒 Ta-êrh-nao-êrh (\*Dar-nör, ? possibly \*Dal-nör) in the Mongol period (cf. *Br*, I, 48-49; II, 162-163). BRETSCHNEIDER explains it as Tal<sup>a</sup>-nör, «Flat-land Lake», and PRŽEVALSKIĭ and POZDNEEV have adopted Dalai-nör, «Sea Lake». The original form is doubtful, and may not be either Tal<sup>a</sup>-nör, or Dalai-nör, since the transcription of the Mongol period would regularly suggest \*Dar-nör, which is indeed supported to some extent by the transcription 達里泊 Ta-li-po, «\*Dari Lake», of Chinese geographical works, and by the modern form Dari-yangya (if the two are actually connected; cf. TIMKOVSKI, *Voyage à Péking*, I, 206; POPOV, *Mêng-ku yu-mu czi*, 281, 291; *TP*, 1931, 166). But no form approaching any of these Chinese and Mongol names occurs in the accounts of Qubilai's campaign against Nayan, and we are left entirely in the dark as to the name which PALLADIUS equated with «Tal-nor».

But, whatever the truth on this point may be, it does not affect the question of the location of the Imperial tombs. In a passage translated above (p. 342), Rašid says that when the princes who were under the orders of Nomoyan rebelled (in 1276; see «Nomogan»), most of the chiliarch guarding the Imperial tombs joined the troops of Qaidu. It is quite probable that, either at that moment, or at the time of Nayan's rebellion, which Qaidu favoured though he did not actually come to Nayan's rescue as he had promised, Qaidu took possession of the «great *qoriq*» itself. This would explain Qubilai's grief, as expressed in the text of undetermined origin alluded to by PALLADIUS.

III. — Long after Qubilai's death, the *ordos* of defunct Emperors were still maintained, though we cannot ascertain whether they remained at their original location or were transferred to the vicinity of the tombs; the Chinese custom, which would favour the second solution, need not have been binding for the Mongols. As an illustration, I may quote the four following passages of the *YS*, which occur in the course of three years :

a. (33, 9 a) : «The second year *t'ien-li* . . . , in the eleventh month, . . . on the day *ping-ch'ên* (November 24, 1329) . . . , an Imperial Edict prescribed that the Empresses and concubines and the servants who accompanied (陪從 *p'ei-ts'ung*, the usual term for those who live at the tomb of a deceased Emperor) the [defunct] Saints (*i. e.* Emperors) should be given for ever clothing and grain, fodder and millet.»



β. (33, 9 b) : «The second year *t'ien-li* . . . , in the twelfth month, . . . on the day *kêng-yin* (December 27, 1329), a purifying sacrifice was offered at the tent (幄殿 *wu-tien*, lit. «tent-hall») of T'ai-tsu (= Chinghiz-khan).»

γ. (35, 1 b) : «The second year *chih-shun* . . . , in the first month, . . . on the day *kêng-yin* (February 27, 1331) . . . According to state regulations, guards (衛士 *wei-shih*) were established at the *ordos* (行帳 *hsing-chang*) of the various reigns (*i. e.* of all the late Emperors), to serve there as at the time when [the Emperors] were on the throne. Recently, the numbers [of the guards] which were excessive have been reduced. For each of the Emperors Wu-tsung (1307-1311) and Jên-tsung (1311-1320), [guards] have been fixed at eight hundred, and for Ying-tsung (1320-1323) at seven hundred. The members of the Grand Secretariat (*chung-shu-shêng*) have reported that [as a consequence,] men who were old servants [of the late Emperors] had lost their office. An edict prescribed to restore to the office [of guards] one hundred men.» Although the text is not clear on the point, it may be that the purpose of the edict was to bring the guards at the *ordo* of Ying-tsung to the same number of eight hundred which had been adopted for Wu-tsung and Jên-tsung.

δ. (35, 1 b) : «The second year *chih-shun*, in the first month, . . . on the day *hsin-mao* (February 28, 1337), the Imperial Heir-Apparent (*huang-t'ai tzü*) 阿剌忒納荅刺 A-la-t'ê-na-ta-ra (\*Ratnadhara; Wên-tsung's son; on him cf. T'u Chi, 77, 3-4) died. On the day *jên-ch'ên* (March 1, 1331), the official of the Heir-Apparent's Household 法里 Fa-li (\*Fakhri?, in which case he would be a Mussulman) and fifty-eight men who were in the service [of the Heir-Apparent] were ordered to accompany (*lit.* to protect) the funerary hearse (靈輿 *ling-yü*) to the north, to be buried by the side of [the other Emperors and princes] (附葬 *fu-tsang*) at the mountain tumuli (山陵 *shan-ling*, *i. e.* at the «*great qoriq*»; cf. p. 338). Moreover, Fa-li and the others were ordered to keep guard at the [tomb].» The procedure adopted for \*Ratnadhara's funeral was in fact that in use for an Emperor. In the same way, his woven portrait was installed and sacrificed to in the Ancestral Hall, and his household maintained at full strength as when he was alive. All this was done because of Wên-tsung's deep grief for the death of his son. The account gives us, at the same time, an interesting glimpse on the practice followed at the death of the Emperors themselves. It is not certain, however, that Fa-li, ordered to «keep guard at the [tomb]», had to remain there for the rest of his life. He was expressly detailed to accompany the hearse and to attend to the funeral. When an Emperor has been buried, the old Mongol ritual was (*YS*, 77, 8 b) that «three officials who had escorted [the hearse] and buried [the late Emperor] settled at beyond five *li* [from the tomb]; every day they 'burnt food' (燒飯 *shao-fan*; on this ancient Altaic rite, cf. WANG Kuo-wei, *Kuan-t'ang chi-lin*, 16, 18-19; the corresponding technical term in Mongolian, *tüläšilä-*, occurs in the *Secret History*, §§ 161, 177) and offered sacrifice for three years, after which they went back». Fa-li was such an official, and there is little likelihood that an Heir-Apparent should have been granted a longer official attendance at his tomb than a regnant Emperor.

IV.—I have given above reasons, which seem to me conclusive, for locating the tomb of Chinghiz-khan at the Burqan-qaldun, towards the source of the Onon. There is, however, an early text which runs counter to my argument; it is that of Hsü T'ing which I have already

translated (p. 333), though without discussing it from that point of view. P'êng Ta-ya, *c.* 1232, had described the tomb of Tämüjin, *i. e.* of Chinghiz-khan, without saying where it lay. In his notes dated 1235-1236 to P'êng Ta-ya's account of the Sung embassy of *c.* 1232, Hsü T'ing adds: «I, [Hsü] T'ing, have seen the tomb of Tämüjin. It is on the side of the 瀘溝河 Lu-kou-ho (Lu-kou-River); mountains and rivers surround it. It is reported that Tämüjin was born there and that for that reason, on his death he was buried there; I do not know whether it be true or not».

The first point to be examined is the identification of the Lu-kou River. We do not know the exact places where the Sung embassies of *c.* 1232 and of 1235-1236 were received by the Mongols, but it must have been west of the great bend of the Kerulen, in the region of the Tūla. There is not much likelihood that any of them should have gone as far north as the sources of the Onon and the Kerulen in the Kentei Range.

The form of the name used by Hsü T'ing is not quite certain. Lu-kou is given in Lo Chên-yü's edition without any comment, but HUNG Chün (I B, 47) gives 瀘渚 Lu-chu; this is also the reading adopted by T'ü Chi (III, 33 b), but with a note to the effect that the ms. possessed by Lo Fêng-lu (1850-1903; cf. GILES, *Biogr. Dict.* No. 1385) gives 瀘局 Lu-chü (cf. also NAKA, 581). It may be that Lu-kou is due to the graphic similarity of 渚 *chu* and 溝 *kou*, coupled with the attraction of 盧溝 Lu-kou, another name of the Hun-ho, well known on account of the Lu-kou-ch'iao, «Lu-kou Bridge», the so-called «Marco Polo Bridge» west of Peking (see «Pulisanghin»). But, if we remember the various transcriptions cited above (p. 331) for a name \*Lügü or \*Lüngü of the Kerulen, it can hardly be doubted that the Kerulen is meant (cf. *TP*, 1935, 166-167). This is the view taken by T'ü Chi (III, 33 b), while CHARIGNON (*Ch*, I, 201) silently substitutes «Kerulen» for «Lu-kou River» when translating Hsü T'ing's passage.

T'ü Chi says that Rašid is in agreement with Hsü T'ing, since both speak of the Kerulen in connection with the tomb of Chinghiz, and, confusing apparently the ɣala'utu Camp of Sa'ari-kä'är with the tomb, finds it natural that P'êng Ta-ya and Hsü T'ing should have seen the tomb when following the main road from Kuei-hua-ch'êng (= Sui-yüan) to the camp of Ögödäi. As a matter of fact, I cannot find anywhere that Rašid speaks of the Kerulen in connection with the tomb, but only of the Burqan-qaldun, which is expressly stated by the *Secret History* to be at the source of the Onon. On the other hand, we do not know whether the Sung envoys travelled to Mongolia *via* Kuei-hua-ch'êng, and not direct from Peking which, since 1215, had been in the hands of the Mongols.

P'êng Ta-ya makes no positive statement that he himself saw the tomb, but his text almost implies it; Hsü T'ing, for himself, says this in so many words. We are thus faced with two possible solutions. Either both envoys of *c.* 1232 and of 1235-1236 were taken to the site of the tomb, perhaps to do homage to the *manes* of Chinghiz-khan, and Hsü T'ing may have spoken of the Kerulen because the Burqan-qaldun was near the sources of both the Onon and the Kerulen; or P'êng Ta-ya may have given second-hand information, and Hsü T'ing may have mistaken one of Chinghiz-khan's *ordos* (? that of Sa'ari-kä'är) for his tomb (the *YS*, I, 8 a, speaks of Chinghiz's return in the spring of 1216 to the «ordo [hsing-kung] of the 臚胸河

Lu-chü River [*i. e.* the Kerulen]», while the *Secret History* says [§ 250] that he returned to Sa'ari-kä'är; we have seen [p. 323] that Sa'ari-kä'är lay in fact somewhat west of the great bend of the Kerulen). Although serious objections may be raised against it, I am inclined to accept the first hypothesis. As to the burial-site of Chinghiz being also the place of his birth, Hsü T'ing himself relates it only as hearsay tradition, which we know to be wrong.

V. — PALLADIUS (*Elucidations*, 12, and cf. *Y*, 1, 248; *Ch*, 1, 195) has quoted a passage of Chin Yu-tzü's *Pei-chêng lu*, the account of Yung-lo's Mongolian campaign of 1410, which has a reference to the tombs of the Mongol Emperors. No detailed study of Yung-lo's itinerary has as yet been made, and most of the names mentioned by Chin Yu-tzü are unidentified (we know, however, that he passed through Dari-yangya; cf. *TP*, 1931, 166). But, since Yung-lo left Peking *via* Hsüan-hua-fu and the Lake of the Brahminy Ducks (see « Ciagannor ») and went on to the north-west, he must have followed what has since been the main track from Peking to Ulän-bätor, and reached the Kerulen towards the southern part of its great bend from the south to the east-north-east. The passage reads as follows (ff. 19-20) : « [In the fourth month . . .] on the 27th day (May 30, 1410), we halted at an old Buddhist shrine (古梵場 *ku fan-ch'ang*). Early on the 28th day (May 31), we left the old Buddhist shrine, and marched several tens of *li*. There was [then] to the north-east a mountain very high and large, with towering peaks, and gloriously verdant; it looked like the mountains south of the [Yang-tzü-] chiang. At the foot of the mountains, a lone peak stood high, with much white stone on it. The princes (王 *wang*) of the Mongol dynasty (元氏 *yüan shih*) lie buried at the foot [of this peak]. In the evening, we arrived at 長清塞 *Ch'ang-ch'ing-sai* ('Ever-pure Barrier'); there was [there] a spring with very pure water; [the Emperor] bestowed on it the name of 玉華泉 *Yü-hua-ch'üan* ('Jade-flower Spring'). At the first beat of the night clepsydra, the Emperor stood in front of his tent and, pointing to the Northern Dipper (= the Great Bear), said : 'This is the Northern Dipper looking south'. He retired only after a long talk. On the 30th day (June 2; the diary is silent about June 1, which was probably a day of rest), we arrived at 順安鎮 *Shun-an-chên*. The Emperor stood in front of his tent and, pointing to the mountains outside of the camp, said : 'These are mountains of the land of the slaves (*i. e.* the Mongols) which form good subjects for paintings'. Consequently, he ordered painters to paint them. In the evening, it rained. On the first day of the fifth month (June 3, 1410), at an early hour, in light rain, we left *Shun-an-chên*; after we had marched more than ten *li*, the mountains had many white clouds. The Emperor called [me], and pointing with his finger to the mountain in front [of us] said : 'This is to be called 白雲山 *Po-yün-shan* (« White cloud mountain »)'. Marching again several *li*, there was in the white clouds a greenish (*ch'ing*) vapour that reached to the ground; from afar, it looked like white clouds on a greenish mountain. The Emperor said : 'This mountain is very high and is a site worth looking at'. [I, Chin] Yu-tzü, [said that] it was certainly so. [Then] the Emperor laughed and said : 'This is vapour, and not a real mountain. If it were actually a mountain, there would be no mountain in the world that could surpass it'. Crossing a range (岡 *kang*), we saw from afar the 臚胸河 *Lu-chü-ho* (Lu-chü River, the Kerulen). When we crossed another range, the Emperor shortened his reins

and climbed to the top; the four quarters all lay below. Going again several *li*, he came near the Lu-chü-ho (the Kerulen), and remained a long time erect on his horse. He [then] bestowed on the river the name of 飲馬河 Yin-ma-ho ('River where the horses are watered'). The river flows to the north-east. Its course is swift. On both banks, there are numerous luxuriant trees; near the banks are many elms and willows. In the river are islands (洲 *chou*), with much rush and green grass, over a foot long. It is said that it cannot be used for feeding of horses, and that when horses eat it, many fall sick. Fish is plentiful in the river; immediately people presented some. Camp was pitched on [the bank of] the river. The place was called 平漠鎮 P'ing-mo-chên ('Garrison of the pacified desert').

Since the Emperor Yung-lo, coming from Hsüan-hua-fu and the Lake of the Brahminy Ducks, reached the Kerulen and found it flowing «to the north-east», the spot at which he arrived must have been about that of the confluence of the Sängkür (cf. p. 322) and the Kerulen, or somewhere east of it, and the peak at the foot of which the Mongol «princes» were said to have been buried, and which was seen to the north-east two days (three days in fact, but one must have been a day of rest) before reaching the Kerulen, must have been to the south-east of and not very far from the river. The Darhan-ūla («Ironsmith Mountain») of our maps, «Tarhan Alin» (ma. *alin* = mountain) of D'ANVILLE's *Atlas*, Tartarie chinoise, 7th sheet, where, according to TIMKOVSKI (*Voyage à Péking*, I, 173, 179), Chinghiz-khan's anvil was then said to be still preserved and where the Mongols used to assemble every summer to commemorate the conqueror, stands south of the Kerulen, but apparently somewhat too far west to meet the conditions required by the data in Chin Yu-tzü's diary.

But whatever the actual position of the place may be, I cannot agree with the deductions made by PALLADIUS. According to his abridged version of the passage, the «sovereigns» of the Yüan house used to be buried at the foot of the lone peak. The word used by Chin Yu-tzü, however, is not 帝 *ti*, «Emperor», but 王 *wang*, «prince». Now, I see no cogent reason why Chin Yu-tzü, although writing under the first Emperors of the dynasty which had overthrown the Mongols, should not give to the Mongol sovereigns their ordinary title of «Emperor». When speaking of the last of them (fol. 2 *b*), he calls him Shun-ti as everybody does. So it seems to me that by «princes», Chin Yu-tzü may mean members of the Mongol Imperial family, and not the Emperors themselves. Only the Emperors and some of their next-of-kin were buried at the «great *qoriq*»; but, if the statement put by Chin Yu-tzü into the mouth of Yung-lo has any foundation at all, it may very well be that there existed at the lone peak, during the Mongol period, a *qoriq* of those princes of the Imperial blood who were not carried to the Burqan-qaldun. I do not think that, taken at its face value, Chin Yu-tzü's diary can be said to substantiate the statements of P'êng Ta-ya and Hsü T'ing.

VI. — In *Trudy členov Ross. dukh. misstī v Pekinē*, IV [1866], 252, PALLADIUS says: «According to the tradition among the Mongols, the tombs of Chinghiz-khan and of his descendants [who lived] in China lie at the Tas Mountain, north of Dolōn-nōr, on the way to the Kerulen; the Mongols say that they assemble there every year to pay homage on the 7th of the 7th moon, on the supposed day of the death of Chinghiz-khan. In all likelihood, it is this place

which is mentioned, for instance, in the accounts of the campaign of Yung-lo, . . . and in one of GERBILLON's diaries». The note to the same effect in PALLADIUS's *Elucidations* of 1876 (p. 358), though reproduced in *Y*, I, 248, is different enough to be quoted here : « Our well-know Mongolist, N. GOLOVKIN, has told us that according to a story actually current among the Mongols, the tombs of the former Mongol khans are situated near Tas-ola hill, equally (i. e. *like the mountain in Chin Yu-tzū's account*) in the vicinity of the Kerulen. He states also that even now the Mongols are accustomed to assemble on that hill on the 7th day of the 7th moon (according to an ancient custom), in order to adore Chinghiz-kan's tomb».

PALLADIUS's statements have at least importance in that they show that, in the sixties of the last century, the « Ordos » theory of the tomb was not yet current or commonly accepted in outer Mongolia, since neither GOLOVKIN nor PALLADIUS seems to have even heard of it. Moreover, from the similarity between the two statements in PALLADIUS, we can safely deduce that GOLOVKIN was his sole informant. It seems to me very probable that PALLADIUS was right in connecting GOLOVKIN's information with that given in the *Pei-chêng lu*. Yet there are difficulties and inaccuracies. I cannot identify the « Tas-ola » (? « Grey-eagle Mountain »), or find even any other mention of the name; while the location between Dolôn-nôr (= Polo's « Ciandu ») and the Kerulen is most vague. No Mongol text speaks of Chinghiz-khan's death as having taken place on the 7th of the 7th moon, but either on the 12th (cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 105; an information which the Mongols seem to owe to Chinese official history), or on the 15th according to the *Altan tobči* (cf. p. 309, 317); the date of the 7th day of the 7th moon may be due, however, to the attraction of the Chinese popular calendar, which celebrates on the eve of that day the meeting of the Weaving Damsel (*α Lyrā*) with the Cow-herd (*βγ Aquila*). In spite of PALLADIUS, I can find nothing on the subject in the various diaries of GERBILLON published by DU HALDE. It may be that PALLADIUS's reference is based here on a faint and erroneous remembrance of a passage in TIMKOVSKI, who quotes (*Voyage à Péking*, I, 170) a sentence of Gerbillon's diary dated June 11, 1696 (DU HALDE, IV, 327; not « June 16 » as in TIMKOVSKI), where, however, nothing is said of the Mongol Imperial or princely tombs.

On the other hand, the gathering of the Mongols in the seventh month fits in very well with their meeting « in the summer » at the Darḡan-ūla in TIMKOVSKI's text. Unless we suppose that there were two successive annual gatherings at neighbouring places, we are here confronted with the same difficulty as when translating Chin Yu-tzū's text : the Darḡan-ūla seems to be too much to the west. Moreover, the names Darḡan-ūla and Tas-ūla are not reconcilable.

While admitting that in both the *Pei-chêng lu* and in PALLADIUS's second-hand information derived from GOLOVKIN we have to do with one and the same tradition, the fact that GOLOVKIN spoke of the Mongol Emperors need not be accepted as the expression of the truth. Quite evidently, the details of the tradition — for instance that of Chinghiz-khan's anvil at the Darḡan-ūla — are later developments. If there were princely tombs of the Mongols at the place referred to by Chin Yu-tzū, it is only too natural that in later Mongol legend they should have become the tombs of the Emperors themselves, the true location of which was no longer known.

VII. — 張鵬翮 CHANG P'êng-ho (1649-1725; cf. GILES, *Biogr. Dict.* No. 95) was one of the

members of the embassy which was sent to the Russians in 1688, and to which the Jesuits GERBILLON and PEREIRA were also attached. He has left an account of the embassy, entitled 奉使俄羅斯行程錄 *Fêng shih O-lo-ssü hsing-ch'êng lu*, which has been reprinted in several collections; I quote it from the one included in the fourth series of the *I-hai chu-ch'ên*. As GERBILLON's diary of the same embassy appeared in DU HALDE (IV, 87-162), we can check one with the other, and their complete agreement as to the dates and events does great credit to both.

In CHANG P'êng-ho's work, we read (fol. 14 *b*) the following passage regarding the departure of the embassy from Kuei-hua-ch'êng or K'u-k'u-ho-t'un (Kökö-hoton; GERBILLON's « Quei hoa tchin » or « Hou hou hotun » in DU HALDE, IV, 102; the present Sui-yüan outside the north-eastern angle of the great bend of the Huang-ho) : « [In the fifth month. . .] on the 21st day (June 18, 1688) we went nine *li* and entered the 祁連山 Ch'i-lien-shan (Ch'i-lien Mountains). [Note by the author : These mountains too are called Ch'i-lien, (but) they are not the Ch'i-lien Mountains of the departments (*chou*) of 甘 Kan, 伊 I and 西 Hsi (mentioned) in the 元和志 *Yüan-ho chih* (that is to say, they are not the Ch'i-lien Mountains of Hsiung-nu memory, which extended from Kan-su to the north-eastern part of Chinese Turkestan).] There are remains of an earthen wall; I suspect that it is what the inscription (an inscription dated 1320 from which CHANG P'êng-ho had given extracts before) calls 甸城 Tien-ch'êng. Looking afar, stone peaks pile up verdant; once entered, the whole extends in flat hills. The tradition is that the Mongol Emperors and Empresses were all buried on that mountain, though no funeral mounds were erected. » There is no parallel passage in GERBILLON.

The painter and poet 徐蘭 Hsü Lan (early 18th cent.), who travelled extensively in eastern Mongolia, wrote, among others, a poem entitled 謁元世祁陵詩 *Yeh Yüan-shih tsu-ling shih* (« Poem on a visit to the ancestral tombs of the house of Yüan » [not « to the tomb of Shih-tsu of the Yüan », *i. e.* Qubilai, as might be thought at first sight]). I have no access to the literary collection of Hsü Lan, but this poem, with its preliminary notice, is partially copied by CHANG Mu (*Mêng-ku yu-mu chi*, 6, 10 *a*), and more completely by the compiler of the *Kuo-ch'ao chi-hsien lei-chêng ch'u-pien* (429, 10 *b*). The text reads as follows : *Preliminary notice* : « The ancestral tombs (*ling*) of the house of Yüan have no mounds or trees. When hunters happen to tread upon their site, strange phenomena (異 *i*) of wind and rain take place. » *Poem* : « I have heard that when the Ming or the Chu family (= the Ming dynasty) established the rites of sacrifice, they ordered members of the [Board of] Rites to proceed to the region of Yü (the Great). From Fu-hsi down to Li-tsung of the Sung, thirty-six [Imperial] mounds were sacrificed to in succession. Only at the 祁連 Ch'i-lien since no mound had been raised, there was, in spite of the [Imperial] wishes, nowhere to offer incense and silk. The steps were swept and the tent laid with mats in Shun-t'ien-fu (= in the department of Peking), and [so] in spring and in autumn the green *fu-jung* (青芙蓉 *ch'ing-fu-jung*; *fu-jung* usually designates the hibiscus, but the name seems to refer here to another plant) was offered to from afar. The *fu-jung* is quite green, harmonizing with the cloudy resting place (?), in the middle of which there are three compartments of old tiled rooms. Those who advance there see from afar the green glaze, and know that it is the 起輦谷 Ch'i-lien-ku (Ch'i-lien Valley) of the house of Yüan. At the entrance of the valley (*ku*), a Tibetan (*fan*) monk understood Chinese characters; the 'settled guest', in his hermitage, spoke of things that were

past. Of himself he said that through the *kalpa* (= the ages of the world) he remembered his former lives, and that he had personally seen the dark abode built up on the empty green slope. When the witch came, leading the 'horse of the soul' (cf. p. 332), the holy monk, crossing the Moving Sands (Liu-sha), had already arrived.» [Note of the author : The name of the monk was 朝爾吉 Ch'ao-êrh-chi.] The rest of the poem is devoted to the earthquakes and tempests which raged whenever some one trespassed on the burial precincts. I need only remark here that, after a line on the apparition of corporal relics (*shê-li* > *śarīra*) of the late Emperors, Hsü Lan adds in a note that « many *śarīra* were obtained at the cremation of the Yüan ancestors (元祖 *Yüan tsu*) »; but his informant the Tibetan monk, who remembered the things of so many *kalpa*, had evidently forgotten that cremation was not resorted to by the Emperors of the Yüan dynasty !

The statements of CHANG P'êng-ho and of Hsü Lan have already been contradicted by CHANG Mu (6, 10 a, where CHANG P'êng-ho's diary is quoted under the erroneous title of 漠北日記 *Mo-pei jih-chi*, « Diary [kept] north of the Desert »). HUNG Chün (1 B, 46) added that an inspection of the map sufficed to establish that the region of Kuei-hua-ch'êng could not be the place where Chinghiz-khan was buried, and that we had perhaps to do here with the tombs of some of the Chinghiz-khanids who, according to Rašidu-'d-Din, had been buried in other places; T'u Chi (3, 34 a) concurs with HUNG Chün.

I see things in a somewhat different light. I have not succeeded in tracing the name of a 祁連 Ch'i-lien Mountain north of Kuei-hua-ch'êng apart from the works of CHANG P'êng-ho and of Hsü Lan. The *Chung-kuo ti-ming ta tz'u-tien*<sup>1</sup> (p. 528) mentions only one Ch'i-lien Mountain, the famous westerly one of Han and T'ang times. The *Ta-Ch'ing i-t'ung chih*, 124, 1 b, states that 20 li north of Kuei-hua-ch'êng is the 翁公谷 Wêng-kung-ku (« Valley of Master Wêng ») and 35 li north of the same city the Wêng-kung-shan (« Mountain of Master Wêng »), adding that it is part of the 陰山 Yin-shan which begins north of the great bend of the Huang-ho, and that it is known under many local names. I take it for almost evident that 祁連山 Ch'i-lien-shan is one of these local names, and that it has not been invented independently by both CHANG P'êng-ho and Hsü Lan. But if we discard the boasts of the Tibetan monk, this local name itself provides us with a most plausible solution. Though written with different characters, it is the phonetic similitude of the two Ch'i-lien which must have given rise to the local tradition according to which the Ch'i-lien-shan north of Kuei-hua-ch'êng was believed to be the Ch'i-lien Valley where the Mongol Emperors had been buried. There is no reason to suppose, as HUNG Chün did, that there may have been somewhat north of Kuei-hua-ch'êng the burial-ground of any Chinghiz-khanid at all.

VIII. — I have no access at present to the 後出塞錄 *Hou ch'u-sai lou*, « Memoirs of a second journey beyond the Barrier », by 龔之鎰 KUNG Chih-yo (the preface is dated *ping-ch'ên*, probably 1736), in which the Ch'i-lien Valley of the Mongols is said to be in the district of Fang-shan (about 40 kilometres south-west of Peking). CHANG Mu, who opposes it (6, 10 a), says that it is the result of a confusion with the sacrifices offered to the *manes* of the Mongol Emperors in the department of Shun-t'ien-fu (= Peking, to which Fang-shan belongs). I have no doubt that

a location of the Ch'i-lien Valley in the district of Fang-shan is out of the question, but CHANG Mu's explanation is of no great value. It is true that the sacrifices to the Mongol Emperors were offered in the department of Shun-t'ien-fu, but in its northern section, whereas Fang-shan is to the south-west. Without the original text of KUNG Chih-yo, I can say nothing more on the subject.

IX. — Allusions have been made above to the sacrifices offered by the Ming and Ch'ing dynasties to the Emperors of the Yüan dynasty. While the sovereigns of all other dynasties were sacrificed to at their funerary mounds, there were no *tumuli* erected over the tombs of the Mongol Emperors, and their exact site was unknown. As a substitute, sacrifices were offered in the department of Peking, north of the 清河 Ch'ing-ho, which flows to the south-east north of the capital. The regulations therefore in the 禮部則例 *Li-pu tsé-li* (« Regulations of the Board of Rites ») of the Manchu dynasty were taken over from those in force under the Ming (cf. PALLADIUS, in *Trudy élenov Ross. dukh. Missiit v Pekiné*, IV, 253; CHANG Mu, 6, 10 a). As these sacrifices have no bearing on the actual site of the tombs, I abstain from further investigation of the point.

X. — Some years ago, I jotted down a note to the effect that in the 宣化府志 *Hsüan-hua fu chih* of 1743 there was a reference (7, 33) to a tradition according to which the Mongol Empresses and princes were buried on the 尖帽山 Chien-mao-shan (« Pointed-hat Mountain ») or 氈帽山 Chan-mao-shan (« Felt-hat Mountain »), north of 獨石 Tu-shih. The Pass (口 *k'ou*) of Tu-shih, north-east of Hsüan-hua-fu, is well known, and a place in the mountains north of it ought to be just beyond the outer Great Wall. There is in itself nothing impossible in the fact that there should have been there a funerary *qoriq* for Empresses and princes who were not carried to the Burqan-qaldun. Unfortunately, I have for the present no access to the *Hsüan-hua fu chih*, and can find no other reference to this tradition, which may, however, be late in date and groundless.

To sum up these somewhat desultory remarks, the only texts which are really puzzling as to the location of the Mongol Imperial tombs are those of P'êng Ta-ya and Hsü T'ing, and to some extent that of Chin Yu-tzü. But I do not think that they are of such a nature as to overrule the evidence found in Rašidu-'d-Din and in the *YS*. I may add two further considerations. The Liao Emperors, even after they had settled in North China, had their bodies carried to eastern Mongolia for burial; it is therefore only natural that the Yüan should have followed the same practice, since, though they lived under a growing Chinese influence, they could wish to enjoy their final rest in the land of their origin. Moreover, the absence of funerary mounds does not explain everything. If the Imperial tombs of the Mongol Emperors had been in North China or just outside it, some memory of them should have survived; that they should have sunk into complete oblivion was because they lay in northern Mongolia, from which China was cut off during the three centuries of the Ming dynasty. For all these reasons, I hold that Chinghiz-khan and the line of Tolui, including Qubilai, were buried on the Burqan-qaldun, *alias* the Ch'i-lien Valley, the « Valley where the hearse was raised ».



## 159. CINGIU (c. 143)

*cinçu* Z  
*cingiu* F  
*cingui* R  
*cyngiu* L

*siguy* FA  
*tinguy* P  
*trigui* V

*tyguy* FB  
*zingoi* VB  
*zingui* VA, VB

Polo, when describing the route from Pao-ying and Kao-yu to Yang-chou, goes out of his way to speak of a place « Tigiü », in which PAUTHIER, YULE, CHARIGNON, BENEDETTO, etc., have agreed to see T'ai-chou (« Tigiü » is probably altered from \*Taigiü). His purpose is to speak of the sea which is, as he says, three days to the east of « Tigiü », and from which much salt is extracted; there he mentions a city « Cingiu », from which he comes back expressly to « Tigiü » and goes on to Yang-chou. Evidently, « Cingiu » is east of « Tigiü », that is to say of T'ai-chou; CHARIGNON's argument (*Ch*, III, 44-45), following KINGSMILL in spite of YULE (*Y*, II, 154), and according to which « Cingiu » would be I-chêng on the Yang-tzü west of Yang-chou, is *a priori* false. It is a pity it should have been accepted by BENEDETTO (*B*<sup>1</sup>, 441). Nor do I think that MOULE has hit the mark in *TP*, 1915, 413.

T'ai-chou, being out of the way, is not mentioned of course in the diary of 1276. But we can establish where the road to T'ai-chou branched off the main road. In *Yung-lo ta-tien*, 19426, 5 *b*, we have the list of postal relays south of Huai-an. *i. e.* Pao-ying, Kao-yu, and then 邵伯 Shao-po. The text adds : « Arrived here, there are two ways; one is a water-route which, going straight to the east, leads through T'ai-chou to Hai-chou; the other goes straight to the south, and passing through Yang-chou, arrives at Kua-pu [see « Caigiü »] and the Yang-tzü-chiang. . . ». On that eastern water-route the text gives, without indication of distances, the following relays : T'ai-chou, Ju-kao, 通州 T'ung-chou, 海 Hai[-chou].

The place called Shao-po, south of Kao-yu, was a *chên*, and is duly mentioned in the itinerary of 1276. Even to-day, it is from Shao-po that the canal starts and later joins the « salt-canal » north of Ju-kao (cf. *Ch*, III, 44); Polo is quite justified in speaking of the junks at T'ai-chou. Moreover, the text of the *Yung-lo ta-tien* says expressly that from Shao-po to Hai-chou, the postal relays were river relays, that is to say were covered by boat.

YULE, with his common sense, had already seen that the city where salt was manufactured was to the east of T'ai-chou; he thought very naturally of (Southern) T'ung-chou, and adopted the reading « Tingiu » instead of « Cingiu ». He may be right, but I think that another solution is preferable.

Polo speaks of three days' journey from T'ai-chou to « Cingiu », and the *Yung-lo ta-tien* names also three stages, the last of which is Hai-chou. Ch. *h* is always *c* (= *k*) in Polo, and we should expect \*Caigiü; now, « Cingiu » is to \*Caigiü in exactly the same state of alteration as the numerous « Tingiu » readings of « Tigiü » are to \*Taigiü (= T'ai-chou). So I think that the place meant by Polo is not T'ung-chou, but another place one days' journey farther on, the

place called Hai-chou in the *Yung-lo ta-tien*. Of course, this Hai-chou has nothing to do with the Hai-chou (now Tung-hai-hsien) in Northern Chiang-su, and can only be Hai-mên, on the coast east of T'ung-chou, and a great centre of salt production at least since Sung times. The difficulty is that the name of Hai-chou has not been registered for Hai-mên in the *YS*, nor in the *Ta-Ch'ing i-t'ung chih*, nor is it given for this place in the *Ti-ming ta-tz'ü tien*. With changes of locality corresponding to those of the sea-line, the place has only since the 11th cent. been officially registered as the *hsien* (and the *t'ing* under the Ch'ing) of Hai-mên. But we know of a number of cases when the data of official histories have to be supplemented by information from other sources. And the express mention of Hai-chou in the *Yung-lo ta-tien*, coupled with Polo's «Cingiu» < \*Caigiu, obliges us to conclude that Hai-chou was one of the names given to Hai-mên during the Yüan dynasty, and probably the one which was popularly current.

From Polo's acquaintance with the local conditions at Hai-chou = Hai-mên, it seems evident that he had been there; he personally knew also the gabelle works at Ch'ang-lu (see «Ciangu»), and was acquainted with those of I-chêng (see «Singiu»). In the introduction to my notes, I state what conclusions we may be entitled to draw from these facts.

#### 160. CINGSAN and see BAIAN CINGSAN

Used by Polo in «Baian cingsan», *q. v.* This is the Chinese 丞相 *ch'êng-hsiang*, «Minister». F writes «cinq san» (not «cincsan» as in *B*, 134, followed by *RR*, 412, and *B<sup>1</sup>*, 438), but Polo's original form can only be «cingsan», and we find «Bajam Cingsan» in Pipino's chronicle published by MURATORI, *Script.* IX, b. iii, ch. 47; «cinq san» is perhaps a copyist's error due to the French word «cinq».

*Ch'êng-hsiang* has passed into Mongolian under the form *čingsang* (cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 121, 123); it is written *činksan* in Armenian (with the Armenian hardening of -g into -k; cf. PATKANOV, *Istoriya Mongolov*, I, 52, 89-90). Rašidu'-d-Dīn writes regularly جينكسانك *čingsang* (cf. QUATREMÈRE, *Hist. des Mongols*, 76-78, where we find also the spelling جنكسانك *čingsang* from Waššāf; *Bl*, II, 470, 472). But the «Chyansam» of *Y*, II, 150, must be excluded; this is a personal name, Hsiang-shan (cf. *Mo*, 264).

The Mongol emperors of Peking sometimes awarded the title of *ch'êng-hsiang* to high officials of the *ilkhān* in Persia, as in the case of Buya (see «Boga») in 1286 (cf. *Ha<sup>1</sup>*, I, 422). BLOCHET (*Bl*, I, 235-236) says that the ambassador from Peking who arrived in Persia in 1326 conferred on the emir Čuban a high Chinese title which he believes to be *ch'êng-hsiang*, although he adds that the *YS*, dealing only with China, is silent on this event. But BLOCHET is mistaken. The mission which arrived in Persia in 1326 was dispatched from China in 1324; it is duly recorded in *YS*, 29, 7 *b*, with the mention of the Chinese titles which were conferred on Čuban; but the title of *ch'êng-hsiang* is not one of them.

## 161. CIORCIA

<i>ciorcia</i> LT	<i>facirocia</i> VA	<i>sargia</i> VB
<i>ciorcia</i> F, FA, FB, L, TA <sup>1</sup> , VL(?)	<i>fuciorcia</i> P	<i>siorcia</i> FB
<i>ciorza</i> R	<i>furcirocia</i> VL	<i>trocirocia</i> L
(c. 160)	<i>giorgia</i> LT, TA <sup>3</sup>	<i>zorzanía, zorzie</i> V
<i>çořa</i> Z	<i>zorca</i> R	

This name is written *Zorça* on Fra Mauro's Map (not «*Zorza*» as in ZURLA, 36, and HALLBERG, 572 [who apparently did not suspect that it was identical with his «*Ciorcia*» of p. 150]). It has long been recognized that it represented the name of the tribe of south-eastern Manchuria which founded the Chin or «Golden» dynasty (see «*Roi Dor*») in northern China and which we generally call «*Jučen*»; the «*Jučen*», or more correctly \**Jürčen* or *Nü-chên*, were Tungus, and near kin of the later Manchus.

In principle, Polo's «*Ciorcia*» represents \**Čörčä*; some of the readings may point to an original \**Giorcia* = \**Jörčä*, though this seems to be less probable.

The form used by Polo is, as usual, the one then current in Persian-speaking circles. Rašidu-'d-Dīn mentions more than once the *جورچه* (cf. *Bl*, II, 446, 485, 498, and several times in Rašid's *History of China*; also QUATREMÈRE, *Hist. des Mongols*, XC, XCII). BLOCHET, without comment, always writes it *جورچه* Čürčäh (= Čürčä), but this is arbitrary; the mss. do not distinguish between *č* and *j*. As a matter of fact, Rašid's spelling gives no clue to enable us to determine whether we should transcribe it *Jürčä* or Čürčä. The form *جورجت* Jürjät (or Čürčät) of the *Nuzhab al Qulüb* (LE STRANGE, *Pers. text*, 257<sup>12</sup>; transl., 250), although stated by the author himself to be based on Rašid, is a misreading in which the vowel of the first syllable has been omitted and the final *č* developed into *č* = *č* *t* (this error, aggravated by a misplacing of dots, occurs sporadically in Rašid's mss., hence the *جورچب* «*Jürčäb*» or «*Čürčäb*» in QUATREMÈRE, XCII); it has nothing to do with the plural in *-t* which I shall discuss farther on. Whatever the explanation of the form \**Jürčä* or \**Čürčä* with a final *-ä* may be, the form is confirmed by Polo's «*Ciorcia*». Moreover, even in the Ming period, the Persian form, though slightly different, was without a *-t*; it is given as *جورچی* and phonetically transcribed *Jörji* (or *Jörji*) in the Sino-Persian Vocabulary of the Board of Translators (13 *b*; this ms. does not distinguish between *č* and *j* in Arabic writing). For the vocalic ending in Persian the probabilities are that it is due to a form ending with a quiescent *-n* as in the Chinese transcriptions. If the name came to the Persians direct from the \**Jürčen* or through a Mongol channel, Rašid's form must be transcribed \**Jürčä*, and Polo's «*Ciorcia*» would stand for \**Giorcia* = \**Jörčä*. On the other hand, if the Persians received the I name from the Uighurs, \**Čürčä* in Rašid and «*Ciorcia*» (= \**Čörčä*) in Polo might be correct. BARTHOLD (12 *Vorlesungen*, 121) says that the Mussulman form of the name was *Jürji*; but find *Jürji* (or *Jörji*) only in the Sino-Persian Vocabulary of the Ming period, the authority of which is of course very small for earlier times in comparison with Rašidu-'d-Dīn.

In Uighur, the name occurs in the legend of Oyuz-khan as Čürčät. In *TP*, 1930, 336,

I have interpreted this as representing Jürčät, because the initial of the original name must have been *J*-, not *č*- (the ms. does not distinguish between *č* and *J*, nor does it mark the palatalization of *-u* after palatal consonants). But there was no *J* in true Uighur words (although the Uighurs could pronounce it), and BANG and RACHMATI (*Die Legende von Oyuz Qayan*, in *SPAW*, 1932, 698<sup>261</sup>) may have been right when they retained Čürčät; an initial *č*- in Uighur finds some support in a Tibetan transcription which will be mentioned farther on. At a later date, the Sino-Uighur Vocabulary of the Board of Translators, 46 *b*, seems to give Čürčük (cf. also TERRIEN DE LACOUPERIE in *JRAS*, 1889, 438; F. W. K. MÜLLER, *Zwei Pfahlschriften*, 33; not « Čürčür » as was proposed as a second « possible » reading by *Bl*, II, 446), so that Čürčük has been entered in RADLOV's Dictionary (III, 2197, where it is so transcribed, but miswritten « Čurcuq » in Uighur writing). But there is certainly a mistake in the text, due primarily to the misplacement of a hook, and the Chinese phonetic transcription 主兒扯 *Chu-êrh-ch'ê* requires the name to be Jürčät.

YULE has said (*Y*, I, 231) that the Mongol form of the name was « Churché », *i. e.* Čürčä, although CORDIER, quoting PALLADIUS, elsewhere gives « Churchin », *i. e.* Čürčin (*Y*, I, 344). But this is not correct. The name occurs several times in the *Secret History* (§§ 247, 248, 253, 274), always as Jürčät, and the derived adjectival form, used as a proper name, is always Jürčädäi (< \*Jürčä[n] + dai; §§ 130, 171, 176, etc.; but the biography in *YS*, 120, 3-4, gives 朮赤台 *Shu-ch'ih-t'ai*, \*Ju[r]čitai). « Sanang Setsen » writes Jürčit (cf. SCHMIDT's Index, 496), which is also given in the *Altan tobči* (COMBOEV, 22<sup>1</sup>, although the translation, p. 133, gives « Jürčüt »; I shall come back to this « Jürčüt » farther on).

A Tibetan text, translated in the middle ages from Chinese into Uighur, and from Uighur into Tibetan, speaks of the bČur-či dynasty (cf. P. CORDIER, *Catal. du fonds tibétain*, Index du *Tanjur*, p. 247), which would tend to show that, in the Mongol period, the name was pronounced in Uighur with an initial *č*-, not *J*-. A more recent author 'Jigs-med nam-mkha speaks of the « Man-ju Jür-čhid » and of the « Jür-čhid Dynasty gSer » (= Golden; cf. HUTN, *Gesch. des Buddhismus*, II, 20, 29); but, although writing in Tibetan, the author was a Mongol, and he merely writes in Tibetan letters the same Mongol form which we have met with in « Sanang Setsen » and in the *Altan tobči*.

The various transcriptions in Persian, Uighur, and Mongolian, with an alternation of *-ä-* and *-i-* in the second syllable, suggest an original form \*Jürčën, with a quiescent *-n*-, on which a Mongol (not Jučën) regular plural in *-t* was formed, to wit \*Jürčed > \*Jürčid. The palatalization of the first vowel (*-ü-*) may have been less marked in the Tungus original form than in Uighur and Mongolian. The Chinese transcriptions, however, raise new difficulties.

It is from the Chinese transcriptions that the form « Jučën », now in general use, has been adopted since GRUBE published his *Die Sprache und Schrift der Jučën* in 1896. This adoption is unfortunate. The initial *j* of « Jučën » has been sometimes wrongly supposed to have the German value of *j* = *y*, and consequently, perhaps also under the influence of the « Yuché » which was probably a slip or a misprint for « Juché » in *Y*, I, 231, we find « Yučën » twice in BANG and RACHMATI, *loc. cit.* 709, and also « Iucen » in BENEDETTO (*B*<sup>1</sup>, 441). But the main objection is that « Jučën », in spite of its scientific appearance, represents neither the Chinese transcriptions

themselves, nor the original form which certainly had an *-r-* at the end of the first syllable. As a matter of fact, the form «Djurtchen», *i. e.* Jürčēn, adopted by TERRIEN DE LACOUPERIE in his paper *The Djurtchen of Mandshuria*, was much more satisfactory, BARTHOLD'S «Čur-Čen», *i. e.* Jur-Jen (12 *Vorlesungen*, 121), being a pseudo-reconstruction from the Chinese, is misleading.

The usual Chinese transcriptions of the name are 女真 and 女直, the normal pronunciations of which are Nü-chên and Nü-chih, respectively (the 如真 Ju-chên of P. CORDIER, *Catal.* p. 247, does not exist). Since the days of KLAPROTH and ABEL-RÉMUSAT (cf. KLAPROTH, *Asia Polyglotta*, 292; ABEL-RÉMUSAT, *Recherches sur les langues tartares*, I, Additions et Corrections, after p. 298), it has been customary to assert that in these Chinese transcriptions, 女 was not to be read *nü* as is usual, but *ju*, «in accordance with the etymology of the name». Following in their wake, BRETSCHNEIDER (*Br*, I, 224) said that «the original spelling of the name by the Chinese was *Ju chen*»; GRUBE spoke of the «Jou-tchen» in *TP*, 1894, 334-340; and CHAVANNES of the «Jou-tche» in *JA*, 1895, II, 144, and of the «Joutchen» in *JA*, 1897, I, 377. But GRUBE'S «Jučēn» is nothing more than a pseudo-scientific representation of Ju-chên itself (the *j-* is here used with the value of *ž-*, as in our common transcriptions of the Chinese, but this is contrary to its use in scientific phonetics; GRUBE did not employ the form «Jučēn» attributed to him by MÜLLER, *Zwei Pfahlschriften*, 33). Though he goes astray at the end of the paragraph, GIBERT (*Dictionnaire hist. et géogr. de la Mandchourie*, 140) has already remarked that, although 女 is sometimes pronounced *ju* in Chinese (when it has the value of 汝 *ju*, «you»), there is no indication that it was ever so read in the name of the Nü-chên or Nü-chih. As a matter of fact, the modern Chinese and Japanese always speak of the Nü-chih, not of the \*Ju-chih or \*Ju-chên. That such was already the case in the middle of the seventeenth century is established by the Manchu version which was then made of the *History of the Chin dynasty*, where the name is written «Niu-či» (DE HARLEZ, *Histoire de l'Empire de Kin*, 1). We can even go farther back. Rašidu-'d-Dīn expressly says that the Chinese knew the Jürčä (or Čürčä) under the name of نرچی Nüči (cf. *Br*, I, 224; *Bl*, II, 446). It is thus clear that the only pronunciation from the thirteenth century down to our day has been Nü-chih, not \*Ju-chih or \*Ju-chên. The form «Jučēn», however, is now so well established in linguistic works that, while I speak of the people as Nü-chên, I retain the usual «Jučēn» as the designation of their language.

Although Nü-chên is the only form used in the Chinese version of the *Secret History* (§§ 247, 248, 255, 274) at the end of the fourteenth century, Nü-chih was prevalent during the Ming and Ch'ing dynasties (but the Koreans have retained Nü-chên, which they pronounce Nye-tyin; cf. COURANT, *Bibliogr. coréenne*, Nos. 1863, 2103, 2316). The reason for the double form is well known. Nü-chên is the older one. But the Liao Emperor Hsing-tsung had 宗真 Tsung-chên for his personal name; so, on his accession to the throne in 1031. the character *chên* was tabooed, and the name of the Nü-chên changed to Nü-chih (cf. *Wên-hsien t'ung-k'ao*, 327, 3 a; *JRAS*, 1889, 439). It was the form Nü-chih which came to the knowledge of Rašidu-'d-Dīn, although the taboo of the Liao had of course not been binding for the Sung, who went on speaking of the Nü-chên until the end. Because of the old pronunciation \**d''iak* of 直 *chih*, TERRIEN DE LACOUPERIE (*JRAS*, 1889, 439, 444) read Nü-chih as «Niutchik» (= Niučik) and connected this *-k* ending with that of the pseudo-\*Čürčük of the Ming Sino-Uighur Vocabulary;

but the old occlusive finals had already been dropped in northern Chinese when the transcription Nü-chih was adopted; it was already sounded Nü-chih, as in the Nü-chi of Rašidu-'d-Dīn and in the modern pronunciation. GIBERT (*Dictionnaire*, 375) mentions two secondary transcriptions, 女貞 Nü-chêng and 女質 Nü-chih. The latter form, Nü-chih, is given in 洪皓 Hung Hao's 松漠紀聞 *Sung-mo chi-wên* (I, 1 b; it is the account presented by that distinguished scholar in 1143, when he came back after fourteen years' captivity among the Nü-chên) as a «popular corruption» of 女直 Nü-chih; thence it passed into the *Wên-hsien t'ung-k'ao* (327, 3 a); it may have arisen from the fact that 質 *chih* means «hostage»; I do not think that it has so far been met with in any text. As to Nü-chêng, it seems to be a faulty reading of Nü-chên (and not, as might be thought, a taboo due to the personal name 趙禎 Chao Chên of the Sung Emperor Jên-tsung, 1022-1063; 趙禎 is a misprint in GILES's *Biogr. Dict.* No. 144), probably due to graphic similarity and to the attraction of the Chinese botanical term *nü-chêng*, «virgin[-tree]», *Ligustrum lucidum* (the wrong reading Nü-chêng, for Nü-chên, occurs in GILES's *Biogr. Dict.* No. 2445; and in the Sao-yeh-shan-fang edition of the *Ta-Chin kuo chih*, 39, 1 a, though it is always written Nü-chên in the rest of that work).

According to GIBERT (*Dictionnaire*, 140, 375), the Ch'i-tan or Liao had first called the Nü-chên 慮真 Lü-chên; GIBERT adds that the change from *l* to *n* «is a common phenomenon». Before him TERRIEN DE LACOUPERIE (*JRAS*, 1889, 440-441) had dwelt at length on this form Lü-chên, which he said dated as far back as the T'ang dynasty, as was proved by ch. 199 of the *Chiu T'ang shu*, where «it is distinctly stated» that Lü-chên was the name by which the Ch'i-tan called the Nü-chên. It is true that, if the name Lü-chên occurred in the *Chiu T'ang shu*, it ought to be in ch. 199; but I have twice read through the two sections of that long chapter without discovering any mention of Lü-chên. As a matter of fact I did not expect to find it there, since I believe that the name Nü-chên, either in that form or in the form of Lü-chên, was not known in China before the tenth century. The only authorities I can find for the form Lü-chên are the 大金國志 *Ta-Chin kuo chih* and Ma Tuan-lin's *Wên-hsien t'ung-k'ao* (327, 3 a). The *Ta-Chin kuo chih* in 40 chapters is said to have been presented to the Sung Emperor in 1234; Ma Tuan-lin's work was compiled at the close of the thirteenth century and first published between 1317 and 1322. But we are here confronted with a very intricate problem, that of the authorship and date of the *Ta-Chin kuo chih*, «Description of the Kingdom of the Great Chin», and of its almost sister-work, the 契丹國志 *Ch'i-tan kuo chih*, «Description of the Kingdom of the Ch'i-tan», in 27 chapters.

VASIL'EV (in *Trudy VOIRAO*, iv [1859], 169, 196) says that the *Ch'i-tan kuo chih* was written by 葉隆禮 Yeh Lung-li in 1179 (a miscalculation for 1180), and the *Ta-Chin kuo chih* in 1234 by 宇文懋昭 Yü-wên Mou-chao, both authors being «well-known» writers of the Sung dynasty. But we know nothing about Yü-wên Mou-chao beyond the superscription and the memorial to the Throne at the beginning of his book, and this memorial is surely spurious. As to Yeh Lung-li, his memorial of presentation to the Throne is dated 1180 and, in agreement with its contents, the Ch'ien-lung Commissioners (*Ssü-k'u* . . . , 50, 19 b-22 a) state that the *Ch'i-tan kuo chih* was written in compliance with an Imperial edict of the Emperor Hsiao-tsung (1162-1189); in the same notice, however, they say that Yeh Lung-li received the doctorate in 1247, but neither to

them nor to recent authors like CHANG Yü in his *Ch'ien Tsun-wang Tu-shu min-ch'iu chi chiao-chêng* (II A, 15-16) did it occur that a man who had already published a work in 1180 cannot well have passed his doctorate in 1247, that is to say 67 years later. That a Yeh Lung-li, native of Chia-hsing in Chê-chiang, received the doctorate in the seventh year of 淳祐 *ch'un-yu* (1247) is not open to doubt, since he is mentioned among the Chia-hsing men of that promotion in the *Chê-chiang t'ung-chih* and in the *Chia-hsing fu chih* (44, 42 b); but I find no other information about him. A possible solution would be that the date of the memorial to the Throne, «the seventh year *ch'un-hsi*» (1180), is corrupt for «the seventh year *ch'un-yu*» (1247), the very year in which the Chia-hsing man Yeh Lung-li became a doctor. The «seventh year *ch'un-hsi*», however, is the date already given in the surviving copy of the edition engraved in the Yüan period (cf. *T'ieh-ch'in-t'ung-chien-lou ts'ang-shu mu-lu*, 9, 23 b, 29 a; *Pi-Sung-lou ts'ang-shu chih*, 23, 14 a). Moreover, internal evidence, some of which has already been adduced by the Ch'ien-lung Commissioners, seems to prove that the book was not written by Imperial order, nor presented to the Throne. If the memorial be apocryphal, Yeh Lung-li's authorship may well be questioned. As a matter of fact, we must take the *Ch'i-tan kuo chih* as a spurious compilation of uncertain authorship and date, written perhaps at the end of the Sung dynasty, but perhaps also only in the Yüan dynasty. Already in the fourteenth century some of its blunders were denounced by Su T'ien-chio (1294-1352) in a dissertation which is quoted in the *Ssü-k'u* . . . , 50, 20 a (it is probably to be found in Su T'ien-chio's literary collection entitled *Tz'ü-ch'i wên-kao*, which is not at my disposal). Nevertheless, the supposed Yeh Lung-li had access to some works which have now perished, and some of the information he has preserved is not to be found elsewhere. The paragraphs of the *Ch'i-tan kuo chih* relating to the origin and the customs of the Ch'i-tan were extracted from the complete work by T'ao Tsung-i who reprinted them as the work of Yeh Lung-li, under the title of *Liao chih*, «Notice of the Liao», at the beginning of the 86th chapter of his *說郛 Shuo fu* of c. 1360-1366. From the *Shuo fu* they were copied under the Ming dynasty into the *Ku-chin shuo-hai*, and have been translated both by VASIL'EV (*Trudy VOIRAO*, IV, 171-196) and by R. STEIN (*TP*, 1939, 1-154).

The case of the *Ta-Chin kuo chih* is exactly parallel with that of the *Ch'i-tan kuo chih*. It is headed by a memorial of presentation to the Sung Emperor dated on the 15th of the first month of the first year *tuan-p'ing* (February 14, 1234), but it tells of the fall of the Chin dynasty which occurred at K'ai-fêng on February 9, 1234. How could an event which took place in Honan have been mentioned in a book presented to the Sung Emperor at Hang-chou in Chê-chiang five days later? Moreover, while Chinghiz-khan is spoken of as «the leader of the Ta-ta (Tartars) Tämü-jin» (22, 2 b), which is what would be expected from a subject of the Sung dynasty, the author speaks elsewhere of the Mongol reigning family as «the Great dynasty», of the Mongol troops as «the Great army», and of Mongol envoys as «Celestial messengers». These contradictions, and some others, have been denounced by the Ch'ien-lung Commissioners (*Ssü-k'u* . . . , 50, 22-23) and by CH' IEN Ta-hsin (1728-1804; in a notice on the *Ta-Chin kuo chih* which is found in ch. 28 of his *Ch'ien-yen-t'ang wên chi*). The Ch'ien-lung Commissioners were of opinion that the *Ta-Chin kuo chih* had been written by Yü-wên Mou-chao under the Sung, but that it had been tampered with at the beginning of the Yüan dynasty. CH' IEN Ta-hsin, more clear-sighted, maintained that

the whole work was a production of the beginning of the Yüan dynasty, with a spurious memorial and an apocryphal ascription. This is certainly right, but we may even go one step farther. The plan of the *Ch'i-tan kuo chih* and of the *Ta-Chin kuo chih* is the same. Both begin with a memorial of presentation, a paragraph on the origin of the nation (初興本末 *ch'u-hsing pên-mo*), and a table of the Emperors; then follow the annals of the various reigns, and some biographies; at the end are various notices and diaries of envoys. Some Chinese scholar of the nineteenth century must have expressed the view that both works were in fact due to Yeh Lung-li, since this has passed into WYLIE'S *Notes on Chinese Literature* (1867, p. 25), and thence into GILES'S *Biogr. Dict.* Nos. 2457 and 2536. It seems to be a vain task to try to determine the name of the author, but I have no doubt that both works are really due to one and the same man, and that this man lived under the Yüan dynasty. In both cases his spurious compilation was made up of extracts, which accounts for the discrepancies in his attitude towards the Mongols: some of the sources from which he drew were of Sung origin, others had been written under the Mongols, and he did not take the trouble to harmonize their statements. But, in spite of many errors, the *Ta-Chin kuo chih*, like the *Ch'i-tan kuo chih*, is of value, since it preserves anonymous quotations from works which have since perished. As in the case of the *Ch'i-tan kuo chih*, the paragraphs of the *Ta-Chin kuo chih* relating to the origin and the customs of the Nü-chên were extracted by T'ao Tsung-i and, under the title 金志 *Chin chih*, «Description of the Chin», they follow the *Liao chih* in the 86th chapter of the original *Shuo fu*. From there they too have passed into the *Ku-chin shuo-hai* and have been translated by VASIL'EV (*Trudy VOIRAO*, IV, 196-215). There is no copy extant of a Yüan edition of the *Ta-Chin kuo chih*, although it seems to have been published together with the *Ch'i-tan kuo chih* (cf. *T'ieh-ch'in-t'ung-chien-lou ts'ang-shu mu-lu*, 9, 23 a-b); the work is generally quoted from the edition which HSI Shih-ch'ên published at the Sao-yeh-shan-fang in 1797. Unfortunately this edition is based on the text adopted for the *Ssü-k'u ch'üan-shu*, in which the Imperial Commissioners have «changed what was objectionable»; I have not had access to a text collated on a ms. following the Yüan edition, like the one mentioned in 章氏四當齋藏書目 *Chang-shih ssü-tang-chai ts'ang-shu mu*, I B, 61-62.

We can now discuss the name «Lü-chên». It occurs in the preliminary paragraph of the *Ta-Chin kuo chih*, entitled *ch'u-hsing pên-mo*, «Account of how the fortune started». Unfortunately, the sheet containing this paragraph is missing from the only copy of the Sao-yeh-shan-fang edition I have at my disposal, and I must cite it from the text given in the *Chin chih* of the ancient *Shuo fu* (Commercial Press ed. 86, 10 a). The paragraph begins as follows (cf. also LI Wên-t'ien's commentary on the *Secret History*, 7, 2 a): «The kingdom of Chin had as original name 朱里真 *Chu-li-chên* (\*Jurçen or \*Jurjen), [but] the lingual sound (舌音 *shê-yin*; this is the Chinese term for our 'palatals', i. e. ç and j, also and more properly called in Chinese 舌上音 *shê-shang-yin*. 'supra-lingual sounds') of the foreign name (番語 *fan-yü*) became Nü-chên through corruption. Some call them Lü-chên. To avoid the personal name of the Emperor Hsing-tsong of the Ch'i-tan, they have moreover been called Nü-chih (all the editions of LI Wên-t'ien's commentary, in citing this passage, print it as though it were the form Lü-chên which was due to the taboo, which is absurd). They are the remnants of the 肅慎 *Su-shên* clan (氏 *shih*),



and a special branch (別族 *pieh-tsu*) of the 渤海 P'o-hai. Some say that they belong to the Na (?) clan (拏氏; the text is corrupt; cf. VASIL'EV, *loc. cit.* 197) of the division 辰韓 Ch'en-Han of the 三韓 San-Han (the San-Han, 'or Three Han', were three small kingdoms in Corea in the first centuries of our era). They were the weakest and the poorest among the tribes of the northern region. In the [period] *chêng-kuan* (627-649), the 靺鞨 Mo-ho came [to render homage] to the Court; the Middle Kingdom (*i. e.* China) [then] heard for the first time of the name of the Nü-chên. For generations [the Nü-chên] lived east of the 混同江 Hun-t'ung-chiang (Hun-t'ung River, the Sungari), at the foot of the Ch'ang-pai-shan (the 'Long White Mountain'); this mountain is [the place where] the Ya-lu-shui (Ya-lu River) takes its rise. To the south, they bordered on Kao-li (Corea); to the north, they adjoined the 室韋 Shih-wei; to the west, they bordered on P'o-hai and 鐵離 T'ieh-li; to the east, they reached the sea. What the *San kuo chih* calls 抱婁 Pao-lou (read 挹婁 I-lou), what the Wei of the Yüan [family] called 勿吉 Wu-chi, and what the T'ang called Hei-shui Mo-ho ('Mo-ho of the Black River', *i. e.* of the Sungari), is at present their territory. The [people] in their dependency were divided into six tribes (*pu*); [amongst them] there was the tribe (*pu*) of the Black River, which is the same as the present Nü-chên. If one should take in the hand water of this [River], its colour is slightly black; the Ch'i-tan gave it the name of Hun-t'ung-chiang. . . .

Ma Tuan-lin's chapter on the Nü-chên (327, 2-3) begins thus: «The Nü-chên are the Su-shên clan of ancient times. For generations they lived east of the Hun-t'ung-chiang, [at] the Ch'ang-pai-shan, [where] the Ya-lu-shui takes its rise. To the south, they bordered on Kao-li; to the north, they adjoined the Shih-wei; to the west, they bordered on P'o-hai and 鐵甸 T'ieh-tien (read 'T'ieh-li'); to the east they reached the sea. The Later Han called them I-lou; the Wei of the Yüan [family] called them Wu-chi; the Wei and T'ang called them Mo-ho. Their clan name (*hsing*) was Na 姓拏氏; among the northern Barbarians, they were the weakest and the poorest. In [the period] *k'ai-huang* (581-600) of the Sui, they offered tribute; their clans (*tsu*) were divided into six tribes (*pu*); [amongst them] there was the tribe (*pu*) of the Black River, which is the same as the present Nü-chên. If one should take in the hand water of this [River], the colour is slightly black; it was given the name of Hun-t'ung-chiang. . . . In [the period] *chêng-kuan* (627-649) of the T'ang, the Mo-ho came [to render homage] to the Court. [The Emperor] T'ai-tsung inquired about their customs, and so the talk touched on the affairs of the Nü-chên. It was at that [moment] that the Middle Kingdom (= China) first heard of their name (*i. e.* of the Nü-chên). The Ch'i-tan called them Lü-chên. . . . In [the period] *k'ai-yüan* (713-741) of the T'ang, their leader (*i. e.* apparently of the Nü-chên, but in fact of the Mo-ho) came [to render homage] to the Court, and he was appointed prefect (*tz'ü-shih*) of the department of 勃利 Po-li. Subsequently, a clan (部 *pu*, an error for 府 *fu*, 'department'; on the Hei-shui-fu, cf. GIBERT, *Dictionnaire*, 220) of Hei-shui ('Black River') was established, and the leader of the clan (*pu*) was made its governor (*tu-tu*); the Court established there a *ch'ang-shih* to control him. . . . At the time of the Five Dynasties (907-960), [these tribes] were for the first time called Nü-chên. Later on, to avoid the personal name Tsung-chên of the Ch'i-tan leader (*chu*; Hsing-tsung of the Liao), this name was changed to 女直 Nü-chih, which became 女質 Nü-chih through popular corruption. . . ».

The *Ta-Chin kuo chih* and Ma Tuan-lin's *Wên-hsien t'ung-k'ao* are, as far as I am aware, the only sources where the form *Lü-chên* occurs as a name of the *Nü-chên*. But, to pass a judgement on the value of these two texts, two other works of earlier date must be adduced.

One is the 三朝北盟會編 *San-ch'ao pei-mêng hui-pien*, the author of which, 徐夢莘 Hsü Mêng-hsin, lived from 1124 to 1205 (cf. CHAVANNES, in *JA*, 1897, 1, 387-388); it is an invaluable repository of texts and documents ranging from 1117 to 1161. In his commentary on the arrival of the *Nü-chên* envoys at the Sung Court on February 22, 1119, Hsü Mêng-hsin says (3, 1 *a-b*; almost the whole of the text which I translate was copied *verbatim* by Hsü Mêng-hsin from Hung Hao's slightly earlier *Sung-mo chi-wên*; I prefer to translate from Hsü Mêng-hsin because the passage on the name *Chü-li-chên* does not occur in Hung Hao's account): «The *Nü-chên* are the ancient kingdom of *Su-shên*. Their original name was 朱理真 *Chu-li-chên*, [but] the foreign name (*fan-yü*) became *Nü-chên* through corruption. They were originally the descendants of 朱蒙 *Chu-mêng* of *Kao-li* (Corea). Some make them [belong] to the race (*chung*) of the *Mo-ho* of the *Black River*, as (?) a special branch (*pieh tsu*) of the *P'o-hai*, [or to? (the text seems to be corrupt)] the *Ch'ên Han* of the *San Han* («Three Han»). In truth, all [of these] were small kingdoms of the eastern Barbarians. For generations, [the *Nü-chên*] lived east of the *Hun-t'ung-chiang*, [at] the *Ch'ang-pai-shan*, [where] the *Ya-lu-shui* takes its rise... To the east, they reached the sea; to the south, they bordered on *Kao-li*; to the west, they adjoined *P'o-hai* and *T'ieh-li*; to the north, they came near the *Shih-wei*. What the *San-kuo chih* calls *I-lou*, what the *Wei* of the *Yüan* [family] called *Wu-chi*, what the *Sui* called *Hei-shui-pu* ('Tribe of the *Black River*'), and what the *T'ang* called *Hei-shui Mo-ho* is their territory... When, in [the period] *chéng-kuan* of the *T'ang*, the Emperor *T'ai-tsung* led an expedition against *Kao-li* (Corea), ... more than 100,000 *Mo-ho* troops made their submission... In [the period] *k'ai-yüan*, their leader (*i. e.* of the *Mo-ho*) came [to render homage] to the Court, and he was appointed prefect (*tz'ü-shih*) of the department (*chou*) of *Po-li*. Subsequently, a department (*fu*) of *Hei-shui* was established, and the leaders of the clan were made its governors (*tu-tu*) and prefects (*tz'ü-shih*)... At the time of the *Five Dynasties*, [these tribes] were for the first time called *Nü-chên*... ».

Though of uncertain date and authorship, another work dealing with the *Nü-chên* must certainly be earlier than the *Ta-Chin kuo chih* and the *Wên-hsien t'ung-k'ao*; it goes under the title of 裔夷謀夏錄 *I-i mou Hsia lu*, «Accounts of the plots of the Barbarians against China», and is usually given as the work of 劉忠恕 *Liu Chung-shu*. Certainly because of the manner in which the book speaks of the *Nü-chên*, which the *Ch'ien-lung* Commissioners felt to be offensive, the *I-i mou Hsia lu* is not mentioned in the *Ssü-k'u*..., though the *Man-chou yuan-liu k'ao* (dated 1777; 1, 3 *a*) quotes from «the Sung author *Liu Chung-shu*» the form «*Chu-li-chên*» of the name of the *Nü-chên*. I know of no modern edition of the *I-i mou Hsia lu*, and have never had a ms. copy of it at my disposal. Nothing seems to be known of its alleged author *Liu Chung-shu*. The text, as now extant, is in one chapter (*Pi-Sung-lou ts'ang-shu chih*, 28, 21-23), or in three (*Shan-pên-shu-shih ts'ang-shu chih*, 10, 10 *a-b*), and begins with a preface by 胡潛 *Hu Ch'ien*, probably a Sung man, about whom I know nothing. On the other hand, the *Chih-chai shu-lu chieh-t'i*, in which no work later than c. 1235 is listed,

mentions (5, 21 b; cf. also *Wên-hsien t'ung-k'ao*, 197, 2 b) the *I-i mou Hsia lu* in seven chapters (not in one or in three chapters, as said above) as the work of 汪藻 Wang Tsao, and the same authorship is given in the bibliographical section of the *Sung chih* (203, 11 a), where, however, the *I-i mou Hsia lu* is said to be only in three chapters. Now, it is most improbable that there should have been two different works of the Sung period with the same rare title of *I-i mou Hsia-lu*; one of the attributions (or perhaps both of them) must be erroneous. Wang Tsao (1079-1154) was a well-known statesman and writer (cf. GILES, *Biogr. Dict.* No 2236, to be combined with the pseudo-Wang Kung-tso of No. 2192), but a cursory examination of his literary collection, entitled 浮溪集 *Fou-ch'i chi* (ed. *Ssü-pu ts'ung-k'an*), has failed to reveal to me anything to suggest his possible authorship of the *I-i mou Hsia lu*. Whether the *I-i mou Hsia lu* be due to Wang Tsao or to Liu Chung-shu, and whether or not it be a work of the first half of the twelfth century, the fact remains that it already existed c. 1235 when the *Chih-chai shu-lu chieh-t'i* was completed, and so antedates both the *Ta-Chin kuo chih* and the *Wên-hsien t'ung-k'ao*. Although I cannot cite the *I-i mou Hsia lu* at first hand, the following passage is quoted in LI Wên-t'ien's Commentary on the *Secret History* (ed. *T'ung-yin-t'ang*, 1896, 7, 2 a): «The *I-i mou Hsia lu* of Liu Chung-shu of the Sung says: 'The original name of the kingdom of Chin was 朱里真 *Chu-li-chên*. To avoid [the use of] the personal name Tsung-chên of Hsing-tsung of the Ch'i-tan, [the *Nü-chên*] have also been called *Nü-chih*. In [the period] *chêng-kuan* (627-649) of the T'ang the Mo-ho came [to render homage to the Court]; the Middle Kingdom [then] heard for the first of the name of the *Nü-chên*. For generations, [the *Nü-chên*] lived east of the Hun-t'ung-chiang; [to the east] (there is clearly a lacuna here, due to haplography between the two *tung*, «east», of the other texts, and so the intervening limits of the *Nü-chên* have been omitted) they reached the sea. They are what the *San-kuo chih* calls I-lou. They lived on the Ch'ang-pai-shan, [where] the Ya-lu-shui takes its rise'.»

When comparing these four texts, one cannot but be struck by their close connection and even, in many cases, by their identical wording. The most sober and accurate version is that of the *San-ch'ao pei-mêng hui-pien*. There the author draws a clear line between hearsay tradition and accurate information, and says neither that the name of the *Nü-chên* was known in the seventh century, nor that there was another form *Lü-chên* of *Nü-chên*. The statement that the name of the *Nü-chên* was known in early T'ang times occurs for the first time in the *I-i mou Hsia lu*. It was certainly from the latter work that it passed into the *Ta-Chin kuo chih* and the *Wên-hsien t'ung-k'ao*, but neither of the authors of these later works noticed that it was in contradiction with another statement, which they copied from the *Sung-mo chi-wên* or the *San-ch'ao pei-mêng hui-pien*, to wit that the name *Nü-chên* made its appearance only in the tenth century. That the author of the *Ta-Chin kuo chih* knew either the *Sung-mo chi-wên* or the *San-ch'ao pei-mêng hui-pien* (or perhaps both) is quite certain. If an additional proof should be required, it would be provided by the passage of the *Ta-Chin kuo chih* (32, 2 b) in which it is said that the *Nü-chên* terms for «wife» and «husband» were 薩那 *sa-na* (the 薩 *sa-sa* in the *Chin chih* [*Shuo fu*, 86, 12 b] is corrupt) and 愛根 *ai-kên* respectively. This is clearly drawn from the *Sung-mo chi-wên*, 14 a, or the *San-ch'ao pei-mêng hui-pien*, 3, 4 b, where we read, more correctly, that the *Nü-chên* terms for «wife» and «husband» were 薩那罕

*sa-na-han* and *ai-kên*; these forms represent the words transcribed *sa-li-an* and *o-i-o* in the late Jučen Vocabulary, equivalent to the Manchu *sargan* and *eigen* (cf. GRUBE, *Die Sprache und Schrift der Jučen*, pp. 97, 98). But it is no less evident that the pseudo-Yü-wên Mou-chao made use of the *I-i mou Hsia lu*, and moreover this can be established by a detail in the text. In both the *I-i mou Hsia lu* and the *Ta-Chin kuo chih* we read: 唐貞觀中靺鞨來中國始聞女真之名. This is a clumsy redaction. In LI Wên-t'ien's commentary, the text is punctuated after *chung-kuo*, so that the translation would be: «In [the period] *chêng-kuan* of the T'ang, the Mo-ho came to the Middle Kingdom, and [the Middle Kingdom then] heard for the first time of the name of the Nü-chên». But «*lai chung-kuo*» is hardly possible in Chinese, and I hold that, at an early date, before the *Ta-Chin kuo chih* was compiled, the word 朝 *ch'ao* had been accidentally omitted in the passage of the *I-i mou Hsia lu*. This is the reason why I have translated: «...the Mo-ho came [to render homage to the Court]; the Middle Kingdom [then] heard...». Such must also have been Ma Tuan-lin's opinion since he writes: «...the Mo-ho came [to render homage] to the Court... It was at that [moment] that the Middle Kingdom...».

So all the elements of the accounts in the *Ta-Chin kuo chih* and the *Wên-hsien t'ung-k'ao* can be traced back to the *San-ch'ao pei-mêng hui-pien* or to the *I-i mou Hsia lu*, with the exception, however, of what relates to the form «Lü-chên» of the name of the Nü-chên. It is true that, as GIBERT says, there are many cases of alternations between *l-* and *n-* at the beginning of words (see «Lop» and «Lambri»), and many words beginning in Chinese with *l-* have an initial *n-* in the Sino-Corean pronunciation; such are the Sino-Corean 路 *no*, 綠 *nok*, 鹿 *nok*, 離 *ni*, etc. The alternation between Ch. T'u-yü-hun and Tib. Thu-lu-hun (cf. TP, 1921, 323) is mysterious; and so are the different values, *yin*, *lin* and *shên*, of one and the same Jučen character in GRUBE, *Die Sprache und Schrift der Jučen*, p. 51, No. 104 (I leave out *k'u*, which seems to be corrupt). The present case, however, is peculiar. In the Altaic languages, the most frequent occurrence is a change from *l-* to *n-* in languages which show a reluctance to an initial *l-*; for instance, the Persian *la'al*, «ruby», becomes *nal* in mediaeval Mongolian, and Mongolian uses both *lačïn* and *načïn* for the Turkish *lačïn*, «falcon». In spite of TERRIEN DE LACOUPERIE, there is no apparent reason why the Ch'i-tan, who spoke a Mongolian dialect, should have changed to a form beginning in *l-* a name of Tungus origin which began either with *ʃ-* as Jurčen or with *n-* as the usual Chinese transcription Nü-chên. The very ascription of the form Lü-chên to the Ch'i-tan, which occurs only in Ma Tuan-lin, seems, moreover, to rest on some error. In the *Ta-Chin kuo chih* we are told, in a sentence relating to the Black River, that «the Ch'i-tan gave it the name (契丹目爲 *Ch'i-tan mu wei*) of Hun-t'ung-chiang», which is perfectly correct (cf. GIBERT, *Dictionnaire*, 327-328), and «Lü-chên» is merely given as another form of Nü-chên. In Ma Tuan-lin we read that the river «was given the name» (*mu wei*), without the «Ch'i-tan» which ought to precede *mu wei*. But in the next line, instead of the «some call them (或曰 *huo yüeh*) Lü-chên» of the *Ta-Chin kuo chih*, Ma Tuan-lin gives «the Ch'i-tan called them (契丹目之曰 *Ch'i-tan mu chih yüeh*) Lü-chên». It seems quite probable that the ascription to the Ch'i-tan of the name Lü-chên is due to a confusion with the sentence which correctly attributed to them the name Hun-t'ung-chiang of the Hei-shui.

Finally, and without absolutely discarding the possibility of the form Lū-chên, I must say that it seems to me to be a very doubtful form and perhaps due merely to a bad blunder of the *Ta-Chin kuo chih*. The character 慮 *lü* is not altogether unknown in Ch'i-tan transcriptions; a place-name 且慮 *Chü-lü* is mentioned in the *Liao shih* (116, 13 b); but it is of rare occurrence. Now there is a sentence which in the *Chin chih* (copying the *Ta-Chin kuo chih*) reads: «The Ch'i-tan were afraid (恐 *k'ung*) that the Nü-chên [might] make trouble» (契丹恐女真爲患 *Ch'i-tan k'ung Nü-chên wei huan*), but, in the *San-ch'ao pei-mêng hui-pien* (3, 2 a), the original text is: «A-pao-chi was concerned (慮 *lü*) about [the fact that] the Nü-chên [might] make trouble» (慮女真爲患 *lü Nü-chên wei huan*), which becomes in Ma Tuan-lin «A-pao-chi was concerned about [the fact that] they (*i. e.* the Nü-chên) [might] make trouble» (慮其爲患 *lü ch'i wei huan*). It looks as though it were this 慮其 *lü ch'i* which was misread 慮真 *lü chên* by the author of the *Ta-Chin kuo chih*, perhaps in some text already corrupt, and thus gave birth to his alternative name «Lü-chên» for the Nü-chên. This was then taken over by Ma Tuan-lin, who, owing to an additional mistake, specified that this name «Lü-chên» had been given to the Nü-chên by the Ch'i-tan. If, however, «Lü-chên» should unexpectedly turn out to be a genuine form, a last hypothesis might be suggested: Lū-chên could be an apheretic form of a more complete \*Nü-lü-chên < \*Jurçen. Such aphereses are not unknown. I lay no stress on the names Ta-Chên = Ta [Nü-]chên and Tung-Chên = Tung [Nü-]chên which will be mentioned in the last part of the present note, because they are half Chinese. But ancient names of the Nü-ku mu-li of the Ch'i-tan period (the present Šira-mürän; cf. *infra*, p. 379), like 如洛壤 *Ju-lo-kuai* (\*Nzi<sup>w</sup>o-läk-kuai), which occurs also as «Lo-kuai», would provide an exact parallel to \*Nü-lü-chên > Lü-chên (on the ancient names of the Šira-mürän, cf. SHIRATORI, *Beiträge zur historischen Geographie der Mandschurei*, Tökyō, 1914, I, 365). But, whatever the truth about «Lü-chên» may be, it must not be adduced to explain the «Longa» in Rubrouck's «Longa et Solanga», as has been suggested by ROCKHILL (*Rubruck*, 153; cf. *Wy*, I, 234).

The name Chu-li-chên, which occurs in the *San-ch'ao pei-mêng hui-pien*, the *I-i mou Hsia lu*, and the *Ta-chin kuo chih* is of course nothing but a transcription of \*Jurçen; it was a bad mistake of Li Wên-t'ien, in his Commentary on the *Secret History* (7, 2 a), to have stated that both Chu-li-chên and Nü-chên represented the name of the Mongol tribe Ĵirgin (occurring in the *Secret History*, §§ 170, 171, 185-187, 208). The opinion expressed in the *San-ch'ao pei-mêng hui-pien* that the Chu-li-chên, *alias* Nü-chên, were the descendants of Chu-mêng of Kao-li is also an error. Chu-mêng was the name of the traditional founder of the kingdom of Kao-li or Kao-ku-li (Corea) just before the Christian era (cf. GIBERT, *Dictionnaire*, 429, 880). But the alleged connection between the names is fictitious, since it is based on the tacit assumption that the names are to be read \*Chu Li-chên and \*Chu Mêng, as though both began with the Chinese surname Chu; but both Chu-li-chên and Chu-mêng are mere transcriptions of foreign names. The correct equivalence of Chu-li-chên with \*Jurçen was given long ago by BRETSCHNEIDER (in *JNCB*, x [1876], 108; cf. also TERRIEN DE LACOUPERIE in *JRAS*, 1889, 437; *Br*, I, 224).

In the 登壇必究 *Têng-t'an pi-chiu*, published in 1598, DEVÉRIA found the name 主兒赤 *Chu-êrh-ch'ih*, rendered in Chinese 海西 *Hai-hsi*, «West of the Sea» (cf. *Revue de l'Extrême-*

*Orient*, I, 175). Here again TERRIEN DE LACOUPERIE (*JRAS*, 1889, 438) absurdly transcribes «Tchu-erh-tch'ek», as if a pronunciation *êrh* could be contemporaneous with a pronunciation in which the ancient *-k* of *ch'ih* (*t's'jâk*) was still preserved. As to Hai-hsi, TERRIEN DE LACOUPERIE, unable to explain it, thinks that it probably «is a simple pun». I see things differently. I have no access to the original edition of the *Têng-t'an pi-chiu*, nor do I suppose had DEVÉRIA either. In my nineteenth-century reprint (ch. 22), Chu-êrh-ch'ih occurs twice; once (No. 77) it is given as the equivalent of Hai-hsi, but the second time (No. 183) the Chinese equivalent has been suppressed. The Manchus, holding themselves to be the descendants of the Chin, expurgated most of the works in which the name of the Nü-chên or Nü-chih occurred, and I have little doubt that the original edition of the *Têng-t'an pi-chiu* had in one place Nü-chih, which has been left blank in modern editions, and in the other place Hai-hsi, which was a regular geographical name in the region in northern Manchuria occupied by the Nü-chên of Ming times (cf. SHIRATORI, *Beiträge*, II, 527). As to Chu-êrh-ch'ih itself, the term, contrary to what DEVÉRIA and TERRIEN DE LACOUPERIE thought, has no real bearing on the forms used in China and by the Chinese; the section in which it appears is a Sino-Mongolian Vocabulary and Chu-êrh-ch'ih merely represents the Mongolian form, which we know to have been Jürçit in late Ming and early Ch'ing times. In the transcriptions of the *Têng-t'an pi-chiu*, the final *-t* is omitted in *künji*[*t*] = *künjit*, «sesamum» (No. 445); the case is almost certainly the same with Chu-êrh-ch'ih, which is accordingly to be restored as the regular Mongol form Jürçit.

The true name of the Nü-chên appears in native writing in the Vocabulary of the Board of Translators of c. 1500 published by GRUBE (No. 324); it is written with two Juçen characters which are phonetically transcribed 朱先 Chu-hsien, with the Chinese equivalent Nü-chih. But this Chu-hsien must be explained. Seemingly, it renders a native pronunciation \*Jusen, but this need not have been the pronunciation of the eleventh and twelfth centuries. The first Juçen character used in the name does not occur elsewhere, but its original value was probably \**jur-*, not *ju-*. As to the second character, it is also used as the second element of the Juçen word meaning «kettle», transcribed phonetically 木先 *mu-hsien* (GRUBE, No. 244), \**musen*. But the same word is transcribed 木徹 *mu-ch'é*, \**muçe*, in the Sino-Juçen Vocabulary of the Board of Interpreters acquired by AUROUSSEAU for the Ecole Française d'Extrême-Orient, and the corresponding word in Manchu is *muçen*. We may therefore safely conclude that the forms in *-sen* are due to dialectical variations or to an unsatisfactory notation of the Board of Translators, but that the true Juçen forms were Jürçen (? > \*Juçen > Jusen) and *muçen*, respectively.

The late form \*Jusen receives apparent support from a Manchu word. In his preface to the *Man-chou yüan-liu k'ao*, the Emperor Ch'ien-lung says: «When the Dynasty was founded, the ancient name which designated those belonging to the 滿珠 Man-chu was 珠申 Chu-shên; later, this name [Chu-shên] was changed to Man-chu (Man-ju), but in the end the Chinese characters [Man-chu] became altered to 滿洲 Man-chou. The truth is that the ancient [name] 肅慎 Su-shên is a phonetical change from Chu-shên, and this gives an additional proof in favour of the territorial identity [of the Su-shên and the Man-chu].» DEVÉRIA's translation (*Rev. de l'Extrême-Orient*, I, 174-175) is somewhat different, but would imply that the text is self-contradictory (cf. moreover I, 3a). Ch'ien-lung's assertion that the Chinese characters were

«altered» means that it was Man-chu which correctly rendered Manju, and that, if *chou* was commonly substituted for *chu*, it was because *chou*, «island», lent itself better in Chinese to a geographical use (cf. I, 1 b, 2 a; as a matter of fact, the very name «Manju», unknown before the seventeenth century, has never been satisfactorily explained; cf. GIBERT. *Dictionnaire*, 589, 602-603). I shall speak of the ancient Su-shên farther on; but it is self-evident that if there should be a connection between Su-shên and Chu-shên, the chronology would require a process of evolution just the opposite of that formulated by Ch'ien-lung (the text in ch. I, 3 a, is in this respect more logical than Ch'ien-lung's preface).

There can be no doubt that the name Chu-shên adduced by Ch'ien-lung in connection with Su-shên and said by the compilers of the *Man-chou yuan-liu k'ao* (I, 3 a) to be fundamentally identical with Chu-li-chên (Jurčen) is the Manchu *jušen*, *juše*, meaning «Manchu servants» and «family people» (cf. ZAKHAROV'S *Dictionary*, 1005; TERRIEN DE LACOUPERIE, in *JRAS*, 1889, 438). GIBERT (*Dictionnaire*, 141), while admitting that *jušen* was the designation not of the ruling class of the Manchus, but of an inferior class maintained in a state of bondage by the other, accepts the Chinese view which connects the name of the *jušen* with that of the Jurčen (cf. also *Tz'ü yüan*, *ch'ou* 253, s. v. «Nü-chên»). The transcription Chu-hsien, \*Jusen, given for the native name of the Jurčen in the late Jučen Vocabulary published by GRUBE, may be adduced as providing an intermediary form. Yet I retain some doubts as to the correctness of the equivalence. Since we have the Manchu *mučen* as corresponding to the \**musen* of GRUBE'S Vocabulary and to *muče* of the Vocabulary acquired by AUROUSSEAU, we should expect, as the Manchu form corresponding to \*Jusen, not *jušen*, but \**jučen*. As to the Manchu *jušen* itself, its historical use remains to be investigated, and I am not certain that it ever was, as Ch'ien-lung's text would imply, an ethnical and not a class name. ZAKHAROV'S etymology from the Chinese 主人 *chu-jen*, «master», is not acceptable, but we may have to do with an independent Manchu word, unconnected with Jurčen. I am all the more in doubt about any such connection because the very form Jurčen may have survived in Manchu. *Jurčen* in Manchu means «opposition», «disobedience», from a root *jurče-* which has many derived forms. But ZAKHAROV (p. 1014) says that it is also the «name of a clan». It is true that he says this with a question-mark. But it looks as though he had actually found the name as that of a clan, but felt some hesitation on account of the identity of that name with the word *jurčen*, «disobedience». If such be the case, the absolute identity of the clan-name Jurčen with the form which we have reconstructed from the various transcriptions for the native name of the Nü-chên would make it certain that it is this native name which has survived as a clan-name in Manchu, and the very doubtful connection proffered between the name of the Jurčen and the Manchu class-name *jušen* should be definitely abandoned.

As to the date of the first occurrence of the name Nü-chên, we have seen (p. 376) that, according to the *San-ch'ao pei-meng hui-pien*, the Ch'i-tan Emperor A-pao-chi was afraid that the Nü-chên might create trouble. This is confirmed by the *Liao shih* (in which work the name always occurs as Nü-chih, as required by the taboo of 1131), where we read (I, 1 a-b) that in 903 A-pao-chi subdued the Nü-chih, and again, in 906, those of the Nü-chih who had not submitted before. But A-pao-chi died on September 18, 926, and it was only under his suc-

cessor that for the first time, on January 16, 928, an embassy from the Nü-chih came to the Ch'i-tan Court (*Liao shih*, 3, 1 b; 70, 2 b). From that date, others followed in rapid succession, almost every year and sometimes twice in the same year.

If we pass from the Ch'i-tan to the purely Chinese dynasties in the south, I do not find the name of the Nü-chên in the *Chiu Wu-tai shih* (*Ancient History of the Five Dynasties*). This is not, however, a decisive proof that it never occurred in it, since the text of that book has been reconstructed and is incomplete and has moreover been tampered with by the eighteenth-century editors precisely in the passages which concerned the north-eastern tribes, out of respect for the kinship of the latter with the reigning Manchu dynasty (cf. on this point *Chang-shih ssü-tang-chai ts'ang-shu mu*, 1 B, 10 a-b). In the later *Wu-tai shih*, written by Ou-yang Hsiu (1007-1072), the Nü-chên are mentioned in the chapter devoted to the Ch'i-tan (72, 2 b), where we read that A-pao-chi, suspicious of the Chinese to the south, « was vexed that the Nü-chên and the P'o-hai were in his rear ». But what follows only refers to the campaign against the P'o-hai, and the possibility is not to be excluded that Ou-yang Hsiu inserted the name of the Nü-chên, not from original documents transmitted from the Chinese Five Dynasties, but from information which had come from the Ch'i-tan dominions at a later date. The only certain mention I can trace of the Nü-chên in a Chinese text actually written under the Five Dynasties is that of the narrative of Hu Chiao, preserved in different works the most ancient of which is Ou-yang Hsiu's *Wu-tai shih* (73, 3 b). Hu Chiao was detained among the Ch'i-tan from 947 to 953. In his narrative a paragraph is devoted to the Nü-chên, who lived east of the Ch'i-tan (cf. CHAVANNES, in *JA*, 1897, 1, 404). It seems that no embassy from the Nü-chên came to the Chinese Court during the Five Dynasties. But diplomatic intercourse began almost immediately after the Sung ascended the throne (960) : in 961, the Sung Emperor received an embassy from the Nü-chên (*Hsü tzü-chih t'ung-chien ch'ang-pien*, 2, 13 b; *Sung shih*, 1, 4 b; the *Sung shih*, *Chin shih*, and *Liao shih* were compiled under the Mongols by the same author and always employ the form Nü-chih, though the works actually written under the Sung always give Nü-chên); and embassies then followed one another from year to year without interruption.

The conclusion is that the name of the Nü-chên does not occur before the beginning of the tenth century, and that the Chinese owe it to the Ch'i-tan. The very form « Nü-chên » favours this view, since 女 *nü* (*ni*<sup>vo</sup> in middle Chinese, but this pronunciation is older than the Ch'i-tan period) is not used, as far as I remember, in the transcriptions of foreign names made under the T'ang, whereas it sometimes occurs in transcriptions made under the Ch'i-tan. Such are, for instance, 女壤 Nü-hsiang, a man's name (*Liao shih*, 116, 15 b; I have not found the corresponding passage which ought to occur in ch. 68); 女里 Nü-li, whose biography is given in the *Liao shih*, 79, 1 b-2 a; and above all 女古 Nü-ku. Nü-ku is a man's name; four different Nü-ku are listed by WANG Hui-tsu<sup>1</sup>, 3, 1 a, to whom I think we should add the 女右 Nü-yu of *Liao shih*, 68, 4 a, probably a graphic corruption of Nü-ku. Nü-ku tribes are mentioned in the *Liao shih*, 1, 5 a; 69, 11 a. Nü-ku also occurs as the name of a river in 女古沒里 Nü-ku mu-li, where *muri* is the Ch'i-tan form of Mong. *mörän*, *mürän*, « river » (cf. SHIRATORI, *Beiträge zur historischen Geographie der Mandschurei*, 1, 365). The place-name 女古底 Nü-ku-ti of the *Liao shih*, 29, 1 b, and 116, 9 a, is probably a derivative form of the same word. Moreover,



*nü-ku* transcribes the Ch'i-tan word for «gold» (*Liao shih*, 31, 4 a; 116, 12 b). Unfortunately we do not know the true Ch'i-tan original forms of any of these names, and consequently, while they prove the use of *nü* in the transcription of the first syllable of Ch'i-tan names or words, they do not help to establish the real phonetic value of the initial consonant. So we are left with the troublesome discrepancy between *Nü-chên* and \**Jurčën*. The case of *nü-ku* is particularly puzzling. The Ch'i-tan spoke a Mongol dialect. The usual Mongol word for «gold» is *altan*, clearly connected with Turk, *altin*, *altun* (see «Altai»). We know also the Jučen word, \**alč'u'un*, \**alču*, and the Manchu word is *aisin* (see «Roi Dor»). But *nü-ku* recalls none of them. It would be a desperate solution to read *nü-ku* as \**ju-ku* and to see in the latter term an apheretic form corresponding with the Jučen \**alč'u'un*.

I have translated above a sentence in which Ch'ien-lung, followed by the authors of the *Man-chou yüan-liu k'ao*, connects the name of the *Nü-chên* with that of the ancient *Su-shên*. The starting point of this theory is the statement in the *San-kuo chih* of the third century (30, 7 b) that the 挹婁 I-lou (\**l̥əp-ləu*) of the beginning of our era were the same as the ancient *Su-shên*. During the Six Dynasties, the I-lou were replaced by the 勿 謁 Wu-chi (\**Mjuət-kjöt*), whose name was transcribed 𣪠 𣪠 Mo-ho (\**Muät-γât*) in the seventh century, and the *Nü-chên* are often said to be scions of the Mo-ho. The consequence was that the *Nü-chên* were already in the middle of the twelfth century said to be descendants of the *Su-shên* in the *Sung-mo chih-wên* and the *San-ch'ao pei-mêng hui-pien*, followed by the subsequent writers of late Sung times. The Emperor Ch'ien-lung could not but accept an identification which provided such an ancient pedigree for the *Nü-chên*, considered by him as the true predecessors and almost as the ancestors of his own dynasty. TERRIEN DE LACOUPERIE (*JRAS*, 1889, 436-437) believed that he could corroborate the identification both historically and phonetically, and this solution is unreservedly accepted by CHARIGNON (*Ch.*, I, 168) and GIBERT (*Dictionnaire*, 140, 375, 812). It is however untenable.

First of all, TERRIEN DE LACOUPERIE's phonetic discussion is valueless. The name of the *Su-shên* occurs in texts prior to the Christian era as 肅慎 *Su-shên* (\**Sj̥uk-žj̥ën*), 息慎 *Hsi-shên* (\**Sj̥ək-žj̥ën*) and 稷慎 *Chi-shên* (\**Tsj̥ək-žj̥ën*). All these transcriptions suppose a *-k* at the end of the first syllable, and it was a grievous mistake of TERRIEN DE LACOUPERIE to maintain that the ancient Chinese used to represent a foreign syllable ending with an *-r* (as in \**Jurčën*) with a character which was pronounced with a final *-k*. Not one example of it has been adduced hitherto.

But it is mainly on historical grounds that the equation of the *Nü-chên* with the *Su-shên* must be rejected. GIBERT says that the *Su-shên* came to the Court of the Emperor Shun in «2231 B. C.». Everybody would wish to have genuine Chinese records of such an early date; but unfortunately all the stories about the *Su-shên* prior to the middle of the first millennium B. C. are of a legendary character. From a passage of the *Tso chuan*, under the 9th year of Duke Chao (553 B. C.), we may infer that there was probably a tribe then known as *Su-shên* on the northern borders of the Chinese territory, but certainly not so far away as eastern Manchuria. All the other mentions of the *Su-shên* belong to the stock-in-trade of folk-lore. The virtue of the Emperor extends all over the world, and is proved by the arrival of far-away tribes; the

Su-shên play their part in such homage by the side of the Long-legged, the One-armed, and the Three-bodied. Such is the conclusion arrived at by IKEUCHI Hiroshi in a remarkable memoir, *A Study of the Su-shên* (first published in Japanese in Vol. 13 of the *Man-Sen jiri*. . . *hōkō*, 1-61, but more accessible in English in *Memoirs of the Research Department of the Toyo Bunko*, No. 5, 1930, 97-163), and I can only endorse it. I also concur with IKEUCHI in his further view that the name of the kingdom of Su-shên which reappears under the Six Dynasties in reference to the I-lou and Wu-chi tribes, the last embassy of the Su-shên dating A. D. 559, is not due to any kingdom which really bore that name, but is an archaistic designation of purely Chinese origin. The tradition about the Su-shên, famous on account of a passage in the biography of Confucius, was that they had brought as tribute a certain kind of arrow-head. The I-lou, and later the Wu-chi, were identified with the Su-shên and given their name because they had offered a similar tribute. But there was no longer a Su-shên tribe in the Christian era, and there is not the slightest chance that this half-legendary name should have survived, even if the two were not phonetically irreconcilable, in that of the Nü-shên or \*Jurčen.

Another attempt has been made to connect the name of the \*Jurčen with forms known in other times and in other regions. In his *Studien zur vergleichenden Grammatik der Türk-sprachen* (SPAW, 1916, 1239), BANG drew attention to Čürčüt and Šüršüt (< \*Čürčüt), which is a Turkish name of the Mongols among the Kurdak and the Kirghiz respectively. I am not aware of the exact value of these forms, nor of the extent to which they were or are in use; it may, however, be that this Čürčüt was also the form used among the Mongols in the second half of the last century, since GOMBOEV, himself a Mongol, transcribed as «Jurcut» the name which he found written Jurčit in the *Altan tobči* (cf. p. 367). In any case, it does not seem open to doubt that this Čürčüt or Šüršüt is the outcome, with a new ascription, of the Mongol name Jürčät of the \*Jurčen.

But BANG goes farther, and proffers two other assumptions with which I must disagree. One, which he expressed in *Türán* (Nos. 6-7 [1918], 437), is that Čürčüt is a plural of čürčü, čürčü, a word occurring in various Turkish dialects with the meaning «brother-in-law»; but there is not the slightest likelihood that the \*Jurčen should have owed their native name to a dialectical Turkish word, unattested moreover in Uighur or in Mongolian.

BANG's second suggestion (SPAW, 1916, 1239), which requires more detailed comment, is that Čürčüt, identical on the one hand with the name of the \*Jurčen, represents at the same time «the Chinese designation Dzut-Dzut of the Avars»; for this last form, he refers to DE GROOT, in MARQUART, *Ueber das Volkstum der Komanen*, 87. These «Dzut-Dzut» are the same which we generally call Juan-juan, and a connection between «Juan-juan» and «\*Jurčen» had already been proposed by BLOCHET, in a note to which I have alluded in *JA*, 1920, I, 147, but which I cannot trace at present. DE GROOT's remarks were to the effect that 蠕蠕 Juan-juan, «on account of its phonetic element, could only be pronounced 'Dzu-Dzu'», so that «the usual pronunciation Juan-juan must be abandoned». In my review of MARQUART's book (*JA*, 1920, I, 143-144) I replied that the most ancient Chinese dictionaries register the reading *juan* for 蠕, that the name Juan-juan still occurs in the *Liao shih* (116, 15 b) with a phonetic gloss indicating that very pronunciation, and that moreover the Chinese consider 蠕 *juan* as another graphic

form of 𪛗 *juan*, the phonetic of which is precisely *juan*. But FRANKE (*Gesch. des chines. Reiches*, III, 283-284) has since dissented from my view, so that I must return to that point.

The name of the Avars occurs in Chinese texts in several forms, some southern, some northern (in the fifth and sixth centuries, China was divided between northern dynasties of nomadic origin and those which were purely Chinese in the south). The southern forms are 芮芮 Jui-jui (Ñǐwät-ńǐwät; in *Sung shu*, 95; *Nan-Ch'i shu*, 59; *Liang shu*, 54; I shall revert to the ancient pronunciation of Jui-jui later) in the fifth century and first part of the sixth, and 茹茹 Ju-ju (\*Ñǐwo-ńǐwo) in the second half of the sixth century (*Chou shu*, 50, 1 a; *Sui shu*, 83, 3 a; 84, 1 a; but at the beginning of the paragraph in *Sui shu*, 83, 3 a, the « northern » form Juan-juan also occurs once, no doubt because the authors of the *Sui shu* drew there from a northern source; the *T'ung tien*, 196, 5 a, is mistaken when it attributes to the *Sui shu* the same form Jui-jui which occurs in the *Sung shu* and the *Nan-Ch'i shu*; this error has passed into the *T'ai-p'ing huan-yü chi*, 93, 9 b). Chêng Ch'iao (1104[?]-1162) cites in his *T'ung chih* (28, 8 a), as occurring in ch. 113 of the *Wei shu*, a sentence saying that « when the Juan-juan entered China, they took the surname 茹 Ju »; this would imply that, before the shortlived Chou dynasty reigned in the south, the name Ju, as a shortened form of Ju-ju, was already given to the Juan-juan among the northern Wei. But there is in fact no such passage in the *Wei shu*, which only says (113, 21 a) that the Wei clan-name 普陋茹 P'u-lou-ju was changed to Ju; it must have been a slip of Chêng Ch'iao, who also quoted this second passage, to attribute the same origin to the first, which was probably drawn from the *Yüan-ho hsing-tsuán* (written c. A. D. 800). Apart from men with the surname Ju who were of Wei origin, this surname has also been borne by families of pure Chinese descent, and no man of a Ju clan has hitherto been shown with certainty to have belonged to the Juan-juan stock. But the case is different with the double surname Ju-ju; it certainly represents the form Ju-ju of the name of the Juan-juan which is given in the *Chou shu* and the *Sui shu*. According to the *T'ung chih*, 29, 4 a, Ju-ju existed already under the northern Wei as a surname borne by Juan-juan people who had sought refuge in China after the Juan-juan empire had been destroyed by the T'u-chüeh (Turks), i. e. not earlier than 555, or two years before the northern Chou ascended the throne in 557. Chêng Ch'iao mentions a member of the Ju-ju clan who, he says, held office under the Wei, but that man seems to have been known only from a document concerning one of his grandsons who was in office in the seventh century, and naturally this grandson spoke of his grandfather under the name by which the latter had become known in later years. The only other member of the Ju-ju clan I can trace lived under the Sui (*Sui shu*, 45, 8 a). On the Ju and Ju-ju clan-names, cf. *T'u-shu chi-ch'êng*, Shih-tsu-tien, 63 and 565; CHANG Shu's *Hsing-shih hsün yüan*, 6, 14; CH'ÊN I's *Wei shu kuan-shih-chih shu-chêng*, ff. 25-26 of my ms. copy. In the works of the northern dynasties (*Wei shu*, 103, 1 a; *Pei shih*, 98), we read that these people called themselves 柔然 Jou-jan (\*Ñǐǰu-ńǐǰän; WATTER's transcription « Niu-yen », on *Yuan-Chwang's Travels*, I, 79, is without foundation; cf. BEFEO, v, 442), but that Shih-tsu (or T'ai-wu-ti, i. e. the Wei Emperor T'o-pa Tao, 424-452), finding that « they were ignorant and looked like 'worms' » (虫 *ch'ung*, which means « worms », but also « animals » in general), changed their name to 蠕蠕 Juan-juan (\*Ñǐwän-ńǐwän; in the T'ang period another pronunciation of 蠕 was \*ńǐǰuën,

cf. *Tōkyō Tripit.* of Meiji, 爲, IX, 78 b, 81 b; *juan* means «to wriggle like worms»). The *Nan shih*, which is a general history of the southern dynasties, always speaks of the «Juan-juan», that is to say gives the northern form of the name (4, 7 a; 6, 12 a; 7, 5 a; 79, 6 b-8 a), though, in the corresponding passages, the original dynastic histories spoke of the Jui-jui and Ju-ju respectively. But this must be due to the fact that Li Yen-shou († c. 676-678), the author of the *Nan shih*, was also the author of the *Pei shih* devoted to the northern dynasties, in which he naturally employed the «northern» form Juan-juan, and he wanted to use for the same people the same form in both works (Hu San-hsing's remark, reproduced in the *K'ang-hsi tzü-tien*, s. v. *jui*, that the *Nan shih* employs Jui-jui is erroneous. Juan-juan and the allied forms have been discussed by FUJITA in *Tōyō gakuho*, XIII [1923], 64, 68, but I do not think that any point of his argument can be retained).

FRANKE maintains (III, 283, 291) that Jou-jan is a purely Chinese term, meaning «weak», «flexible», and that the depreciatory designation Juan-juan, created by T'o-pa Tao, was chosen in phonetic agreement with Jou-jan; moreover he writes this new name as «蠕蠕 J(o)u-juan», and in the course of his work always speaks of the «Jou-juan». But this is quite arbitrary, since no ancient source makes a distinction between the two components of Juan-juan, always written 蠕蠕, and since in no system, even if we should admit of such a distinction, is either of the two characters ever read *jou*. Finally, FRANKE, like DE GROOT, lays stress on the graphic composition of the characters, and on that account asserts that the original pronunciation of 蠕 can have only been *ju*, which he finds indicated as a secondary reading in the *Chi yün* of the eleventh century. In the same way GILES, in the second edition of his *Chinese-English Dictionary*, gives Ju-ju as a possible pronunciation instead of Juan-juan (cf. also SCHLEGEL's similar opinion in MARQUART's *Erānšahr*, 54). But the same *Chi yün*, in the case of the nation which we call Juan-juan, says that 蠕 is the same as 蠕, and for 蠕, the phonetic element of which moreover is *juan*, the dictionaries give no other pronunciation than *juan* (FRANKE's statement to the contrary, III, 284, is due to the fact that he inadvertently misread 竟 *yen*, \**iwän*, as 兌 *tui*, \**d'uäi*). The interchange of phonetics between 蠕 *juan* and 蠕 *juan* is paralleled by the indifferent use of 擗 and 擗, 擗 and 擗, etc. Accordingly, Ju-ju as well as J(o)u-juan is erroneous, and we must retain the pronunciation Juan-juan as it is given in the dictionaries since the T'ang period, and confirmed by phonetic glosses ranging from Tu Yu's *T'ung tien* of the end of the eighth century (196, 5 a) to the *Liao shih* of the fourteenth.

FRANKE believes on the one hand that Jou-jan is a purely Chinese name, and Juan-juan a derisive deformation of Jou-jan, while on the other he admits (III, 290-291) a probable semantic connection of these names with that of *Κερμηχίωτες*, or «Worm-Chionites», as suggested by MARQUART and accepted by CHAVANNES (*Doc. sur les Tou-kiue*, 232). He is thus led to believe that the meaning of «flexible», «wriggling like worms», attached to a native name which would be no other than the very name «Avar». I agree only with part of this argument. «Jou-jan» makes sense in Chinese, as the later Juan-juan does, but so does also 芮芮 Jui-jui which, with another ancient pronunciation (\**ńzi'äi-ńzi'äi*, not \**ńzi'ät-ńzi'ät*), is an old Chinese term meaning «flourishing vegetation». The 茹 *ju* of Ju-ju has, among other senses, the meaning «flexible», and, in the commentary to the well-known *Li sao* of Ch'ü Yüan, it is explained as 柔擗 *jou-juan*

(where 𢀓 = 𢀓 *juan*), «flexible», so that the southern form Ju-ju may in a way be said to mean the same thing as the northern form Jou-jan. Even *juan-juan*, in the equivalent form 𢀓𢀓, is an old term which occurs in a well-known writing of Ma Jung, dated A.D. 115, which is preserved in his biography (*Hou-Han shu*, 90 A, 3 b); the commentary, citing the *Shuo wên*, explains *juan* as meaning 動 *tung*, «to move». So I readily concede that some semantic background lies behind the various forms, and that they are not, as is often the case, mere transcriptions made with characters chosen irrespective of their meaning. But, at the same time, while Jou-jan may make sense, it has then in principle an adjectival or, rather, an adverbial value which would make it awkward as a purely Chinese name, even if it were a translation. Moreover, when a text says that a foreign tribe gives to itself this or that name, it would be almost unparalleled to find that the name was anything but a transcription of the native form. Finally, there is too much of a phonetic connection between Jou-jan and Juan-juan on one side and Jui-jui and Ju-ju on the other, to allow of a solution according to which these two groups of names, although referring to the same nation, should either be separated, or all of them considered as translations, of which the phonetic analogy would be of a purely fortuitous character. My conclusion is that all four forms are transcriptions, though partly influenced by some semantic consideration. This consideration may be due either to the meaning attached to the unknown original of the various transcriptions, or to a current tradition about the character of the nation. There is no chance that the origin should be sought in the meaning of the name «Avar» itself, as supposed by FRANKE, and still less that «Avar», as maintained by BLOCHET in one of his later articles (*Le pays de Tchata*, in *Rendiconti d. R. Acc. Naz. dei Lincei*, 1925, 334), should be the very name which Jou-jan and Juan-juan were intended to transcribe. Though the *Κερυχίωτες*, or «Worm-Chionites», are represented in Western sources as Turks (while the Avars must have been Mongols), the similarity between the meaning of their name and that of Juan-juan can hardly be fortuitous. To these two names I may add a third one hitherto unnoticed. In the Chinese version of the *Tathāgataguhya* made by Chu Fa-hu in 288, a curious list of Central Asiatic countries mentions, between 劍浮 Chien-fou (which may be Kamboja, but may just as well render the ancient name of Aqsu usually transcribed 姑墨 Ku-mo in Chinese), and Kučā and Khotan, a country called 擾動 Jao-tung. S. LÉVI, when translating this list in *BEFEO*, v, 289-290, rendered Jao-tung with «the Agitated», adding that the name was otherwise unknown to him. Though *jao-tung* is not listed in the *P'ei-wên yün-fu*, the term is well attested in Chinese. For instance, it occurs in a *sūtra* which forms ch. 112 of the same collection to which the *Tathāgataguhya* belongs and the translation of which is anonymous, but was made prior to A.D. 400 (*Tōkyō Tripit.* of Meiji, 地 VI, 57 b); in 817 Hui-lin commented on the term in ch. 15 of his *I-ch'ieh ching yin-i* (*ibid.* 爲 VIII, 107 a; cf. also ch. 46, *ibid.* IX, 78 b). Hui-lin explains *jao* as 亂 *luan*, «to disturb», «to trouble»; in the *sūtra*, *jao-tung* refers to an illness which is «shaken» by the doctor's prescription. But Hui-lin also insists on the graphic formation of the character, the original phonetic of which was not 擾 *yu*, still retained in KARLGREN's *Analytic Dictionary*, p. 101, but 擾 *nao* (now written 擾 *nao*), a sort of bustling monkey; this explanation, which is based on the *Shuo wên*, is also given in the *K'ang-hsi tzü-tien*. Now, a frequent expression in early Chinese Buddhist texts, which moreover goes back to the *Huai-nan-tzū* (I, 1 b), is 𢀓動 *juan-tung*, which the commentary of Yen Shih-ku on *Ch'ien-Han*

*shu*, 94 A, 7 a, explains as 蠕蠕動貌 *juan-juan tung mao*, «*juan-juan* (the same as that of Ma Jung, and fundamentally the same as Juan-juan; *juan-juan* itself also occurs in the *Huai-nan-tzū*) is descriptive of movement». The two forms 蠕 *juan* and 蠕 *juan* had at an early date become so interchangeable that we find *juan-tung* written 蠕動 in the independent text of Hsüan-ying's *I-ch'ieh ching yin-i* (Tōkyō Tripit. of Meiji, 爲 VII, 38 b), but 蠕動 in the section where Hui-lin incorporates this part of Hsüan-ying's text in his own larger work (*ibid.* IX, 81 b); the *P'ei-wên yün-fu* has adopted 蠕動 *juan-tung* in all its quotations, including that from the *Huai-nan-tzū*. In Buddhist texts *juan-tung* is used in reference either to «animals which move by crawling» or, sometimes, to all animated beings moving on earth (in contradistinction to birds; cf. DEMIÉVILLE, in *BEFEO*, XXIV, 83-84; it occurs with the sense of reptile in CHAVANNES, *500 Contes*, III, 162). I believe that the idea underlying the unknown original name of which the «Jao-tung» of Chu Fa-hu is the translation is also that of a nation «crawling» or «wriggling», a forerunner of Juan-juan. It is not likely that it refers to the Avars themselves, since the Jao-tung appear in the list among countries much to the south-west of Mongolia, and since, moreover, the name of the Jou-jan or Juan-juan makes its first appearance in Chinese texts a whole century after Chu Fa-hu's translation. This last objection applies also to another name in the list, in which S. LÉVI thought we might have a different transcription of the alternative name Ta-t'an or T'an-t'an indicated for the Avars in *Sung shu*, 95, 16 b. But tales may have circulated in Central Asia concerning a «crawling» or «wriggling» nation; we find them first attached to the «Jao-tung» of Chu Fa-hu's translation, while they became affixed later to the Juan-juan or Avars in the Far East, and to the *Κερμυχῶνες* or «Worm-Chionites» in the West.

We may now return to BANG's theory that the name of the Juan-juan, or of the «Dzut-dzut» as he calls them, is identical with that of the Čürčüt, a name now applied to the Mongols by some Turkish tribes, but which was originally the same as that of the Nü-chên or, in early Mongolian, Jürčät. I must first of all remark that, in view of all the ancient Uighur and Mongolian forms of the name, the second -ü- of Čürčüt can only be of secondary appearance, and due to the influence of the -ü- in the first syllable; we must start from Jürčät, which is already less similar to «Dzut-dzut». As to «Dzut-dzut», it is DE GROOT's interpretation of the ancient pronunciation of 芮芮 *Jui-jui*, which we render as \*Ńẓ̌i<sup>w</sup>ät-ńẓ̌i<sup>w</sup>ät. But even \*Ńẓ̌i<sup>w</sup>ät-ńẓ̌i<sup>w</sup>ät, with its two final -t, is not unassailable. It is the pronunciation indicated in the *Chi yün*, written in the eleventh century, for *Jui-jui* as the name of the Avars. But it will be noticed that none of the other forms of the name, either «southern» as Ju-ju or «northern» as Jou-jan and Juan-juan, ever had a final -t. Since the purely Chinese term *jui-jui*, meaning «flourishing vegetation», was \*ńẓ̌i<sup>w</sup>äi-ńẓ̌i<sup>w</sup>äi, and since it is extremely probable that the «southern» Chinese had this term in mind when they transcribed the native name of the Avars as «Jui-jui», I am much tempted to doubt the accuracy of the information given by the *Chi yün* and to say that, even in the case of the Avars, *Jui-jui* is an ancient \*Ńẓ̌i<sup>w</sup>äi-ńẓ̌i<sup>w</sup>äi, without a final -t. In such an event the phonetic analogy with Jürčät > Čürčüt would be still less satisfactory. I am the more reluctant to propose a reconstruction of the native name under which the Chinese knew the Avars because the various transcriptions seem to have been made with semantic adaptations which could easily lead to distortions. But there does not seem to have ever been an -r- at the end of the first syllable, and the nearest

approach to the original form may be something like \*Žužan or \*Žužun, which is hardly reconcilable with Jürčät. Above all, the Avars or Juan-juan must have been Mongols, whereas the Nü-chên or Jürčät were Tungus; neither the geographical location nor the ethnical character agrees; in my opinion BANG's theory cannot be retained.

When I say that there must have been no *-r-* at the end of the first syllable in the name of the Jou-jan or Juan-juan because none of the Chinese transcriptions shows it, it might be objected that there was almost certainly an *-r-* in \*Jürčên although it does not appear in Nü-chên. But the two cases are not the same. The Chinese pronunciation of the Six Dynasties still possessed occlusive finals, and *-t > -ḍ* could so well represent a final *-r* of foreign words or names that it was heard as *-r* in the T'ang period by the Turks and the Tibetans and has given *-r > -l* in Sino-Corean. But the occlusive finals had already practically disappeared from the pronunciation of northern Chinese when the transcription Nü-chên was adopted; from that time, a final *-r* of foreign words had to be represented by an additional character, either *li* or *érh*; and that additional character was often omitted. This would more or less account for Nü-chên = \*Jürčên, if it were not for the initial, which I am still at a loss to explain.

For the sake of completeness, I may add a last hypothesis, which would trace the name of the \*Jürčên farther back than the tenth century. Under the name of Shih-wei the Chinese of the T'ang period have designated many tribes of eastern Mongolia and Manchuria, most of them Tungus, but some also Mongol. Among these tribes are those of the 大如者 Ta Ju-chê, «Great Ju-chê», and 小如者 Hsiao Ju-chê, «Little Ju-chê»; these Ju-chê sent an embassy to the Court in 748 (cf. *Ts'ê-fu yüan-kuei*, 971, 16 b). Just as we find in this list of the Shih-wei tribes the earliest mention of the Mêng-wu, *i. e.* the Mongols (see «Mongul»), it would be very tempting to recognize in the Ju-chê the ancestors, at least in as far as the name is concerned, of the Nü-chên or \*Jürčên. The geographical position would not contradict such a hypothesis. Yet there is a serious difficulty. Ju-chê is an ancient \*Nž̄wo-t'šja, that is to say it never had at the end of the first character the occlusive final which would have rendered the *-r-* of Jürčên. For this reason, and without entirely discarding the possibility of a connection between the two names, I mention the Ju-chê only as an incentive to further research.

From the name of the \*Jürčên we may now pass to the historical conditions which prevailed when Polo heard of «Ciorcia». According to him (I, 161) the original seat of the Tartars, *i. e.* Chinghiz-khan's Mongols, was «between (I believe this is the correct interpretation of the passage, from a French 'entre', rather than the 'about' given in the translation) Ciorcia and Bargu», which evidently means between Manchuria and the region south-east of Lake Baikal (see «Bargu»), and this is quite correct. Elsewhere (I, 200) we are told that Nayan was lord of four great provinces, «Ciorcia», «Cauli», «Barscol», and «Sichintingiu». In a third passage (I, 363), which we shall leave out of consideration for the present, Polo narrates how Qubilai sent one of his barons into exile «to the desert island named Ciorcia», where he was murdered. The identification of «Barscol» and particularly of «Sichintingiu» (*q. v.*) is difficult, but «Cauli» is of course Corea, and «Ciorcia», the Jürčä or Čürčä of the Persians, certainly refers to the whole or part of Manchuria. When the \*Jürčên had conquered northern China, their empire soon ceased to be called by their name, and came to be known as the kingdom of the «Golden King» (see «Roi Dor»), or they were

referred to in Mongolian as Kitat, because they had replaced the true Kitat (a Mongolian plural of Kitan < Qītañ), *i. e.* the Ch'i-tan or Liao of Chinese texts; and in their turn the true Kitat, those who had remained in northern China as well as those who had migrated to Russian Turkestan, were henceforth known as Qara-Kitat or Qara-Ĥītai, the «Black Kitan» (see «Catai»). As to the name Jürčä, or in its Mongolian form Jürčät, it survived only as the designation of the original home of the nation in eastern Manchuria. In the *Secret History* the Jürčät of §§ 247, 248, 255, and 274 are only the Manchurian Jürčät. Such is the case also when Rašidu-'d-Dīn (*Bl*, II, 498) speaks of Japan as of a great island in the ocean, «in the vicinity of the coasts of the provinces of Jürčä and Kōli (Corea)». So Polo's use of «Ciorcia» is in perfect agreement with the custom of the time. His «Ciorcia» is in principle the country beginning east of the Liao River, and extending south of the Ch'ang-pai-shan to the Japan Sea, but it may also have extended to the north-east as far as the lower Amur, and even corresponded to the whole of Manchuria. About the time of the capture of Peking by the Mongols, the Chin officer 滿鮮萬奴 P'u-hsien Wan-nu had started east of the Liao River an independent kingdom, which he first called 大真 Ta-Chên («Great [Nü]-chên»), and afterwards 東夏 Tung-Hsia («Eastern Hsia») and 東真 Tung-chên («Eastern [Nü]-chên»); he was reduced by the Mongols in 1233 (cf. GIBERT, *Dictionnaire*, 754-755). Prior to the downfall of P'u-hsien Wan-nu, Chinghiz-khan had allotted the whole of his territory as part of the appanage of his youngest brother Tämügä-otčigin, and Nayan (see «Naian») had inherited it as great-great-grandson of the latter (cf. T'u Chi, 75, 1 a). So Polo's information is once more correct.

While all commentators have agreed that, in the first two cases, Polo's «Ciorcia» represents the same name as \*Jürčen and Nü-chên and is in principle a designation of south-eastern Manchuria, more hesitation has been felt about «the desert island named Ciorcia». YULE (*Y*, II, 262), citing RAMUSIO, speaks of «a certain island named Zorza (Chorcha?)», but does not seem expressly to identify the two names, and CORDIER's *Index*, while referring under «Zorza» to «Chorcha», and under «Chorcha» to «Churchin», leaves out the reference to II, 262, under this last name. BENEDETTO, who correctly identifies «Ciorcia» with the \*Jürčen (*B*<sup>1</sup>, 441), says under «Zorza» (*B*<sup>1</sup>, 449): «Unidentified . . . I doubt that it can be the same as Ciorcia, since it is spoken of as being an island». ROSS alone (*RR*, 417, 439), although separating under two different entries the names «Chorcha, Manchu country» and «Zorza, island Chorcha or Juchin», shows by his very explanation «Juchin», *i. e.* Ju-chen, that he held the two to be identical.

Leaving for the moment the name itself, I shall try to determine which was the place referred to by Polo. During the Mongol dynasty people were banished to various parts of the Empire. For instance (*YS*, 29, 3 a), on January 22, 1324, princes who were guilty of conspiracy were respectively banished to Yün-nan, Hai-nan, 奴兒干 Nu-êrh-kan, and «the Sea Islands» (海島 *hai-tao*). In Polo's text, «Yün-nan» is excluded, being neither in the sea nor near the sea. «Hai-nan» too is out of the question, since it designates the island of Hai-nan, which was not «desert» and the name of which was well known to Polo (see «Cheynam»). The anonymous «sea islands» might be taken into consideration if it were not that Polo would probably not have called them by a distinct name when the annals themselves do not; moreover these «sea islands» must have been off the coasts of China proper (perhaps the Chu-san islands, where Nayan's partisans had been



deported; see « Naian »), whereas the name used by Polo does not seem to be Chinese. The main centre of deportation was in fact at Nu-êrh-kan, and I have little doubt that Nu-êrh-kan actually is the place meant by Polo.

The location of Nu-êrh-kan is well established. It lay east of the Amur, near the mouth of the river (cf. GIBERT, *Dictionnaire*, 676, where, however, the forms of the name given for the Yüan period are in Ch'ien-lung's « reformed » spelling, and perfectly valueless). Most of the ancient texts concerning Nu-êrh-kan (with the exception of the important one in the *Cho-kêng lu*) are mentioned in the *Beiträge zur historischen Geographie der Mandschurei* published under the supervision of SHIRATORI (cf. the index in II, 544).

It was in 1272 that a Mongol official, who had been sent to subdue the island of Sakhalin, reached « the land (*ti*) of 努兒哥 Nu-êrh-ko » (*Yüan wên lei*, 41, 32 a); Nu-êrh-ko is certainly the same as Nu-êrh-kan, with which I think we must also identify the 訥里干 No-li-kan occurring in a text of 1308 (*ibid.* 41, 32 b). On September 23, 1320, an official named I-lien-chên (Mong. Iränjin < Tib. Rin-chen, « Great Jewel ») was bamboed and banished to the land (*ti*) of Nu-êrh-kan » (YS, 27, 4 b-5 a). The YS mentions (59, 3 b) the gerfalcons which fly from beyond the sea and are caught and offered as tribute by « the natives of Nu-êrh-kan ». In the 山居新話 *Shan-chü hsin-hua* (ed. *Chih-pu-tsu-chai ts'ung-shu*, 11 a-b), dated 1360, the author, Yang Yü, speaks of a man of Hang-chou who was a friend of a certain native of Hsin-ch'êng; and when the latter, in 1333-1334, was banished to Nu-êrh-kan, the Hang-chou man showed so much devotion that he accompanied his friend as far as 肇州 Chao-chou (in Manchuria; see « Barscol »). T'ao Tsung-i's *Cho-kêng lu*, written in 1366, contains (8, 5 a) the following paragraph on « dog-relays » (狗站 *kou-chan*, Mong. \**noyai-jam*; on these « dog-relays » and the dog-sledges, cf. TP, 1904, 398; Br, I, 129; Y, II, 481-483) : « North of Kao-li (Corea) is [the region] which is called 別十八 Pieh-shih-pa (< Turk. Beš-baliq, 'Five Cities'), which in Chinese means 'Five contiguous cities' (連五城 *lien-wu ch'êng*; this can only be another designation of the 五國城 *Wu-kuo-ch'êng* of the Chin, the region of Manchuria where the Sung Emperors Hui-tsung and Ch'in-tsung died in exile; cf. GIBERT, *Dictionnaire*, 696-698; SHIRATORI, *Beiträge*, II, 140-147). The criminals who are banished to Nu-êrh-kan are obliged to pass through this region. It is extremely cold, and even the sea freezes. The ice sets in the eighth month and does not thaw before the fifth or sixth month of the next year. People walk on it as if treading on even ground. Every year the 'moving Grand Secretariat' (*hsing-shêng*) of 征東 Chêng-tung ('Subduing the East'; the 'moving Grand Secretariat' of Chêng-tung was established in 1283 in Corea, then abolished, and re-established in 1299 and 1321; cf. YS, 63, 13 a; 91, 2 a; 208, 7 a-b; also YS, 28, 5 a, s. a. 1323; but already in 1273, the official who reported on the advance to Nu-êrh-kan in 1272, was a Chêng-tung *chao-t'ao-shih*, this being an office of which nothing else is known except its bare mention in YS, 63, 13 a; cf. SHIRATORI, *Beiträge*, II, 334) details officials to Nu-êrh-kan to distribute vic-tuals to the prisoners who are scattered there. These officials use cars of the official relays (*chan-ch'ê*), every one of which is drawn by four dogs. The dogs are well aware of the feelings of men. The postal relays (*chan*) have regulations for the rations of the dogs; if these are reduced, the dogs will bite their masters and not leave them until they are dead.» I do not know when these « dog-relays » were first started; but on July 22, 1295, ten silver ingots in paper-money were

given to « every family of the twelve (? read 'fifteen') relays of the dog-post (狗遞 *kou-ti*; YS, 18, 7 a) ». On August 19, 1320, « the myriarchy (*wan-hu-fu*) of the Nü-chih (= Nü-chên) and the *t'o-t'o-ho-sun* ('director') of the dog-relays were abolished » (YS, 27, 4 a). On November 10, 1330, « since in the circuit (*lu*) of the Shui Ta-ta ('Water Tatar') of the *hsing-shêng* of Liao-yang rain had been falling heavily since the preceding summer, the two rivers Hei-lung (Amur) and Sung-wa (Sungari) had been flooded and the people had had no fish for their food, and it had come to the point that many of the dogs of the fifteen dog-relays of Mo-lu-sun [etc.] had died of starvation; grain [sufficient] for two months was distributed to [the population] and paper-money was given to replace by purchase the dogs which had died » (YS, 34, 9 b; cf. SHIRATORI, *Beiträge*, II, 347, 396). According to the YS, 101, 1 a, 4 b, the fifteen dog-relays originally comprised 300 families (*hu*) with 3,000 dogs; but the numbers had gradually dwindled to 289 families and 218 dogs. The above texts give a clear view of the difficulties which beset the deportation of criminals to Nu-êrh-kan; and this explains why new regulations had been adopted in the spring of 1320: only the criminals guilty of a grave offence were henceforth banished to Nu-êrh-kan (but we know of two cases of banishment to Nu-êrh-kan in 1321; cf. YS, 27, 7 a and b); those who were sentenced to banishment for a slight offence were to remain at the agricultural settlement (*t'un-t'ien*) of Chao-chou (*Yüan tien chang*, *hsin-chi*, sect. *hsing-pu*, 1 a-b; cf. SHIRATORI, *Beiträge*, II, 335). After the fall of the Yüan dynasty, Nu-êrh-kan was again occupied by the Ming dynasty. As early as in 1403 Yung-lo had sent envoys to Nu-êrh-kan (*Shu-yü chou-tzũ lu*, 24, 1 a), and a Commissariat (都司 *tu-ssũ*) was established there in 1409; its centre was on the Amur, one stage to the « east » of the station Man-ching (near the town of Tyr) where the inscriptions of 1413 and 1433, known as the « Tyr inscriptions » or « inscriptions of the Yung-ning-ssũ », were discovered (on these inscriptions, cf. POPOV, in *ZVOIRAO*, XVI [1906], 012-020, 077, and pl. II; SHIRATORI, *Beiträge*, II, 563; from personal examination of the original monuments, now in Vladivostok, I can say that POPOV's decipherment of the Chinese text can be corrected and completed in many cases; cf. also IKEUCHI Hiroshi in *Man-Sen jiri*. . . *hōkō*, IV, 321; the first of these inscriptions is trilingual, in Chinese, Mongolian, and Jučen; the Mongolian text has not been published, but was translated by POZDNEEV in *Lekcii po istorii Mongol'skoï literatury*, III, 70-75; as far as I know the Jučen text is still unpublished and untranslated).

I have little doubt that Nu-êrh-kan, the « Botany Bay » of the Mongol dynasty, located in a dreadful spot at the extreme north-eastern end of the Empire, is the « desert island » spoken of by Polo. Without being an island, it is in fact a peninsula, between the lower Amur and the sea, and was quite probably called an « island » in Persian-speaking circles (for analogous extensions of the notion of « island » in Polo, see « Çanghibar » and « Mogedaxo »).

But if the « desert island » be the region of Nu-êrh-kan, we have to account for the name which Polo gives to it. The ancient original form which Nu-êrh-kan transcribes is not known with certainty. I thought at one time that the name of Nurhači > Nurhaci († 1626), the founder of the Manchu dynasty which in 1644 became that of the « Great Ch'ing », might be an ethnical form derived from Nu-êrh-kan. But for Nu-êrh-kan the chances are in favour of an original form with palatal vowels. Although POZDNEEV, in his translation of the « Tyr » inscription of 1613, transcribes the name once as « Nurhan », at other times he gives « Nurgan » in Russian, and even, in one passage

«Nūrgän» in Mongolian writing. It may be that this Mongolian form is merely retranscribed from the Chinese, but both the usual «Nu-êrh-kan», and the earlier «Nu-êrh-ko» of the report of 1273 point to an original \*Nūrgä or \*Nūrgän. With such an original, Polo's form cannot be reconciled. It would be futile to imagine that on the one hand, there should have been in the name an alternation between *j*- and *n*- as in \*Jūrčēn and Nū-chēn, and that, on the other, Polo's median -ç-, though attested as occurring in the prototype of Z by the readings in Fra Mauro, RAMUSIO, and Z, may be an early corruption of -c-, so as to reconstruct for Polo an original form representing \*Jörgä. The simplest solution, I think, would be to admit that Polo spoke of the island as «Ciorcia», *i. e.* Jūrčä, because «Ciorcia» was the north-easternmost country of which the name was familiar to him, and also because he probably believed, and not without some foundation, that the «desert island», *i. e.* Nu-êrh-kan, was still within the limits or the sphere of influence of «Ciorcia». Moreover, the way in which Polo speaks of the original seat of the Mongols as lying between «Bargu» and «Ciorcia» implies that his notion of «Ciorcia» was a very extensive one, practically identical with ours of «Manchuria», and thus would naturally comprise Nu-êrh-kan.

### 162. CIUGIU (c. 138).

*cignj* TA<sup>1</sup>

*cinçu* Z

*cinghi* TA<sup>2</sup>

*cingiu* Ft

*cingni* TA<sup>1</sup>, TA<sup>3</sup>

*cingui* DT

*cinguy* FAt

*ciugiu* F, L

*congaj, conganj* (?), *congnj* VB

(? *militen* two days from

*pequi* VB)

*cuigiu, cuigui* F

*siguy* FA

*singuy* FB

*vigui* V

The readings of the mss. point to «Ciugiu»; but, from Polo's itinerary, the only possible identification is 宿遷 Su-ch'ien (or Hsü-ch'ien), on the northern bank of what became after 1324 the course of the Huang-ho, between the ancient P'ei-chou and Huai-an; the Commissioners of 1276 passed through Su-ch'ien (*TP*, 1915, 397, 415). On this point I agree with PAUTHIER, YULE and CHARIGNON. It is more difficult to account for the name. In Polo's time, what was from 1324 to 1853 the lower Yellow River, was still the 泗州 Ssü-ho, and there existed until quite recently a Ssü-chou, the seat of which is north of the Huai and far to the south-west of Su-ch'ien. But we cannot accept outright PAUTHIER's statement that Su-ch'ien was once called Ssü-chou, nor is there any good ms. that gives the form «Sigiū» adopted by YULE, PAUTHIER and CHARIGNON as the basis for their transcriptions.

The place was a district (*hsien*) already in Han times, and was renamed 宿豫 Su-yü towards A. D. 400. In the beginning of the T'ang dynasty and until 735, Su-yü was the seat of the department of Ssü-chou. In 762, the name of Su-yü was changed to Su-ch'ien on account

of a taboo. In 806-820, Su-ch'ien became a dependency of Hsü-chou and of P'ei-chou from 976 down to our days, almost without interruption (*Ta-Ch'ing i-t'ung chih*, 69, 4a).

In spite of PAUTHIER, there is little probability that Su-ch'ien was popularly called Ssü-chou in Polo's days, as a survival, from T'ang times, of the century during which it was the seat of the department of Ssü-chou, and notwithstanding the fact that the new seat of Ssü-chou had already had, in the 13th cent., a long tradition of the name. But it is not much more likely that the name of Su-yü, tabooed in 762, should have still been in current use, as CHARIGNON thinks, at the end of the 13th cent. Chinese geographical names have never had the fixity and duration we are accustomed to in the West. If CHARIGNON was right, we should have to suppose that Polo uses *-g-* here before *-i* not as he usually does in value of *ǰ-*, but to mark the hiatus between *su* and *yü*, as he does perhaps with the first *-g-* of «Coigangiu» (though not in «Cauyu»); before *-i*, however, it ought to be *-gh-* (see «Paughin»). The whole of this argument is not very satisfactory.

Provisionally, I incline to the following solution. The first part of the name would represent the *su* of Su-ch'ien, but read *hsü* (cf. «Cuigiu» < \*Siugiu = Hsü-chou in Ssü-ch'uan). The character has both pronunciations, and although the *K'ang-hsi tzü-tien* and most of our Western authors (PLAYFAIR, GILES, etc.) write Su-ch'ien, a pronunciation Hsü-ch'ien («Siu-ts'ien», in French spelling) may have been and is perhaps still in use locally. CHARIGNON, who was there himself, writes «Siu-ts'ien», unfortunately without any comment on the possible dual pronunciation *su* and *hsü*. As to the second part of the name, the *-giu* of the best mss. generally represents *chou* in Polo. But it may be that the name was altered by copyists under the influence of the frequent *-giu* = *chou*. Suppose Polo wished to refer to Hsü-ch'ien; he, or rather Rustichello, would put it down as «Siucin» or «Ciucin»; it is an easy change for a copyist of the archetype (all our mss. have common faults) to turn this into «Ciugiu».

### 163. CIUGUI (c. 177)

*caiguy* FA  
*chongiughati* TA<sup>3</sup>  
*cinguos* S  
*ciugui* F, L(?)

*ciuguy* FB  
*congu ighati* (?) TA<sup>1</sup>  
*cuigui* F, Z, L  
*cyngui* VL

*tingui* R  
*zengui* V  
*zinguj* VA

This represents a pronunciation *çugi* (for *ǰugi*?; but we would expect «gioghi») of Skr. *yogīn* > Hind. *ṣogī*, «ascetic». The form 濁肌 *cho-ki*, noted in India by Ma Huan some time before 1433, represents also *ṣogī* (cf. *TP*, 1915, 450). For other transcriptions, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 461-462; DALGADO, *Glossário Luso-Asiático*, I, 490-494; Mircea ELIADE, *Yoga*, Paris, 1936, 254-265. LOKOTSCH, *Etym. Wörterbuch*, No. 2169, is very brief on the point.

## 164. COBINAN

*cabanant* FA, FB  
*cabanat* Fr, t  
*chobian* V  
*chobinan* VA

*cobian* F  
*cobina* Z  
*cobinam* P, VB, R  
*cobinan* F, Z, VA, L

*conbinam* VB  
*giobiam* TA<sup>3</sup>  
*gobiam* TA<sup>1</sup>, LT

Identified with كوبنان in Kerman, variously transcribed «Kuh Banán», «Kooh Benan», «Kuh banān», «Kúbenán», «Kubunān». It is only certain that the first vowel is now *u*, but Polo may still have heard it as *o*. The name, when read Kuh-bānān, means «Mountain of the Wild Pistachioes». On the place, and the route followed by Polo to reach it, cf. *Y*, I, 125-126; III; 25-30; *LS*, 309.

The name has passed as «Chobinam» and «Cobina[n]» into Fra Mauro (*HALLBERG*, 87).

## 165. COCACIN

*caciése* TA<sup>1</sup>  
*chaciesi* TA<sup>3</sup>  
*chogazin* V  
*cocacin* F  
*caganyn* P

*cogara* FA  
*cogatin* F, L; R  
*cogatra* FA, FB  
*cogitnyn* P<sup>5</sup>

*cogratin* VB  
*cozacin* VL  
*gogatim* VA  
 (cotroco LT)

Rašidu-'d-Dīn (*QUATREMÈRE*, *Hist. des Mongols*, 94) writes كوكاجي Kōkāči. The name is not common. YULE (*Y*, I, 38), relying on DE GUIGNES, says it was also the name of the Qonyrat wife of Chên-chin (see «Cinchim»). Chên-chin's wife, a Qonyrat, the mother of the Great Khan Tāmūr Ūljāitū, is called Baīram (?)-ākāči in *YS*, 106, 3 *b* (with a clerical error *chien* for *lan*), and Chên-chin had besides a concubine named \*Aljinmiš (*ibid.*). But it is true that *YS*, 116, 1 *a*, states that Baīram-ākāči was also called K'o-k'o-chên, Kōkōjin, and this is the only name by which Rašīd knows her, writing as he does كوكجين Kōkājin (*Bl*, II, 535, 540, 582). The name is Mongolian, derived from *kōkā* (> *kōkō*), «blue», plus suffix *či*, which often, and in the present case even more correctly, takes the form *-čin*, weakened in *-ji*, *-jin*.

The great Buluyan (see «Bolgana»), Abaya's and Aryun's wife, had died in 1286, and expressed the wish to be replaced by some woman belonging to her own tribe. This is implicitly confirmed by Rašīd, whose text, more precise than in *QUATREMÈRE*'s translation (*Hist. des Mongols*, 94), states that Kōkāči was brought from China on account of her kinship with the «great *hatun*» Buluyan. She must thus have been a Bayaut, like Buluyan herself. The Bayaut were then in high favour in China. A Bayaut, Bayaučin, was Qubilai's fourth wife and

had given him a son (cf. *YS*, 106, 3 a; *Ber*, I, 178; *Bl*, II, 370); another Bayaut, Buluyan, was the wife of Qubilai's grandson and future successor Tämür-Öljäitü (*YS*, 106, 2 b; 114, 2 b).

HAMMER, in his account of Kōkāčīn's reception by Ghazan, says that Ghazan himself had sent an envoy to China to ask for her (*Ha*<sup>1</sup>, II, 20). This is impossible; Ghazan's father Aryun did not die until 1291; there can be no doubt that the embassy had left Persia long before, and Polo's very detailed account about the sending of the embassy by Aryun is to be entirely trusted. A passage in R, omitted by BENEDETTO (*RR* and *B*<sup>1</sup>), but certainly genuine, says that, by the date of Polo's return from the Southern Seas, the envoys had already been away three years. The interval between Buluyan's death and Kōkāčīn's arrival, of about seven years, may seem almost excessive; but we must not forget that the same journey to China and back took Ghazan's envoys nine years, from 1298 to 1307 (see «Caçan»).

According to HAMMER (*Ha*<sup>1</sup>, II, 20), Kōkāčīn came to Ghazan while Ghazan was at «Abher» (Abhar, Ābhār, W.S.W. of Qazwīn; cf. *Br*, II, 113), on his way back from Täbriz to Ḥorasan. The date of the meeting is important, as it has much bearing on the chronology of Polo's return from China. YULE (*Y*, I, 24) has supposed that the travellers reached Ormuz about November 1293, and Ghazan's camp one or two months later; in I, 38, he proposes for the meeting «the end of 1293 or quite the beginning of 1294». This seems to be too late. From the date in *Ha*<sup>1</sup>, II, 19-20, and *Oh*, IV, 94-95, it seems to me that Ghazan's second stay at «Abher» (the one meant here) cannot be later than July-August 1293. Before that, the three Polos had taken Kōkāčīn to Gāihatu, and it is Gāihatu who had her sent along to Ghazan. All that must have taken a fairly long time. The conclusion seems to me unavoidable that the travellers crossed the western Indian Ocean in the winter-monsoon of 1292-1293. Since it took them three months to sail from Zaitun to Sumatra, where they waited five months for the monsoon and spent eighteen months crossing the Indian Ocean, they must have left China not in 1292 as is generally stated, but early in 1291. As, at the time the three Venetians were permitted to go back to the West, Polo was just back from the Indian seas, it may be possible to determine the date and nature of the mission of which he was a member, though probably not the head (see «Seilan»).

Ghazan married Kōkāčīn. By that time, the *ordo*, that is to say the camp and property, left by Hülägü's wife Toquz-ḥatun had become available; it was assigned to Kōkāčīn. Here we must go back a little. Toquz-ḥatun, a Christian Kerait princess, had died in 1265, shortly after her husband, and her *ordo* had been assigned to her Christian niece (or sister?) توقتانی Toqtani (or Tuqtani). This last name has been misread «Toukiti» by QUATREMÈRE (p. 95), «Tuktai» by D'OHSSON (*Oh*, III, 553), «Tuktai», «Tuktini» and finally «Tukini» and «Takteni» by HAMMER (cf. *Ha*<sup>1</sup>, II, 554; *Ha*<sup>2</sup>, 262); «Tuqtai» by BEREZIN (*Ber*, I, 100); «Takteni» by HOWORTH, III, 312; but it is an old Mongol feminine form Toqtani (< Tuqtani ?) of the name Toqtai (< Tuqtai < Tuytai ?), correctly written «Tuḥtani» in Armenian by Vardan (PATKANOV, *Istoriya Mongolov*, I, 25); cf. *TP*, 1932, 49-50. In 1288, two letters had been addressed by the Pope to two princesses whose names are always quoted as «Elegag» (or «Elagag») and «Tuctan» (cf. *Oh*, IV, 69; *Ha*<sup>1</sup>, I, 360, 395; HOWORTH, III, 349; CHABOT, 203, 231; GROUSSET, *Hist. des Croisades*, III, 722), but in the letters they are called in fact «Elegagi», i. e. El-egäči, and «Tuctani». One

of them is our Kerait princess Tuqtani (<Toqtani). She had died on February 20, 1292 (QUATREMÈRE, 95), and that is how it was possible to assign her *ordo* to Kōkāčīn in the second half of 1293 or in the beginning of 1294.

But Kōkāčīn herself died soon afterwards, in fact between June 4 and July 2, 1296, and the former *ordo* of Toquz-ḡatun was then given to Kārāmūn-ḡatun («Ermine-ḡatun»), a Qonyrat, first-cousin to the second Buluyan-ḡatun (QUATREMÈRE, 95; *Ber*, I, 150). Kārāmūn died probably on January 23, 1300 (there is something wrong in the date given by Rašid; QUATREMÈRE, 95). In 1305, Ūljāitū married the Kerait Qutluy-šāh, whereupon the *ordo* of Toquz-ḡatun came back to her family (*Ha*<sup>1</sup>, II, 182).

For the daughter of the king of «Mangi», the name «Caciese», found in some mss., seems to be an erroneous reduplication of «Cocacin» (cf. Vol. I, 92).

## 166. COGACIN

*chogozun* V  
*cocaiio* VL  
*cogaam* P

*cogacin* F, LT  
*cogatim* FA

*cogatin* FB; R  
*zogatin* VB

This is Rašid's *مركاچی* Hügäči (*Bl*, II, 364), in Chinese 忽哥赤 Hu-ko-ch'ih, Hügäči, in literary Mong. Ūkärči, «Cowherd». He was Qubilai's fifth son according to *YS*, 107, 8 *b*, and sixth son according to Rašid, whose genealogical data for that period are generally more reliable; his mother was Dörbäjin, of the Dörbän tribe. The final *-n* is used almost *ad libitum* in many Mongol words, and particularly with the suffix *-či* of a *nomen agentis*. The initial *h-* is correct, and represented in Persian as in Chinese; *hükär* (> *ükär*), «ox», was one of the numerous Mongol words which, in the Mongol period, were still pronounced with an initial *h-* (< \**p-*), although the Uighuro-Mongol writing did not register it (cf. *JA*, 1925, I, 240); it must have been aspirated in a rather pronounced manner to appear as *c-* in Polo. The Chinese transcription and that due to Polo (Rašid is here ambiguous) seem to point to a voiced or at least a weak pronunciation Hügäči instead of the more regular Hükäči. The fall of the *-r* in Hügäči is not accounted for, but is confirmed in early Mongol documents; for instance we have *hükäčīn* in §§ 232 and 234 of the *Secret History*; the case of Chinghiz-khan's personal name Tämüjin, probably a *nomen agentis* formed with *tämür*, *temür*, «iron», is very similar.

Hügäči was appointed prince of Yün-nan in 1267, and, in the month following his appointment, was sent to govern Ta-li, Shan-shan (Yün-nan-fu), 茶罕章 Ch'a-han-chang (Čayān Jang; see «Caragian»), 赤秃哥兒 Ch'ih-t'u-ko-êrh, Chin-ch'ih (see «Čardandan») and other places (*YS*, 6, 5*b*). He was poisoned in 1271, a number of years before Polo went to Yün-nan; the plot against him seems to have originated in Mussulman circles (cf. the names of the culprits in *YS*, 7, 4 *a*). A biographical notice of Hügäči is compiled in *T'u Chi*, 76, 10 *a-b*.

The name of Ch'ih-t'u-ko-êrh mentioned above raises an interesting question of Sino-Mongolian nomenclature. It appears also as Chih-t'u-ko in *YS*, 121, 2 *b*, 3 *a*, and simply as 赤科 Ch'ih-k'o in *YS*, 8, 2 *b*. All are more or less complete transcriptions of Čitkür, in Mongolian «Devil» (but read here Čitgör and Čitkör), and this gives us the Mongol name of the barbarian kingdom of Eastern Yün-nan which other Chinese texts of the period name 鬼國 Kuei-kuo, «Devilish kingdom», or «Lo[羅]-Kuei-kuo», or «Lo-shih[氏]-Kuei-kuo», «Devilish kingdom of the Lo family». The Chinese name goes back to late T'ang times, when we find a 大鬼主羅殿王 Ta-kuei-chu Lo-tien-wang, «Great Chief of the Devils, Prince of Lo-tien» (often named in *Ts'ê-fu yüan-kuei*; also for instance in *Chiu Wu-tai shih*, 38, 4 *b*); and the tradition of a 鬼方 Kuei-fang, «Region of Devils», in Yün-nan is still more ancient. Other Mongol names of Yünnanese tribes are well known, such as Qara-Ĵang, Čayān-Ĵang, to which must be added the Iki-büsä or Yäkä-büsä, «Big belts».

## 167. COGATAI

### *cogatai* R

Most of the names given in their versions of Aĥmad's murder in 1282 by Polo, Rašidu-'d-Dīn, and the Chinese sources are hardly reconcilable (see «Acmat<sup>1</sup>»), and that of «Cogatai» is no exception to the rule. In Rašīd, the nearest counterpart to Polo's «Cogatai» is a certain 𐰽𐰺𐰍; \*Türgān, or \*Tärgān (*Bl*, II, 517; BARTHOLD, in MINAEV, *Marco Polo*, 126), or, with a correction, 𐰽𐰺𐰍 \*Tügān. There is some similitude in sound between this \*Türgān (or \*Tügān) and the Toyōnčar of Chinese sources, while «Cogatai», corrected to \*Togacai, would be another distortion of the same name; but the difference of phonetic «class» practically disposes of any connection between \*Türgān (or \*Tügān) and Toyōnčar (cf. MOULE, in *JNCB*, 1927, 27).

MOULE (*ibid.*) has alluded to my former suggestion that the best representative of Polo's «Cogatai» seemed to be 高 髡 Kao Hsi, and I still hold the same view. But this should not be interpreted as an attempt to suggest a phonetic identification of Kao Hsi (pronounced «Kao Hi» in the Mongol period) with «Cogatai». It seems to be beyond doubt that «Cogatai» renders a genuine Mongolian name \*Kökätai = Kökötai, an adjectival form derived from Mong. *kökä*, *kökö*, «blue», and the exact masculine counterpart of the woman's name Kököčīn or Kökäčīn (see «Cocacin»). WANG Hui-tsu<sup>1</sup>, 29, 3 *a-b*, enumerates eight different Kökötai in the *YS* alone. The most natural solution would be to suppose that Kao Hsi, who belonged to a family of P'o-hai descent which had settled in China and served under the Chin, was known under a Mongolian as well as his usual Chinese name; instances of such double names are numerous even in the *YS*, and not always easy to detect. As a matter of fact, we know that Kao Hsi had a Mongolian personal name, bestowed on him by Qubilai (*YS*, 169, 6 *a*); unfortunately, this was not Kökötai, but 失 刺



Shih-la, *i. e.* Mong. Šira (< Sira), «the Yellow one» (for other Shih-la, cf. WANG Hui-tsu<sup>1</sup>, 26, 3 *a-b*; add the 賈昔刺 Chia Hsi-la = Chia Sira, Chia the Yellow, of YS, 169, 2 *a*, also a name imperially bestowed; for an analogous case, cf. the Mongolian name Qara, «the Black», bestowed by Qubilai on a man of the Liu family, YS, 169, 3 *b*, and the name Čayān, «the White», bestowed on a Tangutan, YS, 120, 1 *a*, and also given to, or taken by, a man from Balkh, YS, 137, 1 *a*). One way out of the difficulty would be to suppose that Kao Hsi was known as Kōkötāi before he was renamed Šira by Qubilai; but, if Kao Hsi's biography be right, the name Šira was given him before 1281, and it is hard to believe that an earlier name should still have been used in 1282 rather than the new one bestowed by Imperial favour.

If Kao Hsi was never known as Kōkötāi, we should look for another man with whom we might identify «Cogatai». The two other officials who play something of the part attributed by Polo to «Cogatai» are 博敦 Po-tun and 張九思 Chang Chiu-ssü (cf. MOULE, *JNCB*, 1927, 27). But Po-tun was a Mongol, and there is no reason why he should have also been known as Kōkötāi. As to Chang Chiu-ssü, he was a pure Chinese, and his biography does not speak of any Mongolian name which had been given him. Among the various Kōkötāi who appear in the *pên-chi* of the YS, one in 1276 was commander-in-chief (*tu-yüan-shuai*) of the Chinese troops (*Han-chün*) serving under the Mongols (YS, 9, 5 *b*); another was one of the officers in charge of lost property (see «Bularguci»), and was pardoned in 1278, when he and some of his colleagues had illegally disposed of some of the property confided to their care (YS, 10, 1 *a*). But neither of them could possibly be Polo's «Cogatai». On the whole, it may be that the true Mongolian name Kōkötāi was misapplied by Polo and, provisionally, I feel inclined to believe that the man really referred to is Kao Hsi.

## 168. COGATAL

*chogatal* VA; R  
*cochobal* VL

*cogatal* F, FA, FB, P, P<sup>5</sup>, VB  
*coghotal* TA<sup>3</sup>

*cogotal* LT  
*gogotal* VA

«Cogatal» is the only reading given by the best mss. for the name of the «baron» who was sent by Qubilai to the Pope with the two elder Polos at the end of their first journey, but it is not known as a Mongol name, and Ross may be right (*RR*, 418) in his surmise that «Cogatal» is perhaps an early clerical error for \*Cogatai, Kōkötāi (see «Cogatai»). None, however, of the Kōkötāi mentioned in Chinese sources can be identified with Qubilai's envoy.

According to most mss., «Cogatal» fell ill and asked to be left behind in a city which TA alone names, surely without authority (cf. Vol. I, 80; see «Alau»). According to VB, the travellers had then marched «twenty days», which have become thirty in Santaella (cf. *Pe*, 17). Only in VL and the texts derived from it (cf. *Pe*, 17, 156, 351), «Cogatal» is said to have died in that city.

Nothing more is known of «Cogatal», who never reached Europe. The information found in Jacopo d'Acqui, that Gregory X *baptizavit principem illum Tartarorum dictum Cogatal* must be based on some confusion.

## 169. COLACH

*choiach* Z; R

*coiach* Z

This word, as a designation in Ma'abar of unlucky hours, was only known before BENEDETTO's edition from a single mention of «Choiach» in RAMUSIO; but RAMUSIO seems to have abbreviated the text of his ms.; in Z (Z<sup>1</sup> is slightly altered), the word occurs seven times, and is spelt «choiach», «coiach», and even «coiath». Moreover, in Z, we read the first time *unam horam infelicem id est uciacham quam appellant choiach*, and the next one *hora uciacha id est coiach*. A very similar paragraph, without, however, the words *uciacha* or *coiach*, is found in the chapter on «Lar», and would have almost appeared to be a misplaced duplication were it not that both passages occur in Z.

For «coiach» CALDWELL (Y, II, 368) has suggested Skr. *tyājya*, «rejected», pronounced «*tiyācham*» (more exactly *tiyāḷḷiyam*); possibly reduced to \**tiyāch* (> \**toiach* > «coiach») by Polo's Mussulman informants. In a letter dated August 31, 1937, K. A. NILAKANTA SASTRI declared himself in agreement with CALDWELL. But, even if we start from \**toiach*, the -o- of the first syllable remains unexplained. Moreover Polo's -ch is -k, not -č as in \**tiyāch*. Other words, such as *tiyakkam*, «faintness», «swoon», or *kośakkam*, «confusion», do not belong to the technical terms of Indian astrology.

ROSS (RR, XIII-XIV) has successfully shown that, even now, popular Malabar superstition observes the same unlucky hours (*ragu kalam*) in the various days mentioned by Polo (but Ross is mistaken when he says that Polo speaks of «Malabar»; Polo's chapter is on Ma'abar, *i. e.* Coromandel). Ross adds that the Dravidian term for the evil hours is *karippu kal*, «which would be pronounced *Kachukka* by a lowcaste Tamil or Malayalam». I must state quite frankly that I do not believe in any phonetic connection between *karippu kal* and «uciacha» or «coiach».

Moreover, the relation of «uciacha» to «coiach» is not clear. It is not evident that RAMUSIO's ms. had both words, and in the two places in which they appear side by side, it is just possible that they betray the hesitation of a copyist who later decided for «coiach» alone. But, if we take Z at its face value, it looks as though «uciacha» were a term of Western astrology, and «coiach» alone a native term used in Ma'abar. I have nothing to add to MOULE's ingenious suggestion that «*uciaca*», read \**uciata*, might be the participle of either *hucciare* «to proclaim», or of *bruciare*, since *ora bruciata* is known in fact in the required sense of «unlucky».

To YULE's indications on Indian divination and lucky or unlucky hours, add al-Bīrūnī's *India*, I, 344.

## 170. COIGANGIU

<i>cagangui, caygangui</i> LT	<i>cingo, jngjngo</i> (?), <i>zigoj,</i>	<i>coiganzu</i> R
<i>caigam, caygam, coygangui</i> P	<i>zingo</i> VB	<i>conganguy</i> FB
<i>chaygiagui</i> TA <sup>1</sup>	<i>coginganguy, cogniganguy,</i>	<i>couganguiu</i> F
<i>chayngu</i> TA <sup>3</sup>	<i>coguigangui</i> FA	<i>coygagin, coygagin</i> S
<i>choigangui, chorgangui, gan-</i>	<i>coigançu</i> Z	<i>coygangiu</i> F, Ft, L, VL
<i>gui</i> VA	<i>coiganguiu</i> F, L	<i>coyganguy</i> FB, P, P <sup>5</sup> , S
<i>chonguagui, chuiangui, cui-</i>	<i>coigangui</i> Fr, LT, VA	<i>curgagiu</i> G
<i>gangui, zinguagui</i> V	<i>coiganguy</i> FAt, FBt	<i>coyganguy</i> Pr
<i>choygangui</i> VL		

We are here on safe ground. Nobody has ever doubted that «Coigangiu», where Polo crossed the Yellow River (his «Black river»; see «Caramoran»), is 淮安州 Huai-an-chou. The name goes back to the end of the Sung (1234), but, when Polo used it, though it was still current among the people, it had ceased to be official. In 1277, a *tsung-kuan-fu* had been created at Huai-an, and the place had become the seat of a real *fu* in 1283 (YS, 59, 10 a). Of course, the Commissioners of 1276 also crossed the Yellow River at Huai-an (TP, 1915, 396, 414).

Polo's transcription of Huai-an-chou is interesting in its first *-g-*. The initial sound which we note in French by *ng-*, and which is omitted in English transcription, is very weak, and I do not think that it has left any trace in Mongol or Persian transcriptions. It may be that Polo's first *-g-* is not so much a real transcription of that initial sound as a substitute for the hiatus stop between *huai* and *an*, something (quite independently) like one of the values of the intervocalic *-g-* in Mongolian writing. Other examples may be «Paughin», and perhaps «Ciangan» (*q. v.*); but Polo writes «Cauyu» without *-g-* (or *-gh-*), and I have decided against a similar use of *-g-* in «Ciu-giu» (the place west of Huai-an).

Another point in Polo's transcription, «Coigangiu» is worth mentioning : it is the notation of Ch. *-ua-* (*-wa-*) through *-o-*. This is exactly what occurred in Uighur, Mongol and Persian transcriptions of Chinese, where *wang* became *ong*, and Kuan-shih-yin became Konšim (but not as a final *-ua*, which of course gives also a final *-a* in transcription).

## 171. COILOMIN

<i>cholonbini</i> V	<i>coilomini</i> L <sup>1</sup>	<i>combi</i> VB
<i>chome luuini</i> VA	<i>columi</i> FA, FB (bresil and	<i>grandes</i> and <i>linuini</i> P
<i>coilomin</i> F, Z, L	<i>ginger</i> )	[ <i>mirabolani enblici</i> TA <sup>1</sup> , TA <sup>3</sup> ]

An epithet derived from «Coilum» (? «\*Coilom»), Quilon. Polo uses it for a kind of brazil-wood and also for a kind of ginger.

On brazil-wood in general, its other name *sappan*, and on the particular sort called *columbino*, *colombino*, etc., see «Brazil» and cf. *Y*, II, 380-381; *Hobson-Jobson*<sup>2</sup>, 113, 794; EVANS, *Pegolotti*, 433; HEYD, II, 587-590; *HR*, 217; *Fe*, 246.

For the ginger *colombino*, cf. *Y*, II, 381; EVANS, *Pegolotti*, 419; and, above all, HEYD, II, 619-623.

Although I have retained the «coilomin» of F, Z and L, it may be that «\*coilomi», perhaps a Persian adjectival form in *-ī*, also existed. Some mss. readings seem to point to it, and Pegolotti, though generally writing «colombino», speaks twice in the same passage of the sort of brazil-wood called «colonmi» (EVANS, 361). «Colonmi» may either be a copyist's error for «\*colomin», or represent «\*colomi».

## 172. COILUM

<i>cailan</i> Ft	<i>coillon</i> Fr	<i>coulam</i> R
<i>cailem</i> Lr	<i>coillum</i> VA	<i>coylum</i> P
<i>cailon</i> L	<i>coilom</i> L <sup>1</sup>	<i>orbai</i> VLr
<i>choilon</i> V	<i>coilon</i> F, Z, V	<i>orbay</i> VL, S
<i>choilu</i> TA <sup>1</sup>	<i>coilum</i> F, FA, FB	<i>sorlym</i> G
<i>choylu, choyul</i> TA <sup>2</sup>	<i>colim</i> VB	

All the mss. point to *coi-* for the beginning of the word («Coulam» of *R* is probably a case of RAMUSIO'S «editing»), although we should expect *co-*, or *cu-*, or *cou-* (in value of *cu-*); as to the final, the original French text perhaps had «\*Coilom» (> \*Coilō > Coilon), or even «\*Coulom». The name is the same as that of modern Quilon on the coast of Travancore; in Tamil, «Kullam», though «Kollam» occurs an inscription of 1265 (cf. *JA*, 1922, II, 49).

Most of the ancient quotations of the name are assembled in *Y*, II, 377-380; *Hobson-Jobson*<sup>2</sup>, 751-752; DAMES, *Barbosa*, II, 3, 96-97; and HALLBERG, 153-157. Since a would-be Syriac mention of «Colon» in the middle of the 7th cent. has been retained in *Y*, II, 377, it may be remarked that YULE himself had justly renounced it in *Hobson-Jobson*. Quilon appears for the first time in the Kūlam-Malāi (= Kūlam of Malabar) of the Arabic travellers of the 9th cent.; Sulaymān, in 851, speaks of the taxes paid by Chinese ships at Kūlam-Malāi (FERRAND, *Voyage du marchand arabe Sulaymān*, 40). The mediaeval Western form is «Columbum», «Columbo», etc., occurring in many sources, from Jourdain Cathala (see the documents added to CORDIER'S edition) and Marignolli to the Catalan Map; a sanskritized form Kolamba is known epigraphically. To the various quotations already produced, I may add «Colom» in Guillaume Adam (*Hist. des Crois.*, Arm. II, 552). A name «Khlmbū» appears in a Hebrew document found in the Cairo Geniza,

and I have taken it for Quilon rather than Colombo (cf *TP*, 1928, 460; 1930, 435); but I have been wrong in adducing, besides «Columbum», etc., the *Kulanbū* (first two vowels uncertain) of Ibn Baṭṭūṭah, which is really Colombo; and, if the Hebrew document be late enough, it may after all refer to Colombo too.

In 983, a Chinese Buddhist priest went to India with a series of letters, one of which was for the king of 柯蘭 *K'o-lan* (*Sung shih*, 490, 2*a*); although this transcription supposes \**Kalan*, it is supported by a later form of the Ming period, and the place meant is probably Quilon (in FERRAND's note, *JA*, 1924, 1, 115, 枸 is a misprint, and it is wrongly stated that the transcription can be restored to \**Ka-lam*, with a final *-m*). In 1778, the *Ling-wai tai-ta* writes 故臨 *Ku-lin* (= \**Kulom*), and in 1225, we have the same spelling *Ku-lin* (= \**Kulom*) in Chao Ju-kua (cf. *BEFEO*, iv, 319; *HR*, 88, 91).

Under the Mongol dynasty, there are many mentions of Quilon, particularly in the notice of «*Ma'abar* and other countries» of *YS*, 210, 7*a-b*, abominably translated in *Pa*, 603-605, and not very satisfactorily by ROCKHILL in *TP*, 1914, 431-436. PAUTHIER is responsible for the pseudo-royal residence «*A-pu-ho-ta*» which has puzzled YULE; the text really speaks of the distance from *Ma'abar* to the domains of the great (*ta*) king *A-pu-ha*, i. e. the *ilkhān* *Abaya* (see «*Abaga*»). I shall only remark here that the name in that notice is written 俱藍 *Chū-lan* (= \**Kulam* or \**Kulom*), and that the king reigning in 1280 is named 必那的 *Pi-na-ti* (or *Pi-no-ti*), his younger brother being \**Kenokā* *Buramušing*. Quilon is also mentioned, with the same spelling *Chū-lan*, in *YS*, 12, 4*a* (s. a. 1282); 12, 6*a*, 6*b* (s. a. 1283, the name of the king being 瓦你 *Wa-ni*); 14, 5*b* (s. a. 1287, the name of the envoy from Quilon being \**Buruwunai*); 16, 9*a* (s. a. 1291); there are certainly other mentions too which I have not noted. In 1293 \**Qultuqa*, and in 1294 \**Tügü-Tämür* were sent as envoys to 閣藍 *Ko-lan* (*Ko-lam*); *YS*, 17, 9*b*; 18, 3*b*; this too is Quilon.

In the same *YS*, 94, 11*a*, mention is made of regulations published in 1296 for the trade with *Ma'abar*, 俱喃 *Pei-nan* and *Fandaraina*; as CORDIER had it from me already (*Y*, III, 120; *Y*<sup>1</sup>, iv, 27; cf. also *TP*, 1914, 425), we should read 具南 *Chü-nan* (= \**Kunam* or \**Kunom*), Quilon. The same correction must be adopted for the «barbarian kingdom» (*fan-pang*) of 咀喃 *Chü-nan* whose ambassador, *Ma Burhanu'd-Din*, arrived in 1291 (*YS*, 16, 8*b*); and the spelling *Chü-nan*, instead of *Chü-lan* and *Ko-lan*, is adopted in 1349-1350 by the *Tao-i chih-liao*, which has a notice on «Small [小] *hsiao* *Chü-nan*», «Small Quilon» (cf. *TP*, 1915, 445-447). In the first half of the 15th cent., Quilon is marked as 小葛蘭 *Hsiao Ko-lan*, «Small *Ko-lan*» on the map published by PHILLIPS (*JNCB*, XIX, 222), in the *Ying-yai shêng-lan*, in the revised *Hsing-ch'a shêng-lan* and in the *Ming shih* (cf. *TP*, 1915, 447-448; 1933, 288-289); the original *Hsing-ch'a shêng-lan* writes *Hsiao Chü-nan*, the same as the *Tao-i chih-liao*. The *Hsing-ch'a shêng-lan* has besides a notice on a «Great Quilon», the name of which is written 大 [大] *Chü-nan* in the original text, and 大 *Ko-lan* in the revised edition; but this «Great Quilon», with both spellings, in an arbitrary creation of the *Hsing-ch'a shêng-lan*, whose notice of it is simply copied from that of the «Small Quilon» in the *Tao-i chih-liao*. In spite of the fact that the *lan* of *Ko-lan* never ended in *-m*, and that, in the beginning of the 15th cent., *Ko-lan* ought to have represented an original \**Golan*, it cannot be doubted that the name meant is *Kollam* or *Kulam*, our Quilon.

Although the « Great Quilon » is a fiction of the *Hsing-ch'a shêng-lan*, the fact remains that, in the middle of the 14th cent., we find the name of Quilon without epithet changed to that of « Small Quilon ». Much has been written about it already, and the problem is not an easy one.

ROCKHILL (*TP*, 1915, 446) supposed that « Small Quilon » was « Kayan Kulam or Kain Colan, the Singuyli of Friar Jordanus, as suggested by YULE, . . . which in turn is the 僧急里 Sêng-ki-li of *YS*, 210 »; cf also *TP*, 1914, 441. CORDIER also says (*Les Merveilles de l'Asie*, 84) that YULE places « Singuyli » at Kayan-Kullam. But « Singuyli » must be left out. YULE had thought of this equivalence in his translation of Jourdain Cathala published in 1863, but justly abandoned it in *Cathay*<sup>2</sup>, I, 82; II, 133-134; III, 249; IV, 78; *Hobson-Jobson*<sup>2</sup>, 828-829. HEYD, II, 661, who started from Odoric's « Cyngilin », etc., and knew that YULE had identified it with Cranganore, has tried to maintain its identity with Kayan-Kullam by starting from « Caincolon »; « de là à la forme Cyngilin, il n'y a qu'un pas ». But since we know a real name Kayan-Kullam and a real name Singili, it is of course impossible to bring them together phonetically. Singili (this is the correct form, not « Šinkali », etc.) is certainly an old name of Cranganore.

As to Kayan-Kullam, it is a port on the backwater, a few miles north of Quilon. The name is written « Caincoulam » in RAMUSIO's Italian version of Barbosa (ed. 1606, I, 312 *b*), « Caymcolan » in the Spanish one, and « Cale Coilam » in the Portuguese text; in the anonymous *Sommario* of RAMUSIO (I, 332 *a*), we find « Caicolam » and in a supposed letter of Amerigo Vespuce, « Caincolon » (HEYD, II, 661). Arabic sources of the first half of the 16th cent. have كاین کولم Kāin-Kūlam (*Fe*, 530; *JA*, 1924, I, 115). DAMES (*Barbosa*, II, 96) has adopted « Cale Coilam », and supposes it to be « an Arab form of the Malayālam name », « Cale being clearly the Arab *Kal'a* 'a fort' ». This seems to me most unlikely. We know the Arabic form, Kāin-Kūlam, which is in agreement with the native Kayan-Kullam, and one does not see how the Italian and Spanish translators of Barbosa could alter another supposed and unattested Arabic form to hit on just the correct native name. There are other cases where the Italian and Spanish versions of Barbosa are superior to the Portuguese text as we have it now (see « Chisce »). I take « Cale Coilam » to be an error for « Cain Coilam », even if Barbosa wrote « Coilam » at all and not, both here as well as in the rest of his work, « Coulam ».

FERRAND, following in ROCKHILL's wake, has considered as an established fact (*JA*, 1920, II, 101; 1924, I, 115) that « Little Quilon » was Kayan-Kullam and that the *Hsing-ch'a shêng-lan*'s « Great Quilon » was our Quilon. But we have seen that this « Great Quilon » never existed. The whole problem is to decide whether the change of Chü-lan (Quilon) to Hsiao Chü-nan and Hsiao Ko-lan, « Little Quilon », is due to a change of harbour and, if it is, what has been the change. I am not in a position to explain why the Chinese name was changed in the middle of the 14th cent., but I rather feel that in all cases Quilon only is meant, and DAMES (*Barbosa*, II, 97) has evidently taken the same view. PHILLIPS's map, which is a Chinese translation of an Arabic map, names only « Little Ko-lan », and it seems to me most improbable that, in the beginning of the 15th cent., the original Arabic map should have mentioned Kayan-Kullam and omitted Quilon. It was and has remained the main harbour on this part of the coast; the Chinese Imperial fleets, bringing Imperial orders to the local prince, could not well have failed to enter it.

The *Burhan-i Qaṭī*\* (cf. VULLERS, II, 920) gives a word كزك *kaūlam*, meaning in Persian «black pepper». It is evidently the name of Quilon adopted as a designation of one of its staple products.

## 173. COJA

<i>choia</i> VB	<i>cor</i> LT	<i>edila</i> VL
<i>choila</i> VA	<i>cora</i> FB	<i>edilla</i> S
<i>chorza</i> V	<i>coya</i> TA <sup>1</sup>	<i>goza</i> R
<i>coia</i> F, FA, TA <sup>3</sup> , L	<i>coyla</i> (?) P, P <sup>5</sup>	

In view of «Goza» in R, I read the «Coia» of F as «Coja» (Coža) for «Cogia» = جوجه *Ḥōja* (< *Ḥwājah*), in Persian «master». I think YULE means the same when he writes «Koja» in Y, I, 33, and B<sup>1</sup>, 441, has adopted «Cogia». The name is not characteristic enough to support an attempt at identification. For Western quotations of the word, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 235; it is written «coya» in Marignolli (*Wg*, 537). In *Codex Cumanicus* (KUUN, 105), we find *dominus* rendered «ghoya» in Persian and «Coia» in Turkish.

## 174. COMAIN

<i>chaynari</i> LT	<i>comain, comainz</i> F	<i>commains</i> FB
<i>chumani</i> TA <sup>1</sup> , VB	<i>comains</i> FA	<i>cumani</i> Z, L, V, R
<i>chumanni</i> TA <sup>3</sup>		

The Comans appear for the first time in Byzantine history in 1078, and as *Κόμανοι*; in 1154, Edrisī calls them Qomān, and speaks of their country as Qomāniya. Although Russian chronicles refer to the «Kumani», they usually give them the name of Polovcy, which does not seem to mean «Dwellers of the Plain», as is said in Y, II, 491, but is probably derived from *polóvyi*, «fallow», «grey-fallow». The Hungarian form is, in the plural, Kúnok (hence Cuni in Hungarian Latin). Although the origin of the name is still uncertain, the Comans were a Turkish-speaking nation, and their name was frequently used in the Middle Ages, by Western travellers and chroniclers, as a synonym of Qipčaq. Polo is right when he says that the Tartars did not exist in the time of Alexander the Great (see «Alexandre<sup>1</sup>»), but he is wrong when he replaces their name by that of the Comans. On the Comans, see J. MARQUART, «Ueber das Volkstum der Komanen» (*Abh. d. K. Ges. d. Wiss. zu Göttingen*, ph.-hist. Kl., N.F., XIII, No. 1, 25-238); PELLIOT, «A propos des Comans», in *JA*, 1920, I, 125-185; *Mi*, 315-317. For the various forms of the name in Western documents, cf. HALLBERG, 159-161.

## 175. COMANIE

*chomania* TA<sup>1</sup>*comanie* F*cumania* Z, L

The land of the Comans (see «Comain»). It represents the plains of southern Russia, including a portion of western Siberia, but the extent of «Comania» varies in the accounts of the different travellers.

## 176. COMARI

*camari* Z m*chomate* Sr*comary* FA, FB*chomacci* TA<sup>1</sup>*chomati, comati* VL*comate* S*chomacri* TA<sup>3</sup>*comam* VB*cormari, cormary* FA*chomain, comain* V*coman* G*cumari* R*chomari* VA*comari* F, Fr, t, P, Z, VA, L*quomari* Z

Cape Comorin and the surrounding region. The name comes from Skr. Kumāri, «the Virgin», and occurs already in Ptolemy. The Arabic form, at least since the 13th cent., has been كُمهري Kumhari, the origin of the *-h-* remaining obscure. Cf. Y, II, 382-383; YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «Comorin»; Fe, 698; DAMES, *Barbosa*, II, 102-103. Here as in a few other cases, Polo gives the height of the North Star in «goues», that is to say in «cubits» (see «Goue»); this is surprising, as the then current practice among Mussulman sailors on the Indian Ocean (reflected in Chinese portulans of the 15th cent.) was to count in *isba'* or «digits»; cf. FERRAND, *Instr. nautiques*, III, 157. Fra Mauro writes «Chomari» (*Zu*, 42; HALLBERG, 143).

## 177. COMO (&lt; \*COINE)

*chemo* S*come* FA, FB*goino* LT*chomo* VA[*comesauastcaserie* FB]*gomo* P, P<sup>5</sup>*cogno* R*como* F, L (L, *lecomo*)

This is Qonyā, «Konieh» of our maps, the ancient Iconium. B<sup>1</sup>, 20, writes «Conia», and says that, in Rustichello's French, the final *-o* has almost the value of a final mute *-e*. I must say I doubt it, as I believe the *-o* to be an early corruption of *-e*. The corruption already occurred in the source of the Catalan Map, which gives «Cunnyo» (BUCHON, in *Not. et Extr.* XIV, II, 100). On the other hand, R's «Cogno» (< \*Coyno; again «Cogno» in R's *Dichiaratione*, 13 b) and LT's



«Goino» make me think that «Como» or «Come» does not stand for «Conie», but for «Coine»; this very form «Coine» occurs for instance in the *Gestes des Chiprois* (*Hist. des Crois.*, Arm., II, 843), and in the *Jeu de Saint Nicolas* (cf. Y, I, 132) and Joinville also writes «Coyne» (cf. Y, I, 44). «Coine» seems to have been corrupted into «Turchia» in Z and V; the -r- may then represent the -i- of «Coine». Polo must have here used the form then current among «Franks» in the East.

Konieh, before the Mongol conquest, was the capital of the Seljuqid sultans of Rüm. Pope Gregory IX sent a mission to them in 1233 (GOLUBOVICH, *Bibl. bio-bibl.* II, 298), and Rubrouck passed through Konieh in 1255 (*Wy*, 328). About 1330, Konieh became the capital of the Qaramān princes.

On Konieh, cf. CL. HUART, *Konia, la ville des derviches tourneurs*, Paris, 1897, in-8, and LS, 140-142.

### 178. CONCI (< \*CONICI)

*canci* Fr, t, Zr  
*chanachon*, *gangi* V

*choccholini*, *chonci* TA<sup>1</sup>, TA<sup>3</sup>      *zanzi*, *zenzi* VB  
*conci* F, Z, L

The original is certainly Qoniči, in Mongolian «shepherd» («Conici» > \*Comci > F and Z Conci). In spite of F and Z, B<sup>1</sup>, 440, has adopted «Canci», because YULE (Y, II, 481) had called attention to the arrival in Persia, in 1293, of envoys from «Kaunchi», one of Jöči's great-grandsons. But YULE had kept «Conchi» in his text, and says that he changed «Kaunchi» from HAMMER's forms «Quwindschi» (*Ha*<sup>1</sup>, II, 479) and «Qubindschi» (*Ha*<sup>3</sup>, 663); both are misreadings, قوينچی Q<sup>u</sup>winči and قوبينچی Qubinči standing for قونينچی Qoniči. In RR, 415, through some misunderstanding, a restitution Kōnčā is attributed to me, but, at my request, this was corrected to Qoniči in the Addenda. In Oh, II, 454, «Kotchi-ogoul» is an error for Qoniči-oyul, «Prince Qoniči». I leave out of consideration the «K'uan-sa» (to be read K'uan-ch'ê, Kōnčāk) of Br, II, 15. HOWORTH (II, 217-220) has confused three or four different names.

There were two Qoniči (Bl, II, 95, 120, 443, 611; the names are misread «Qoinči»; BARTHOLD, *12 Vorlesungen*, 188, adopts «Qončy»), but the one that Polo speaks of is certainly Sartaqtai's son, who was ruling over the former appanage of Ördü (or Ordu?), Jöči's eldest son. That branch, although acknowledging in theory the authority of Batu's successors, was practically independent; its members were called «Princes of the Left Hand», that is to say «of the East» (cf. BARTHOLD, in MINAEV's, *Marko Polo*, 332). Qoniči died about 1300.

### 179. CONDUR, see SONDUR and CONDUR

When leaving Ciamba (Champa), not Java as the mss. have it wrongly (see «Java» and «Lochac»), Polo sails 700 miles south-south-west and finds two islands, the greater called Sondur,

the smaller Condur, which should be treated together. Although the estimation of distance seems to be excessive, there can be no reasonable doubt that, in Polo's mind at least, the double name applied to Pulo Condor as well as to one of the minor islands of the group. There can be no question of locating them just east of the isthmus of Kra, as is done on BENEDETTO's map (*B*<sup>1</sup>, at the end of the volume).

Since 1904, when I collected for the first time the Chinese sources concerning both the 崑崙 K'un-lun races and K'un-lun itself, the Chinese name of the island Pulo Condor (*BEFEO*, IV, 217-231; add *TP*, 1923, 271-272; *Études asiat. EFEO*, II, 257-263), the problem has been studied again by FERRAND in «Le K'ouen-louen», a memoir of 267 pages, published in *JA*, 1919, I-II, which abounds in useful information from Mussulman and Indonesian sources. I would have had much to add, to correct, or to qualify, but *non est hic locus*, and I wish simply to note here FERRAND's agreement (I, 327) that «from the end of the 13th cent., K'un-lun clearly means the island of Pulo-Condor in certain texts», and that, in Polo's text, «Condur» can only be the very name of Pulo Condor. Malay Pūlau Kundur means «Gourd Island», and so it is with the Cambodian name Koh Tralàch; the Annamese Côn-nôn is simply the Annamite pronunciation of K'un-lun. On the early 15th cent. Chinese map, «K'un-lun» is marked where we would expect it, as Pulo Condor. A notice of K'un-lun = Pulo Condor is given in 1349-1350 by the *Tao-i chih-liao*, where we are told that this is the ancient «Mount K'un-lun», but that it is also called «Mount 軍屯 Chün-t'un». It has been supposed that this was a more correct transcription of the name of Pulo Condor. But we must not forget that K'un-lun is an old Chinese mythical name, applied to the K'un-lun races and to Pulo Condor through sheer phonetic analogy. On the other hand, one text writes here 渾屯 Hun-t'un, and, if written 渾沌 Hun-tun, we would have not only the ancient Chinese name of «Chaos», but also the name of a monstrous animal which Chinese mythology places in the legendary K'un-lun mountains; moreover, precisely the «Sea of K'un-lun» (cf. *infra*) is called the «Great Sea of 混沌 Hun-tun» in Shih P'i's biography (*YS*, 162, 5b). So I think no conclusion can be derived from the alternative form in the *Tao-i chih-liao*.

In the Chinese texts, the sea south of Pulo Condor towards the Straits is called «Sea of K'un-lun». Its approach was much dreaded, and that is why there was a saying: «Above are the Seven Islands, below is K'un-lun; if the needle goes wrong or the rudder fails, of men and ships what will remain?» In translating this notice of the *Tao-i chih-liao* (*TP*, 1915, 112), ROCKHILL has missed half of the saying (and partly also on p. 112 in translating the notice of the *Hsing-ch'a shêng-lan*, simply copied from the *Tao-i chih-liao*). Moreover the Seven Islands (Ch'i-chou) are the Taya Islands at the north-eastern angle of Hainan (cf. *BEFEO*, IV, 208), and not the Paracels as is still said by ROCKHILL, by FERRAND and by FUJITA (66 b). I must add that the place «Ting-chih» which is mentioned in this notice by ROCKHILL (p. 112-113) and which has passed into FERRAND's memoir (*JA*, 1919, I, 258), does not exist; the text says that the port of Champa, Pulo Condor and the Tung-Hsi-chu Islands (probably Pulo Aor) are placed respectively at the feet of a tripod (*ting-ch'i*), a usual Chinese expression.

The «Sea of K'un-lun» of the Chinese is the «Sea of كندرنج Kundurunj» (= Kundurung) of the early Arabic travellers, which name is of course identical with the Chün-tu-lung (\*Kundurung)

of Chinese transcriptions of the 8th cent.; in spite of FERRAND (*Fe*, 14-17), I still believe that \*Kundurung is the old name of Pulo Condor, not of Cape Saint-Jacques. Polo's Condur simply shows that the modern Malay form was already current at the end of the 13th cent.

So much for Polo's Condur, but what of Sondur? It occurred already to YULE that Sondur must be the same name which appears as *صندر فولات* *Şundur-fülât* in the early Arabic narratives, and I think he is right, but the case is complex and requires some explanation.

FERRAND considers that *Şundur-fülât*, which for a long time he transcribed *Čundur-fülât*, is Pulo Condor, and, although he never said so, I think that is what prevented him from admitting that Kundurung was Pulo Condor. The fact is that the Arabic relations first speak of Kundurung, then of Champa, and afterwards of *Şundur-fülât*, so that *Şundur-fülât* cannot be for them the same as Kundurung. As to *Şundur-fülât*, FERRAND has explained it as a Persian plural of an arabized *Şundur-pūlāu*, *Şundur* itself being the outcome of *Kundur* > *K<sup>1</sup>undur* > *Čundur* > *Şundur* (*Fe*, ix, 2; *JA*, 1919, i, 328). But he did not explain why *k* became *č* in *Kundur*, but remained *k*, at the same time, in *Kundurung*.

In *JRAS*, 1914, 496, BLAGDEN raised several objections to FERRAND's solution. 1. That in Malay *k*- does not change to *č*- in the initial position; 2. that in Malay and in Indonesian languages generally, *pulau* ought precede the name of the island, and not to follow it; 3. that, if *Şundur-fülât* is Pulo Condor, there is no reason why the Arabic travellers, on their way to China, should go to *Şundur-fülât* after leaving Champa.

On the first point raised by BLAGDEN, I am not in a position to make any definite statement. To the second, some answer can be given. It is true that the usual Malay, and generally Indonesian, construction requires *Pulau Kundur*, but we know of other cases when *pulau* has been transferred to the end of the compound. The name of the island off the coast of Annam called *Culao Cham* on our maps means «Cham island», *culao* being Cham *pulau*, *kalāu*, *kulau*, «island» (Annamese *cù lao* is borrowed from the Cham), identical with Mal. *pulau*; but the name is *Chan-pu-lao* (= *Čam-pulau*) in Chinese texts of the 8th cent., *Chan-pi-lo* (\**Cam-pilo*) in the Ming period (cf. *BEFEO*, iv, 198-201; *JA*, 1919, i, 323); I may add «*Champiloo*» in F. M. Pinto (cf. COLIN-PASTELLS, *Labor Evangelica*, i, 364<sup>3</sup>). Another example is provided by Polo's «\**Gaumispola*», the «*Gomispola*» of other travellers, and the *Ĵāmis-fulah* (= *Gāmis-pula*) of Arabic texts, which is «*Pulo Gomme*s» etc. of ancient maps. So the order of the words is not an obstacle to the explanation of *fülât* in *Şundur-fülât* with the Mal. *pulau*.

The geographical objection is of much greater weight. All Arabic texts place *Şundur-fülât* after Champa on the way to China, while Pulo Condor is passed before reaching Champa. FERRAND himself seems to have later yielded tacitly to this argument since in his *Instructions nautiques*, III, 166-167, speaking again of the same Arabic texts, he gives the island of Hainan as the equivalent of *Şundur-fülât*. I also think that *Şundur-fülât* is not Pulo Condor, so that nothing more stands in our way to identify with Pulo Condor the *Kundurung* of the 8th-10th cents.

But that does not mean, in my opinion at least, that *Şundur-fülât* is Hainan. Without being dogmatic about it, I think that *Kundurung* is Pulo Condor, that the early Arab travellers mean by Champa the coast of Binh-dinh with Quinhon harbour, that their *Şundur-fülât* is *Culao*

Cham, and that their «China's gates» are the «Seven Islands» of the Chinese texts, that is to say the Taya Islands off the north-eastern corner of Hainan. Even then, the possibility should not be excluded that there might be some connection between the name Şundur and that of Kundur. Annamite texts, of late date though they may reproduce a fairly ancient tradition, speak of an event of the early 5th cent., which is said to have taken place at Culao Cham, but which, an authoritative Chinese work of the early 6th cent. states, took place at K'un-lun (cf. G. MASPERO, *Le royaume de Champa*, 93; FERRAND, in *JA*, 1919, I, 323); and, as Pulo Condor was still called Kundurung by the Chinese in the 8th cent., it is just possible that they first applied their legendary name of K'un-lun to Culao Cham, and transferred it to Pulo Condor only at a later date. The phonetic connection between K'un-lun, Kundur and Şundur remains doubtful, but the name Şundur must still have been current in Polo's time among Arabo-Persian sailors, since it is evidently represented by «Sondur». Perhaps Şundur moved south, and was transferred from Culao Cham to Pulo Condor in the same way as K'un-lun. Polo had heard both names, Kundur, the real «Malay» name (< Kundurung), and the Arabic Şundur, and applied them to two islands of the group. And perhaps there is some unconscious survival of the old sense of Şundur = Culao Cham, when Polo says that «Sondur» is the greater island and «Condur» the smaller one.

The suggestion made with some diffidence by HIRTH and ROCKHILL, *HR*, 49, 50, that, in 1225, Chao Ju-kua's 蒲羅甘兀 P'u-lo-kan-wu might be Pulo Condor cannot be sustained; phonetically, one would think of a name like that of Pulo Gambir.

## 180. CONSTANTINOPLE

<i>chconstantinopoli</i> V	<i>constantinopla</i> S	<i>costantinoble</i> O
<i>chostantinopoli, costantinopoli</i>	<i>constantinople, gonstantinople,</i>	<i>constantinopoli</i> VB
VA	<i>gostantinople</i> F	<i>costentinoble</i> FA
<i>constantineapolis</i> LT	<i>constantinopoli</i> VL, R	<i>ghostantinopoli</i> TA <sup>s</sup>
<i>constantinoble</i> FB	<i>constantinopolis</i> LT, P, L, Z	

Constantinople, under its Mussulman name of Kostanţinyah (REINAUD, *Géogr. d'Aboulféda*, II, I, 315; «Constanthynyé» as in the Index, or «Constantinah» as in *China Review*, v, 239, or *Br*, II, 135), appears twice in Chinese texts. On the Chinese map of c. 1330, 吉思答你 Chi-ssü-ta-ni (read Ku [古] -ssü-ta-ni) is marked west of the dominions of the *ilkhan*. In a Chinese itinerary of the Ming period, 苦思且 K'u-ssü-tan is said to be four months to the west of Tabriz. BRETSCHNEIDER has already recognized Constantinople in both cases; but it seems that the transcription was made from a shortened form, and with *u* instead of *o* in the first syllable. The denasalization of the first syllable is not due to the Chinese, as we find Qusţanţiniya in the *Hudūd* (*Mi*, 79), and regularly in Abū-'l Fidā (but the *u* or *o* nature of the first vowel cannot be ascertained from the Arabic spelling). The denasalized form survives in Italian «Costantinopoli».

## 181. COTAN

<i>catan</i> Lr	<i>cocan</i> FA	<i>cotan</i> F, FA, FB, L, VB
<i>chatan</i> V	<i>coram</i> LT	<i>cotran</i> Z
<i>chota</i> VLr	<i>cota</i> VL	<i>gottan</i> L <sup>4</sup>
<i>chotam</i> TA <sup>1</sup> , TA <sup>3</sup> , VA	<i>cotam</i> P, VA; R	<i>sitan</i> FB
<i>chotan</i> V, VB		

The name meant is of course ختن Ḥotan, our «Khotan». The Catalan Map gives «Cotan»; Fra Mauro, «Gothan» for the «province» and «Gothā» (= «Gothan») for the city (HALLBERG, 166; RUGE's «Cothan» is a misreading). The earliest mediaeval mention in Western texts occurs in the Latin translation made at Damietta in 1221 of an Arabic document relating to the Mongol campaigns against the Mussulmans; the name is there written «Chatem» and «Chacen», both corrupt for \*Chaten (cf. ZARNCKE, *Der Priester Johannes*, II, 34, 49). In 1603-1604, Bento DE GOES, the first European to visit Khotan after Polo, writes «Cotàn» with the same spelling as the Venetian traveller (cf. TACCHI-VENTURI, *Opere storiche del P. Matteo Ricci*, I, 531, 539, 540).

The name of Khotan became known to the Chinese c. 125 B.C., as a result of the mission of Chang Ch'ien. It is written 于窠 Yü-t'ien (and 于窠 Yü-t'ien) in *Shih chi*, 123, 2a (cf. also TAKIGAWA Kametarō, *Shiki kaichu kōshō*, 123, 7a), 于闐 Yü-t'ien in *Ch'ien-Han shu*, 96A, 1a. This second form is the one which remained in common use, although 于填 Yü-t'ien may be found occasionally (cf. Tōkyō *Tripit.* of Meiji, 爲, x, 71b; BEFEO, v, 263, 293; TP, 1920, 322, 419), and even 于鑛 Yü-t'ien (*ibid.* 39b, 41a). BRETSCHNEIDER is strangely mistaken when he says (*Br*, II, 47) that «the first sound of the name is generally rendered by a character meaning 'jade'» and sees there an allusion to Khotan's most famous product. STEIN (*Ancient Khotan*, 155), influenced by RICHTHOFEN, says something of the same sort. So it may be useful to state definitely that Yü-t'ien is *never* written the character 玉 yü, «jade».

The difference between the various *t'ien* of Yü-t'ien is of a purely graphic order, for characters which can be written in several ways. All the phonetic glosses leave no doubt that c. A. D. 600 Yü-t'ien was pronounced \*Jju-d'ien (in KARLGRÉN's system); the only hesitation is that some read \*d'ien in the first (*p'ing*) tone, and others in the fourth (*ch'ü*); cf. the various references given above for the name itself, and add 爲, VIII, 88a; IX, 1b, 51a, 151a; x, 65a; *T'ang shu shih-yin*, 24, 2a. The 翻梵語 *Fan Fan yü* (Taishō *Tripit.* 54, 1036; cf. *JA*, 1915, I, 110) says that 于填 Yü-t'ien ought to be written 優地邪那 Yu-ti-yeh-na, Udyāna, which means 後堂 *hou-t'ang*, «back-rooms», «ladies' apartments» (the real meaning of *udyāna* is «pleasure-garden», or «park»). Although the correction is absurd and clearly rests upon a confusion between Yü-t'ien (= Khotan) and the usual wrong sanskritization of Uḍḍiyāna as Udyāna, the note shows that Yü-t'ien was actually read with -d-, \*Jju-d'ien.

But we have to interpret this \*Jju-d'ien. The second element is clear, and there is no reason to think that it did not represent a foreign *-dan* or *-d'an* (yodized) in Han times as it did

seven centuries later. The case of 于 *yü* (\**jju*) is more difficult. In KARLGREN's transcription *j-* is a *yod*, with an approximate value of *y-*. There is some reason to believe that in archaic Chinese the word began with *g-*, \**giu*, but we cannot say that this is certain, nor that an initial *g-* still existed in the pronunciation of Han times. On the other hand, 于 *yü* is not regularly used in Buddhist transcriptions, but frequently occurs under the Han in the transcription of Hsiung-nu names or titles, for instance in the title of the 單于 *shan-yü* or Hsiung-nu sovereign. Unfortunately none of these early transcriptions has so far been restored to a certain original. But we must keep in mind the use of *yü* in the transcription of terms of Hsiung-nu origin; it will be of some moment in the discussion of the various forms mentioned for the name of Khotan.

The principal text to adduce for such a discussion is a note in Hsüan-tsang's *Memoirs* which has passed, though in a more or less curtailed form, into various works, including the *Hsin T'ang shu* (cf. *JA*, 1895, II, 362-363; CHAVANNES, *Doc. sur les Tou-kiue*, 125; WATERS, *On Yuan Chwang's Travels*, II, 299; LÉVI, in *BEFEO*, IV, 560; V, 260; STEIN, *Ancient Khotan*, 153; *Ta T'ang hsi-yü chi*, ed. of Kyōto University, XII, 24, and *Apparatus criticus*, 125-126). Here is the complete text :

«...One arrives at the kingdom of 瞿薩旦那 *Ch'ü-sa-tan-na* (\**G'ju-sât-tân-nâ*). [Note :] In the language of the T'ang (=in Chinese), [this] means 'Earth-teat' (地乳 *Ti-ju*); it is the elegant name [used] locally (卽其俗之雅言). The local language (俗語 *su-yü*) says 'Kingdom of 渾那 *Huan-na* (\**Xuân-nâ*)'. The 匈奴 *Hsiung-nu* call it 于遁 *Yu-tun* (\**Jju-d'uən*); the Hu (Iranians), 豁旦 *Ho-tan* (\**Xuât-tân*); the Hindus (Yin-tu), 屈丹 *Ch'ü-tan* (\**K'juet-tân*). Formerly [the Chinese] said *Yü-t'ien*; it is an incorrect [form].»

This note is of much greater value than has been assumed hitherto, and can be verified in most particulars.

*Ch'ü-sa-tan-na*, said to mean «Earth-teat», was restored by JULIEN as *Kustana*; and this is still the only form mentioned by STEIN (*Ancient Khotan*, 153-154), by HERRMANN (*Southern Tibet*, VIII, 440), and by THOMAS (*Tibetan Texts and Documents*, I, 18). A seeming confirmation was provided by the form *kustañāgasa* of a Kharoṣṭhī document (STEIN, *loc. cit.*; RAPSON, *Kharoṣṭhī Inscriptions*, 339<sup>3</sup>); STEIN did not revert to the question in *Serindia* or in *Innermost Asia*. It is to be regretted that no greater attention was paid to the remarks published by LÉVI in 1905 (*BEFEO*, V, 258-260). LÉVI showed that 瞿 *ch'ü* was always used by Hsüan-tsang to render *go-* or *gu-*, not *ko-* or *ku-*, and that *Gostana* meant «Earth-teat» just as well as and even better than *Kustana*. Moreover, Indian lexicographers mention *gostanī* as a name of the grape, and LÉVI explained *gostanī* as «Khotanese», the name being that of the grape of Khotan spoken of by the Chinese and by Polo. LÉVI's restoration «*Gostana*» ought to have been accepted at once; it the only one which is in agreement with the Chinese transcription (KARLGREN's *Dictionary* is here misleading; while giving the two pronunciations *chü* and *ch'ü* of 瞿 in modern Chinese, it restores only \**kju* as the ancient sound; but *ch'ü* is \**g'ju*, and it is only with this second pronunciation that the character occurs in transcriptions). As to *kustañāgasa*, it is an isolated example, with a first consonant of ambiguous value, and it occurs in a text where it may mean something other than Khotanese (cf. *BSOS*, IX, 541), particularly if we remember that the name of Khotan

in these Kharoṣṭhī documents is everywhere else Khotamna or Khotana. Moreover, we are now in a position to prove definitely that LÉVI was right. Among the documents in Khotanese which I brought back from the Tunhuang caves, BAILEY found a sort of a bilingual « manual of conversation » in Sanskrit and Khotanese; the Sanskrit name of Khotan is given there as Gaustana and Gāmstana, both graphic renderings of Gostana. But Hsüan-tsang tells us that this was « the elegant name [used] locally », which I understand to mean that, though it was not the name of Khotan in Khotanese current speech, yet the Khotanese language used Gostana as a more literary name of the country. This is confirmed by the occurrence of *Gāmsta* (= \**Gaustam*) and Gaustamā in a Khotanese document (cf. BAILEY, in *BSOS*, ix, 522, 541). BAILEY explains Gaustamā as « an inverse spelling of \**gaustam*, after *-ana* and *-ama* had become *-am*. It may be so, but the solution depends to some extent upon the very history of the name Gostana.

At the same time as LÉVI, WATERS (*On Yuan Chwang's Travels*, II, 299) was also struck by the constant value of *ch'ü* as *go-* or *gu-* in transcriptions. Starting from the other forms Yü-t'ien, Ch'ü-tan, etc., WATERS supposed that they represented « a word like Go-dan or Gothān ». « The former », he added, « is found in Türki, and the latter in the Indian vernacular, and they represent the Sanskrit *Gosthāna*. These words denote a *place* or *station* for *cattle* ». According to WATERS, it is this *Gosthāna* which, mispronounced and misunderstood as Kustana, led to « the silly legend invented to account for the name ». It is true that Skr. *gosthāna*, lit. « cow-station », means a « stable », or an « enclosure for cattle »; but the Türki word intended by WATERS, *gotan*, « enclosure for cattle (mainly for sheep) », identical with Mong. *gotan* > *hoto*, « enclosure for cattle » and « city », has of course nothing to do with *gosthāna*, nor with the name of Khotan. Something of WATERS's hypothesis may however be retained, and was in fact retained by THOMAS (in *Asia Major*, II, 261; *Tibetan Texts and Documents*, I, 18). Skr. *sthāna*, « station », « country », occurs mainly in the geographical nomenclature of the North-West, and in some cases seems to be due to the much more frequent use of its Iranian equivalent *-stāna*. I have no doubt that for instance the well-known Skr. *Cīnasthāna*, « China », which is used in a Kharoṣṭhī document (RAPSON, *Kharoṣṭhī Inscriptions*, 12, No. 35, where there is once *cina* and twice *cinasthanade*), is the Indian recasting of an Iranian \**Činastāna*, the original of the Sogdian *Činstan* (see « Cin »). The correct form of the name restored by LÉVI as « Jaguḍavardhana » (*JA*, 1915, I, 84) is Jāguḍasthāna, Zabulistan. Tibetan texts mention an Āryastana on the Gośṛṅga hill of Khotan (THOMAS, in *Asia Major*, II, 261; *Tibetan Texts and Documents*, I, 8, 18, 27, 108, 109). A Gostana, actually formed of *go* + Iran. *stāna* > *stana*, « country », could of course be interpreted as a Skr. compound of *go* + *stana* meaning « earth-teat », and this mistaken interpretation would be at the basis of the whole legend recorded by Hsüan-tsang and in Tibetan texts (I find no trace of the intermediary Prākṛit form « Wothana » mentioned by LÉVI, *JA*, 1915, I, 110); that Gostana in its turn may very well have become \**Gostan* at an early date in Khotanese, and be used as such, in « elegant » speech, alongside with what we know to have been the current form of the name in Khotanese in Hsüan-tsang's time. As to the value of the first element *go-* of Gostana, I shall come to a discussion of it farther on. It may be that a form related to Gostana left traces in Tibetan

texts, since a text translated by Sarat CHANDRA DAS (*JASB*, LV [1887], 202; cf. also WATERS, *On Yuan Chwang's Travels*, II, 299) speaks of «the country called Gosthan, or place of virtue, now vulgarly called Khothan (or Khoten)...». «Gosthan» is impossible as such in Tibetan writing (unless written «Gos-than», which is unlikely), and I do not know how the translator came to his «place of virtue». It seems that «place» is not intended by Sarat CHANDRA DAS to render *sthan* = Skr. *sthāna*, but rather *go*, which means «place» in Tibetan. With all due reserve as to the authority of the text (which is recent, and which shows traces of Russian influence) and of its translation, the fact remains that we have here, based on the Tibetan and long before the remarks of LÉVI and WATERS, a form of the name of Khotan beginning with *go*- and not with *ku*-.

The current Khotanese form was evidently the one meant by Hsüan-tsang when he says that the name in the local language was Huan-na (\*Xuân-nâ). This statement finds a striking confirmation in the late Khotanese texts, where the usual name for Khotan was Khotanese Hvatana-, Hvatāna-, later Hvamna-, Hvana-, Hvam, locative sing. Hvamnya, Hvanya; adjective *hvatanaa-*, *hvamnaa-*, *hvaṇaa-*, «Khotanese» (cf. Sten KONOW, in *JRAS*, 1914, 342; *Saka Studies*, 145; BAILEY, in *BSOS*, IX, 522, 540). Sogdian used Xwōnyk, quoted by W. B. HENNING in his *Sogdica*, p. 10, from a list of peoples. Hsüan-tsang's transcription is a faithful rendering of Hvamna = \*Hvanna, \*Ḥvanna; it shows that the original intervocalic dental of the name was already assimilated to the following *-n* in the first half of the 7th cent. But the original form is preserved in more ancient Khotanese mss. as Hvatəna-, Hvatana- (cf. LEUMANN, *Das nordarische Sakische Lehrgedicht des Buddhismus*, 3rd Part, in *Abh. f. d. Kunde d. Morgenl.*, XX, No. 3 [1936], 528).

According to Hsüan-tsang, the Hu, *i. e.* the Iranians, said Ho-tan (\*Xuât-tan); this transcription, which supposes an original \*Ḥwātan, already heralds the ختن Ḥotan of later Mussulman tradition, the modern «Khotan».

By a «Hindu» form Ch'ü-tan (\*K'juət-tân), Hsüan-tsang of course does not mean a Sanskrit form, but the one which was then in current use in the spoken language of Northern Hindus. Middle-Indian language having no *h-*, a *kh-* was the nearest possible equivalent, and this is how we must understand \*K'juət-tân, which supposes \*Khutan or \*Khōtan. But, shortly after Hsüan-tsang, a different pseudo-Sanskrit form appears in the *Fan-yü tsa-ming*, to wit Kōrttana, with the insertion of an unetymological *-r-* (cf. BAGCHI, *Deux lexiques sanskrit-chinois*, 77, 295; HENNING, in *BSOS*, IX, 570).

A last form given by Hsüan-tsang remains to be examined, the Yü-tun (\*Jju-d'uən) which the traveller says was the name of Khotan among the Hsiung-nu. Apart from the texts which repeat the gloss of the *Memoirs* on the names of Khotan, this transcription only occurs, so far as I am aware, in the *K'ai-yüan shih-chiao lu* (*Tōkyō Tripit.* of Meiji, 結, IV, 72 b) and in Hsüan-tsang's biography in the *Hsü kao-sêng chuan* (*ibid.* 致, II, 107 b); but both passages are also derived from the *Memoirs* themselves. Hsüan-tsang's information is however of the highest interest. The first point is to know what he meant by «Hsiung-nu». Of course there were no longer true Hsiung-nu in the 7th cent. At the end of the 3rd cent., Chu Fa-hu used Hsiung-nu where the corresponding Tibetan translation gives Hu-na = Hūṇa (cf. BEFEO,



v, 289-290; some of the names in the Chinese list are somewhat surprising at such an early date); the same rendering of Hūṇa with Hsiung-nu occurs in 308 in the *P'u-yao ching* (BEFEO, iv, 575-576). But the days of the Hūṇa in North-Western India were already passed when Hsüan-tsang visited India and Central Asia. In the 7th cent., the Hsiung-nu had been replaced in Central Asia by the T'u-chüeh (Turks), who, rightly or wrongly, called themselves in their inscriptions the descendants and successors of the old Hsiung-nu. On the other hand, Hsüan-tsang often speaks of the T'u-chüeh, and if he had only wanted to say that Yü-tun was a T'u-chüeh form of the name of Khotan, he might have said it in so many words. My impression is that he wanted to convey the idea that Yü-tun was the name of Khotan among the nomads of Central Asia, Mongols as well as Turks. At any rate, there can be no doubt that such was actually the case, since Yü-tun certainly represents what will soon be shown to have been the Turkish and Mongolian name of Khotan in the Middle Ages, to wit Odon.

We have seen above that the 于 *yü* (\**jju*) of Yü-t'ien and Yü-tun, which is hardly ever used in Buddhist transcriptions, is of rather frequent occurrence in early transcriptions of Hsiung-nu words. Here again, it was for a «Hsiung-nu» term that Hsüan-tsang adopted it. We may therefore conclude that it probably had an initial element which did not occur in Indian speech, perhaps some sort of laryngeal opening, followed in the present case by a labial vowel. In other words, Yü-t'ien would be \*'Odan, and Yü-tun would be \*'Odon. If we remember the History of Chang Ch'ien's missions, it would be a natural inference to suppose that the name of Khotan came to his knowledge through a Hsiung-nu channel. In agreement with the native form, the Hsiung-nu pronounced it with an *-a-* in the second syllable, as \*'Odan, but at a later period, the initial labial vowel, as has often been the case in Altaic languages, and particularly in Mongolian, labialized the vowel of the second syllable, hence \*'Odon. The case is absolutely parallel with that of Mong. *qotan*, «enclosure for cattle» and «city», which has given *qoton* > *hoton* and *hoto* (the word is already *qoton*, plur. *qotot*, in the *Secret History*, §§ 124, 195, 247, 272, except once with plur. *qotat*, § 248). In such circumstances, it may not be necessary to resort to a hypothetical pronunciation \**giu* for 于 *yü* (\**jju*) in Han times. The native name must really have begun with a guttural consonant, perhaps sonant or spirant, but we are certain that there was none at the beginning of the Altaic form in the Middle Ages. As the absence of a guttural initial in the Altaic form is of course independent of the hypothetical evolution \**giu* > \**jju* in Chinese, the simplest solution seems to be to conclude that Yü-t'ien already transcribed \*'Odan in Han times, as Yü-tun rendered \*'Odon in the 7th cent. Perhaps the Hsiung-nu language did not any more admit of an initial *g-* or *γ-* than the Turkish of T'ang times.

The result of our examination of all these ancient forms is that the name of Khotan must have existed at a very early date, at least in the 2nd cent. B. C., in a form \*Godan, which, in the first centuries of our era, was doubled by a form \*Gostāna > Gostana. It seems reasonable to suppose that the two parallel forms have the same value in different languages, or at least in different dialects, and that both mean «Place (or Country) of the \*Go». Further speculation would be risky. It may be, but it cannot be proved for the present, that the *-dan* of \*Godan is the same as the *-dan* of Ḥumdan, the ancient name under which Hsi-an-fu was

known in Central Asia. As to \*Go, the frequent occurrence in the Khotan region of names which were sanskritized as Gomati, \*Gomasālagandha, etc., is certainly striking, but I do not see that much more can be said for the present. \*Go may be the name of the country, or of the people. It has certainly nothing to do with Ch. 五 *wu* (\**nguo*), «five», nor can it, in my opinion, mean «west» or «mountains of the west», as has been suggested by THOMAS (*Asia Major*, II, 257, 259).

HERRMANN sees Khotan in Ptolemy's *Χαύρανα* (VI, 15, 4), which he proposes to correct to *Χαύρανα* (*Southern Tibet*, VIII, 452; *Das Land der Seide*, 121, 145 [where *Χαύρανα* seems to be a misprint]). *Χαύρανα* is supported, however, by *Χαυραναῖοι Σκίθαι* in Ptolemy VI, 15, 3, and by Chauriana in Ammianus Marcellinus. Although the oldest native forms I have deduced begin with *g*- it may be that the name was already sounded with an initial *h*-, in Sogdian, in Ptolemy's time. But the identification of the Ptolemean nomenclature is still for the most part so uncertain that I must abstain from dogmatizing in the present case.

While Yü-t'ien and Yü-tun presuppose \*Odan and \*Odon respectively, other ancient transcriptions show a median *-t-* instead of *-d-*. One is in the Syriac catena which mentions «the Śākiamunaye, *i. e.* the Tuptaye and the black 'Otnaye», that is to say the disciples of Śākyamuni, who are the Tibetans (see «Tebet») and the black Khotanese (cf. BIDEZ and CUMONT, *Les mages hellénisés*, II, 117). Because of the mention of the Tibetans, I do not think that this part of the catena can be older than the 7th-8th cent. (see «Sagamoni Burcan»). By its *-t-*, 'Otnaye, an ethnical name derived from 'Otn (= 'Otän), is in the line of the Iranian (Sogdian ?) Ḥwātan, but does not show the same strong aspiration at the beginning of the word. It is nevertheless certainly a «Western» form, which is not directly connected with \*Odan or \*Odon.

The Tibetan form is more surprising. There is a purely Tibetan name of Khotan, Li-yul, «the Li country», *li* being the Tibetan word for «bell-metal», in Skr. *kaṃṣa*; the name has not been accounted for (cf. STEIN, *Ancient Khotan*, 155; THOMAS, in *Asia Major*, II, 255). But Tibetan chronicles also speak occasionally of Khotan as 'U-then and 'U-ten (cf. THOMAS, in *Asia Major*, II, 256-257; *Tibetan Texts and Documents*, I, 129, 306). This is no doubt a form borrowed from the Chinese Yü-t'ien. The use of 'U-ten or 'U-then instead of \*U-den finds a parallel in the name of the Gośṛṅga Hill of Khotan, called in Chinese 牛頭山 Niu-t'eu-shan (\*Ngjəu-d'əu-šan), «Ox-head Mountain»; this Chinese name appears in the Tibetan texts relating to Khotan as 'Ge'u-to-šan or Gau-to-šan (cf. *JA*, 1914, II, 144-145; THOMAS, *Tibetan Texts and Documents*, I, 6). We have seen that the *t'ien* of Yü-t'ien (\*Jju-d'ien) is recorded as pronounced in two different tones; our modern reading Yü-t'ien is based on the *p'ing-shêng* pronunciation; read in the *ch'ü-shêng*, it would regularly give a modern *tien*. The passage from the sonant initial to the surd (with or without aspiration according to the tone) dates from *c. a. d.* 1000 in Northern China, but did not take place simultaneously over the whole of China. It may be that the Tibetans who wrote down the chronicles of Khotan in Tibetan had already heard the ancient sonant initials of the *p'ing-shêng* as surd aspirates, which would account for 'U-ten or 'U-then and for 'G'eu-to-šan or Gau-to-šan (S. C. DAS's explanation of 'U-then as Iṅa-ldan or Pañcavatī, «the Quintuple», in *JASB*, LV, 195, is absurd, and ought to have been alluded to by THOMAS, in *Ancient Khotan*, 583).

This is also the only explanation I can think of for a document concerned with Kan-chou in Kan-su, probably dating from the 10th cent. It is written in Khotanese, but the name of Khotan is given there as Yütimñi Kūhi, *i. e.* \*Yütin-kūg, a transcription of the Chinese Yü-t'ien-kuo (\*Jju-d'ien-k<sup>w</sup>ək), «Kingdom of Khotan» (cf. BAILEY, in *BSOS*, IX, 541). The transcription betrays a Chinese pronunciation which is no longer that of the 7th cent. The final consonant of *kuo* (\*k<sup>w</sup>ək) is still heard, but the initial of *yü* (\*jju) is already the modern *y*-, and *d'ien* is rendered with a surd initial, as in the forementioned Tibetan transcriptions, which are perhaps of approximately the same date.

The ancient capital of Khotan, probably abandoned in the 11th cent., began some five miles north by west of the new Chinese town of Khotan; its remains under ground, discovered c. 1870, are within the area of the group of detached villages called «Yōtkan» (STEIN, *Ancient Khotan*, 191). CORDIER quotes (*Y*, III, 44-45) a passage of PARKER according to which «Yōtkan» probably «furnishes a clue to the ancient name of Yü-t'ien». BARTHOLD (in *EI*, *s. v.*, «*Khotan*») is positive on the identity of the two names. THOMAS too says (*Tibetan Texts and Documents*, I, 8) that «Yōtkan» is «certainly the site, and perhaps retains a perversion of the name, of the ancient city of Khotan». At the same time, and while speaking of «the Chinese name *U-then*», thus admitting that Ch. Yü-t'ien and Tib. 'U-then are one and the same name, Thomas adds that we connect that name with the word «Khotan» «naturally (but perhaps mistakenly)», and thinks of explaining «Uthen» with the name of a certain *stūpa* of «A-dha-ma», mentioned in a Tibetan chronicle of Khotan (*Asia Major*, II, 257, 270). I shall not try to reconcile three hypotheses which appear self-contradictory, since I think that they must all be abandoned. Neither the history nor the meaning of يوتقان Yōtqān (such is the true spelling; cf. GRECARD, *Mission dans la Haute-Asie*, III, 127; STEIN's transcriptions do not distinguish between *-q-* and *-k-*) is known to me (does *-ō-* mark the slurring of a following *-r?*). But there is no likelihood that the specifically Chinese *y*- initial of Yü-t'ien should have had an independent unetymological counterpart at Khotan itself. It is still more difficult to imagine, since Yü-t'ien and Khotan are fundamentally one and the same name, that that name should have developed on the spot such divergent duplicates as Yōtqān and Khotan. In spite of a partial phonetic analogy, my opinion is that the two names are not connected.

I have said that the Altaic name of Khotan in the Middle Ages was Odon, in agreement with the form supposed by Hsüan-tsang's Yü-tun, \*Odon. There are other examples of such double names. The ancient name of Kučā was \*Kuči, duly rendered in Chinese transcriptions and in Brahmī spelling. But at the same time, there was a Turkish name of Kučā, Kūsān, which we can follow from the 10th. cent. down to the 16th, in Turkish as well as in Mongol and Chinese sources, and which may be much more ancient than the date at which it is attested in the texts. When Kāšyārī compiled his Turkish dictionary in 1076, \*Kuči had already passed to Kučā (? Kūčā), but, alongside of Kučā, Kāšyārī gives the synonym Kūsān (BROCKELMANN, 245). The case of Khotan is quite parallel, and we find in Kāšyārī both «Ḥotan» (Khotan) and «Odon» (BROCKELMANN, 251, where the name is transcribed «Udun»; of course Arabic spelling does not permit of a distinction between *u* and *o*).

Except for the double form Ḥotan and Odon in Kāšyārī, all mentions of Khotan in Mussul-

man sources only give Ȥotan, and we must turn to Central Asiatic and Far Eastern works for fresh mentions of the Altaïc form.

After Kāšyari, the next mention in date occurs in Yeh-lü Ch'u-ts'ai's *Hsi-yu lu*, written in 1229. Yeh-lü Ch'u-ts'ai had accompanied Chinghiz-khan in his campaign to the West (1219-1224). He never visited Khotan, but when speaking of Beš-baliq, the great centre to the north of the T'ien-shan, he devotes a few words to places of Chinese Turkestan situated more to the south; amongst them, he mentions « 五端 Wu-tuan (\*Udon) which is the same as the kingdom of Yü-t'ien of the T'ang dynasty » (*Br*, I, 16). Yeh-lü Ch'u-ts'ai was not a very strict phonetician, and the -o- of the second syllable implies in Altaïc an o- also in the first. The form he had heard must have been Odon.

A few years later, the name of Khotan appears again in the *Secret History* (§ 263) in the form Wu-tan = Udan. But this form calls for comment. The *Secret History* was compiled in Mongolian in 1240, but the scholars who transcribed it with Chinese characters at the end of the 14th cent. had no tradition to fall back on as to the true pronunciation of many proper names. The name written in Uighur-Mongol writing which they rendered as Udan could also be read \*Utan, \*Odan, \*Otan. The other Chinese transcriptions of the name during the Mongol dynasty leave no doubt that we should read Odan, not Udan (the form in the Mongol manuscript of Ulän-bätör lacks the final -n and is of no avail as it can be read Oda, Uda, Ota, or Uta). The Chinese transcription of the *Secret History* is nevertheless important in two respects. First, it shows that, in 1240, the official spelling in Uighur-Mongolian writing was still Odan, in agreement with the oldest Chinese borrowed form Yü-t'ien, \*'Odan, and though the real pronunciation must have then been Odon as in Yü-tun, \*'Odon. Secondly, if we had only Chinese transcriptions giving Odon and the very faulty Ulän-batör manuscript, we would be in doubt whether the name was pronounced Odon or Ödön. But \*Ödön would be based on a more ancient \*Ödän, and even without an actual and ancient mention of the name in Uighur-Mongol writing, we know that the original name was Odan, not \*Ödän, because if it had been \*Ödän, the transcribers, guided by the Uighur-Mongol spelling of the initial syllable, would have transcribed it \*Wu-tien, not Wu-tan.

Khotan is very often mentioned in *YS*, either as Yü-t'ien or more often as 斡端 Wo-tuan, Odon. The two forms betray the origin of the documents used by the compilers : those which give Yü-t'ien had originally been written in Chinese, while those which give Wo-tuan, Odon, had been translated into Chinese from the Mongolian. BRETSCHNEIDER (*Br*, II, 49), quoting passages from the *YS*, transcribes in five lines « Wu-duan », « Wa-duan » and « O-duan »; but the Chinese characters are the same, Wo-tuan, in all three cases. I know of no exception, save for the frequent corruption of 斡 *wo* to 幹 *kan* (for instance in *Yüan tien chang*, 9, 3 a; 34, 25 a). At the beginning of the Ming dynasty, however, the name of Khotan was given to a military colony on the western frontier of China, in the same manner as another was called Kūsän, i. e. Kučā, and this time the name is written 阿端 A-tuan, which would seem to represent \*Adon (*Br*, II, 208-210). But we have there very probably a case in which 阿 *a* is used with its *o* reading, just as we find, in the Mongol period, the name of the Russians transcribed Wu-lu-ssü, Wo-lo-ssü and A[阿]-lo-ssü (or O-lo-ssü); it is however quite certain that the name always was Urus or Oros, not

\*Aros. Consequently, even the apparent A-tuan should be read O-tuan, Odon. BRETSCHNEIDER, while allowing that the pseudo-A-tuan was a designation of Khotan in one case, thought that the military colony owed its name to the region of the «Su sing hai» (read 星宿海 Hsing-hsiu-hai) situated at the sources of the Huang-ho, the name of which means «Starry Sea» and appears in YS, 63, 1 a, as «O-duan nao-rh», a Mongol form of the same meaning as the Chinese name. But here again BRETSCHNEIDER is inaccurate. The Mongol name of the Hsing-hsiu-hai is written in the YS *Huo-tun nao-êrh*, i. e. *Hodun na'ur*. *Hodun*, «star», in written Mongol *odun*, is one of the words which were still pronounced in the Mongol period with an initial *h*-, which the Uighur-Mongol writing however did not note. No such *h*- ever occurs in the transcription of the name of Khotan, nor does it exist in the pseudo-A-tuan (to be read O-tuan). Moreover, the second character of *huo-tun* renders *-dun*, while the second character of A-tuan or O-tuan can only represent *-don*. The two names have nothing in common.

I have said that Khotan appears in the YS either as Yü-t'ien, or as Wo-tuan (Odon). There is however at least one exception : in the map of c. 1330 and in the corresponding list of YS, 63, 15 b, the name is written 忽炭 *Hu-t'an*, *Hutan* (= *Hotan*; cf. *Br*, II, 47). But the exception is only apparent, since we know that the map (and consequently the list derived from it) is of Mussulman origin. I do not think that the official workshop of 忽丹八里 *Hu-tan-pa-li* (\**Qudan-baliq?*), mentioned in YS, 85, 16 a, after the workshop of Beš-baliq, has anything to do with Khotan.

At the time of the re-conquest of Chinese Turkestan in the 18th cent., the Manchu dynasty, combining the living name *Hotan* with the ancient Chinese transcription Yü-t'ien, gave to Khotan the new administrative name of 和闐 *Ho-t'ien*. In more recent times, the old Chinese name Yü-t'ien was itself revived, but misapplied as the official designation of the district (*hsien*) created at Kerya.

The Turco-Mongol form Odon of the name Khotan can be found elsewhere. The Tibetan chronicle partly translated by SCHLAGINTWEIT, *Die Könige von Tibet (Abh. d. Bay. Ak. d. W. x [1866], No. 3, 847)* mentions in the North what SCHLAGINTWEIT renders as «the tribe of O-donkas-dkar». While thinking of Kāšyar for «kas-dkar», the translator could do nothing with «O-don». But it is evident that the chronicler spoke of «the people of Khotan (O-don) and of Kāšyar (Kas-dkar)».

I think that Odon also occurs in a Syriac text. When Rabban Çauma and the future Mār Yahbalaha III started from Peking for the Holy Land, they reached c. 1274 a city in which we all agree to see Khotan, although the name is written «Lōṭōn» in the ms. CHABOT (*Hist. du patriarche Mar Jabalaha III*, 22), followed by BUDGE (*The Monks of Kūblai Khân*, 138), corrected «Lōṭōn» to «Hōṭōn», putting the alteration to the account of a confusion in Syriac writing. I am in favour of another solution. We know that when Rabban Çauma was sent to the Pope and the kings of Europe, he wrote his diary in Persian (CHABOT, 93), and this accounts, in my opinion, for the wrong form «Önbâr» which occurs for \*Lōnbar, «Lombardy» : the confusion was made by misreading لونبر \*Lōnbär as اونبر Önbär (cf. CHABOT, Suppl. 2; BUDGE, 181). But it is not only the diary of his travels in Europe which must have been originally written in Persian. In a later part of the work, the double corruption «Baidar» instead of «Baidū» (BUDGE,

206) is difficult to explain if we start from the Syriac writing, but very easy to account for and common if the confusion be between Arabic , *ū* and , *r*. It seems to me extremely probable that the diary of the journey from China to Mesopotamia also passed into Syriac from a Persian original. The Syriac form «Hōṭōn» postulated by CHABOT's correction is unlikely, since there is no known example of the Iranian and Mussulman form Ḥōtān being written with vowels in a Semitic writing, and particularly with an -ō- in the second syllable. Our two pilgrims, of Altaic origin and living under the Mongol domination, must have known Khotan under its Turco-Mongol name Odon. Rabban Čauma wrote اوتون \*Oton (although we should have rather expected «Odon»), and it is this «Oton», which, misread لوتون \*Lōtōn, became the «Lōṭōn» of the Syriac version. The case, in my opinion, is the exact counterpart of that of \*Lōnbār misread Ōnbār.

The old Turco-Mongolian form Odon had been forgotten when the Kalmuks extended their power over Chinese Turkestan in the 17th cent. The Kalmuk map brought back by RENAT when he came back to Sweden in 1734, gives Ḥoton as the name of Khotan (BADDELEY, *Mongolia, Russia, China*, I, cxcviii). In Eastern Mongolian, the same form had prevailed, and this explains why the Mongol author 'Jigs-med nam-mkha, writing in Tibetan in 1819, mentions Khotan as «Ho-thon» (HUTH, *Geschichte des Buddhismus*, II, 17; despite BLOCHET, *Moufazzal*, 691, «Sanang Setsen's» Ḥara-ḥotan [cf. SCHMIDT, *Gesch. der Ost-Mongolen*, 259] is not Khotan, but the now well-known Ḥara-ḥoto [«Kara-khoto»] to the north of Kan-su; see «Eçina»).

But this Mongol form Ḥoton raises a new problem. In modern Mongolian, Ḥotong is the name of the settled population of Chinese Turkestan (cf. KOWALEWSKI, 916-917), and the same obtains in Kalmuk, with the pronunciation Ḥotū. RAMSTEDT (*Kalm. Wörterbuch*, 190) is in doubt whether that name is derived from *hoto*, *hotq*, «city», or from the name of Khotan (RAMSTEDT actually says in German «Chotand» [=Khotand] and speaks of «die chotandische Sprache»; it can only be a slip for «Chotan» = Khotan). The use of the term goes back at least to the beginning of the 18th cent., since RENAT has added «Kottonner», as an ethnic, to the names really given by the Kalmuk map (BADDELEY, *loc. cit.* I, CLXXIV, ccv). In the same manner, the notice due to FILISOF, written in 1734 and describing a lost companion map to that of RENAT, speaks of Western Chinese Turkestan as «the Khoton towns» and «the Khoton country». When referring to Khotan, FILISOF uses the curious expression «Khotoni Khoton» (BADDELEY, *loc. cit.*, I, CLXXXI). This seems to be Ḥoton-i Ḥoton, «Ḥoton of Ḥoton». The second Ḥoton is certainly Khotan, and I feel inclined to believe that the first one also represents it. It seems to me that Khotan, on account of its importance, had become a sort of a generic term for the whole region among the Kalmuks, so that «Ḥoton-i Ḥoton» really means «[the city] Ḥoton of [the country] Ḥoton», or «Khotan of Chinese Turkestan». The Ḥotong of modern Mongolian and modern Kalmuk is a nasalized pronunciation of Ḥoton (we know of many similar cases) which is easier to explain if we start from Ḥoton < Ḥotan, Khotan, the final -n of which had to be preserved and was actually preserved by -ng, than if we think of *qotan* > *qoton* > *χoton*, «city», with a quiescent final -n which is usually dropped in the current form *χoto*. The use of «Khotan» as a generic name of Western Chinese Turkestan finds some support in the late account translated by S. C. DAS (*JASB*, LV, 203), when it mentions, to the east of «Akasu» (= Aq-su) «Guchhe of Li-yul», which would literally mean «Kučā of Khotan»; Li-yul, «Khotan», is clearly here the equivalent of the

Mong. Hotong in its sense of «Western Chinese Turkestan and its settled Mussulman population».

On a legendary primitive name «Chia-lo-sha-mo» of Khotan, see «Cascar».

Khotan played an important part in the transmission of the Buddhist scriptures to China. At the end of the 2nd cent., the fictitious opponent in the polemical Buddhist treatise *Mou-tzū* is alleged to have visited Khotan and seen the Buddhists there (cf. *TP*, 1920, 322, 419). The first Chinese Buddhist whom we know for certain to have travelled abroad, 朱士行 *Chu Shih-hsing*, left for Khotan in 259 and died there at the beginning of the 4th cent. (cf. *BEFEO*, x, 225).

According to the *Hsin T'ang shu*, the family name of the king of Khotan was 尉遲, which nowadays is generally pronounced Wei-ch'ih (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 126, 375). STEIN (*Ancient Khotan*, 173) proposed to see in Wei-ch'ih a transcription of Vijaya, which is the first element in a long line of the names of kings of Khotan, and there is no doubt that he was fundamentally right. But we can now reach a greater precision, both from the Khotanese and from the Chinese side.

The character 尉 has two readings, *wei* (\*<sup>u</sup>ʔi) and *yü* (\*<sup>u</sup>juət), and the dictionaries and glosses, as well as the special works devoted to Chinese surnames, leave no doubt that with the second reading it was one of the components of the double surname Wei-ch'ih, which, regularly, ought to be read Yü-ch'ih (\*<sup>u</sup>juət-d'i). As a matter of fact, the popular deified 尉遲恭 Wei-ch'ih Kung, *tzū* 敬德 Ching-tō (GILES, *Biogr. Dict.* No. 2267 [585-658]) is always known in Peking as Yü-ch'ih Ch'ing-tel. This *yü* reading explains why the name of the 尉頭 Wei-t'ou or Yü-t'ou of Han times, which lay between Kāšyar and Uč-Turfan (cf. *TP*, 1907, 207), should have been revived as a *chou* of 鬱頭 Yü-t'ou, and moreover misapplied, in the administrative organization of the Chinese Turkestan created by the T'ang in the 7th cent. (cf. *Hsin T'ang shu*, 43 B, 14 b; the same name is written 蔚頭 Wei-t'ou, or Yü-t'ou, *ibid.* 8 a). In the biography of a monk who lived in China in the first quarter of the 8th cent. and who belonged to the family of the kings of Khotan, the name is written in the usual way by the *Sung kao sêng chuan*, but 鬱持 Yü-ch'ih (\*<sup>u</sup>juət-d'i) in two earlier sources (cf. BAGCHI, *Le canon bouddhique*, 552; the form given in NANJIO, *Catalogue*, App. II, No. 152, is a combination of the two forms, which I do not find in his sources). In a manuscript brought back from Tun-huang by STEIN (No. 910 of the provisional inventory), I have also noted a name 鬱遲佛德 Yü-ch'ih Fo-tê, which is perhaps the same as the Yü-ch'ih Hsien[|||]-tê found by WANG Kuo-wei in another Tun-huang document (*Kuan-t'ang pieh-chi*, *pu-i*, 24 a; either Wang or I may have wrongly copied the third character). I shall accordingly write Yü-ch'ih, not Wei-ch'ih.

CHAVANNES, finding in an inscription of A. D. 495 at Lung-mên the mention of a woman whose surname was Yü-ch'ih, thought that she belonged to the family of the kings of Khotan (*Mission archéologique*, 474). But this is a mistake. The surname Yü-ch'ih never occurs in connection with Khotan before the beginning of the T'ang dynasty; originally it was the transcription of a foreign clan name or title of the T'o-pa Wei (cf. *Wei shu*, 113, 22 b; CHANG Chu, *Hsing shih hsün yüan*, 41, 2 b-3 a; above all SU Yü's *Wei shu kuan-shih chih k'ao-chêng* [on which see «Calacian»], ff. 62-63 of my ms. copy). It is certainly to that T'o-pa clan [that the lady of the inscription of Lung-mên belonged.

The king of Khotan whose surname we are told for the first time was Yü-ch'ih sent out

his first embassy in 632. It may be that the transcription was adopted only then, though there is another possible solution. Already before 618, a Central Asian painter who, despite certain minor difficulties, was probably a Khotanese, and connected with the royal family, had come to China, where his foreign style of painting had found great favour; his fame was however surpassed by that of his son, who was summoned to the Chinese Court c. A. D. 627, and who did, among other paintings, portraits of the Khotanese royal family. The father is known as 尉遲跋質那 Yü-ch'ih Pa-chih-na (\*·l̥uət-d'i B'uât-t'í-nâ, \*Vijayavardhana ?), the son as 尉遲乙僧 Yü-ch'ih I-sêng (\*·l̥uət-d'i ·l̥ët-səng, but the personal name need not be here a transcription); on the two painters, cf. WALEY, *An Index of Chinese artists*, 98. It may be that both received the Chinese surname Yü-ch'ih only after 632, but it is perhaps more likely that, in imitation of the already existing surname Yü-ch'ih, it was given to Yü-ch'ih Pa-chih-na at an early date, perhaps under the Sui, and was extended only in 632 to all the members of the royal family of Khotan.

Since the surname Yü-ch'ih existed before it is recorded as that of the royal family of Khotan, we should expect it to be an accurate transcription, and to have been chosen only on account of a certain phonetic analogy. On the other hand, the form to be rendered was not so much Vijaya as the form which Vijaya had taken in Khotanese, and we know now that this form was Viṣa, whatever may be here the value of the small semicircle, generally indicating «that some sound had ceased to be spoken» (cf. KONOW, in *JRAS*, 1914, 342-343; *Saka Studies*, 12, 195).

But, if Yü-ch'ih was not used as the surname of the kings of Khotan before the beginning of the T'ang dynasty, that does not mean that the «surname» Vijaya does not occur earlier as a mere transcription. There was in the 1st cent. a king of Khotan called 位侍 Wei-shih (\*J'i-zi; cf. *TP*, 1907, 201), though it would be too risky to restore as Vijaya this Wei-shih, occurring as it does at such an early date and among names none of which can be restored with any probability. More important is the text of the *Sui shu* (83, 5 b) saying that the surname of the king of Khotan was 王 Wang and that his personal name was 卑那閉練 Pei-shih-pi-lien (altered to Tsao [早]-shih-mên[明]-lien in *Pei shih*, 97, 3 a). Pei-shih-pi-lien is \*Pjie [or B'ji]-d'z'i-piei-lien. We do not know how the family name Wang, so common in Chinese, came to be attributed to the king of Khotan in the *Sui shu*. But Pei-shih-pi-lien, though it is not a very strict transcription, seems to have a fair chance of representing a name well known in the Khotanese royal genealogies, Vijayavikrama (cf. *Ancient Khotan*, 582; THOMAS, *Tibetan Texts and Documents*, I, 126, 142, 143, 147, 162, 163; HOERNLE's «Turkish» hypothesis, in *JASB*, 1899, Extra-Number, 7, cannot be retained). The final part -n instead of -ma would be in agreement with a Khotanese form of the name; both -ana and -ama had given -an in that language (cf. *BSOS*, IX, 541). It seems difficult to suppose that this Vijayavikrama (?) should be the same person as the one who figures in our fragmentary lists, and who seems to have lived in the 8th cent. (KONOW, in *JRAS*, 1914, 349; THOMAS, *loc. cit.* 163); but there may have been two Vijayavikrama, just as THOMAS is led to admit that there were two Vijayavarman and two Vijayakirti.

The Khotanese king of 632 about whom the *Hsin T'ang shu* says that his family name



was Yü-ch'ih is stated to have had as personal name (*tzü*) 屋密 Wu-mi (\*Uk-mjět), and the same reading occurs in the *Ts'ê-fu yüan-kuei* (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 126; *TP*, 1904, 4). In texts of the T'ang period, the *wu* of Wu-mi is often corrupt for 屈 *ch'ü* (\*k'juət); and the *Chiu T'ang shu* (198, 8 a) actually gives Ch'ü-mi, which is no doubt correct. But neither \*Uk-mjět nor \*K'juət-mjět suggests any foreign form reconcilable with what we know of the royal lists.

The names of most of the successors of Yü-ch'ih Ch'ü-mi are given in Chinese sources as beginning with 伏闍, followed by a last character which is different for every king. CHAVANNES had read the first two characters as Fu-tu (*Doc. sur les Tou-kiue*, 325; *TP*, 1904, 99, 109). Sten KONOW was told by sinologists that the proper reading was «Fu-ch'a», and saw there «another rendering of Vijaya, or, rather, of Viśa» (*JRAS*, 1914, 347). To this LÉVI added that the second character was the regular transcription of *-ja-*, and that, in his opinion, 伏 *fu* was certainly corrupt for 吠 *fei*; *fei-shê* would render Vija = Vijaya, and would even be a more accurate rendering than the earlier Yü-ch'ih (*JRAS*, 1914, 1020-1021). THOMAS, in his recent *Tibetan Texts and Documents*, I, 162, retains, however, «Fu-tu», because he believes it to represent Boydo (*JRAS*, 1931, 832; 1937, 309-313).

The case is somewhat embarrassing. One point is not open to doubt: the first two characters are a transcription; moreover, it is not an «adopted» transcription as in the case of Yü-ch'ih, since there is no other example in Chinese of the combination of the two characters which CHAVANNES read Fu-tu. THOMAS's theory cannot be accepted. *Boydo*, «holy», is a comparatively late form in Mongolian. The Mongol spelling is *boyda*, plur. *boydas*. In the *Secret History*, we still find *boḥda* (§§ 200, 254), *boḥta* (read *boḥda*, § 254), and only once *boḥdo* (§ 121), which must be the result of a popular pronunciation at the end of the 14th cent., since the Ulān-Bātor ms., in the corresponding passage, reads *boda* (corrupt for *boḥda*). The evolution *boḥda* > *boḥdo*, *boydo*, is of the same type as *Odan* > *Odon*. Tib. *bog-to*, *pog-ta*, *Yatuk bogdo* are borrowed from the Mongolian (cf. LAUFER, in *TP*, 1916, 495; *Sino-Iranica*, 576). The word does not exist in Turkish at an early date. It is not given in Kāšyārī. The forms *poḡda*, *pyda* of some modern Turkish dialects (RADLOV, II, 1267, 1362), the pseudo-Uighur forms *poktas* of RADLOV, II, 1264, and *boydaz* of RAMSTEDT, *Kalm. Wörterbuch*, 49, are misreadings of *boydas*, which is actually given in the Sino-Uighur Vocabulary of the Ming period and is merely borrowed from the Mong. plural *boydas*. We cannot so far trace the word beyond the Mongol period; it is just as arbitrary to connect it, as THOMAS does, with the Juan-juan name 伏 闍 Fu-t'u (\*B'juk-d'uo) as it would be to identify with it the T'o-pa Wei name 伏 阨 Fu-t'o (\*B'juk-d'â; on which cf. LO Chên-yü, *Wei shu tsung-chih chuan chu*, piao, 22 b). SCHOTT's Indian etymology *bhakta* is certainly wrong (*Abh. Bay. Ak. d. W.* 1877, 5); BLOCHET's derivation from Pers. *boḥta* (*JRAS*, 1915, 308) would be more satisfactory from the phonetic point of view, but it lacks all other support. I would rather suppose with RAMSTEDT (*loc. cit.* 49) that *boyda* > *boydo* is a purely Altaic word, connected with Tung. and Oroč. *buga*, Goldi *boa* (cf. P. SCHMIDT [ŠMITS], *The Language of the Oroches*, Riga, 1928, 27).

But, apart from the history of the word *boyda*, there is a point of Chinese phonetics which I consider fatal to THOMAS's argument. Nobody knows why CHAVANNES transcribed the term as

«Fu-tu». The character 圖 has two pronunciations, *tu* (\**tuo*) and *shê* (\**d'z'ia*), but it occurs in transcriptions thousands of times, and it has never been found to be used in these transcriptions with any other value than *shê*. Its regular value in transcription is *ja* (or *jā*); LÉVI was absolutely right on this point. There is something more. In the name of one of the Khotanese kings, the texts give as the first two characters not Fu-shê, but 伏師 Fu-shih (\**B'juk-ši*), which in all likelihood is the rendering of a dialectical pronunciation of the same name or title (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 127, 207, 273; *TP*, 1904, 48); but, although *shih* (\**ši*) has no *tu* reading, THOMAS adds *tu* between brackets as an alternative sound after *shih* (*Tibetan Texts and Documents*, I, 162); this is clearly impossible.

Although I agree with LÉVI that the second character must be read *shê*, with a transcription value *ja*, I am in great doubt as to his correction of the first character as 吠 *fei* (\**b'ix'vi*). It is true that this character occurs in transcriptions, but in rendering *ve* or *vai* rather than *vi*. What is more important is that Fu-shê, and for one king Fu-shih, occur in a number of Khotanese royal names, in both dynastic T'ang histories, in the *Ts'ê-fu yüan-kuei*, and in the *T'ang hui yao* (73, 23 a), and there is not one instance in which *fei* is used instead of *fu*. My conclusion is that we must retain Fu-shê. If this is, as I think, another transcription of Vijaya, Vi-a or the like, we may imagine different solutions. One would be that the vowel of the first syllable was slurred and that the use of the first character with a final consonant chosen by a Court interpreter who was not, like Hsüan-tsang, a trained phonetician, was intended to render something like \**Vijä* or \**V'ši*. Another hypothesis would be to read *fu* with its other and less usual pronunciation \**b'iz'u*. In such a case, \**B'iz'u-d'z'ia* or \**B'iz'u-ši* would render \**Vujä* or \**Vuši*, both forms in which the initial labial consonant has developed after it a labial vowel as in Skr. *viṣaṇa* > Arab. *buṣan*. At the present state of our knowledge it would be premature to try to choose between the two explanations.

But, while I believe that Fu-shê and Fu-shih are only other transcriptions of the name Vijaya adapted somewhat earlier as Yü-ch'ih, a serious difficulty remains which LÉVI, KONOW and THOMAS have overlooked. Since both Yü-ch'ih and Fu-shê or Fu-shih represent Vijaya, we ought not to find names in which both forms occur simultaneously. We find, however, Yü-ch'ih Fu-shih (without the last character of the name, the one which is really personal) in *Chiu T'ang shu*, 198, 8 a, and in *Ts'ê-fu yüan-kuei* (cf. *TP*, 1904, 48), Yü-ch'ih Fu-shih Chan in *Hsin T'ang shu* (CHAVANNES, *Doc. sur les Tou-kiue*, 127), Yü-ch'ih Fu-shê Ta and Yü-ch'ih Fu-shê Hsiung in *Ts'ê-fu yüan-kuei* (cf. *TP*, 1904, 23, 24; p. 56, CHAVANNES's index is misleading as it gives Fu-t'u [屠] as if it were the actual reading in both passages; but this *t'u* is only a clerical error for 圖 *shê* in the text translated, p. 23; in that of p. 24, the text has *shê* as usual; moreover, the *Ts'ê-fu yüan-kuei* correctly gives twice Fu-shê Hsiung in ch. 999, 8 b, 9 a, for two embassies of 674 and 687 respectively, overlooked by CHAVANNES). If we had only the case of Yü-ch'ih Fu-shih or Yü-ch'ih Fu-shih Chan, we might suppose that, in spite of what seems natural, Fu-shih is not the same as Fu-shê, and that Fu-shih or Fu-shih-chan really is the personal name of that king. But the solution would not be possible for the Yü-ch'ih Fu-shê Ta and the Yü-ch'ih Fu-shê Hsiung of the *Ts'ê-fu yüan-kuei*. I incline to the following explanation: since the current notices of Khotan give, from the beginning of the T'ang dynasty, the family

name of the king as Yü-ch'ih, compilers have prefixed Yü-ch'ih to names which already had Vijaya represented by the later transcription Fu-shê. This is what occurred in the *Ts'ê-fu yüan-kuei*, though not completely, since it only speaks of Fu-shê Hsiung, not Yü-ch'ih Fu-shê Hsiung, for the embassies of 674 and 687. As to Yü-ch'ih Fu-shih or Yü-ch'ih Fu-shih Chan, the only instance of such a duplication in both the *T'ang shu*, we may finally have to leave its case apart and to decide that the personal name of this individual was Fu-shih or Fu-shih-chan, though I rather think that we have here the beginning of what took place on a larger scale with the *Ts'ê-fu yüan-kuei*, and that Fu-shih too represents Vijaya or Viśa.

The real personal names of the kings of Khotan in T'ang times, represented by the character which follows Yü-ch'ih, Fu-shê or Fu-shih, are not transcriptions, and do not even look like translations; it may be that the members of the Khotan royal family, which was strongly under Chinese influence, were given purely Chinese «personal names», which would not prevent them from having native names omitted from the Chinese records. Whatever the case may be, all the attempts made to connect these «Chinese» names with native royal names, by treating them either as transcriptions or as translations, have been so many failures (KONOW, in *JRAS*, 1914, 347-350; A. H. FRANCKE, *Königsnamen von Khotan*, in *SPAW*, 1928, 671-676; THOMAS, *Tibetan Texts and Documents*, I, 162-163). I must add that the Chinese list of these kings is neither complete nor necessarily correct. For the king who is called 徹 Ching in both the *T'ang shu*, CHAVANNES remarks (*TP*, 1904, 24) that Ssü-ma Kuang's *Tzū-chih t'ung-chien* gives 環 Hsia instead of Ching. Ssü-ma Kuang was a serious historian, and this reading may have some foundation; moreover, it is mentioned as a variant in the very text of the *Ts'ê-fu yüan-kuei* to which CHAVANNES appended his note. At the tomb of Kao-tsung († January 684), there are still the statues of vassals which had been placed in front of the monument, but their short inscriptions are entirely worn away. These inscriptions are partly given, however, from old rubbings, in YEH I-pao's *Chin-shih lu pu* (ed. of *Huai-lu ts'ung-shu*, 22, 7-10), and one of them reads 于闐王尉遲瓌, «the king of Yü-t'ien (Khotan), Yü-ch'ih Shu». YEH I-pao's readings are not faultless, and we might think that *shu* is a misreading for the somewhat similar *ching*. But Ching became king only in 691 (*Ts'ê-fu yüan-kuei*, 964, 9b; *TP*, 1904, 24) or 692 (*Chiu T'ang shu*, 198, 8a), and must have reigned until 728; there is no reason why his statue should have been erected among those of the vassals of an Emperor who had died in 684. I am far from being positive on the point, but it may be that Shu was Ching's elder brother; one does not, however, see how to date his reign if Fu-shê Hsiung really was Ching's immediate predecessor and only died in 691 or 692. Whatever the case may be, the inscription has the advantage of providing a contemporary instance of Yü-ch'ih (and of course not Yü-ch'ih Fu-shê) at a time when the family name of the kings of Khotan is generally written Fu-shê by the dynastic histories.

During the 11th and the 12th cents., Khotan, which had become Mohammedan, was under the domination first of the Iläk-khan, afterwards of the Qara-Ĥitai. In the beginning of the 13th cent., it was conquered by Kūčlüg the Naiman, a bitter opponent of Islam. Then came the armies of Chinghiz-khan. In the division of the Mongol Empire as appanages of the conqueror's sons, Khotan fell to the share of the house of Čayatai. At the beginning of Qubilai's reign, Čayatai's

grandson Aluyu fixed his capital at Khotan (the true form of the name, Aluyu, and not Alyu as is said generally, is guaranteed by an unpublished Uighur inscription of 1326). He first tendered his allegiance to Qubilai, but afterwards opposed him. Qubilai, after trying to bring his cousin to reason through the intermediary of envoys, finally sent an army under Buqa-tämür to conquer Khotan (YS, 120, 2 a; 133, 6 b; Br, I, 161; T'u Chi, 148, 42 b). Aluyu is supposed to have died in 1266 (BARTHOLD, *12 Vorlesungen*, 185). Qubilai's authority seems to have been supreme at Khotan for some years, and must still have been so when the Polos arrived there, probably late in 1273 or early in 1274. But the situation was to change soon afterwards. Güyük's youngest son, Hoqu (> Oqu; the «prince of Ta-ming» of Br, I, 161; cf. WANG Hui-tsu<sup>2</sup>, 33, 13 b; T'u Chi, 148, 48 a), who resided in the West and had received gifts from Qubilai at the same time as Baraq (see «Barac») in 1268, rebelled against the Great Khan and gained forcible possession of Khotan. A text of the beginning of 1276 speaks of people who had been prevented by Hoqu's rebellion from going to work in the mines of Badaḥšān for Qubilai (see «Badascian») and had then been detailed for other work; so the rebellion cannot have taken place later than 1275, and under «Yarcan» I have proposed to connect with this rebellion certain measures taken in the beginning of 1274 to ensure the security of the official postal relays in the regions of Khotan, Yärkänd and Kāšyar. But the most precise information we have on Hoqu's rebellion comes from the *History of Mār Yahbalaha III*. When he and Rabban Çauma reached Khotan on their way to the Holy Land, «there had been a war between the King of Kings (= Qubilai-khan) and the King Oqu who had fled and had come to this country where he put to death thousands of men. The roads and ways had been cut; wheat was scarce and could not be found; many people died of hunger» (CHABOT, *Hist. du patriarche Mar Jabalaha III*, 23-24). The two monks stayed six months at Khotan and then went on to Kāšyar, which they found just as badly ravaged. CHABOT (p. 181) dates their departure from the Far East in 1278. This would be irreconcilable with what we know of the events at Khotan, and CHABOT is certainly mistaken, since Mār Yahbalaha III, in 1310, says himself that he «came from the East» thirty-five years before, *i. e.* in 1275. With a margin of one year at the utmost, the stay of the two Nestorians at Khotan must be dated at the end of 1274 or the beginning of 1275; and it is likely that they had crossed the caravan of the Polos in Northern China.

Polo, who speaks of Christians at Kāšyar and Yärkänd, makes no mention of them at Khotan. It is at Khotan, however, that we hear of Christians from other sources. Gardēzi, who wrote in 1050-1052 from older sources, says that «there are at Khotan two Christian churches, one in the central city, the other in the suburbs» (BARTHOLD, *Otčēt o poézdké v Srednyuyu Aziyu* [1897], 119). According to Ĵuwainī (I, 49), copied by Rašīdu-'d-Dīn (*Ber*, III, 39), when Küčlüg the Naiman conquered Khotan, he obliged the Mussulmans to become either Christians or Buddhists. A late tradition has it that the village of Ujat, on the left bank of the Qara-qaš River, was once «inhabited by Nestorian Christians, or at least by people who were considered as insincere Muhammadans» (STEIN, *Serindia*, 96). With all due caution, STEIN draws attention to a small cave in the cliff opposite Faizābad. «That it is of pre-Muhammadan date seems to me highly probable», STEIN says, «and if that is the case attribution to either Nestorian Christians or Manichaeans naturally suggests itself.» But the explorer could only examine the cave from a distance.

As a matter of fact, the only certain Christian relic brought back from Khotan is the small bronze cross acquired by GRENARD and now belonging to the Musée Guimet. It is reproduced in GRENARD, *Mission dans la Haute Asie*, III, 134-135, with explanations by DEVÉRIA which can also be read in the latter's memoir of *JA*, 1896, II, 436-437. DEVÉRIA read the signs which adorn the Cross as Chinese characters; I have already expressed my conviction that we have there merely a cross between A and Ω, which is in agreement with the Greek sigla above and below (*TP*, 1914, 644, where I was mistaken in saying, after DEVÉRIA, that the cross had been enamelled). But the result is that we have to deal with a Melchite, not with a Nestorian monument. There have actually been Melchite communities in Russian Turkestan, and it is not surprising that some of their adepts should have reached the region of Khotan.

Khotan was always famous for its jade. The Turkish name is *qaš* (> Mong. *qas*, Kalm. *ḡaš*), on which see «Cascar». The Persians use the name *yāšm*, which is to a certain extent a misnomer, since *yāšm*, also written *yāšp* and *yāšf*, is the same as *ἰασπις* and ought to designate «jasper». Although the ancients may not have been very strict in the use of the name *ἰασπις*, Abel RÉMUSAT's long argument (*Hist. de la ville de Khotan*, 119-239), attempting to establish that the *ἰασπις* of the ancients was not our jasper, but jade (and moreover that *yāšm* [and «jasper»] and *qaš* were etymologically one and the same word) has not been countenanced by further research (on the use of the word *yāšm*, cf. also QUATREMÈRE, in *Not. et Extr.* XIV, I, 476-477). It is in agreement with the Persian use of *yāšm* that Polo (Vol. I, 147; cf. *Y*, I, 193) speaks of jade as «jasper and chalcedony»; he had in mind, as usual, the Persian term. So does also, after him, Bento DE GOES, whose language of intercourse in Central Asia was Persian, and who speaks of «jasper» when he is at Yärkänd (cf. TACCHI-VENTURI, *Opere storiche del P. Matteo Ricci*, I, 539; most of the text in *Y*<sup>1</sup>, IV, 219, is not GOES's own, but represents additions by TRIGAUT). In Russian texts, *yašma* (< Pers. *yāšm*) is often used in the sense of «jade». The Persian word has also passed into Turkish: *yašim* is said to mean «agate» at Kazan (RADLOV, III, 247), but it is quite certain that Osm. *yāšäm* or *yāšöp* means «jade», despite RADLOV (III, 380) who, labouring under the same misapprehension as under *qaš*, wrongly translates it «Regenstein», which is the meaning of *yada* or *yada*, not of *qaš* or «jade».

Our dictionaries generally explain Fr. and Engl. *cacholong*, applied by mineralogists to a sort of chalcedony, as formed with «Kash», name of a river in «Bucharia», and a Mongol word meaning «stone». The term actually seems to have come from Mongolia in the 18th cent., and to represent a somewhat undue extension of the meaning of Mong. *qas-čila'un* > *ḡas-čolön*, «jade-stone» (cf. Abel RÉMUSAT, *loc. cit.* 121-122, 127-130, 161-163; this undue extension already existed in Kalmuk, where *ḡaš-tšölün* is «marble»; cf. RAMSTEDT, *Kalm. Wörterbuch*, 171). The «Kash» river is a very small error, since «Bucharia» here means «Little Bucharia», *i. e.* Chinese Turkestan, and it is *qaš*, «jade», which enters into the name of the two main branches of the Khotan River, *Qara-qaš* («Black Jade») and *Yörüng-qaš* («White Jade»). The word «cacholong» is not given at all in BLOCH's *Dictionnaire étymologique*, nor is it included in LOKOTSCH's *Etym. Wörterbuch*.

As to the word «jade» itself, the error which connects it with *yada*, *yada*, is so widespread and tenacious that it may be of use to denounce it once more: the *yada* or *yada* stone is a bezoar and has nothing to do with «jade», either with the word or with the thing (cf. *TP*, 1912, 436-

438; 1930, 299-301). Our word «jade» comes from the Spanish *pedra de ijada*, «groin-stone», because it was supposed to cure pain in the groin (cf. the name «nephrite»). Following *Hobson-Jobson*<sup>2</sup>, 445, we generally credit Max MÜLLER with being the first exponent of this etymology, in a letter to *The Times* of January 10, 1880. But this was only a third discovery, at most. Before MÜLLER's letter, the true explanation was indicated in Father BRÜCKER's *Benott de Goès*, published in 1879 (p. 30 of the reprint). Still earlier, Abel RÉMUSAT had given the derivation «jade, de l'espagnol *ijada*, flancs, reins», in his *Histoire de la ville de Khotan* (p. 231), published in Paris in 1820.

### 182. COTROCO, see COCACIN

The name, occurring only in LT (cf. Vol. I, 88), is given as that of the tribe to which the Queen Buluyan (see «Bolgana») belonged. This might have been taken for an alteration of the name of the Onyrat or Qonyrat, who gave so many empresses and princesses to the Mongol reigning family, were it not that the latter name occurs in Polo as «Ungrat» (*q. v.*), and had we not known for certain that Buluyan was not a Qonyrat, but a Baya'ut. The mention of «Cotroco» in LT must be due to some misreading.

### 183. COTTON

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*bambace, bambax* LT

*bambagia* TA<sup>1</sup>

*bambagio, bambasina, bambaso, bombagini, bombaso* R

*bambasio, barbasteli* (?) VL

*bambasso* VB

*banbace, banbacin, banbance,*

*banbasin, banbaxe, banbece, bansin* F

*banbagia* TA<sup>1</sup>, TA<sup>3</sup>

*banbaso* V

*banbaxio* VA

*banbaxo* V, VB

*bombacinus, bombax* L

*bombatium, bonbacinus, bonbacinus, bonbix, bumbatium* Z

*bombicinus, bombicium* P

*bombix* P, Z

*coton* FA, FB

*cotoun* O

Although Polo speaks of cotton in many places, most of the mss. designate it with a different word, and «cotton» occurs only in the fragments of FO in «Franco-Italian» dialect (written «cotoun»), and (written «coton») in the mss. in Court French. This may be adduced as a further argument for a close connection between the original of FO and the *rifacimento* in Court French

(cf. *B*, xxxiii). In a notice on Persia taken over from Polo, Fra Mauro says that *goton* grows there (cf. *Zurla*, 44), but he may have modernized the text.

According to HEYD (II, 614), who cites a Genoese document bearing on 1289, the word *cottonum* occurs, occasionally, in Western texts from the end of the 13th century. As a matter of fact, in the first half of that century, Jacques de Vitry (I, 84) mentions in Palestine «arbusta quaedam ex quibus colligunt bombacem, quae Francigena cottonem seu coton appellant», and the word is found in Rubrouck (1255), who speaks once of *telas de cotone sive wambasio* (*Wy*, 166; ROCKHILL, *Rubruck*, 44, says that Rubrouck «uses the word *cottone* in several passages»; I can find only *cotone*, as a Latin ablative, and only once). In French, both Villehardouin (§ 68) and Joinville (§ 94) speak of *chapel de coton*, «cotton caps». The word is traced to the 12th century by BLOCH (*Dict. étymol.* I, 181). In the 14th century, Odoric mentions *opera goton, id est bombicis* (*Wy*, 468; *Y*<sup>2</sup>, II, 207). Pegolotti employs both *cotone* and *bambagio* (or *bambagia*; cf. EVANS's Index).

COTTON. — It is well known that «cotton» is borrowed from the Arabic قطن *quṭn* or *quṭun*, vulg. *qoṭon*, itself perhaps of Egyptian or Indian (?) origin (cf. LOKOTSCH, *Etym. Wörterbuch*, No. 1272; J. Löw, *Aramäische Pflanzennamen*, Leipzig, 1881, No. 65). From *qoṭon*, with the Arabic article, are derived Span. *algodón*, Port. *algodão*, Prov. *alcoto*, Old Fr. *auqueton*, Fr. *hoqueton*, and, without the article, It. *cotone*, Fr. *coton*, Engl. *cotton*, Port. *cotão*, Germ. and Pol. *katun*.

Practically all our dictionaries and encyclopaedias, including LOKOTSCH, connect with the Arabic *quṭn* the Hebrew *k'tōneʿ* (or *k'ttōneʿ*), «jerkin», > Greek χιτών, κιθών, and, indirectly, German *Kittel*. But it cannot be a direct derivation. The true Arabic representative of *k'tōneʿ* (where *-eʿ* is a feminine ending) is Aramaic *kettān* or *kittān* > Arabic كَتَان *kättān* (vulg. *kātān*), which does not mean «cotton», but «flax». NÖLDEKE had already supposed that the word may have reached the Greeks from the Phoenicians, who perhaps pronounced it \**kuṭ'ōn*, without the feminine ending (cf. H. LEWY, *Die semit. Fremdwörter im Griechischen*, Berlin, 1895, 82; Löw, *loc. cit.* No. 172). As a matter of fact, VIROLLEAUD informs me that in the Semitic texts of Ras-Šamra, where *k-* and *q-* are carefully distinguished, the corresponding word is written *ktn*. It is certainly this word which lies at the origin of the Gr. χιτών, although it is not even mentioned by LOKOTSCH. The question of an etymological connection between Aram. *kettān* > Ar. *kättān*, «flax», and Ar. *quṭn*, «cotton», is a different problem. As there is no Semitic etymology for either, and as the names for «flax» and «cotton» have more than once been confused (cf. *infra*, pp. 529-530, for a discussion on the meaning of βύσσανος), it may well be that both represent, as was supposed by NÖLDEKE, one and the same foreign word, but it must then have passed into Arabic from different sources and at different dates. However it may be, one fact is certain, namely that from the early Middle Ages, the words *quṭn*, «cotton», and *kättān* (*kātān*), «flax» or «linen», must be kept separate.

*Quṭn* does not seem to have reached Central Asia. KLAPROTH's pseudo-Uighur word «kedin», which he derived from *quṭn*, will be shown farther on to be a mistaken emendation. A

word *küddän* occurs in the *Qutadyu bilig* (RADLOV ed., II, 212), and RADLOV renders it «cotton stuff», but, at the same time, in his *Dictionary* (II, 1486), refers the reader to *kädän*, a dialectical form of *kätän* (< *kätän*), which is not cotton, but linen; moreover, *küddän*, in the said passage, is not the name of a textile, but means «guest» (BANG retained RADLOV's erroneous translation). It is *kätän*, not *qoṭon* as DULAURIER says, which is transcribed *ḥthan* and translated *tella* (= *tela*, «linen», not «cotton stuff» as in DULAURIER) in an Armenian document of 1288 (*Rec. Hist. des Croisades*, Arm., I, 750). The *chetan* of the *Codex Cumanicus*, always rendering Latin «*tella*» (*tela*), does not represent the Ar. *quṭun* as is said in KUUN's edition, p. 270, but *kätän*, as already corrected by RADLOV (*Das türk. Sprachmaterial des Codex Cumanicus*, 31). The same mistranslation «cotton» of *kättän* occurs in A. GRAY, *Pyrard of Laval*, II, 416. A Chinese term *ku-chung*, formerly, though erroneously, derived from *quṭun*, will be dealt with later. Only in Persian, a derivative form *quṭunī*, *quṭnī* (hence late Osm. *quṭnī*, *quṭnu*, Russ. *kutnya*) has been adopted, as a designation of a mixed textile of silk and cotton (cf. VULLERS, II, 730; BERNEKER, *Slav. Etym. Wörterbuch*, I, 653). From Persian *quṭunī*, and not from *kättän* as doubtfully proposed by YULE (*Hobson-Jobson*<sup>2</sup>, 289), are derived Anglo-Indian «cuttanee», Port. *cotonia*, French *cotoni* (in TAVERNIER), Dutch plur. *cotonias* and *cotonyen* (in KERN, *Linschoten*, I, 42, 153), Konkani and Mahrati *kutnī* (cf. DALGADO, *Influência do Vocabulário Português*, 65).

BAMBACE. — The word generally used for cotton in Polo's mss. is «bambace» in French (spelt in different ways), and «bumbatium», «bombax» or «bonbix» (gen. «bonbicis») in Latin. Its history is of some interest (cf. LOKOTSCH, No. 1617). Low Latin *bambagium* and *bumbatium* (< *bambacium*), like Greek *πάμβαξ*, *παμβάκιον* are borrowed from Middle Pers. *pānbāk* (*pāmbāk*), «cotton». The latter word occurs also as *bambak* in Ossetian and Armenian (cf. HÜBSCHMANN, *Arm. Grammatik*, I, 116; *Rec. Hist. des Croisades*, Arm., I, 750). The modern Persian *pānbā* (< *pānbāk*) seems to be the basis of Osm. *pāmbā*, meaning «pink». Under the influence of the preceding labial consonant, in many forms the -ā- has been changed into -u- in the second syllable: such are, in Turkish, Uiy. *pamuq* (γ uzz dialect; BROCKELMANN, 138), Türkm. *banbuq* (HOUTSMA, *Ein Türk.-Arab. Glossar*, 64), Com. *mamuy* (*Cod. Cum.* 41 a, KUUN ed., 92 [where «magugh» is a copyist's error], 139), *mamiq* (HOUTSMA, 101), Čay. *pamuq* and *mamuq* (POPPE, *Mongol'skii slovar' Mukaddimat al-Adab*, 218), Osm. *pamuq* and *pambuq* (> Serb. *pamuk*). But, in another group of forms, the action of the labial consonant has been retrogressive: hence Rouman. *bumbac*, Bulg. *bubak*, Russ. *bumaga* (now meaning «paper», but the adjectival form *bumažnyī* is still used in the sense of «of cotton»), Lat. *bumbacium*, *bombacium*, *bombax*, French (originally adjectival) *bombasin* (cf. GAY, *Glossaire archéologique*, I, 173; HUGUET, *Dict. de la langue française du xv<sup>e</sup> siècle*, I, 618; W. VON WARTBURG, *Franz. Etym. Wörterbuch*, II, 229; > *basin* [but the *bansin* of F, in B, 38<sup>85</sup>, seems to be an instance of haplography, for *ban(ba)sin*, given in B, 203, 212]; > French *beige* is highly hypothetical), Engl. *bombasine*, etc. *Bombax* is now the botanical name of the silk-cotton tree; so the «cloth of bombax» of ROCKHILL, *Rubruck*, 44, is misleading, not to speak of the «cloth of Bombay» of his index, 286. As for «bonbix», this word is the result of a confusion with Greek *βόμβυξ*, «cocoon of the silkworm». But, curiously enough, this Greek word too must have been borrowed from the Persian at an early date, and was so adopted on



account of the similarity between the capsule of the cotton plant and the cocoon of the silkworm (cf. E. BERNEKER, *Slav. etymol. Wörterbuch*, I, 101). This similarity struck the Chinese too, and is made use of in the first Chinese description of the cotton plant growing in the Turfan region (cf. *infra*, p. 433). The mediaeval use of a name of cotton which resembled, and sometimes was identical with, that of the cocoon of the silkworm has given rise to serious misunderstandings. In his papers on the Comans, RASOVSKII (*Seminarium Kondakovianum*, IX [1937], [Reference ?]) mistakes the *bonbecium* of the *Codex Cumanicus* (KUEHN ed., 92), which is «cotton», as meaning «silkworm». YULE himself, partly following HAKLUYT, II, 60, went astray on this point. When Odoric says that the inhabitants of Cansay (*i. e.* Hang-chou; see «Quinsai») pay every year to the Great Khan a tax of one *balis* (< Pers. *balīšt*, «cushion», the Persian name of the silver «shoes» or ingots, *ting*, in the Mongol period), *id est quinque cartas bombicis*, YULE, following moreover a secondary reading which inserts *ad instar* after *cartas*, translated (*Y*<sup>1</sup>, II, 196) «five pieces of paper like silk»; he had clearly forgotten the *opera goton, id est bombicis* of the other passage. In Odoric's time, the *ting* of paper money was again valued at one-fifth of its nominal value in silver; it was only one-tenth in Polo's time.

This leads us to another remark. Just as Odoric speaks of the Chinese paper-money as *cartae bombicis*, Rubrouck says that the usual money in China was *carta de wambasio*. Apart from PAUTHIER (*Pa*, 327), who, reviving an old error of BAYER, *Museum Sinicum*, De litteratura, p. 76, absurdly maintained that *wambasium* meant «bamboo», all the translators and commentators have seen that *wambasium* was the same as *bambacium*, «cotton»; moreover, Rubrouck had said so himself (*de cotone sive wambasio*; cf. *supra*, p. 426; MALEÏN, *Istoriya Mongalov*, 66, was mistaken when he translated the two words as designating two different cotton goods). Consequently, all the translators have spoken of the Chinese paper money as having been made of cotton paper. The tradition of the «cotton paper» is an old one among scholars, but it has been established by KARABAČEK (*Das arabische Papier, Mitteil. a. d. Sammlung Papyrus Erzherzog Rainer*, II-III, 43 ss.), KOBERT (*Ueber das älteste in Deutschland Gefindliche echte Papier*, 5, reprinted from *Der Papier-Fabrikant*, Festheft 1911), and WIESNER (in *Sitz. d. K. Ak. d. Wiss. in Wien*, Ph.-Hist. Kl., vol. 168, 5th Abhandl., 5-6) that cotton never entered into the fabrication of any mediaeval paper. For its use as one of the three constituents of Turkestan paper in the 18th century, cf. LAUFER, *Sino-Iranica*, 562. As a matter of fact, *carta bombacina*, of which Rubrouck's *carta de wambasio* and Odoric's *carta bombicis* are merely other forms, was a ready-made term which meant nothing more than «paper».

This is not without some importance in the case of the Chinese paper money, because there was not yet very much cotton grown in China, save in Kuang-tung and Fu-chien, in the days of Rubrouck, Polo, and even Odoric. Polo, who speaks of cotton as growing abundantly in Persia, in the western oases of Chinese Turkestan, and in India, does not mention any place in China proper where it was cultivated. A passage which is found only in FB says of the Chinese that «it is true that they have cotton and hemp in some places, but not enough to satisfy them» (Vol. I, 237). As a matter of fact, although cotton had begun to be cultivated in many parts of China when Polo visited that country, most of the cotton goods, and the best ones, were still imported from abroad. But the history of cotton in China, either imported or indigenous, is still far from being elucidated.

The preliminary results reached by WATTERS (*Essays on the Chinese language*, 439-440), HIRTH and ROCKHILL (*JNCB*, XXI, 230-231, and *HR*, 217-220), and LAUFER (*Sino-Iranica*, 488-492 and 574) are sadly in need of correction and elaboration. Although I am not a botanist, and moreover cannot undertake here a thorough examination of the problem, I think it will be worth while to record a certain number of facts which I have ascertained.

GOSYPIUM AND BOMBAX. — Before examining the various terms which have been used in Chinese to designate cotton, some preliminary explanations are necessary. Cotton, properly so called, is the produce of a malvaceous plant, the *Gossypium*. There are many species of *Gossypium*; for our present purpose, they may be reduced to two, the *Gossypium herbaceum*, which is a plant of little size, and the *Gossypium arboreum*, which is a small tree, reaching three and sometimes even six or seven yards in height. In the Middle Ages, it was *Gossypium arboreum* which was most generally planted in tropical or subtropical regions like Indo-China, and in parts of Yün-nan, Kuang-tung, and even Fu-chien, whereas only *Gossypium herbaceum* was grown in the provinces of Chê-chiang and Chiang-su which soon produced most of the cotton stuffs used throughout the country. But there is also a lofty tree, of imposing proportions, the *Bombax*, or silk-cotton tree, the fruit of which yields a floss, the kapok, which is used for stuffing and padding purposes. Although recent botanists are inclined to speak of it as *Bombax pentandrum*, or *Bombax ceiba*, or *Ceiba pentandra*, there is only one kind of *Bombax* in Asia, with varieties, and we may retain for it the earlier and more common name *Bombax malabaricum*. Now, the whole problem of the history of cotton in the Far East is dominated by the confusion which has been made by Chinese scholars between *Gossypium arboreum* and *Bombax malabaricum*. But Western scholars might have known better. Speaking of *Gossypium herbaceum*, STUART (*Materia Medica*, 197) says that « this malvaceous plant, which yields the cotton wool, and which is the same as *Gossypium indicum*, is not distinguished in Chinese works from the sterculiaceus *Bombax malabaricum*, the cotton tree. The reason for this probably appears in the fact that the cotton tree was known in China from very ancient times, and its cotton was used by the Chinese in the manufacture of cloth before the introduction of the cotton plant ». But, as we shall see, there is no indication that the Chinese ever used the floss of the *Bombax* for making any cloth. Moreover, there is not one single word in STUART's book about *Gossypium arboreum*, which is precisely the one species which lies at the basis of the confusion. In the same way, LAUFER (*Sino-Iranica*, 491), finding in a Chinese work a plant the name of which was said to be synonymous with *mu-mien*, « cotton », automatically equated it with *Bombax malabaricum*, because *mu-mien* literally means « tree floss », and in spite of the fact that the very author he cited has expressly stated that the plant in question was « the *mu-mien* which was like a plant », not « the *mu-mien* which was like a tree »; but the *Gossypium arboreum* is absent, too, from LAUFER's notes on cotton. The only Western authors who have written on cotton in China and introduced *Gossypium arboreum* are HIRTH and ROCKHILL (*HR*, 219); they give it as the equivalent of *mu-mien*, and translate the latter term « tree-cotton ». But we shall see that *mu-mien* applies to both *Gossypium arboreum* and *Gossypium herbaceum*, not to speak of its confusion, perhaps even at an early date, with *Bombax malabaricum*. To avoid misunderstandings in the following notes, I shall never speak of *Bombax malabaricum* as the « cotton tree », but always as

the «silk-cotton tree», which is its accepted designation in English. By «cotton tree», I shall always mean *Gossypium arboreum*, and, by «cotton plant», *Gossypium herbaceum*, both being designations of true cotton.

TOU-LO-MIEN. — A first term which Chinese commentators have generally equated to cotton is 兜羅綿 *tou-lo-mien*, «*tou-lo* floss», in which *tou-lo* (\**t<sub>2</sub>u-lā*) undoubtedly transcribes Skr. *tūlā*. HIRTH was in error when he said (*HR*, 219) that *tou-lo-mien* did not occur before Chao Ju-kua, *i. e.* before c. 1225; I have found it in a Buddhist translation as early as the end of the 3rd cent. A. D. (cf. *TP*, 1933, 429-430; *Tōkyō Tripit.* of Meiji, 爲, VIII, 84 b, 163 a; cf. also CHAVANNES, *500 Contes*, II, 179, 265). Translators of the early T'ang period adopted new forms, 妬羅綿 *tu-lo-mien* (*ibid.* VII, 98 a; IX, 192 a), 蠶羅綿 *tu-lo-mien* (*ibid.* VII, 87 a; VIII, 187 a), 堵羅綿 *tu-lo-mien* (*ibid.* VIII, 51 b, 69 b), and 靚羅綿 *tu-lo-mien* (in the *Mahāprajñāpāramitā*; cf. 爲, I, 16 b), which never became of common use. Fa-yün, the Sung author of the *Fan-i ming-i chi*, gives 細香 *hsi-hsiang* as the meaning of *tou-lo* (*ibid.* 兩, XI, 87 b); but this is a corrupt reading for 細粟 *hsi-juan*, «thin and soft», given in an early T'ang gloss (cf. *ibid.* 爲, VIII, 51 b, 69 b). Fa-yün adds that, according to Hui-yüan, *tou-lo* means «ice», and 兜沙 *tou-sha* «hoar-frost». This comes from an absurd note of Hui-yüan, who maintained (*ibid.*, VIII, 151 b; X, 129 a, 147 b) that *tou-sha-lo*, a perfectly correct transcription of Skr. *tuṣāra*, «hoar-frost», ought to be written *tou-sha-tou-lo*, and was formed with *tou-sha*, «hoar-frost», and *tou-lo*, «ice» (he was perhaps led to this etymology by the white and flossy appearance of the hoar-frost); this *tou-lo*, which may have to be connected with the mysterious \**tudyu*, «ice», of BACCHI, *Deux lexiques sanskrit-chinois*, 70, 292, is puzzling, but certainly has nothing to do with the *tou-lo*, *tūlā*, of *tou-lo-mien*. The latter *tou-lo* also occurs either alone (*Tripit.* 爲, IX, 167 b), or as *tou-lo*-[毳] *érh*, «*tou-lo* tuft» (*ibid.*, VIII, 166 a). Despite the glosses, I suspect that 毳 *érh* (\**ńízi*) may have to be here pronounced *jung* (\**ńíziung*)-like 茸 *jung* (\**ńíziung*), and may be in fact the prototype of the later 絨 *jung*, 絨 *jung*, 絨 *jung*, and 絨 *jung*, «floss», «a textile with a nap», and finally «velvet» (cf. the formation of 耳 *érh* [\**ńízi*] and 茸 *jung* [\**ńíziung*]). In a polyglot list of the eighty «secondary signs» (*anuvyañjana*) of the Buddha published by DE HARLEZ (*TP*, 1896, 370), the forty-fourth sign is that, according to the Chinese text, the Buddha's «hands were like *tou-lo-mien*» (in DE HARLEZ's paper 錦 *chin* is a faulty reading instead of 綿 *mien*; on this frequent alteration, cf. *TP*, 1933, 429), and the corresponding Sanskrit term, very corrupt, is *kulapatriśasu*, which DE HARLEZ boldly rendered «the skin of his hands was like the *kulapatri*»; a note adds that the *kulapatri* is the «cotton tree». But *kalapatri* does not exist; the true form was either *tūlasadṛśasu*[*pāṇi*], «having fine hands similar to *tūla* (cotton)», as partly suspected already by BURNOUF, *Le Lotus de la Bonne Loi*, 587-588, or *tūlapuṭikāsu*[*pāṇi*], «having fine hands [like] cotton cloth», for which cf. *tūlapuṭikā* in *Mahāvīryūtpatti* No. 5874.

Most Chinese commentators have explained *tou-lo* as «cotton» (*mu-mien*) or «cotton down», but Tao-hsüan (7th cent.) maintained that it was a designation of willow and reed catkins as well (爲, VIII, 51 b, 69 b). There is in fact a decisive text to that effect in the *Vinaya* of the Sarvāstivādin (*Shih-sung li*, ch. 18, in 張, IV, 15 b); and the opinion has been expressed that *tou-lo* could even be referred to the cocoons of wild silkworms (cf. ODA Tokuno, *Bukkyō dai-jiten*, 1278). The

more extended use is in agreement with the use of *tūlā* in Sanskrit texts, but it is with reference to special kinds of cotton goods that *tou-lo-mien* has long survived in Chinese lay texts of the Southern Sung, Yüan, and Ming dynasties (cf. *HR*, 219; *TP*, 1933, 429-430; 1934, 306). Chao Ju-kua makes of *tou-lo-mien* the name of the best quality of cotton goods (*HR*, 218); at the beginning of the 15th cent., it was the designation of cotton velvets (Ar. *maḥmal*). WANG Kuo-wei's attempt to connect with *tou-lo-mien* the *t'u-lu-ma* of Ch'iu Ch'ang-ch'un's *Hsi-yu chi* (cf. his edition, I, 24-25) is decidedly a failure (cf. *infra*, p. 514). *Tou-lo-mien* is given as a product of Burma in *Ming i-t'ung chih*, 87, 35 *a* (it is altered into *tou-lo-[錦]chin* in the corresponding passage of the *T'u-shu pien*, 85, 51 *a*) and of Bengal in WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao* (33, 14 *a*). The *Ming shih* (324, 10 *a*) mentions in Sumatra 兜羅被 *tou-lo-pei*, «*tou-lo blankets*», for which I doubt whether GROENEVELDT's «*blankets of woollen cloth*» (*Essays relating to Indo-China*, 2nd series, I, 193) is a correct rendering. These are, moreover, certainly the same as *tou-lo-chin pei* mentioned among the objects of tribute offered by the states of Sumatra in *Hsü Wên-hsien t'ung-k'ao* (33, 12 *a*, 13 *a*); here again I suspect that *tou-lo-chin pei* is a corrupt reading of *tou-lo-mien pei*.

The *tou-lo-mien* of the Ming dynasty must have been a strong fabric, since the author of the *Tung-hsi yang k'ao*, after mentioning the *tou-lo-mien* of Siam (2, 13 *b*), adds: «*罽奢夫 CH'ü Jui-fu says: 'As to the tou-lo-mien, neither sword nor arrow can pierce it.'*» Jui-fu is the *tzü* of both 罽智 Ch'ü Chih of the Yüan, and 罽九思 CH'ü Ch'iu-ssü of the Ming; the one here meant is almost certainly CH'ü Ch'iu-ssü (c. 1550-1620; cf. *Ming shih*, 288, 3 *a*).

WATTERS (*Essays on the Chinese language*, 439) says that *tou-lo* was used as a «*general name*» when European velvet, plush, and woollen goods were first introduced into China, and that we thus find «*兜羅呢 tou-lo-ni* (now *to-lo-ni*) and 兜羅絨 *tou-lo-jung*, for woollens and velvets respectively». I doubt the existence of both *tou-lo-ni* and *tou-lo-jung*, or at least I can find no authority for these forms. But it is true that a kind of woollen velvet imported into Canton in European vessels is mentioned in the dictionary 正字通 *Chêng tzü t'ung*, first published in 1670-1672 [cf. COURANT, *Catalogue*, Nos. 4464-4512; I do not know why WATTERS says (*Essays on the Chinese language*, 87) that it was first published in 1705], as 多羅絨 *to-lo-jung* (s. v. 髹 *t'a*; cf. also *K'ang-hsi tzü-tien*, s. v. 髹 *t'a*). On the other hand, broadcloth has been known in China, in modern times, as 哆囉呢 *to-lo-ni*, the origin of which is obscure, and there is a still more obscure 廣東哆囉嗎 «*to-lo-ma* of Kuang-tung», said to be a fabric of hemp or ramie manufactured in Kuang-tung (cf. KIUNG and COURTOIS, *Quelques mots sur la politesse chinoise* [*Var. Sin.*, No. 25], 33 and 109, where the transcription «*tou-lou-ma*» is not correct). I do not know the origin of *ma* in *to-lo-ma*. As to the *ni* of *to-lo-ni*, it enters into a number of names of European cotton and woollen textiles, such as 大呢 *ta-ni*, «*great ni*», another name for «*broadcloth*»; 小呢 *hsiao-ni*, «*small ni*», «*cassimeres, flannels*»; 俄呢 *O-ni*, «*Russian ni*», «*Russian cloth*», etc. The authors of the *Tz'ü-yüan* (s. v. 呢) say that *ni* was originally written 尼 *ni*, and cite a poem of Huang T'ing-chien (1045-1105) in which already this 尼 *ni* occurs. But the chronological gap is a large one, and moreover Huang T'ing-chien does not speak of a cotton stuff, but of camel-hair; and the identification cannot be accepted unless intermediary and more decisive examples are discovered. Pending this, one might suppose that 呢 *ni* in «*great ni*», «*small ni*», etc., is nothing more than an aphoristical abbreviation of *to-lo-ni* itself. As to *to-lo-ni*, HIRTH has already called attention (*JNCB*,

xxi [1887], 230) to a passage in which the Fukienese author of the *Tung-hsi yang k'ao* (*Hsi-yin-hsüan ts'ung-shu* ed., 4, 7 b; cf. also 6, 18 a; 7, 12 b), who wrote in 1617-1618, speaks of the *tou-lo-mien* of the north-western part of Sumatra « which is now called 哆囉哩 *to-lo-lien* ». HIRTH read the last character *nien*, a pronunciation for which I can find no authority in Mandarin; yet he was certainly right when he identified this *to-lo-lien* with the later *to-lo-ni*. Let it be remarked by the way that this older form *to-lo-lien* does not support the connection made by the *Tz'ü yüan* between the *ni* of *to-lo-ni* and the *ni* used alone in Huang T'ing-chien's poem. As to 哆囉 *to-lo*, it has been customary, since WATTERS' day, to say that it was also written 多羅 *to-lo* and that the latter *to-lo* was sometimes used as a designation of the cotton tree (cf. WATTERS, *loc. cit.* 439; GILES, *Chin.-Engl. Dictionary*, No. 11302; TARANZANO, *Vocabulaire des Sciences*, II, 743). But in Chinese mediaeval texts 多羅 *to-lo* (\**tâ-lâ*), as a botanical term (in other contexts it has been a designation of a cosmetic box; cf. *T'ai-p'ing yü-lan*, 717, 5 b), always transcribes Skr. *tâla*, the palmyra, *Borassus flabelliformis* (cf. *Hobson-Jobson*, s. v. « tallee » and « talipot ». In *Essays relating to Indo-China*, 2nd series, I, 193, GROENEVELDT, who adopts a dialectical pronunciation « to-lo » of *tou-lo*, refers the reader to D'HERVEY DE SAINT-DENYS, *Ethnogr. des peuples étrangers, Méridionaux*, 474, n. 33; but this is a palpable error since, in that note, D'HERVEY DE SAINT-DENYS speaks of *to-lo*, *tâla*, not of *tou-lo*. Moreover, although TARANZANO gives both forms, in all the Chinese examples of *to-lo-ni* I know, the characters 哆囉 *to-lo* are used (and these we find also in the earlier *to-lo-lien* of the *Tung-hsi yang-k'ao*), not 多羅 *to-lo*, and I have met the latter form only in the *to-lo-jung* of the *Chêng tzü t'ung*, a woollen velvet. It looks as though WATTERS' assignation of the secondary meaning « cotton » to 多羅 *to-lo* were a mistaken inference drawn from the term *to-lo-ni*. That the *to-lo* of *to-lo-jung* and *to-lo-lien* or *to-lo-ni* is due to a confusion with *tou-lo* is probable enough (as was already suspected in 1868 by MAYERS, in *Notes and Queries*, II, 95; cf. also HIRTH, *China and the Roman Orient*, 249), but the confusion is of late popular origin (later than the phonetic evolution from \**tâ-lâ* to *to-lo*), and it has nothing to do with the old term *to-lo* which, as a botanical name, only represents *tâla*, never *tûlâ*. My provisional conclusion is that *to-lo-ni* is an adaptation in Mandarin of a local term used in Amoy and Chang-chou. In the Amoy and Chang-chou dialect, this name is *to-lô-ni*<sup>n</sup> (cf. C. DOUGLAS, *Chin.-Engl. Dict. of . . . Amoy*, 335-336), and it is a matter of very frequent occurrence in that dialect that an initial *l*, so pronounced in literary usage, becomes *n*- in popular speech. Thus *to-lô-ni*<sup>n</sup> exactly represents the *to-lo-lien* of the author of the *Tung-hsi yang k'ao*, himself a native of Chang-chou. The *to-lo-ni* of modern Mandarin, in its turn, may transcribe the popular Amoy pronunciation *to-lô-ni*<sup>n</sup>, in which the final *-n* is practically mute. If so, *lien* would be the true earlier form of *ni*<sup>n</sup> > *ni*, and its connection with the *ni* used by Huang T'ing-chien must probably be discarded. The only remaining difficulty would be to explain *lien*, since there are in the Amoy dialect interchanges between *l*- and *m*-, but not in the case of 綿 *mien*, which is there pronounced *biên* in literary usage, and *mi*<sup>n</sup> in popular speech. On the other hand, the other solution would remain possible, if we should suppose that, as is often the case in the Amoy dialect, the final *-n* of *lien* and *ni*<sup>n</sup> is itself of secondary appearance, and that the *l*- of *lien* instead of *n*- is a mistaken purism on the part of the author of the *Tung-hsi yang k'ao*; but I hold this to be less probable. *To-lo-jung* may, in its turn, have been created on the model of *to-lo-ni*. In my opinion, *to-lo-lien* and *to-lo-ni*, as well as the later terms formed

with *ni*, are interesting vestiges of the time when most of the foreign trade of China was carried on at Amoy and Chang-chou.

KARPASA > KU-PEI. — Of greater significance than *tou-lo-mien* is another name of cotton, which is in fact one of the few terms to have travelled all over the ancient world. The cultivation of cotton probably started in India. Now, a usual Sanskrit word for cotton and cotton goods of any sort, be they the produce of the cotton plant, or of the cotton tree, or even of the silk-cotton tree (if there have ever been any such, which I doubt), is *karpāsa* (> *kārpāsa*). In the West, it was anciently borrowed in such forms as Hebrew *karpas* (*Esther*, I, 6), Greek *κάρπασος*, Latin *carbasus*. But it has also passed to Central and Eastern Asia. In Persian, the word occurs as *kirpās* in the *Šāh-nāmāh* (VULLERS, II, 811-812); from the Persian it was borrowed as *kirbās* in Arabic and *kerpas* in Armenian (cf. SAINT-MARTIN, *Mém. sur l'Arménie*, II, 394, 468). The Persian word is given as *kārbās* in the Sino-Persian Vocabulary of the Board of Translators, with the translation 布 *pu*, « cloth » (cf. also *TP*, I, 11; *China Review*, XIX, 134). As to Central Asia, it is remarkable that no word for cotton occurs in the Kharosthī documents of the first centuries of our era, which contain so many names of textiles (cf. LÜDERS, *Textilien im alten Turkistan*, in *Abhandl. d. Pr. Ak. d. W.* 1936, No. 3, 38); nor have any fragments of cotton stuff prior to the T'ang dynasty been hitherto discovered in Chinese Turkestan. This goes a long way to show that cotton was not yet much grown in that region (for a seeming exception, cf. *infra*, p. 491). But in the beginning of the 6th cent. conditions had already changed. In the *Liang shu* (54, 13 b), which is based on documents of 502-556 and the author of which died in 637, we read that in the country of Kao-ch'ang, *i. e.* the Turfan region (see « Carachoço »), « there are many shrubs, the fruit of which is like a cocoon; in the cocoon are threads like fine hempen threads, and it is called 白疊子 *po-tieh-tzū* (*tzū* is an affix of the spoken language). The people of the country commonly use it to weave into cloth (布 *pu*). This cloth is extremely soft and white, and it is used in trade exchange (with other countries).» This text, which has generally been quoted from the later *Nan shih* (79, 7 a) into which it has been copied, has been interpreted by CHAVANNES as a possible reference to cotton, but with much qualification (*Doc. sur les Tou-kiue*, 102, 352), and CHAVANNES has been followed in this hesitation by F. W. K. MÜLLER (*Uigurica*, II, 70, 105). LAUFER, not without some reluctance, admitted that cotton was referred to in this text (*Sino-Iranica*, 490). As a matter of fact, no doubt is possible. It will soon be seen that, as early as the end of the Han period, *po-tieh*, « white *tieh* », was probably used in the sense of « cotton goods »; and, at any rate, such was its meaning among the Chinese of the Turfan region and practically all over China in pre-T'ang and T'ang times. Suffice it to say here that, in a Sino-Sanskrit vocabulary of the T'ang period, which is the work of a monk from Kučā, *po-tieh* is rendered *karpāsa*, « cotton » (cf. BACCHI, *Deux lexiques sanskrit-chinois*, pp. 47, 279), and that I-ching expressly gives *po-tieh* as the Chinese equivalent of Skr. *kārpāsika* (𑖦𑖫, VIII, 87 a).

We are not aware of the native name under which cotton went in the Turfan region before the Uighur occupation. But the Uighur word is well known, it is *kübāz*, given by Kāšyar (BROCKELMANN, 101) and occurring several times in Uighur documents from Turfan (cf. MALOV'S

edition of RADLOV's *Uigurische Sprachdenkmäler*, 282). Now, *käbáz* is clearly derived from a Prakrit form of *karpāsa* (cf. Pali, *kappāsa*). In the Ming Sino-Uighur Vocabulary of the Board of Translators, the Uighur word for cotton (*mien-hua*) is given as *kädäs*, which KLAPROTH (*Abhandlung über die Sprache und Schrift der Uiguren*, 14) read *kedis* and proposed to correct to *kedin*; in the latter form he saw an equivalent of the Ar. *quṭun*, «cotton» (cf. also BANG, *Vom Köktürkischen zum Osmanischen*, IV, 14). This is not acceptable, since the Uighur word belonged to the palatal class and so could hardly render *quṭun*, and since moreover *quṭun* did not pass into any Turkish dialect. I have no doubt that *kädäs* is one of the many corrupt forms which have crept into the Vocabulary, and that the original word was \**käbäs* = *käbáz*. The word has survived in modern Turfan Turkī as كَابَز *käbáz*, vulg. *kiwáz* (cf. VON LE COQ, in *Türán*, I [1918], 454); it is transcribed 克頗斯 *k'o-p'o-ssü* \**käpäs*, in *Hsi-yü t'u-chih*, 43, 4 a. But, though *po-tieh* has been used as an equivalent of *karpāsa* > *käbáz*, *käbáz* itself was rather the name of the cotton plant, not of the cotton goods, which latter sense, on the other hand, was given to *po-tieh*. The real Uighur word for cotton goods was *böz*. This word, certainly connected with Greek βύσσος (> Arab. and Pers. *bāzz*, Osm. *bāz*, Russ. *byaz'*), which itself rests on an old Semitic original (cf. LEWY, *Die semit. Fremdwörter im Griechischen*, 125-126; MÜLLER, *Uigurica*, II, 70; LAUFER, *Sino-Iranica*, 574; BANG, *Vom Köktürkischen zum Osmanischen*, IV, 14), is already given with the meaning «cotton goods» in Kāšyarī (BROCKELMANN, 48, where the vocalization *büz* should be discarded), occurs frequently in RADLOV's *Uigurische Sprachdenkmäler* (cf. the index, p. 279), and is perhaps to be recognized in the 華布 *p'o-pu* or «*p'o* (\**b'uət*) cloth» which was offered by the T'u-chüeh in 593 (cf. *TP*, 1929, 216). *Böz* has passed, with the same meaning, into Mong. *bös* (> Dayur *büri*; cf. POPPE, *Dagurskoe narečie*, 56, 141), JUČEN \**busu*, Manchu *boso* (RAMSTEDT, *Kalm. Wörterbuch*, 56, adds Čuvaš *pir*, which is correct, and Ch. 布 *pu* [\**puo*], «cloth»; but *pu*, which moreover did not in ancient times refer to cotton, is too old a Chinese word not to be indigenous). *Böz* being the true Uighur word for «cotton goods», we are not surprised to find it used in the Uighur translation of the *Sitātapatradhāraṇī* in a passage where the Chinese translation gives *po-tieh* (cf. MÜLLER, *Uigurica*, II, 70), nor to find in the Uighur version of the *Suvarṇaprabhāsa*, which was made from the Chinese, *po-tieh*, «white *tieh*», literally rendered *yürüng böz* «white *böz*» (MALOV, *Suvarṇaprabhāsa*, 444<sup>14-15</sup>). There is no foundation for RADLOV's statement (*Uigurische Sprachdenkmäler*, p. 120) that *böz* had been preceded in Uighur by another word, also meaning «cotton stuff», which he read «*kokpu*». Perhaps he laboured under the idea that the *-pu* of his «*kokpu*» rendered Ch. 布 *pu*, «cloth», later «cotton cloth», or he may have connected his «*kokpu*» with the late Mongolian *köböng*, «cotton». But «*kokpu*» often occurs in the documents (cf. RADLOV's index, p. 279), and is clearly the designation of some sort of money. It is the same word which F. W. K. MÜLLER, reading it «*quanbo*», had restored as 官寶 *kuan-pao*, without any translation (*Ostasiat. Zeitschr.* VIII, 324). This was nearer the mark, but still unsatisfactory, since the reading is certainly *qoqbō* (or *qoqpō*). In such circumstances, I think there cannot be any doubt that the original form was 國寶 *kuo-pao* (\**k<sup>w</sup>ək-pāu*), which means both «national treasure» and «national currency». The second *-q-* shows that the term was an ancient one in Uighur, having been borrowed when final occlusive consonants were still heard in northern Chinese, *i. e.* before A. D. c. 1000.

In Mongolian, the modern word for cotton (not «cotton goods») is *kübüŋg* (*köböŋg*, *köbäng*), Kalm. *köwŋ*; it occurs as *kubun* in Manchu. SCHOTT had thought of connecting this word with 古貝 *ku-pei* (\**kuo-puâi*), «cotton» (*ku-pei* will be discussed below). LAUFER (*Sino-Iranica*, 574) declared that such a derivation was «impossible in view of the labial surd» of the Chinese term, and proposed an Iranian origin, adducing *kubas* in Waḥī, a Pamir dialect. The objection is of no value, since there was no *p* in Mongolian, nor in early Manchu, and these languages used *b* in its stead (for instance, the Chinese 螺貝 *lo-pei* [\**luâ-puâi*], «conch» [used as a musical instrument], has given in Mongolian *labai*, «horn»; see «Cowry»). As to the Pamir form, it is true that HJULER, *The Pamir languages*, 38, gives for «cotton» Šuynī *paḥta*, Waḥī *kubâs*. But BENVENISTE draws my attention to the fact that this form has not been confirmed by subsequent inquiries: MORGENSTIERNE, *Indo-Iranian frontier languages*, II, 15\*, and H. SKÖLD, *Materialien zu den iran. Pamirsprachen*, 1936, 162-163, give only *paḥta* for «cotton» in Waḥī, and in other dialects such forms as Yidya-Munġi *karvasë*, *karbos*, «cotton», or Šuynī *kärbôs*, «cotton goods», «muslin» (W. LENTZ, *Pamir-Dialekte*, I, 1933, 173); so that HJULER's *kubâs*, if it was correctly noted, must be a local, recent, and perhaps individual development. It may be due to the retrogressive action of the labial consonant, and is perhaps to be compared with Burušaski *gupas* (cf. LORIMER, *The Burushaski Language*, III, 173), probably borrowed from the Indo-Aryan. But, whatever the truth may be about the Waḥī *kubas* and the Burušaski *gupas*, the Mongolian *kübüŋg* appears at too late a date to be a form directly borrowed from the Iranian. No mediaeval text has given a Mongolian word of the type of *kübüŋg*; on the contrary, we know now that the ancient Mongolian word for cotton was not *kübüŋg*, but كيباز *kebâz* (*kibâz*) or كيبز *käbiz* (POPPE, *Mongol'sko-Tyurkskii slovar'*, Leningrad, 1938, 218), that is to say was fundamentally the same as Uighur *käbâz*. The earliest mention I can trace of a word of the type of *kübüŋg* is the *k'u-pu*, \**kubu*, «cotton» (*mien-hua*), of the late Jučen Vocabulary acquired by AUROUSSEAU (Nos. 382, 975). Moreover, Jučen \**kubu* and Manchu *kubun* cannot be separated from Goldi *kūfa*, «cotton» (cf. GRUBE, *Goldisch-Deutsches Wörterverzeichnis*, in L. VON SCHRENCK, *Reisen*, III, App., II, 30). My impression is that SCHOTT was probably right; it must have been *ku-pei* (\**kuo-puâi*) which passed into the Tungus languages; the original vowel *a* of the second element has been retained in Goldi, but became *-u* in Jučen and Manchu under the influence of the preceding labial consonant; and, if so the *-n* of the Manchu *kubun* must be of secondary appearance, and it is from the Manchu that the word was borrowed by the late Mongols. The earlier occurrence of the word in Tungus languages disposes at the same time of RAMSTEDT's hypothesis (*Kalm. Wörterbuch*, 242), which would trace *kübüŋg* (*köbüŋg*) to the Mongolian root *köb-*, «to swell».

In Central Asia, the word *karpāsa* did not travel beyond the Uighur country and Mongolia; it did reach China, however, from the south, in a transcription which, in pre-T'ang and T'ang texts, is sometimes 吉貝 *chi-pei* (\**kjēt-puâi*), though more usually 古貝 *ku-pei* (\**kuo-puâi*); the term is then always used in descriptions of Indo-China and Indonesia, or for presents sent from these regions. While everybody agrees that these transcriptions are ultimately connected with *karpāsa*, it is evident that they are not a direct representation of the Sanskrit word. WATERS, GROENEVELDT (*Essays relating to Indo-China*, 2nd series, I, 142, 185), and HIRTH had



thought that the intermediary form was the Malay *kapas*; LAUFER (*Sino-Iranica*, 491), taking into consideration only the form *ku-pei*, stated that the Chinese must have received the term from an Indo-Chinese language, and that Bahnar *kōpaih* provided «the nearest approach» to *ku-pei*. I cannot entirely agree with either opinion. The correspondence Indonesian (and Cham) *-a* > Bahnar *-ō* is well known, and the *-ō* of the modern Bahnar form *kōpaih* must be a late development, as is shown by the various Indo-Chinese and Indonesian forms, conveniently collected by CABATON, *Dictionnaire Cam-Français*, 57 : Jav., Dayak, Malay, Sund. *kāpas*, Makass. *kapasa*, Battak *hapas* [and *kapas*], Bis. *gapas*, Cham *kapah*, Bahnar *kōpaih* (add Radè *kapas*, Kuoi *kabas*, Kčo *kopas*, Sedang *kōpè*), Khmer *krābas* (add Khmer *kābbas* < *kāppas*, as a secondary form derived from Pali *kappāsa*). The word seems to be represented in Bima as *kafa*, «thread», and even to have passed into Melanesian and Polynesian languages : Fijian *kava*, Samoan *'afa*, Maori *kafa*, all meaning «string» (cf. O. DEMPWOLFF, *Die Lautentsprechungen der indones. Lippenlaute* . . ., Berlin, 1920, 8vo, p. 13, and BLAGDEN, in *BSOS*, II, 152). But, on the other hand, the Malay *kāpus* is also out of the question. Just as we have Skr. *karpāsa* and Pali *kappāsa*, we find in Indian the modern forms Beng. *kārpās* and *kapās*, Hindī *kapās*, *kāpās*, (for other forms in modern India dialects, cf. J. BLOCH, *La formation de la langue marathe*, 309; for Anglo-Indian, cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «capass»). In the Indo-Chinese forms, only the Khmer form *krābas* shows an *r* (with metathesis) as in the Skr. *karpāsa* (it may go back to the period of Sanskrit influence in Cambodia). There is no etymology of *karpāsa* and the word stands isolated in Indo-European languages (the connection suggested by UHLENBECK, *Kurzgefasstes etymolog. Wörterbuch*, 46-47, with Skr. *kūrpāsa*, «woman's jacket», is highly problematical, and leads nowhere, since the origin of *kūrpāsa* is also unknown); in all likelihood, the word is pre-Aryan in India (cf. the very similar Skr. *karpūra*, Pali *kappura*, modern Indian dialects *kapūr* and *kāpūr*, Jav. *kapur*, Mal. *kāpur*). So far I agree with PRZYLUKSI's views as expressed in *Bull. Soc. Ling.*, xxv, 1924, 69-70; but here he and I must part. PRZYLUKSI, starting from the Indo-Chinese languages, supposed a root \**ḥas*, meaning «to use a bow», both for shooting, and for carding cotton (I do not know why PRZYLUKSI does not adduce in favour of his thesis Stieng *pahi*, Annamite *vai*, and Siamese *fai*, all meaning «cotton»). The forms of the *kāpas* type would represent this root with a *ka-* prefix, enlarged by a liquid in the Skr. *karpāsa*; to the same root, with the dropping of the *k-* and a nasal infix, would also belong the Khmer *ambas*, *ambōh*, meaning «cotton» according to PRZYLUKSI, who, on the other hand, does not cite Khmer *krābas*. None of these hypotheses is in itself impossible, since parallel cases may easily be adduced for every one of the supposed transformations of the root. But it is *krābas* which is the word used in Khmer for «cotton» in general, and *ambas*, or *ambōh*, means «spun cotton», «cotton thread», and I see no sufficient reason to believe that the two words are connected. If there are in Indo-China and Indonesia other words probably connected with but not directly traceable to *karpāsa*, I would rather look for them in the Malay *كافق kāpuq*, *kāpoq*, «floss of the silk-cotton tree», our «kapok» (cf. LOKOTSCH, No. 1067), a word which has passed into the Amoy dialect as *ka-pók-mt'*, «kapok cotton» (C. DOUGLAS, *loc. cit.*, 329), and in the Malay *kābū* or *kabukābū* «cotton», which is also the general name for cotton among the primitive tribes of the Malay Peninsula (cf. SKEAT and BLAGDEN, *Pagan Races of the Malay*

*Peninsula*, II, 566 : Tēmbi *ka*bu, Jēlai *ka*bu' [*ka*buk]; but Marathi *ka*pūs, Guzrati *kā*pūs, Singhalese *ka*pu are attached to *karpāsa* in BLOCH, *loc. cit.*, and so is Maldiv. « *capa* » [PYRARD DE LAVAL, GRAY transl. II, 416] now *ka*fa). Apart from the fact that the hypothesis of a \**ḍās* root is arbitrary, my main objections to PRZYLUKSI's theory are that the cultivation of cotton began more probably in India than in Indo-China, and that all the forms of the *kā*pas type can easily be accounted for by starting from the Pali *kappāsa*, the *-pp-* of which is regularly the outcome of the *-rp-* in the Skr. *karpāsa*. As to another word, *karpaṭa*, which PRZYLUKSI introduces by the side of *karpāsa*, and which he says means « cotton stuff », the only meaning known for Skr. *karpaṭa*, Pali *kappaṭa*, is « tattered rag », and it is perhaps satisfactorily explained from the Indo-European root \**kerp-*, « to cut » (cf. UHLENBECK, *loc. cit.*, 46).

To decide whether the Chinese term represents a form with or without *-r-* depends on the choice we are to make between *chi*-*pei* and *ku*-*pei*, since in *chi*-*pei* (\**kjēt-puāi*) the final *-t* (> *-d*) of the first character would regularly render the *-r-* of the *karpāsa* type, whereas there is nothing similar in *ku*-*pei*. It seems evident that one of the two forms 吉貝 *chi*-*pei* and 古貝 *ku*-*pei* is a graphic corruption of the other; taken in themselves, each of them is possible, since both *chi* and *ku* occur before the T'ang period in the transcriptions of words or names of Indo-China and Indonesia. About the end of the 16th century, LI Shih-chên considered (*Pên-ts'ao kang-mu*, 36, 12 b) that it was *ku*-*pei* which had been altered to *chi*-*pei*, and this may be the reason why LAUFER spoke of *ku*-*pei* only. But the *Chêng-tzū t'ung* of 1570-1572 (*s. v.* 棉 *mien*) is in favour of *chi*-*pei* against LI Shih-chên, and 俞正燮 Yü Chêng-hsieh (1775-1840), in his 癸巳類稿 *Kuei-ssü lei-kao* (*Ch'iu-jih-i-chai* ed., 7, 21 a; 14, 4-5), declares that *ku*-*pei* is nothing more than a misprint in the Sung editions for *chi*-*pei*. WATTERS, *Essays on the Chinese language*, 440, started from *chi*-*pei*, just as I did in *TP*, 1933, 332.

LI Shih-chên, in his turn, may have based his opinion on a passage of a Sung work completed in 1085, the 文昌雜錄 *Wên-ch'ang tsa-lu* of 龐元英 P'ang Yüan-ying. Our texts of the *Wên-ch'ang tsa-lu* are not satisfactory, and the passage in question does not occur in the complete edition, in six chapters, of the *Hsüeh-chin t'ao-yüan*. I have found it, however, among the extracts included in ch. 47 of the *Shuo fu* in 120 chs., and it seems evident that this has been the source of the quotation in the *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *tsa-lu*, 1 b. It is a matter of surprise that neither the Commissioners of the *Ssü-k'u* . . . , 120, 14 b-16 a, nor the editor of the *Hsüeh-chin t'ao-yüan* should say anything about this paragraph, or about others which are only to be found in the *Shuo fu* extracts; the reason may be that the *Wên-ch'ang tsa-lu* is given by mistake in the *Shuo fu* as the work of a man other than P'ang Yüan-ying; it seems, however, that neither the authorship of the book, nor the authenticity of the present passage can be doubted. It says that in Fu-chien, Kuang-tung, and south of these regions the people make cotton (*mu*-*mien*) goods, which they call *chi*-*pei*; but that, reading the *Nan shih*, P'ang Yüan-ying found there the description of the *ku*-*pei* plant, which certainly was the same as *chi*-*pei*; his conclusion is that « it must be that *ku* was vulgarly pronounced as *chi* » (this is of course an error; the case is one of graphic, not phonetic corruption). But, while the *Nan shih* (ch. 78) speaks of *ku*-*pei*, the earlier *Liang shu* (ch. 54) always gives *chi*-*pei*. *Chi*-*pei* alone was heard in southern China by P'ang Yüan-ying, and also evidently by 程大昌 Ch'êng

Ta-ch'ang (1123-1195), who, in his 演繁錄 *Yen fan lu*, completed in 1180 (*Hsüeh-chün tao-yüan* ed., 10, 8 a), remarks that the notice on Champa in the *Hsin T'ang shu* speaks of *ku-pei*, a plant from the «flowers» of which cloth was woven, and that this was also the case with the *chi-pei* of his own time; but, since «*ku* and *chi* cannot be used one for the other», Ch'êng Ta-ch'ang wondered whether the name had become corrupt, or whether the two forms referred to two different products. Another Sung author, 陳正敏 Ch'ên Chêng-min (this is the form of the name in the *Chiün-chai tu-shu chih*, 13, 20 a, the *Sung shih*, 206, 3 a, and the *Wên-hsien t'ung-k'ao*, 217, 1 a; the *Shuo fu* in 120 chs. erroneously gives 范政敏 Fan Chêng-min; Ch'ên Chêng-min is mentioned in the *Hsü po-wu chih*, 7, 3 b, and so cannot be later than the middle of the 12th cent.; according to the *Chiün-chai tu-shu chih*, his work is to be dated 1102-1110), has also noted in his 遜齋閒覽 *Tun-chai hsien-lan* (*Shuo fu* in 120 chs., 25, 3 a-b) that the cotton stuff of Kuang-tung and Fu-chien which in his time was called *chi-pei* was clearly the same as the *ku-pei* of the *Nan shih*. In 1178, Chou Ch'ü-fei, the author of the *Ling-wai tai-ta*, in the course of an interesting notice which will be dealt with at greater length farther on (*Chih-pu-tsu-chai ts'ung-shu* ed., 6, 12-13), noticed that the double form *ku-pei* and *chi-pei* seemed to have originated out of graphic confusion, but remained in doubt about the identity of the «*ku-pei* plant» and the «*chi-pei* tree». Among other Sung and Yüan authors who have used the form *chi-pei*, and never *ku-pei*, I may mention the great poet Su Shih (1036-1101 : «I was presented with a *chi-pei* cloth»); 方勺 Fang Cho (c. 1125) in his 泊宅編 *Po chai pien* (*Pai hai* ed., 2, 5 b); the *Sung shih*, in the notices on Champa, on Java, on P'o-ni (Borneo), on Chu-lien (Coromandel), etc. (489, 1 a, 6 b, 7 b, 9 a); 李材 Li Ts'ai (c. 1360 A. D.), who, in his 解醜語 *Chieh-ch'êng yü* (*Kuang Po-ch'uan hsüeh-hai* ed., 2 a; *Chieh-[醜]hsing yü*, given in the *T'u-shu chi-ch'êng*, whence it has passed into BRETSCHNEIDER, *Botanicon Sinicum*, 1, 159, is corrupt), speaks of ten «*chi-pei* gowns» acquired by envoys sent to Ma'abar in 1285 (only the embassy sent in return from Ma'abar is recorded in *YS*, 210, 7 b), adds a note on *chi-pei*, and mentions (3 a) «*chi-pei* brocades»; 王禎 Wang Chêng, who completed in 1313 his valuable work on agriculture entitled 農書 *Nung shu* (Fu-chou reprint of the *Wu-ying-tien t'sung-shu* ed. recovered from the *Yung-lo ta-tien*, 10, 5 b; 21, 16 a); and T'ao Tsung-i, in his *Cho-kêng lu* (24, 12 b). Both Chou Ch'ü-fei in his *Ling-wai tai-ta*, and Chao Ju-kua, who, in the first quarter of the 13th cent., collected his information at Ch'üan-chou in Fu-chien (see «Çaiton»), always speak of *chi-pei*, never of *ku-pei*. *Chi-pei* occurs in a song by Wang Kuang-yang at the end of the 14th cent. (cf. *infra*, p. 480). It is also mentioned, but as a term used in former times, in the 梧潭雜佩 *Wu-Hsün tsa-p'ei* (quoted in *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *hui-k'ao*, 1 b; *tsa-lu*, 2 a), the work of 張所望 CHANG So-wang, a native of Shanghai, doctor of 1601, who held office in Kuang-hsi and died at the (real) age of 79 (cf. *Sung-chiang fu chih*, 54, 42-43; 72, 25 b). We also find *chi-pei* still later in the first half of the 17th cent. in 王象晉 WANG Hsiang-chin's 群芳譜 *Ch'ün-fang p'u*, published 1630 (*Mien p'u* section, 1 a; cf. *Ssü-k'u* . . . 116, 38; BRETSCHNEIDER, *Botanicon Sinicum*, 1, 70; COURANT, *Catalogue*, 5480; the section on cotton has been translated by STAUNTON, *Narrative on an Embassy*, App. III, 249-257) and in the 農政全書 *Nung-chêng ch'üan-shu* of the famous Christian scholar and minister Hsü Kuang-ch'i (1562-1633), published posthumously in 1639 (35, 2-3). But it is possible that both WANG

Hsiang-chin and Hsü Kuang-ch'i knew *chi-pei* only as a term found in earlier works. *Chi-pei* however, may have remained alive in southern dialects fairly late, since it is still used in explanatory glosses of the *Tung-hsi yang k'ao*, which was completed in 1617-1618 (1, 11 b; 5, 6 a; cf. also 3, 10 a). At any rate, neither *ku-pei* nor *chi-pei* seems to have survived in the modern dialects of Fu-chien and Kuang-tung (nor in Annamite, in spite of WATTERS, *loc. cit.* 440).

Thus *chi-pei* was the only form used in southern China from the 11th till the 14th cent., and perhaps later. Yet I believe that the balance of the argument is in favour of a primitive form *ku-pei*. If we could trust the *T'u-shu chi-ch'êng* (*ibid.*, *hui-k'ao*, 1 b), *chi-pei* would already have been mentioned in the 南州異物志 *Nan-chou i-wu chih* («Cotton [*mu-mien*] is produced by the *chi-pei*. . .»); the *Nan-chou i-wu chih*, a work on the remarkable products of the southern countries, was written in the 3rd cent. (cf. BEFEO, III, 267; IV, 277-278; TP, 1923, 123; *Etudes asiatiques*. . . de l'EFEO, II, 255; I regret not to have at my disposal the recent edition of the extant fragments which was published by 陳運溶 CH'ÊN Yün-jung, on which cf. *The Yenching Journal of Social Science*, II [1939] 127). But, in most cases, the *T'u-shu chi-ch'êng* has altered to *chi-pei* the reading *ku-pei* of the original texts; moreover the quotation, the origin of which I have been unable to trace, cannot, from its contents, go back to the 3rd cent. Part of the quotation, but without the sentence on *chi-pei*, is found in the *Nung shu* of Wang Chêng, dated 1313 (20, 16 a; on this work, cf. *infra*, p. 502); there it is said to be drawn from the *I-wu chih* (without «Nan-chou»); on the next page, another part of the quotation is given with the full title *Nan-chou i-wu chih*, and there *chi-pei* is mentioned; but this does not solve the chronological difficulties, and I believe that the text does not go farther back than the Sung at the earliest, which would account for the form *chi-pei*. In fact, the earliest mention of *ku-pei* or *chi-pei* which I can find occurs in the *Sung shu* (97, 2 b); there it is said that, in the 7th *yüan-chia* year (430), «the kingdom of 呵羅單 Ho-lo-tan (\* $\chi\hat{a}$ -lâ-tân), which has its seat on the island (*chou*) of Shê-p'o, sent an envoy who offered such objects as diamond rings, red parrots, *po-tieh* and *ku-pei* from the kingdom of 天竺 T'ien-chu (an abnormal way of writing 天竺 T'ien-chu, India), and *ku-pei* from the kingdom of 葉波 Yeh-po». Shê-p'o transcribes Java, which may here designate Sumatra as well as Java proper; the kingdom of Ho-lo-tan, which seems to be erroneously duplicated as kingdom of 訶羅隨 Ho-lo-t'o (\* $\chi\hat{a}$ -lâ-d'â) in the same chapter (2 a-b), remains unidentified; Yeh-po, or Yeh-po-[羅]lo (\*[ $\lambda\hat{p}$ -puâ-lâ) is an ancient name of Gandhâra (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 225, 322; BEFEO, IV, 272; JA, 1914, II, 406-408). Being for the moment concerned only with the term *ku-pei* or *chi-pei*, I leave for another paragraph the question of the juxtaposition of *po-tieh* and *ku-pei*; the text is identical in the quotation made by the *T'ai-p'ing yü-lan*, 787, 14 b. This example of *ku-pei* (or *chi-pei*) antedates by two and a half centuries the date given by Wells WILLIAMS (*The Middle Kingdom*, II, 36-37) for the introduction of the term, viz. «A. D. 670». In the *Nan Ch'i shu* (58, 5 a), two pairs of *ku-pei* are listed among the presents offered in 484 by Fu-nan (= Cambodia; cf. BEFEO, III, 260, where, however, «an elephant in white santal» is a slip for «an image in white santal»). *Shuang*, «pairs», is abnormal in the case of textiles, but I find it also used in connection with *tieh*, «cotton stuff», in 張, VIII, 117 a; it may be due to the fact that two strips of cloth were used as clothing, hence they are mentioned in Indian texts as *yuga*,

« pairs » (cf. PRZYLUKSKI, in *JA*, 1919, I, 406). Then comes the *Liang shu* (ch. 54), which always speaks of *chi-pei*, but the *Nan shih*, in copying it, always gives *ku-pei* (except once, 79, 7 b, where it gives *chi-pei* in the notice of K'o-p'an-t'o west of Khotan; but it may be a misprint), and so does the quotation from the *Liang shu* in the *T'ai-p'ing yü-lan* (788, 15 b). It is also *ku-pei*, and *ku-pei* alone, which we find in the *Sui shu* (82, 3 a) and in both *T'ang shu* (*Chiu T'ang shu*, 197, 1 a and b; *Hsin T'ang shu*, 222 c, 1 a and b), as well as in the great compilations of T'ang and early Sung times, such as the *T'ung tien* (188, 4 b), the *T'ai-p'ing huan-yü chi* (176, 3 a, 3 b-4 a, 12 a and b; 179, 19 a, in the section of P'o-ni [Borneo], while the later *Sung shih*, in an otherwise identical sentence, gives *chi-pei*), the *T'ai-p'ing yü-lan* (ch. 788, sometimes with a corrupt form 古具 *ku-chü*, which is certainly a misprint) and the *Ts'ê-fu yüan-kuei* (959, 21 a). The statement in the *P'ei-wên yün-fu* (s. v. « *chi-pei* »), blindly followed by the *Tz'ü-yüan*, and also in the *T'u-shu chi-ch'êng*, that the *Nan shih* and the *Hsin T'ang shu* give *chi-pei* is nothing but an error. In a note in the *China Review*, XIX, 191, PARKER cites the 九域志 *Chiu-yü chih* as speaking of *ku-pei* in a notice on an Indonesian state which he supposed to date from the Sung dynasty (cf. also LAUFER, *Sino-Iranica*, 471; FERRAND, in *JA*, 1921, II, 287). This would be the latest independent use of the form *ku-pei* found hitherto. Unfortunately, PARKER's information is certainly second hand, taken, it seems, from some dictionary or encyclopaedia which he did not trouble to mention. The only *Chiu-yü chih* I know of is the *Yüan-fêng chiu-yü chih*, dated 1080 (cf. *Ssü-k'u* . . . , 68, 7-9), but I can find in it no passage recalling the one mentioned by PARKER. In any case, the passage is not original, but, with a mistake in the name of the state 婆斯 P'o-ssü instead of 婆利 P'o-li, is merely copied from the notice of P'o-li in *Nan shih*, 78, 6 b; the true title may have been *Chiu-chou chi*, a work written prior to 527, on which cf. *infra*, p. 444. In the Buddhist scriptures too, I have found only *ku-pei*, occurring in Paramārtha's translations of the *Abhidharmakośa* (NANJIO, No. 1269; ch. 9; Tōkyō *Tripit.* of Meiji, 冬, I, 54 b) and of the *Fo a-p'i-t'an ching* (NANJIO, No. 1108; ch. 2; 寒, x, 16 b) which are of the end of the 6th cent.; on that account, *ku-pei* is commented upon by Hsüan-ying in his *I-ch'ieh ching yin-i* (*ibid.* 爲, VI, 71 a; VII, 72 a), and Hsüan-ying's text has been taken over by Hui-lin in his ch. 70 (*ibid.* IX, 189 b). Of course, *chi* and *ku* are graphically so much alike that one could easily have taken the place of the other; moreover Chinese texts have often been revised by scholars, and we know cases of wrong forms which have been substituted wholesale for the correct ones, such as *t'ieh-lo* which always occurs in ancient Chinese texts instead of the correct *t'ieh-ch'in*, Avar and Turk. *tegin*. But, in the present case, the agreement of so many sources is the more impressive because it extends to Buddhist texts. Nevertheless, on account of the *chi-pei* of the *Liang shu* and of the later use of *chi-pei* under the Sung, I should have hesitated to express any definite opinion, if it were not that the correctness of *ku-pei* can be established, I believe, by comparison with another transcription.

The word *karpāsa* is of frequent use in Buddhist texts, so that it often occurs in their Chinese translations, though in different guise, once as 劫貝娑 *chieh-pei-so* (\**kjəp-puāi-sā*; mentioned in connection with the flower of the plant; cf. Tōkyō *Tripit.* of Meiji, 爲, VIII, 164 a), more usually as 劫波育 *chieh-po-yü* (\**kjəp-puā-juk*; cf. *ibid.*, VI, 2 b, 33 a; VII, 2 b; VIII, 117 b) and 劫貝 *chieh-*

*pei* (\**kj̥p-puāi*; cf. *ibid.*, VI, 59 a; VII, 58 a; IX, 147 a). The *chieh*-[具] *chü* or *chieh-chü-so* in SOOTHILL and HODOUS's *Dictionary of Chinese Buddhist terms*, 232, is a misprint; and so is «*kiu-pei*», *chü-pei*, instead of «*kie-pei*», *chieh-pei*, in PRZYLUCKI's paper, *JA*, 1919, I, 407; outside of Buddhist texts, *chieh-pei* occurs in a verse of the T'ang poet P'i Jih-hsiu, doctor of 867. A text in *Tripit.*, 辰, III, 99 b, speaks of 迦尸劫貝 *Chia-shih chieh-pei*, «*Kāśi cotton*», i. e. Benares muslin (*kāśi* or *kāśika* is not «*silk*», as said by COWELL, *Divyāvadāna*, 678; the same is true of Pali *kāsika*). One is immediately struck by the fact that none of these transcriptions is made directly from *karpāsa*; all are based on Prākṛit forms beginning like the Pali *kappāsa*. As a matter of fact, if we except Li Yen's Sino-Sanskrit Vocabulary published by BAGCHI, *karpāsa* never occurs as such in Chinese Buddhist texts, where we find only *kārpāsika* (cf. *supra*, p. 433), and the *Mahāvvyupatti* registers only *kārpāsakam* (No. 9164). In the middle of the 7th cent., Hsüan-ying, commenting on the above transcriptions which he declares to be erroneous, states that the true Sanskrit word was 迦波羅 *chia-po-lo* (\**ka-puā-lā*), and Hui-lin copies it on his own account; but *chia-po-lo* can only render *kapāla*, «*skull*». ODA Tokuno has suggested in his Dictionary (p. 470<sup>1</sup>) that the second and third characters may have to be reversed; but the result, \**chia-lo-po*, would only give *karpā-*, without *-sa*. I rather think that Hsüan-ying misunderstood some information imparted to him by Hsüan-tsang, a fact which of course would sadly reflect upon the Sanskrit attainments of both Hsüan-ying and Hui-lin. As to the *chia-lo*-[婆] *p'o-chieh* given in STUART's *Materia Medica*, 198, as a Sanskrit name of cotton, this is a mistake of the *Pên-ts'ao kang-mu* (36, 71 b; it has passed into *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, 1 a, 11 a), due to a partial juxtaposition of *chia-lo-po* and *chieh-pei*, and simply does not exist. *Chieh-pei-so* seems to be an artificial, half-scholarly transcription, in which an attempt has been made to add the final *-sa* of Pali *kappāsa* to the transcription already current *chieh-pei*; but, as will soon be seen, *chieh-pei* is not merely *kappā-*. *Chieh-po-yü* would suppose an original like \**kappāiuk*; it probably represents a Prākṛit form \**kappāik* corresponding to the adjectival form *kappāsika*, «*made of cotton*», «*cotton stuff*», of the Pali. This last form, which is the more usual, is the most interesting, since it supposes a Prākṛit \**kappāi*, corresponding to the feminine Pali form *kappāsi*, «*cotton*». Now this is exactly the stage of the final element in *ku-pei*, which would suppose \**kupāi*, or *chi-pei*, which in its turn would suppose \**kirpāi*. The *Chêng-tzū t'ung* (s. v. 棉 *mien*) says that *chi-pei* is more probable than *ku-pei*, because it is «*nearer*» *chieh-pei*. As to Yü Chêng-hsieh, he boldly states (*Kuei-ssü lei-kao*, 7, 21 a) that *ku-pei* is merely a misprint of the Sung editions, and, in his special section on cotton (*Mu-mien k'ao*; *ibid.* 14, 4-6), always writes *chi-pei*, without mentioning any other reading. But we must not forget that we have no editions older than the Sung; on the other hand, one cannot see why an almost general corrupt reading *ku-pei* should have crept into the Sung editions of the dynastic histories and of the *Tripitaka*, when *chi-pei* was in fact the only form alive in Sung times. The opinion of the authors of the *Chêng-tzū t'ung* and of Yü Chêng-hsieh seems to have been determined by the fact that both *chi* and *chieh* have an *-i*, which *ku* never had, and that both are *ju-shêng* words, while *ku* is pronounced with *shang-shêng*; but they were not aware of the ancient final consonants, one being *-t* in *chi* (\**kj̥ēt*), and the other *-p* in *chieh* (\**kj̥p*). As we have seen, *chi-pei* (\**kj̥ēt-puāi*) would suppose \**kirpāi* < \*Skr. *kārpāsī*, but the vowel of the first syllable would be abnormal, and moreover it seems impossible that a Prākṛit form in which <sup>o</sup>*pāst* had been

reduced to *-pāi* should have retained the consonantic group *-rp-* of Skr. *kārpāsī*, instead of assimilating it in *-pp-* as in Pali *kappāsī*. On the other hand, *kupāi* and *\*kappāi* can easily be reconciled, since we have only to suppose a retrogressive action of the first *-p-*, finally melting into the new labial vowel. This is possibly what occurred in quite a different part of Asia with the modern form *gupas* of Buruṣaski. My conclusion is that the Indian name of the cotton which the Chinese received in the 5th cent. from the southern seas reached them neither through the intermediary of Jav. and Malay *kapas*, nor of an Indo-Chinese form similar to the modern Bahnar *kōpaih* (where *-h* < *-s*, as in Cham), but as *\*kupāi* < *\*kappāi*, itself < *kappāsī*. It may be that *\*kupāi* is to be traced to the Chams. In Cham, many words with a first *-a-* vowel have a doublet in *-u* : cf., for instance, *kabav* and *kubav*, *kubau*, « buffalo » (Jav. *kēbo*, Mal. *kērbav*, Khmer *krebēi*, etc.), and, in the case of the word for « cotton », it is *kapah* among the Chams of southern Annam, but *kupah* among those of Cambodia (CABATON, *loc. cit.* 57, 74). But I am far from being certain that *kupah*, the final *-h* of which, like that of *kapah*, goes back to an earlier *-s*, can be ancient enough to provide the prototype of *\*kupāi* (which moreover does not really represent *kupah* [< *\*kapas*], but is < *\*kappāi* < *kappāsī*). As to the *chi-pei* of the *Liang shu*, and any other which might be traced, they must be the result of scholarly corrections, after a graphic error had, in early Sung times, substituted in current use *chi-pei* for *ku-pei*. If Jučen *\*kubu*, Manchu *kubun*, Mongol *kübüŋ* and Goldi *kufa* are borrowed from *ku-pei*, we must not be surprised not to find *kübüŋ* in Mongolian during the Mongol dynasty, since the Mongols were then under the influence of Uighur culture, and adopted, as we have seen, the Uighur word *kübüz*. But *ku-pei* must have been borrowed from the Chinese at an early date, before *chi-pei* became the only form in use in southern China. So it may be surmised that the *\*kubu* which so far we know only in late Jučen was already in use when the Nü-chên ruled over northern China, even if it does not go back to the time of the Ch'i-tan.

PO-TIEH. — We have seen that the *Liang shu* mentions cotton as existing in the Turfan region in the first part of the 6th cent., and gives it the name *po-tieh-tzū*, *tzū* being a substantival affix of the spoken language; on the other hand, *po-tieh* can theoretically be conceived either as being a transcription (*\*b'ok-d'iep*), or as simply meaning « white *tieh* (*\*d'iep*) ». That the term was used at least by the numerous Chinese inhabitants of the country is expressly stated in the middle of the 7th cent. by Hsüan-ying, in a gloss repeated almost *ad nauseam* by himself and by Hui-lin; according to that gloss, which has a bearing on the various transcriptions of Prākṛit forms of *karpāsa*, « the people of Kao-ch'ang (= the Turfan region) call [cotton] 疊{ *tieh* » (Tōkyō *Tripit.* of Meiji, 爲, VI, 2 b, 33 a, 59 a; IX, 147 a); 疊{ *tieh* (*\*d'iep*) is merely another graphic form of 疊{ *tieh*. Of course, the use of *tieh* instead of *po-tieh* by Hsüan-ying is not in favour of the view that *po-tieh* is a transcription; it is, however, not conclusive, since it may be a scholarly abbreviation. In the same way, the author of the *Hsin T'ang shu* (222 c, 1 b), copying a passage in which the less literary author of the *Chiu T'ang shu* (197, 1 b) explained the difference between *ku-pei* and *po-tieh*, merely speaks of *pei* and *tieh*, although *ku-pei* is admittedly a transcription.

The main Western exponent of the theory according to which *po-tieh* would be a transcription was HIRTH (*HR*, 218), who proposed to see in this term, « certainly borrowed from one of the Turki languages » (this has passed into COULING, *Encyclopaedia Sinica*, I, 134), an old form similar to the

Čay. Turk. باختا *pāhtā*, which also occurs as باقتہ *pāqta*, بختہ *pahta* and بختہ *bahta*, «cotton (freed from seeds)»; cf. SHAW, *Vocabulary*, 56, 57; RADLOV, *Dictionary*, IV, 1131, 1138, 1464 (the reference added in HR, 218, to GERINI, *Researches in Ptolemy's Geography*, 243, really concerns another etymology, from Thai *pha-tieu*, «loin-cloth», which is not worthy of refutation). HIRTH's hypothesis sounds natural enough, and it occurred to me independently when travelling in Central Asia. As a matter of fact, we had both been anticipated by T'AO Pao-lien who, in 1897, gave the same explanation in his 辛卯侍行記 *Hsin-mao shih-hsing chi* (6, 40 b); FUJITA, in his commentary on the *Tao-i chih-liao* (*Hsüeh-t'ang ts'ung-k'o* ed., 15), follows HIRTH. Yet, with a better knowledge of historical conditions and ancient Chinese phonetics, a connection between Ch. *po-tieh* and Turk. *pāhtā* cannot be retained. LAUFER pointed out (*Sino-Iranica*, 490) that *po-tieh*, in the form 帛疊 *po-tieh* (\**b'vk-d'iep*), was already mentioned in texts relating to the beginning of our era, under circumstances which «do not lead anywhere to the Turks», that we cannot correlate such an ancient Chinese term with one of a comparatively recent Turkish dialect, and finally that *pāhtā* was not of Turkish, but of Persian origin. As a conclusion, LAUFER proposed to explain *po-tieh* as an apheretical form derived from an Iranian original \*[*pam*]bak-dīp, «cotton-brocade»; hence «cotton stuff».

Some of LAUFER's argument should decidedly be rejected. A Middle-Persian \**pambak-dīp* does not exist, since the ancient Iranian word for «brocade» was not \**dīp*, but *dēpāk* (cf. HORN, *Grundriss der neupers. Etymologie*, No. 591), and there is no reason why a hypothetical term «cotton-brocade» should have become a name of «cotton stuff» in general. On the other hand, LAUFER was probably right in stating that *pāhtā* was originally Iranian, but the mention of the word in STEINGASS's *Persian-English Dictionary*, 237, is no sufficient proof of it. It does not occur in VULLERS, or in DESMAISONS, nor is it mentioned by HORN or by HÜBSCHMANN; the only two examples I know of it which are not quite modern are *bahta*, «cotton», in the *Memoirs of Bābur*, and in the Sino-Uighur Vocabulary of the Board of Interpreters (ms. of the London School of Oriental Studies, where it renders *mien-hua*); that is to say, both are Turkish, not Persian. It must be noticed, however, that *pahta*, «cotton», occurs in most Iranian dialects of the Pamir, and has always been considered by the various scholars of the subject to be there borrowed from the Persian. My colleague MASSÉ informs me that, while the Persian dictionaries due to true Persian authors do not give *pāhtā*, the word occurs, explained «cotton» (*pānbā*), in the *Farhang-i Rašīdī*, which is probably the source used by STEINGASS; but the *Farhang-i Rašīdī* was written in the 17th cent. by Abdu'r-Rašīd, a native of Tatta, near the Indus; so *pāhtā* would be peculiar to Indo-Persian. This is confirmed to some extent by Stephens C. PAUL's *Persian-English and Urdu Dictionary* (Allahabad, n. d.), where *pāhtā* is translated «cotton separated from its seeds». We should feel more assured of the Iranian character of *pāhtā* if its etymology were clear, which is not the case. MASSÉ suggests that it may be a form constructed after the analogy of the past participles, and based on *pāht*, another form of *pāhš*, «crushed», «stretched through being crushed or trodden upon»; I think he is right. Apart from the origin of *pāhtā* < *pāhtā*, LAUFER justly observed that this word would not account for the ancient -*p* final of \**b'vk-d'iep*, and that the «Turkish» word was of too late a date to be taken into account here; he might have added that Turkish was not yet spoken in the Turfan region in the first half of the 6th cent. On the other hand, a reverse solution, which



would derive Turk. *pahta*, *bahta* (and from it Pers. *pāhtā*) from Ch. *po-tieh* (\**b'vk-d'iep*) can hardly be thought of, because, at a time when the ancient final *-k* of \**b'vk* was still heard, the *-p* of \**d'iep* ought to have been represented in the transcription, and we should expect in Turkish \**baqtap*, or rather \**bāktāp*.

But the main point is that there is no reason to connect *po-tieh* with Central Asia, since it is not about Central Asia that the term makes its appearance in Chinese texts (PRZYLUKSI's statement to the contrary in *JA*, 1919, 1, 384, is an error). It is generally said to occur first in the *Hou-Han shu*, completed only c. 445, but based in principle on documents going back at least to the first quarter of the 3rd cent. There we read (116, 8*a*) : «The land of the 哀牢 Ai-lao Barbarians is fertile and yields the five cereals, silkworms and mulberries. They know how to dye in colours and [make] embroideries with designs; [they have] woollen textiles 罽毼 *chi-to*, 帛疊 *po-tieh*, and fine *lan-kan* cloth the woven designs of which (織成 *chih-ch'êng*) have the appearance of damasked (綾 *ling*) and variegated silks (錦 *chin*). They have the 梧桐 *wu-t'ung* tree (*mu*), the flowers (*hua*) of which they spin to make cloth, every strip of which is five feet broad; it is clean and white, and does not get soiled. They first cover with it the deceased, and afterwards wear it ». This passage is grammatically and technically difficult, and I am not certain that I have exactly rendered all its niceties. The meaning of *to* in *chi-to* is doubtful. Of course I have made use of the parallel text in the *Hua-yang kuo chih* (*Han hai* ed., 4, 18*b*). My addition «they have» between brackets is supported by the quotation in *T'ai-p'ing yü-lan* (796, 10*b*) and by the *Hua-yang kuo chih*. HIRTH and ROCKHILL say (*HR*, 218) that, according to ch. 101 of the *Wei shu*, *po-tieh* «was a textile fabric of hemp, which was called in their language (= of the Ai-lao) *lan-kan* ». But there is not one word of this in the *Wei shu*, which merely speaks, in the section devoted to the 僚 Lao Barbarians of south-western China (101, 10*a*; the Lao Barbarians were scattered in Yün-nan, Kuang-hsi and Tongking, according to the *Kuang chih* quoted in *T'ai-p'ing yü-lan*, 356), of their habitations built on trees (such was also the case with the aborigines in part of Hai-nan; cf. the *Kuang chih*, quoted in *Ch'u hsüeh chi*, 8, 41*a*), which habitations were called 干蘭 *kan-lan* (on which cf. *BEFEO*, IV, 171). HIRTH and ROCKHILL have certainly confused this passage with a note in which the commentary of the *Hou-Han shu*, completed in 676-678, quotes the *Hua-yang kuo chih* to the effect that *lan-kan* was the Lao word for 紵 *chu*, which, by the way, is not generally «hemp», but «grass cloth» made from the fibres of the ramie, *Boehmeria nivea*. The 華陽國志 *Hua-yang kuo chih*, or «Description of the kingdom of Hua-yang», is a work on the history of Ssü-ch'uan down to 347 A. D., and written shortly after that date; the passage in question occurs in ch. 4, 18*b*; *lan-kan* is clearly a transcription, but the original is unknown. As to *po-tieh*, it is written in the parallel passage of the *Hua-yang kuo chih* with the same characters as in the *Hou-Han shu* (the *T'ai-p'ing huan-yü chi*, 179, 13*a*, quotes from the 九州記 *Chiu-chou chi*, a geographical work by 樂資 Yo Tzū, prior to 527 [cf. *Sui ching-chi-chih k'ao-chêng*, 6, 38*a*], a text which is exactly that of the *Hou-Han shu*, except that it includes the note on the meaning of *lan-kan* and omits the four words *chi-to po-tieh*). Now, we must not forget that, although the *Hou-Han shu* bears in principle on the period of the Hou-Han (25-220 A. D.), it was only written in the first half of the 5th cent., and the study of its chapter on western countries in particular has shown that its author has made great use of works dealing with somewhat later times, in particular

of the lost *Wei lio* of Yü Huan, written in the second third of the 3rd. cent. In the paragraph we are here concerned with, either the *Hou-Han shu* has merely used the earlier *Hua-yang kuo chih*, or both have drawn from a common source, which may be Yü Huan's *Wei lio*. Whatever the case may be, there is no reason to suppose that the passage in which *po-tieh* occurs is older than the second quarter of the 3rd cent. The Ai-lao tribes lived in western Yün-nan and southern Ssü-ch'uan; their centre was in the region of Yung-ch'ang (between Ta-li and Bhamo). According to the *Hou-Han shu* (the *Hua-yang kuo chih* may have here a lacuna), the ears of the leaders of the Ai-lao descended three inches below the shoulders, while those of the commoners were on a level with the shoulders. These stories of «pendant ears» (儻耳 *tan-êrh*), which recur in the description of the island of Hai-nan (hence the *chün* of *Tan-êrh* under the Han, now *Tan-hsien*; cf. also the kingdom of *Tan-êrh* in *T'ai-p'ing yü-lan*, 790, 22 b), are a comparatively sober Chinese counterpart to the *Karṇaprāvaraṇa*, «Those who use their ears as a mantle», of Indian folklore (cf. S. LÉVI, in *JA*, 1918, I, 17, 75). Yet these same barbarians, located on the direct land track from western China to India *via* Burma, clearly were well advanced in the technique of weaving. But this does not necessarily mean, of course, that *po-tieh*, or even *tieh* alone, should be a foreign word transcribed from one of their dialects; the *Hua-yang kuo chih*, which says that *lan-kan* is a Lao word, is silent on *po-tieh*.

Leaving *po* for a while, something has to be said on *tieh*. First of all, 疊 *tieh* means «to duplicate», «to fold» (it is in that sense that it is translated *uparāpara* in BAGCHI, *Deux lexiques sanskrit-chinois*, 316; the editor ought not to have referred there to No. 536, p. 279, where *tieh* is used in its other sense of cotton textile), and has never been the name of a textile in the pre-Han texts, nor even in Han texts if we do not count that of the *Hou-Han shu* which seems to be actually of post-Han origin. As to 疊 *tieh*, which is the designation of a textile, either alone, or in *po-tieh*, it is also a post-Han creation. An attempt, however, has been made at an early date to connect *tieh*, in its meaning of textile, with a term occurring in Han texts. In Ssü-ma Ch'ien's chapter on goods and trade (129, 34-35, in TAKIGAWA Kametaro's critical edition *Shiki kaichu kōshō*), mention is made of a textile called 榻布 *t'a-pu* or 苔布 *ta-pu*, i. e. «*t'a* (or *ta*) cloth»; *t'a* is \**t'âp*, and *ta* is \**tâp*; the parallel text in the *Ch'ien-Han shu* (91, 3 b) gives *ta-pu*. When Ma Yüan was sent to Ssü-ch'uan c. 25 A. D., the local usurper presented him with a garment in 都布 *tu-pu*, «*tu* (\**tuo*) cloth» (*Hou-Han shu*, 54, 1 b); from a note of the commentary of 676-678, we learn that the lost 東觀[漢]記 *Tung-kuan* [Han] *chi* of the 2nd cent., in a parallel passage, gave *ta-pu*, and that *tu-pu* was mentioned as a name of textile by 何承天 *Ho Ch'êng-t'ien* (370-447). For the *t'a* or *ta* of *t'a-pu* or *ta-pu*, there are still secondary forms occurring in various editions or dictionaries, such as 蓊 *t'a* (\**t'âp*) in the *Yü-p'ien* (and the *T'ang yü-n*, according to the *K'ang-hsi tzü-tien*; cf. also TARANZANO, *Vocabulaire*, II, 576), 苔 *ta* (\**tâp*) in some copies of Ssü-ma Ch'ien (cf. TAKIGAWA's edition) and of the *Ch'ien-Han shu* (cf. *K'ang-hsi tzü-tien*, and *Tz'ü-yüan*, s. v. *ta*), and 蓊 *t'a* (\**t'âp*) in the *Shih-shuo hsün-yü* of Liu I-ch'ing (401-444; cf. *Tz'ü-t'ung*, 1760); to these, the *Tz'ü-t'ung* (*ibid.*) adds 納布 *na-pu*, «*na* (\**nâp*) cloth» of *Sung shu*, 71, 1 b, but the connection is doubtful (*na* seems to have been the name of a cloth [cf. the texts, often corrupt, quoted in *Ko-chih ching-yüan*, 27, 25-26, and in *Tz'ü-yüan*, s. v. *na*], but it may also perhaps be taken here with the value of 衲 *na*, «to piece

together»; 衲衣 *na-i*, as a designation of the Buddhist garb, anciently occurs as 納衣 *na-i*, for instance in 寒, x, 16 a, and in 爲, VIII, 166 b; cf. also SOOTHILL and HODOUS, *A Dictionary of Chinese Buddhist terms*, 336, 339; under the Yüan, the tribute from Fu-chien included 200 *na* robes [衲襖 *na-ao*], according to WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 32, 16 a). In his lost commentary on the *Ch'ien-Han shu* entitled *Han shu yin-i*, Mêng K'ang, who lived in the first half of the 3rd cent., had a note saying that «*ta-pu* is 白疊 *po-tieh*» (cf. also *T'ai-p'ing yü-lan*, 691, 1 a; in spite of the *Chêng-tzũ t'ung*, s. v. 棉 *mien*, Mêng K'ang of course does not yet speak of «*chi-pei*»). In the first half of the 7th cent., Yen Shih-ku opposed this view, saying (*Ch'ien-Han shu*, 91, 3 b) : « [*Ta-pu*] was a coarse cloth; it was cheap, and that is why it had to be delivered [in paying taxes] in the same weight as leather; it is not *po-tieh*. As to *ta*, [it expresses] the idea of 'thick and heavy' (厚重 *hou-chung*), but readers have foolishly given it the sound *t'a*, which is not correct.» Later authors have quoted Yen Shih-ku's text, more or less accurately, in particular Chang Shou-chieh, who, in his commentary on Ssü-ma Ch'ien, dated 736 (not «737», as said by CHAVANNES, *Mém. histor.*, I, CCXVI, and *TP*, 1907, 183, followed by LAUFER, *The Story of the Pinna*, 115), added on his own account (TAKIGAWA ed., 129, 35) : «*Po-tieh* is woven from cotton (*mu-mien*), which does not exist in China.»

Yet Yen Shih-ku's dogmatic assertion is not conclusive. The *Tz'ü-yüan*, under *tu-pu*, maintains that the *tu-pu* of Ma Yüan's biographical notice is the same as the hempen light material known in Kuang-tung as 絡布 *lo-pu* or 都落 *tu-lo*, which is worn in summer, and, at the same time, under 苔 *ta*, says that *ta* in *ta-pu* is the same as *tieh*, «to duplicate», and expresses the idea of «thickness». But, in view of the alternation of *ta-pu* and *tu-pu* in the *Hou-Han shu* and the *Tung-kuan Han chi*, it seems probable that both designate the same textile and are doublets of the same term. On the other hand, this double form would almost imply that both represent a foreign southern term borrowed twice at different stages of phonetic evolution; the relation between *ta* (\**tâp*) and *tu* (\**tuo*) would be practically the same as between *chieh* (\**kjəp*) and *ku* (\**kuo*) in the transcriptions *chieh-pei* and *ku-pei* of \**kappāi* < *kappāsī* (cf. above, p. 441). Yü Chêng-hsieh (*Kuei-ssü lei-kao*, 14, 4 a) accepts Mêng K'ang's view that the *ta-pu* was *po-tieh*, «cotton». FUJITA has also hypothetically connected with *ta-pu* the puzzling term 打布 *ta-pu* which occurs several times in the *Tao-i chih-liao*, written in the middle of the 14th cent. (*Tao-i chih-liao chiao-chu*, *Hsüeh-t'ang ts'ung-shu* ed., 15 a); on this late *ta-pu*, cf. ROCKHILL, in *TP*, 1915, 86, 237, where *ta-pu* is supposed to be some cotton fabric (but the reference to a term 達布 *ta-pu* in Chao Ju-kua must be suppressed, since Chao Ju-kua actually speaks of 于達布 *yü-ta pu*; cf. *HR*, 160). I feel very sceptical about FUJITA's hypothesis. The interval in time is so great that the proposed connection remains *en l'air* unless the expression can be traced in some southern dialect. Moreover it may not be necessary. In his *Chên-la fêng-t'u chi*, «Description of the customs of Cambodia», Chou Ta-kuan, also a writer of the Yüan period, speaks of the rules which decided the kind of «cloth» (布 *pu*) which everyone was entitled to «put on» (打 *ta*), and also of the brahmans, who, besides the «cloth put on» (*ta-pu*) by everybody, wore a band (*Ku-chin shuo hai* ed., 6 a, b, 8 a; *BEFEO*, II, 146-148). It may be that the *ta-pu* of the *Tao-i chih-liao* was a term of the spoken language, designating a cheap cotton fabric, a «cloth to wear». One might be also tempted to see the same

*t'a* (\**t'âp*) or *ta* (\**tâp*) of Han times as the first element of 氍毹 *t'a-têng* (\**t'âp-têng*), the designation under the Later Han and until T'ang times of a fine woollen rug with designs (LAUFER, *Sino-Iranica*, 492, has given an ancient pronunciation which would be \**d'iep* or *d'âp*, and referred for \**d'iep* to ch. 19 of the *I-ch'ieh ching yin-i*; but I find no gloss on *t'a-têng* in ch. 19 either of Hsüan-ying's or of Hui-lin's works which bear that title; on the other hand, both works comment more than once on *t'a-têng*, but always say that the pronunciation is \**t'âp-têng*; cf. Tōkyō *Tripit.* of Meiji, 爲, VII, 7 *b*, 17 *a*, 45 *b*, 60 *a*; VIII, 163 *b*; IX, 66 *b*, 110 *a*, 119 *b*, 149 *b*; X, 37 *a*). I think, however, that *t'a-têng* must be left out. First of all, it was not a cotton, but a woollen textile; in Tōkyō *Tripit.* of Taishō, 54, 986, *t'a-têng* (the form is corrupt in the edition) translates Skr. *kambala*, «woollen blanket or rug». On the other hand, I feel inclined, like LAUFER, to see in *t'a-têng* a term of Iranian origin, though perhaps not in the way he meant (cf. my review of LÜDERS's *Textilien* in *Oriental. Literaturzeitung*, 1938, No. 3, 185).

Yen Shih-ku was certainly right in stating that the «*ta* cloth» of Han texts was a coarse fabric and could not be what in his time was known as *po-tieh*, *i. e.* real «cotton stuff», but it is quite possible that, at an early date, before our era, the technique of cotton weaving was not yet perfected, and that a coarse cotton cloth was woven in south-western China, which went under a name «*ta* (\**tâp*) cloth» or «*tu* (\**tuo*) cloth», borrowed from some non-Chinese language of the south. Although I am not in a position to say where the «*ta* cloth» of the Former Han period was made and used in paying taxes, we must not forget that Ma Yüan's garment of «*ta* cloth» or «*tu* cloth» was offered to him in Ssü-ch'uan (the *t'a-pu* in the *Shih-shuo hsün-yü* may be a «literary» survival, inspired by Ma Yüan's biography). *Tieh* (\**d'iep*) may be the 3rd cent. transcription of the same foreign word which was formerly rendered *ta* (\**tâp*) and *tu* (\**tuo*). It is of some moment that a scholar of the first half of the 3rd cent. like Mêng K'ang should say that the «*ta* cloth» (or «*t'a* cloth») is the same as *po-tieh*; he clearly identified *ta* and *tieh* as one and the same word. Even Yen Shih-ku, while opposing Mêng K'ang, certainly thought of *tieh*, «to duplicate», when he said that *ta* implied the idea of «thickness» and «heaviness», and the authors of the *Tz'ü-yüan* have merely given a more express agreement to Yen Shih-ku's opinion when they state that *ta* was used instead of *tieh*. So, in my opinion, it is *tieh* alone, and not *po-tieh*, which is the direct continuation, in the 3rd cent., of the former *ta* and *tu* as the designation of a cotton fabric. The nature of that cotton will be more fully discussed in the last part of this inquiry, but I may already state here that, in my opinion, it was the product of the cotton tree (*Gossypium arboreum*).

Early in the 3rd cent., Chinese maritime trade with Indo-China and Indonesia also brought to China cotton goods, made from the down of either the cotton plant or the cotton tree, or of both. They too were designated as 帛疊 *po-tieh* or 白疊 *po-tieh*. Both forms are easily explainable, the first one, «silky *tieh*», implying an allusion to the glossy appearance of the fabric, and the second, «white *tieh*», referring to its colour, and it is difficult to decide which of the two was adopted first. In view of parallel cases to be quoted next, an original «white» meaning is, however, more probable. The two *po* (\**b'ok*) were absolute homophons, and there are even cases when they were used indiscriminately. The 白旃 *po-peï*, «white streamers», of the *Shih ching* (LEGGE, *Chin. Classics*, IV, 283) is quoted as 帛旃 *po-peï* in the sub-commentary of the Kung-

yang commentary on the *Ch'un-ch'iu* (cf. *Tzū-t'ung*, 18, 9). The Chinese surname of the royal family of Kučā is given as 白 Po in *Chin shu*, 97, 6 a, *Pei shih*, 97, 6 a, *Sui shu*, 83, 5 b, *Chiu T'ang shu*, 198, 7 a, and *Hsin T'ang shu*, 221 a, 8 a, but as 帛 Po in *Liang shu*, 54, 14 a, and *Tzū-chih t'ung-chien* (cf. CHAVANNES, in *Ancient Khotan*, 1, 544). In Buddhist works, the surname is usually written 帛 Po for people who either are of Kuchean origin, or have taken over the surname of a Kuchean master (cf. NANJĪŌ, *Catalogue*, App. II, Nos. 28, 36; BEFEO, VI, 350; x, 224; BAGCHI, *Le canon bouddhique*, II, 129, 319). In his *Hsing-shih hsün-yüan* (43, 4 b), CHANG Chu gives only 帛 Po as the surname of the king of Kuča. In the case of *po-tieh*, the first form, seemingly meaning « silky tieh », occurs in the *Hua-yang kuo-chih* and the *Hou-Han shu*, perhaps both drawing from the *Wei-liao*, and I know only one other example of it (the verse of Chou Pang-yen, 1057-1121, cited in the *P'ei-wên yün-fu* is merely copied from the *Hou-Han shu*), in a *sūtra* translated c. A. D. 300 (NANJĪŌ, No. 165, ch. 4; Tōkyō *Tripit.* of Meiji, 帛, x, 41 a, reading confirmed *ibid.* 爲, VIII, 186 a); here a true cotton fabric is certainly meant, and is described as such in Hui-lin's gloss. The second form, meaning apparently « white tieh », is much more common, and has finally prevailed. We find it first, in the second quarter of the 3rd cent., in Mêng K'ang's gloss on « ta cloth ». Almost at the same date, it occurs in the 吳時外國傳 *Wu shih wai-kuo chuan*, that is to say in one the accounts of the mission of K'ang T'ai and Chu Ying to Fu-nan (Cambodia) c. 245-250 (cf. *TP*, 1923, 121-122; *Études asiatiques*. . . de l'EFEO, II, 245). This account is lost, but many quotations from it have come down to us, including the following one, which has been preserved in the commentary on the *Hou-Han shu*, 116, 8 a, and in the *T'ai-p'ing yü-lan*, 820, 20 b : « The women of the country of 諸薄 Chu-po weave *po-tieh* flower cloth » (白疊花布 *po-tieh hua-pu*, i. e. « cloth made with *po-tieh* flowers »; on this meaning of *hua-pu*, cf. *infra*, p. 475; my old rendering « cotonnades à ramages » in BEFEO, IV, 270, 275, may have to be abandoned; the same may perhaps be said of my translation « étoffes à ramages » for *hua-pu* in BEFEO, II, 146, 147, 176, and of the analogous translations in *HR*, 48 [where I am not certain that 吉貝花布 *chi-pei hua-pu* is not the designation of only one product], 152, 155, 171; although the meaning of *hua-pu* may have changed between Chin and Sui or post-Sui times, I am struck by the fact that, as late as 1618, the *Tung-hsi yang k'ao* [6, 14 b] employs *hua-pu* in the general sense of « cotton goods », lit. « cloth [made] from flowers ». Chu-po (\*T'šj'wo-b'âk) certainly renders a derived form \*Ĵavag (< \*Ĵavaka) of the name of Java, although it may designate Sumatra as well (see « Iava »; cf. BEFEO, III, 264; IV, 270, 275-276; FERRAND, in *JA*, 1919, II, 5-6; 1922, II, 209). The transcription is not satisfactory, although a later 社薄 Chê-po (\*Ĵja-b'âk) provides an interesting parallel form for the second part of the name. As to 諸 *chu* (\*t'šiwō), we may here again suppose a labialization of the first vowel under the influence of the following labial consonant, but the surd nature of the initial consonant, which ought to render č, not ĵ, remains unexplained. In Kuo P'o's (276-324) commentary on the *Shan-hai ching* (*Ching-hsün-t'ung ts'ung-shu* ed., 16, 2 b; from there it has passed into the sub-commentary on the *Êrh ya*, *Shih-san-ching chu-shu* ed., 9, 24 a), we find a quotation which is copied verbatim from the *Wu-shih wai-kuo-chuan* (it is the one on the Huo-shan, the Volcano, and my note in BEFEO, III, 276, has to be changed accordingly), and there the name is written 耆薄 Ch'i-po (\*G'ji-b'âk), clearly corrupt for 者薄 Chê-po; but it would

be of no avail to start from Chê-po (\*T'šja-b'äk), and to imagine a generalized faulty reading in all the numerous quotations of T'ang and early Sung times, since Chê-po would still suppose \*Čavag, not \*Ĵavag. But since Chu-po can only refer to Java or Sumatra, it is of some importance to note that we have perhaps here the earliest Chinese location of a country where a *po-tieh* which was undoubtedly a cotton stuff of some sort, was manufactured.

The *T'ai-p'ing yü-lan* (820, 19 a) cites an edict of the Emperor Wên-ti of the Wei dynasty which, from its position between two passages of the *Wei lio*, can only refer to the Emperor Wên-ti of the first Wei dynasty (= Ts'ao P'ei, who reigned from 220 to 226), and moreover is well in the trend of many other productions of that learned Emperor; I do not know why it has been omitted from Wên-ti's writings as collected in YÊN K'o-chün's *Ch'üan shang-ku san-tai...*, sect. *Ch'üan san-kuo wên*, chs. 4-8. The text says: «The places of origin of precious things are always China and the Western Countries; the products of other countries cannot be compared with them. The 'yellow cloth' (*huang-pu*) of 代郡 *Tai chün* (=the region of Ta-t'ung in northern Shan-hsi) is fine (細 *hsi*), the 練 *lien* (=white boiled gauze) of Lo-lang (in Corea) is refined (精 *ching*), and the T'ai-mo cloth' (太末布 *T'ai-mo pu*; T'ai-mo is an ancient name of Ch'ü-chou fu in Chê-chiang) of 江東 *Chiang-tung* (lit. 'East of the Chiang', *i. e.* the eastern part of the region south of the Chiang; = Chê-chiang) is white (白 *po*), but they are not so fresh and pure as the *po-tieh* cloth (白疊布 *po-tieh pu*).» Although it is difficult to reach a definite conclusion from such a fragmentary text, it looks as though the Emperor Wên-ti wished to contrast good Chinese textiles with the *po-tieh* of western countries which he considered superior. This would be the more interesting since the text, if I have dated it correctly, may be more ancient than even the source of both the *Hua-yang kuo-chieh* and the *Hou-Han shu* and provide the earliest instance of the term *po-tieh* hitherto discovered. I find next that, in 331, Ta-yüan (Ferghâna) offered *po-tieh* to Shih Lo, who reigned in northern China (*T'ai-p'ing yü-lan*, 820, 20 b). From that time, *po-tieh* often occurs in secular literature and in Buddhist translations down to the beginning of the Sung dynasty (cf. *K'ang-hsi tsü-tien*, *s. v.* 疊, and YAMATA's Index to the various *I-ch'ieh ching yin-i*). It would be an error, however, to believe that *po-tieh* was uniformly written with the same second character. Prior to T'ang times, as if *tieh* had retained something of its foreign origin, the second element of *po-tieh* was often written by the translators or authors 繫 *chih* (\*t'siäp), 縹 *hsieh* (\*sjät; or 練 which occurs also in the tale of *Yu-yang tsa-tsu*, 14, 5 a; HUBER, *BEFEO*, VI, 37, has translated 細練 *hsi-hsieh* [= *hsi-tieh*], «*étoffe d'une finesse extraordinaire*»), 襄 *hsieh* (\*sjät), and even 繫 *chieh* (\*kiet; on this last form, faulty, but frequent, cf. *BEFEO*, IV, 357; *TP*, 1912, 463; 1932, 152-153, 183; *BEFEO*, XXIV, 102). These forms are more than once denounced as vulgar or incorrect by the authors of the different *I-ch'ieh ching yin-i* (to the remarks to that effect which may be collected from the passages mentioned in YAMATA's Index, add that of K'o-hung, ch. 18, in 爲, III, 68 a). As Hui-lin says (ch. 35, *ibid.* IX, 25 a), there was no authorized character for *tieh*, so that we find sometimes «emergency» forms created by the translators. What is more important, the same authors often say that the ancient form of *tieh* was 翳 *t'a* (\*t'äp; altered in 爲, VII, 45 b; IX, 72 b, 128 a; but correctly given in IX, 133 b); but 翳 *t'a* is to 翳 *t'a* what 疊 *tieh* is to 疊 *tieh*, that is to say, the authors of the *yin-i* established between the *tieh* of *po-tieh* and the *t'a* of *t'a*

cloth» (or «*ta* cloth») the same connection which had already been asserted by Mêng K'ang.

From all the documentation already examined, the conclusion can be safely drawn that the *po* of *po-tieh* is not a necessary constituent of the term, and that *tieh* alone, often miswritten in the forms indicated above, was a sufficient designation of the textile; hence the many terms like 疊花 *tieh-hua*, 疊衣 *tieh-i*, 織氈 *chih tieh*, etc., tens of examples of which may easily be collected from the quotations in the *P'ei-wên yün-fu*, the *K'ang-hsi tzü-tien* and YAMATA'S Index. *Tieh*, used alone, translates Skr. *paṭa* in *Tripit.* 薩, x, 7b corresponding to COWELL, *Divyāvādāna*. 405<sup>2</sup>. From a T'ang source which I have not been able to identify, Fa-yün, in his *Fan-i ming-i chi* (ch. 18, *Tripit.* 兩, xi, 87a), has culled the following notice: «鉢吒 *Po-ch'a* (\**puāt-t'ia*; Skr. *paṭa*) means in Chinese 'a strip of *man*' (縵條 *man-t'iao*); it is a strip (幅 *fu*) of *tieh* without 'field marks' (田相 *t'ien-hsiang*, i. e. without lines similar to those separating one field from another); the three garments (of a monk) are all made out of the same *man*.» A note by I-ching (張, viii, 109a) also explains *po-ch'a* as meaning *man-t'iao*. In ordinary Chinese, *man* is a name of thin plain silk; but it is constantly used in T'ang and Sung times as the designation of the loincloth, or the sampot, or the sarong of Indo-Chinese and Indonesian native populations, which was certainly not made of silk. In Sanskrit, *paṭa* means a «strip of cloth», and is translated *snam-bu*, «woollen cloth», «serge», in Tibetan (*Mahāvvyutpatti*, No. 5864); it is clearly the word meant by Fa-yün's source and I-ching, and not *paṭṭa*, «silk» (cf. *Mahāvvyutpatti*, No. 5867), with which it is often confused (cf. LÜDERS, *Textilien im alten Turkistan*, 24-28, and my remarks in *Oriental. Literaturzeitung*, 1938, No. 3, 186). But the Tibetans had little knowledge of cotton, and I take Fa-yün's text as meaning that *tieh* was, in principle, the designation of a strip of plain cotton. Speaking of Kao-ch'ang, Hsüan-ying and Hui-lin always say that «cotton» is called there *tieh*; they never say *po-tieh*. The pilgrims Hsüan-tsang and Hui-ch'ao, when describing the cotton garments of the people of India and Central Asia, always use *tieh* alone; when we find once in Hui-ch'ao (FUJITA ed., 70b) the mention that the people of the present Russian Turkestan used to wear 白氈帽子 *po-tieh mao-tzū*, I have no doubt that he intended to indicate the colour, and that we must translate «caps of white cotton». This also explains that *po-tieh* should have been rendered *yürüng böz*, «white cotton stuff» in a Uighur translation made from the Chinese (cf. *supra*, p. 434). An identical case occurs in the Tibetan *'Jaṅs-blun*, translated from the Chinese *Hsien-yü ching*. In Tibetan, *ras*, «cotton stuff», is the exact equivalent of Turk. *böz*, and *po-tieh* of *Hsien-yü ching*, ch. 5, §. 25 (宿, ix, 30b) is rendered *ras dkar-po*, «white cotton stuff», by the Tibetan translator (cf. SCHMIDT, *Der Weise und der Thor*, Tib. text, 120; transl., 149; the other passages in which *po-tieh* or *tieh* occurs in the *Hsien-yü ching*, 20a, 59b, 70b, belong to sentences or paragraphs missing in the Tibetan version).

In many languages, confusion has occurred in the meaning of words referring to cotton, woollen, and even silk fabrics; *tieh* is no exception to the rule. The *K'ang-hsi tzü-tien*, followed by the *Tz'ü-yüan*, gives only one meaning for *tieh*, that of 細毛布 *hsi mao-pu*, «fine woollen cloth» (lit. «fine hair cloth»), and the *Tz'ü-yüan* gives «silken stuff» as the primary meaning of *po-tieh*. D'HERVEY DE SAINT-DENYS (*Ethnogr. des peuples étrangers*, Méridionaux, 539-540) thought that *po-tieh* could be «the *nippis* cloth of the Philippine Islands, although he admitted

that this was not white. When Fa-hsien speaks of the *po-tieh* tied round the bamboo structure of a procession car in Magadha, LEGGE (*The Travels of Fâ-Hien*, 79) gives «white and silk-like cloth of hair», whereas the material meant by Fa-hsien was certainly cotton. As late as 1910, in his *500 Contes*, CHAVANNES translated *po-tieh*, «*étoffe blanche*» (I, 86), and «*tapis blanc*» (II, 187-188), and *tieh* alone «*feutre*» (I, 168; clearly a slip), or «*tapis*» (II, 139, 187), or «*drap*» (II, 229), cotton being adopted only sporadically (I, 165, 166; II, 142). The word «cotton» was not even mentioned under *tieh* in the first edition of GILES's *Chinese-English Dictionary*. In stating that *tieh* was a woollen cloth, the authors of the *K'ang-hsi tzü-tien* merely followed in the wake of their predecessors: the definition of *tieh* as «fine woollen cloth» already occurred in the 博 著 *P'i-ts'ang* of 張 揖 Chang I, written in the middle of the 3rd cent. (cf. Hui-lin, 34, in 爲, IX, 20 a; the contrary statements in chs. 55 and 78, [IX, 128; X, 33 a] are due to corruptions of the text), and it was repeated in 呂 忱 Lü Shên's 字 林 *Tzū lin* (c. 270 A. D.; *ibid.* ch. 52 [IX, 112 a]; the date is obtained by combining the indication of Lü Shên's master in *Pei shih*, 34, 12 a, with the biography of the latter in *Chin shu*, 37, 3 a-b), and in Lu Fa-yen's famous *Ch'ieh yün*, published in 601 (Hsi-lin, ch. 5 [VIII, 20 a; altered in VIII, 21 a]; Hui-lin, ch. 27 [VIII, 171 a]). A similar statement is made in Kuo I-kung's 廣 志 *Kuang chih* (4th or 5th cent.), which says that the «*po-tieh* cloth», a produce of the island (*chou*) of Chu-po (Java or Sumatra), «is woven with wool» (lit. «hair»; 毛 織 *mao-chih*; cf. Ssü-ma Chêng's commentary [c. 730] on Ssü-ma Ch'ien, in TAKIGAWA's ed., 129, 35, and *T'ai-p'ing yü-lan*, 820, 20 b; this passage, which does not occur in the fragments of the *Kuang chih* collected in ch. 61 of the *Shuo fu* in 120 chs., is given in the reconstructed *Kuang chih* of the *Yü-han-shan-fang chi i-shu*, I, 6 a). The only dictionary to express a partly divergent opinion is the 考 聲 *K'ao shêng*, the date of which I do not know (I do not find any mention of this work in HSIEH Ch'i-k'un's *Hsiao-hsüeh k'ao*, nor in the list of ancient lexicographical works tabulated by YAMADA at the end of his Index of the various *I-ch'ieh ching yin-i*; it is often cited, however, by Hui-lin, and from his ch. 39 [爲, IX, 44 b], we learn that the name of the author was 張 戩 Chang Chien; I do not know whether he is the same Chang Chien whose work on mourning rites is mentioned in *Hsin T'ang shu*, 58, 10 b). In the *K'ao shêng*, the definition of *tieh* is: «It is a woollen cloth (*mao pu*); it is also a cloth [made] of the flowers of a plant» 草 花 布 *ts'ao-hua pu*; Hui-lin, chs. 14 and 40, in 爲, VIII, 102 b; IX, 50 b; the text is incomplete in chs. 34 and 55, *ibid.* IX, 20 a, 128 a). In the same manner, K'uei-chi, one of the principal disciples of Hsüan-tsang, says (*ibid.* VIII, 171 a): «According to the *Ch'ieh yün*, [*tieh*] is a 'fine woollen cloth'. The modern use of the term is not in agreement [with this definition]. There are, besides [the old woollen cloth], 氈 *chan* [read 氈 *tieh*] flowers which are woven so as to make cloth. What is made with wool is serge (襪 *ho*) and 罽 *chi*.» So even those who, like the author of the *K'ao shêng* and K'uei-chi, know *tieh* as a cotton fabric, did not dare to depart from the more ancient authorities, but considered that a new meaning had developed. Yet, the examples in Buddhist translations clearly refer to cotton, and to cotton only (with an exception to be discussed in the next paragraph). Whenever the quotations in the *K'ang-hsi tzü-tien* are precise enough to allow of an identification, it is also cotton which is meant. Quite evidently too, the *tieh* or *po-tieh* produced in Java or Sumatra, about which the *Kuang chih* says that it was «woven with wool», is the same as the *po-tieh* woven



by women of the same country according to the reports of K'ang T'ai and Chu Ying, and this was certainly cotton. Although, at first, *tieh*, as the equivalent of the *ta* or *t'a* of «*ta* cloth» or «*t'a* cloth», may have referred to cotton of the cotton tree as well as of the cotton plant, I maintain that it has always been the name of a fabric of vegetable, not of animal origin.

In I-ching's translations of the *Vinaya* of the Mūlasarvāstivādin, there is a passage, however, in which *tieh* is used in a wider sense. Speaking of fine textiles, the translation says (ch. 44; 張, IX, 89 b) : « [Of fine textiles], there are four sorts, which are called 'Chia-shih fine *tieh*' 加尸細疊, 'ch'u-mo fine *tieh*' (芻摩細疊), 'chu-ma fine *tieh*' (紵麻細疊), and 'ku-ch'ê-po-chia fine *tieh*' (孤沾薄迦細疊). The «Chia-shih fine *tieh*» is the «Kāśī (= Benares) cotton muslin», which presents no difficulty; but *ch'u-mo* transcribes Skr. *kṣauma*, «flax», «linen», *chu-ma* is «ramie», and *ku-ch'ê-po-chia*, \**kochavaka*, can be no other than Skr. \**kaucapa*, a kind of «woollen blanket» or «carpet» (cf. *infra*, p. 492). So only the first of the four fine *tieh* is really a cotton stuff (the same may be said of the use of Tib. *ras* in *Mahāvvyutpatti*, 9160-9163). My impression is that this is due to the loose value of Skr. *paṭa* («strip of cotton stuff», but also a designation of strips of other textiles), which was probably the word used in the Sanskrit original which I-ching translated.

I may add that there were many mixed textiles, one-half or one-third of which was cotton; a descriptive list of such fabrics has been preserved in ch. 2 of NANJĪŌ, No. 1107 (寒, x, 16 a-b). The *Hsien-yü ching* (ch. 12, § 50; NANJĪŌ, No. 1322; 宿, IX, 70 b) mentions «a piece of gold coloured *tieh*» (一端金色之疊) made by Mahāprajāpatī. The *K'ang-hsi tzū-tien*, quoting this (s. v. *tieh*), adds that, according to a gloss of the text, this was the designation of a 織成大衣 *chih-ch'êng ta-i*, «great garment in *chih-ch'êng*». As a matter of fact the quotation in the *K'ang-hsi tzū-tien* is second hand, and is drawn from the *Fa-yüan chu-lin* (ch. 41; 雨, VII, 78 b), where the gloss is an addition. We cannot check the T'ang interpretation of the passage from the Tibetan translation, because, in the latter, there is no section corresponding to ch. 12, § 50 of the Chinese text. But there is no reason to doubt the gloss of the *Fa-yüan chu-lin*, which goes back to the 7th cent. Now, the important point is that *chih-ch'êng*, as it is used in the gloss, must be the technical designation of a fabric with designs and ornaments, a kind of brocade without a uniform coloured «ground» (cf. *infra*, p. 507). Mahāprajāpatī's textile recurs in CHAVANNES, 500 *Contes*, III, 46 (cf. also PRZYLUŚKI, in *JA*, 1919, I, 397), where «un vêtement complet tissé en fils d'or» renders 金縷織成衣 *chin-lü chih-ch'êng i*, more accurately «a garment of gold thread brocade» (cf. also *ibid.* I, 281, «vêtements faits en tissu d'or», which really are «gold brocade garment»). The very fact that the said *tieh* was «gold coloured» precludes the possibility that it should have been a strip of plain cotton stuff. So we must be prepared to find *tieh* used in a number of cases with a wider meaning than the one I have deduced as original and fundamental.

Early in the Sung dynasty, a new character 疊 *tieh* was formed to write the *tieh* of *po-tieh*; it occurs for the first time in the 集韻 *Chi-yün*, a dictionary published by Ting Tu (990-1053), and is mentioned in the 饗牖問評 *Wêng-yü hsien-p'ing*, a miscellaneous work written by 袁文 Yüan Wên (12th cent.; *Wu-ying-tien chü-chên-pan ts'ung-shu* ed., 4, 3 a). But it never met with general acceptance, and I know of no actual example of it in a text.

THE RELATIVE MEANING OF KU-PEI (CHI-PEI) AND PO-TIEH. — SOOTHILL and HODOUS (*A Dictionary of Chinese Buddhism*, 232) say that Skr. *kārpāsa* is «cotton, *Gossypium herbaceum*», but that the Chinese transcriptions of the word refer «especially to *kārpāsī*, the cotton tree» (*i. e.* silk-cotton tree). I do not think that in the consideration of Indian sources such a strict line can be drawn between the values of *kārpāsa* and *kārpāsī*, while, on the other hand, Chinese texts certainly do not allow us to regard *ku-pei* (and later *chi-pei*) as referring to the silk-cotton tree. On the other hand, *kārpāsa* is «cotton» rather than «cotton stuff», while *po-tieh* has always been the designation of a cotton fabric. But the fact is that both *ku-pei* (*chi-pei*) and *po-tieh* soon came to be used as designations of textiles, and that a distinction was made between them. We have seen (p. 439) that, in 430, an Indonesian kingdom sent to China «*po-tieh* and *ku-pei* of the kingdom of T'ien-chu (India) and *ku-pei* of the kingdom of Yeh-po (Gandhāra)». By the way, if *ku-pei* were «silk-cotton tree», *Bombax malabaricum*, as seems to have been thought by SOOTHILL and HODOUS, the inferior fabric made out of the floss of the *Bombax* (if any could be made) would have travelled all the way from the north-western Indian frontier to Indonesia and China, which certainly does not seem to be probable. In *Sui shu*, 82, 3 a, we are told that, when holding his Court, the king of Chên-la (Cambodia) wore 朝霞古貝瞞 *chao-hsia ku-pei man*, «a sampot of dawn-rosy *ku-pei*»; in ordinary circumstances, he wore *po-tieh*. (I have refuted, *TP*, 1912, 480, HIRTH and ROCKHILL's mistaken explanation [*HR*, 218-219] of *chao-hsia* as a transcription of Skr. *kaūseyā*, «silken stuff [from cocoons of wild silkworms]»; cf. also my review of LÜDERS's memoir *Textilien im alten Turkistan* [*Abhandl. d. Pr. Ak. d. Wiss.* 1936, Ph.-hist. Kl., No. 3], in *Oriental. Literaturzeitung*, 1938, No. 3, 186; as to *man*, more often written 縵 *man*, which also occurs as *kan-man*, *tu-man*, *ho-man*, it was another mistake of HIRTH and ROCKHILL, *HR*, 64, to derive it from Skr. *kambala*, «wool»; it represents the Chinese adaptation of an Indonesian word, the forms of which vary from Malay *kěmban* to Cham *aban*, Bahnar *habān*.) So, in this case, the coloured *ku-pei* was a cotton stuff regarded as superior to the *po-tieh*, which must have been a plain white fabric. According to the *Chiu T'ang shu* (197, 1 a), the king of Champa (see «Ciamba») wore *po-tieh ku-pei* which covered the upper arm diagonally and went round the waist, while his consort donned *chao-hsia ku-pei*, which she arranged into a short skirt (短裙 *tuān ch'ün*). On the next page (197, 1 b) in the notice of the kingdom of P'o-li (Bali?), we are told that «it has the *ku-pei* plant (*ts'ao*); [the people] take its flowers, and make cloth with [them]; that which is coarse is called *ku-pei*; that which is fine is called *po-tieh*». Practically the same indications are given in the *Hsin T'ang shu* (222 c, 1 a, b), except that, in the passage on the «*ku-pei* plant», the *Hsin T'ang shu* does not say that the coarse is *ku-pei* and the fine *po-tieh*, but merely that they are *pei* and *tieh*, respectively. Here at least, the *ku-pei* is certainly not a «cotton tree» in the sense of «silk-cotton tree», *Bombax malabaricum*, nor even, probably, what I call cotton tree, *Gossypium arboreum*, but a plant (*ts'ao*), the *Gossypium herbaceum*; it will be remembered that *tieh* is defined «a cloth made with the flowers of a plant of the Western countries» by Hui-lin (*Tripit.* 爲, ix, 72 b, 114 a). So, it would seem that *ku-pei* had different values: it was the name of the cotton plant, and of cotton itself, and was also used as a generic term for all cotton stuffs, so that a plain white variety could be called a *po-tieh ku-pei* and a gay-coloured one a *chao-hsia ku-pei*; but, as the specific name of a particular

fabric, *ku-pei* was the coarse, in contradistinction to the fine variety called *po-tieh*. Moreover, *po-tieh*, either from the outset or soon after it had been adopted, meant «white *tieh*»; but, in the absence of any other specification, it was used as a generic name for «fine cotton stuff». *Tieh* alone, however, was always felt to be a word which had this same meaning, and could be used in other combinations. The *tieh* was «fine», and yet we are told of «coarse *tieh*» (麤疊 *ts'u tieh*; cf. Hui-lin, ch. 65, in 爲, IX, 72 b). Side by side with *po-tieh*, «white *tieh*», there are examples of 黑疊 *hei-tieh*, «black *tieh*» (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 133, where the translation «black garments forming folds» for *hei-tieh* is erroneous); one could not have said «black *po-tieh*» without indulging in a contradiction in terms. A distinction is hardly to be traced in Sung times between *ku-pei* (which had become *chi-pei*) and *po-tieh*, because *po-tieh* seems to have already become an obsolete term by that time, occurring only in quotations made from earlier works or in allusions to them. Neither *ku-pei* (*chi-pei*) nor *po-tieh* occurs in Fan Ch'êng-ta's *Kuei-hai yü-hêng chih*, the preface of which is dated 1175; it employs only *mu-mien* (*Chih-pu-tsu-chai ts'ung-shu* ed., 14 a). In the *Ling-wai tai-ta*, *mu-mien* is used only once (10, 9 a), in a passage which seems to be derived from a more ancient source, and it is not mentioned in the special paragraph entitled *chi-pei* (6, 12-13) which is devoted to cotton. In this *chi-pei* paragraph, *po-tieh* occurs only in connection with the Nan-chao (not «Laos» as in *HR*, 219, but the Ta-li kingdom in Yün-nan), but the very name «Nan-chao» betrays the antiquarian nature of the passage, since the former Nan-chao kingdom of T'ang times was known under the Sung as the kingdom of Ta-li; and, moreover, «Nan-chao», in Chou Ch'ü-fei's text, is a misquotation from the *Hsin T'ang shu*, where the passage refers not to Nan-chao, but to Champa. Chao Ju-kua's two mentions of *po-tieh*, in the paragraphs on Champa and India (*HR*, 48 [where the rendering «white muslins» is inaccurate], 111), are also derived from T'ang sources, but nothing is said of *po-tieh* in the special paragraph on «cotton», entitled *chi-pei* (*HR*, 217-218). In the Supplement to the *P'ei-wên yü-n-fu* (*Yün-fu shih-i*, 105, 9 a, s. v. «po-tieh»), a passage is given as drawn from the notice of the kingdom of Ta-li in the *Sung shih*; the Ta-li people are said there to have had almost the same customs and the same dress as the Arabs (Ta-shih), and to have worn around the breast a piece of *po-tieh* which reached to their feet. But it must be a misquotation, since there is not a word of all this in the Ta-li paragraph of the *Sung shih* (488, 8 a-b), and the whole passage is in *Sung shih*, 489, 1 a-b, and refers to Champa. It is the only one which would suggest that *po-tieh* was still a living term in southern China under the Sung, and it may go back to a source dating from the beginning of the dynasty. The only indication that *po-tieh* had more or less survived either in northern China, or in Chinese Turkestan until the Mongol period is provided by the account of Ch'iu Ch'ang-ch'un's travels across Central Asia in 1221-1224. In speaking of the dress of the inhabitants of what is now Russian Turkestan, Ch'iu Ch'ang-ch'un says that it was made of *po-tieh* (WANG Kuo-wei's ed., 2, 1 b), and repeats it in a poem (2, 2 b). In the first case, BRETSCHNEIDER (*Br*, I, 89) has inaccurately rendered *po-tieh* as «woollen stuff» and WALEY (*Travels of an Alchemist*, 107) correctly as «cotton»; both have omitted the poem. The «*po-tieh* cloth» mentioned by Hsü Kuang-ch'i in speaking of the garb of the Buddhist patriarchs (cf. *infra*, p. 489) is a scholarly recollection. We have, I think, instances of misleading archaizing, among hundreds of others, when the *Ming i-t'ung chih* (87, 35 a; followed by *T'u-shu pien*, 89, 31 b)

speaks of *po-tieh* as being made in Burma, or (87, 38 a; *T'u-shu pien*, 89, 51 a) at 干崖 Kan-yai (also called 干額 Kan-ê, at the confluence of the Nam-ti and the Ta-ping in Upper Burma; cf. *BEFEO*, ix, 666; the Sino-Pa-i Vocabulary, 10 b, renders Kan-yai as 猛那 Mêng-na, which then ought to be different from the 猛乃 Mêng-nai of *BEFEO*, ix, 669), or when it says (87, 29 b; *T'u-shu pien*, 89, 30 b) that Chin-ch'ih (= Yung-ch'ang) produces 縹纈 *piao-tieh* (? «purple tieh»), which is «*po-tieh* cloth» (*po-tieh pu*).

In this paragraph on *chi-pei*, Chao Ju-kua enumerates four qualities of cotton cloth, which, in decreasing order of value, are *tou-lo-mien* (cf. *supra*, p. 431), 番布 *fan-pu* («foreign cloth»), *mu-mien* properly so called, and 吉布 *chi-pu*. In Chao Ju-kua's accounts of the various kingdoms, *tou-lo-mien* is mentioned in the kingdoms of India and of the Arabs (*HR*, 88, 97, 116). *Fan-pu* often occurs, sometimes as of various colours (*HR*, 61, 84, 87, 88, 92, 126); we also find this term in *YS*, 16, 9 a, in the account of an embassy of 1291 from Quilon (see «Coilum»). *Mu-mien* is mentioned by Chao Ju-kua in the section on Chiao-chih (Tongking), where *mu-mien* and *chi-pei* follow each other in the list of the native products (*HR*, 46). Moreover, in the section on Hai-nan (*HR*, 183), we hear of the *mu-mien* which the Li aborigines mix up in their textiles with threads unravelled from Chinese silken fabrics to make new cloth, but this is taken over from the *Kuei-hai yü-hêng chih*, 14 a, which speaks only of *mu-mien* (*chi-pei* never occurs in the work), and *chi-pei* is Chao's own and perhaps arbitrary addition. The same may be said of the next sentence, when he adds that the Li women also weave cloth made only of «*mu-mien* or *chi-pei*». It looks as though the distinction apparently drawn by Chao Ju-kua between *mu-mien*, which he uses only for Tongking and Hai-nan, and *chi-pei*, which occurs for these and other countries, were artificial and due primarily to his use of different sources. One might think at first that he understood *chi-pei* as *Gossypium herbaceum*, and *mu-mien* as *Gossypium arboreum*, so that there should be no serious objection to the respective translations «cotton» and «tree cotton», which have been adopted by HIRTH and ROCKHILL. We must remember, however, that, in 1178, Chou Ch'ü-fei knew only the cotton tree (*shu*), not the cotton plant (*ts'ao*), and it was precisely this difference which made him hesitate to accept the otherwise obvious identification of the «*ku-pei* plant» of pre-T'ang and T'ang texts with the «*chi-pei* tree» of his own time (cf. *infra*, p. 437). Unless a great change had taken place between 1178 and 1225, Chao Ju-kua's *chi-pei* ought also to be *Gossypium arboreum*, and, as such, synonymous with *mu-mien*. But the fact is that we lack information as to the place, time, and condition in which the cotton plant progressively took the place of the cotton tree; it is only the cotton plant which we find from the outset in Chiang-su, as results from the details given in HU San-hsing's text (cf. *infra*, p. 501). As to Chao's *chi-pu*, it does not occur anywhere else in his book, and may be a misreading for *chi-pei* or *chi-pei pu* (the 吉費 *chi-pei* of *HR*, 155, is another misreading; as to *pei*, used alone twice, *HR*, 177, 181, it is either also a misreading of *chi-pei*, or a «literary» abbreviation based on its use in the parallel definition of *po-tieh* and *ku-pei* in the *Hsin T'ang shu*; cf. p. 442); *chi-pei*, and its equivalent *chi-pei pu*, «*chi-pei* cloth», are repeatedly mentioned in Chao's descriptions of the various kingdoms. I must add, however, that *chi-pu* occurs in Wang Chêng's *Nung shu* (21, 16 a; 吉衣 *chi-i* is a misquotation in Wang Chêng's text as cited by Hsü Kuang-ch'i; cf. *Chin. Repository*, xix, 469), and it may be, after all, that *chi-pei pu* was

sometimes abbreviated as *chi-pu*, which gave the auspicious meaning «fortunate cloth». Chao Ju-kua's four categories leave out some of the terms he uses elsewhere, like *pu* alone (very frequent), 細布 *hsi-pu*, «fine cloth» (*HR*, 134; this was the designation of a particular fabric in Ming times and perhaps earlier), 雪布 *hsüeh-pu*, «snow cloth» (*HR*, 135), etc. In all such cases, HIRTH and ROCKHILL have rendered *pu* as «cotton cloth» or «cotton stuff», which may happen to be correct for the textiles of certain countries, but is not necessarily so; *pu* has been the ordinary name of «cotton stuff» only after cotton became a staple product in China, as it was not in Chao Ju-kua's time. In particular, it seems highly doubtful that, as would be implied by HIRTH and ROCKHILL's translation (*HR*, 168, 171), cotton cloth should have been generally used in Corea and Japan in the first quarter of the 13th cent. According to the *Nihon-koki* (ch. 8), cotton was introduced into Japan, independently of China, in 799, by a shipwrecked Hindu, or, according to the *Ruiju-kokushi* (ch. 199), in 800 by a shipwrecked man from K'un-lun (Indonesia and Malay Peninsula); cf. MOOKERJI, *A History of Indian shipping*, 1912, p. 174 (quoting TAKAKUSU's lecture published in *Journal of the Indo-Japanese Association*, Jan. 1910). But the adoption of the new textile seems to have been slow, and, moreover, would not affect the Corean aspect of the problem.

KU-CHUNG. — The question of the silk-cotton tree, the cotton tree, and the cotton plant has always been a *crux* for Chinese authors since Ming times. In recent Chinese works (cf. for instance *Chung-kuo* [藥] *yao-hsüeh ta tz'ü-tien*, 234), cotton from the silk-cotton tree is called 木棉 *mu-mien*, lit. «tree-cotton», and the true cotton, *Gossypium herbaceum*, is called 棉花 *mien-hua*, «cotton-flowers», or, still more recently, 草棉 *ts'ao-mien*, lit. «plant-cotton». Not a word is said of the cotton tree, *Gossypium arboreum*. But all this is modern nomenclature, and a mechanical application of it to the past is responsible for many errors. One of the more serious mistakes was made by LAUFER (*Sino-Iranica*, 491) when he said that LI Shih-chên, the author of the *Pên-ts'ao kang-mu*, considered the 古終 *ku-chung* as a «tree-cotton 木綿 (*Bombax malabaricum*)». It is true that LI Shih-chên (36, 71 b, 72 a) speaks of the *ku-chung* as a *mu-mien*, but it is because he always uses *mu-mien* as a designation of both the cotton plant and the silk-cotton tree; and, in the case of *ku-chung*, he most emphatically declares it twice to be «the *mu-mien* which is like a plant», not «the *mu-mien* which is like a tree».

Whether LI Shih-chên be right or not is another matter, and the problem must be approached from a different angle. In his notice on *mu-mien* (cotton), LI Shih-chên quotes a passage which is said to come from the lost 南越志 *Nan-yüeh chih*, or «Description of Nan-yüeh» (= Kuang-tung and Tongking), written by 沈懷遠 *Shên Huai-yüan* in the third quarter of the 5th cent. (cf. *Sung shu*, 82, 7 b-8 a; *Sui ching-chi-chih k'ao-chêng*, 3, 8). In LI Shih-chên, the passage is as follows: «桂州 Kuei-chou produces the 古終藤 *ku-chung t'êng* («*ku-chung* creeper»), the fruit of which is like goose down, and the kernel like 珠珣 *chu-hsün*. [The people] remove the kernel, spin [the down] as [one spins] silk floss (絲綿 *ssü-mien*) and dye it to make 'variegated cloth' (斑布 *pan-pu*)». This is the only text in which the name *ku-chung* occurs. Starting from it, SCHOTT in 1867 (*Altäische Studien*, III, 137, 138), and

independently MAYERS (in *Notes and Queries*, II, 95), followed by HIRTH and ROCKHILL (*HR*, 219), have thought of a Fukienese pronunciation «ku-tüng», and explained the term as a transcription of the Arabic *qutn*, *qutun*, «cotton» (cf. *supra*, p. 427). This is certainly the reason why *ku-chung* is underlined as being a transcription in TARANZANO, *Vocabulaire*, I, 344. LAUFER was right in denouncing such an etymology as impossible for many reasons; but one of his reasons will not stand, when, contrary to LI Shih-chên's plain statements, he says that LI Shih-chên held the *ku-chung* to be a «tree-cotton» (in the sense of *Bombax*). Moreover, had he not neglected the word *t'êng*, he would have noticed that a «creeper» was not a tree. But more serious difficulties have hitherto been left unnoticed. The *Nan-yüeh chih* was certainly written in the 5th cent. But Kuei-chou, which corresponds to the present Kuei-lin in Kuang-hsi, is an administrative name which was adopted for the first time in 507 (cf. *Ta-Ch'ing i-t'ung chih*, 355, 1 a). We shall see farther on that another quotation made from the *Nan-yüeh chih* by LI Shih-chên cannot be pre-T'ang. Perhaps the present one of unknown origin, if it be genuine, may come from the later continuation of the *Nan-yüeh chih*, the 續南越志 *Hsü Nan-yüeh chih*, on which cf. MASPERO, in *BEFEO*, XVIII, III, 7. Neither passage is among the fragments of the *Nan-yüeh chih* published in the *Shuo fu* in 120 chs., ch. 61. The extant fragments of the *Nan-yüeh chih* have been successively collected by CHANG Tsung-yüan and YEN K'o-chün, but unfortunately their work has never been published (cf. WANG Chung-min in *Fu-jên hsüeh-chih*, III [1932], No. 1, 17). In the present case, the authenticity of the passage may all the more be challenged on account of its disturbing similitude with the still more doubtful text quoted in the *T'u-shu chi-ch'êng* as from the *Nan-chou i-wu chih* (cf. *supra*, p. 439). This latter text says: «The *mu-mien* (cotton) is produced by the *chi-pei*. When it is ripe, it is like goose down, and finer (*hsi*) than silk floss (*ssü-mien*). Inside there are kernels like *chu-hsün*. To use the [*mu-mien*], [the people] remove the kernels. Formerly they employed a rolling mill (輾軸 *chan-chou*); now they employ a seed cleansing stand (攪車 *chiao-ch'ê*), which is more convenient. The cloth they make is called 'variegated cloth' (*pan-pu*). That which is gaily ornamented (繁縟 *fan-ju*) is called 城 *ch'êng* (? 'city [cloth]'); a coarser one is called 文縟 *wên-ju* ('ornamented'); a still coarser one is called 烏驎 *wu-lin* ('black piebald')». It seems clear that the passing from the «rolling mill» to the «seed cleansing stand» (this stand is depicted and described in *Nung shu*, 20, 16 b) and all the Chinese trade names for the different kinds of «variegated cloth» could only have been thought of at a time when the manufacture of cotton had reached a certain stage of development in China, that is to say, certainly not before the Sung dynasty. But, at the same time, if we leave out the name of the «*ku-chung* creeper» on the one hand, and the process of the manufacture of cotton stuff on the other, there is between the so-called text of the *Nan-yüeh chih* and that attributed to the *Nan-chou i-wu chih* too striking a coincidence in the trend and the terms of the description to be accidental. The «goose down» (the comparison with «goose down» and the name *pan-pu*, «variegated cloth», are used in the description of the cotton of Champa in *Nan shih*, 78, 1 b), the «silk floss» and, above all, the «*chu-hsün*» would not have spontaneously come twice in the same order to the mind of two different authors. There is even perhaps some ground to think that the so-called *Nan-yüeh chih* quotation may have been

remodelled on that of the pseudo-*Nan-chou i-wu chih* at a late date. Both the *Nan-yüeh chih* in LI Shih-chên's quotation and the *Nan-chou i-wu chih* as reproduced in the *T'u-shu chi-ch'êng* speak of 斑布 *pan-pu*, «variegated cloth», but in the quotation of the latter text made in 1313 by the author of the *Nung shu*, this term is written twice in the form 班布 *pan-pu*, of identical meaning, which is often used in pre-Sung texts (cf. the quotations in the *P'ei-wên yün-fu*); of course, both quotations may have used the more modern form, but it may also be that the so-called *Nan-yüeh chih* quotation was copied from the pseudo-*Nan-chou i-wu chih* after one form of *pan-pu* had, in the latter text, been changed into the other. More striking is the case of 珠珣 *chu-hsün*, which I have left untranslated. It occurs on the one hand in the quotation from the pseudo-*Nan-chou i-wu chih* as given in the *T'u-shu chi-ch'êng*, and on the other in the quotation from the *Nan-yüeh chih* in the *Pên-ts'ao kang-mu*; and it is carefully retained by the *T'u-shu chi-ch'êng* (*ibid.* 303, 11 a) and by the *Chung-kuo yao-hsüeh ta tz'ü-tien* (p. 235) in copying LI Shih-chên's notice (but the *T'u-shu chi-ch'êng*, *ibid.*, *chi-shih*, 1 a, leaves out *hsün*, retaining only *chu*, when reproducing the *Nan-yüeh chih* passage alone). Yet it does not make much sense. *Chu* means «pearl», and *hsün* is a half-precious stone more or less similar to jade (I do not know on what authority TARANZANO, II, 581, says it is a «branch of coral»). Now, instead of *chu-hsün*, the quotation in the *Nung shu* gives 珠珣 *chu-kou*, with a phonetic note expressly stating that the second character is to be read *kou*. *Kou* is also the designation of a kind of jade, and *chu-kou* is not much more satisfactory than *chu-hsün*. But I feel inclined to think that we have here an unauthorized form of some popular term designating perhaps seed pearls, with which cotton seeds could be compared and which was still understood in 1313. At any rate, the *Nung shu*, with its phonetic gloss, must have preserved the original reading, and since the quotation from the *Nan-yüeh chih* gives the same corrupt form *chu-hsün* which we find in the pseudo-*Nan-chou i-wu chih* as quoted in the *T'u-shu chi-ch'êng*, there is a fair chance that the so-called *Nan-yüeh chih* passage is a piecemeal forgery later than 1313. Some clue may perhaps be found to the origin of part at least of the so-called quotation from the *Nan-yüeh chih*. The *I-wên lei-chü*, a T'ang encyclopaedia completed c. 640, quotes a passage from P'ei Yüan's *Kuang-chou chi*, a work prior to 527 (cf. *infra*, p. 462), on the use of *mu-mien* («cotton») by the southern Barbarians to make cloth. The first sentence of the quotation will be translated farther on in the discussion of *mu-mien*. Then come four characters which I hardly dare translate (皮圓當竹, «the skin is round like that of the bamboo»?), and a last sentence: «They peel (剥 *po*) old «green creepers» (古綠藤 *ku-lü t'êng*) and spin (績 *chi*) [the skin] to make cloth (*pu*)». This text is certainly not clear; moreover it looks as if two different quotations, perhaps from two different works, had erroneously become amalgamated into one. The «peeling» of creepers occurs elsewhere: the *Nan-fang ts'ao-mu chuang* (quoted in *Ch'i-min yao-shu*, 10, 36 a) describes a creeper, called 耳藤 *êrh-t'êng*, «tuft creeper», which the people of the south «peel» (*po*) to make tufts (*êrh*). But the interpretation of *ku lü t'êng* is doubtful. It may be taken as the name of a creeper, the «*ku-lü* creeper»; or as meaning «old 'green creepers'», «green creeper» being then the name of a *lü-t'êng* creeper; or as meaning old creepers which are still green; or even, though less probably, as meaning «old [and] green creepers». I have found no trace of a creeper called

*ku-lü t'êng* or *lü-t'êng*; the terms *ku t'êng* and *lü t'êng* occur fairly often in literature, but merely in the sense of *t'êng* that are old or of *t'êng* that are green. Whatever the case may be, the main point is that the 古綠藤 *ku lü-t'êng*, from which cloth was made, is hardly different from the cloth giving 古終藤 *ku-chung t'êng* in the pseudo-*Nan-yüeh chih* text; one of the two forms must be a graphic corruption of the other, but I am not in a position to tell which. One result, however, has been reached, I think, by this provisional inquiry: the «*ku-chung* creeper», which has played too important a part in European research on the history of cotton in China, should disappear altogether from Chinese botanical nomenclature. It is almost needless to add that LAUFER's attempt at connecting *ku-chung* with Lepča *ka-čuk ki-kun*, «cotton tree», Sin-p'o *ga-duñ*, *id.* and Meo *čoa*, «cotton», is valueless.

MU-MIEN. — In discussing the question of *ku-chung*, I have referred several times to cotton as *mu-mien*, and we have seen that LAUFER rendered it «tree-cotton», which is indeed a word-for-word translation (*mu*, «tree»; *mien*, «cotton»), as if the term had been created in contradistinction to *ts'ao-mien*, lit. «plant-cotton», the *Gossypium herbaceum*. But this is a mistake. The word 綿 *mien*, formerly and more regularly written 絛 *mien*, is very ancient in Chinese as the designation of «floss silk». When cotton became known in China, cotton fabrics were designated as *tieh* or *po-tieh*, but unwoven cotton, and by extension the tree or plant which produced cotton was called *mu-mien*, where *mu* does not really mean «tree» in contradistinction to «plant», but vegetal in contradistinction to *mien* alone or to *ssü-mien*, which was «floss silk». The most that can be said is that, if the Chinese then created the term *mu-mien* (tree + floss), and not *ts'ao-mien* (plant + floss), it may be due to the nature of the cotton-yielding vegetable with which they first became acquainted; it may have been first the silk-cotton tree, and the name would have soon been extended by analogy to the cotton tree and the cotton plant. But this is not binding, and the Chinese may have done what the Germans did when they called cotton «Baumwolle», *i. e.* «tree-wool», although they certainly did not have in view a true tree like the *Bombax*. As late as the end of the 16th cent., LI Shih-chên did not know how to distinguish between the tree (or trees) and the plant except by speaking of «the *mu-mien* which is like a tree» and «the *mu-mien* which is like a plant».

The earliest mention of *mu-mien* I have found occurs in a fragment of the geographical section of the 吳錄 *Wu-lu* of 張勃 Chang Po (last quarter of the 3rd cent.; cf. *Sui ching-chih chih k'ao-chêng*, I, 12 *b*, and MASPERO, in *BEFEO*, XVIII, III, 25), which has been preserved in a 5th cent. work, the 齊民要術 *Ch'i-min yao-shu* (*Chin-tai pi-shu* ed., 10, 44 *b*; it is also given in *T'ai-p'ing yü-lan*, 960, 17 *a*, but is not included among the fragments of the *Wu lu* collected in the *Shuo fu* in 120 chs., ch. 59): «In the district (*hsien*) of 定安 Ting-an of [*the chün*] of 交趾 (交趾 in *T'ai-p'ing yü-lan* and *T'u-shu chi-ch'êng*, *ts'ao-ma tien*, 303, 2 *a*) Chiao-chih, there is the *mu-mien* tree (*shu*), which is ten feet high. Its fruit (*shih*) is like a wine cup; at its orifice (口 *k'ou*, retained in the *T'u-shu chi-chêng*; but the *T'ai-ping yü-lan* gives 中 *chung*, 'inside [the fruit]', which seems preferable; the next quotation, parallel to this one, gives *chung*), there is floss (*mien*) like the floss of the silkworm. It can also be used for making cloth, which is called 白縹 *po-hsieh* (on this form, cf. *supra*, p. 449), and by some, 'woollen



cloth' (毛布 *mao-pu*).» We find here a confirmation in an early text of the theory I have maintained above, to wit that, even when ancient dictionaries spoke of *po-tieh* as a «woollen cloth», it was a fabric of vegetal origin. Chiao-chih was the name of the *chün* («commandery») which corresponds to our Tongking, and its seat was practically identical with the present Hanoi. The location of the district of Ting-an of the 3rd cent. is not well ascertained, but it must have lain south of Hanoi in the southern part of the delta (cf. MASPERO in *BEFEO*, x, 582-583, 679; AUROUSSEAU, in *BEFEO*, XIV, ix, 13, 15). The *Shih chi so-yin*, i. e. the commentary of c. 730 on Ssü-ma Ch'ien (TAKIGAWA ed., 129, 35; the passage does not exist in the usual editions of the *Shih chi*, 129, 7 a) quotes one sentence which looks as if it belonged to the present passage, but, instead of the name of «Ting-an *hsien* of Chiao-chih», gives that of «the *chün* of 九眞 Chiu-chên»; the *Tz'ü-t'ung* (p. 2777), probably using another independent edition of the same commentary of c. 730, gives «the *chün* of 九德 Chiu-tê». One of the two names must be corrupt, an intermediary form having perhaps been the 意 form of *tê*. Chiu-chên is modern Thanh-hoa; Chiu-tê corresponds to the present regions of Nghê-an and Ha-tinh (cf. MASPERO, in *BEFEO*, x, 679). But both are much more to the south than the ancient Ting-an, and it would be surprising if the author of the *Shih-chi so-yin*, even if he wished to use a name better known than that of Ting-an, should have chosen such an inaccurate equivalence, instead of retaining at least the well-known Chiao-chih; moreover, there were no *chün* c. 730. Since the *Wu lu* had a geographical section, I feel inclined to believe that more or less similar sentences occurred in the descriptions of various regions, and that the quotation in the commentary of c. 730 was not adapted from that of Ting-an in the *chün* of Chiao-chih, but taken from the description of the *chün* of Chiu-tê or from that of Chiu-chên.

Another quotation from the *Wu lu* is given in the *P'ei-wên yün-fu* (16 B, 100 a, s. v. *mu-mien*), the *T'u-shu chi-ch'êng* (*shih-huo tien*, 312, *tsa-lu*, 2 a) and the *Pên-ts'ao kang-mu* (36, 72 a), but I do not know through what channel it has come down to us (it does not occur in the fragments of the *Shuo fu*, ch. 59) : «In Chiao-chou and 永昌 Yung-ch'ang the *mu-mien* tree (*shu*) is higher than a house (*wu*); there are some which last more than ten years (有十餘年不換者). The fruit (*shih*) is as big as a wine-cup. The floss inside the flower is soft and white (the text in the *T'u-shu chi-ch'êng* adds : 'by breaking one fruit, one obtains several pounds [of floss]', which is absurd), and it can be used to make cotton-wool (縑絮 *wên-hsü*) and 'woollen cloth' (*mao-pu*).» In the first centuries of our era, Chiao-chou included Kuang-tung, Kuang-hsi, Tongking and North Annam. Yung-ch'ang was during the first centuries of our era the name of a vast region in western Yün-nan, between Ta-li and Bhamo, and its name has survived down to our days. During the partition of China into the Three Kingdoms, the Shu Han dynasty of Ssü-ch'uan had created in Yün-nan a Chiao-chou of its own, the seat of which was not far from the present Ch'ü-ching in north-eastern Yün-nan (cf. *Chung-kuo ti-ming ta tz'ü-tien*, 281); but I do not think that this second Chiao-chou can be meant in the text of the *Wu lu*. The manufacture of cloth made from the *mu-mien* tree in the region of Yung-ch'ang in the 3rd cent. is of real interest if we remember that Yung-ch'ang was precisely the seat of the Ai-lao tribes who made the *po-tieh* mentioned in the *Hua-yang kuo chih* and the *Hou-Han shu*.

Almost of the same date as the *Wu lu* is a passage which occurs in the *San-kuo chih*, *Wei*

*chih* section, in the description of the Wo kingdom, or Japan (30, 11 a); it is very embarrassing. We are told that, in the Wo kingdom, «men leave the top of the head bare and tie *mu-mien* round (?) their heads» (男子皆露紒以木綿招頭). Now, scholars are agreed that the Wo people did not know cotton for centuries after the date of this text. *Mu-mien* has, however, been interpreted as meaning cotton in this passage, possibly with the idea that this was cotton stuff imported from abroad. But it seems hardly possible that, at a date when cotton was still unknown in China proper, enough cotton material should have come from the south seas to provide one of the elements of the national costume of the Wo population. I have consulted my colleague Haguenaer on this point. Unfortunately, the history of the Japanese names of cotton, *momen* and *wata*, is hard to trace; in particular, one cannot tell how and when Ch. *mu-mien*, which ought to have given \**moku-men* in Japanese, became *momen*. HAGUENAUER remarks that, in the *Kiki* and the *Manyōshū*, 木綿 is used to represent Jap. *yufu* (\**jup-*) > *yū*, and that this may reflect the change of technique from a tow headband to a cotton one. But 木綿 as representing Jap. *yufu* > *yū*, has also been interpreted as a designation of the ancient coarse cloth made from the «liber» of the paper-mulberry, *Broussonetia papyrifera* (cf. also the cloth and the paper made from the «bark» of the *fu-sang* tree in the half-legendary accounts of Fu-sang [*Liang shu*, 54, 11-12, and SCHLEGEL, in *TP*, 1892, 121, 129-134]). The cloth which, according to the *Sui shu*, 81, 5 a, was worn by the natives of Liu-ch'iu (probably then = Formosa), seems to have been made from the «bark» of a different tree. I have no authority to decide on the purely Japanese side of the problems, but I feel certain that, in the text of the *San-kuo chih*, *mu-mien* does not mean cotton. The term had been created in southern China and Tongking where there were both *Bombax* and *Gossypium*; but the Wei, in northern China, had neither. On the other hand, when speaking of the *mu-mien* of Wo men, the Chinese Wei account cannot have copied a Sino-Japanese term, since Chinese writing was still unknown in Japan at that period. The Wei must have used the new southern term *mu-mien*, «tree-floss», which conveyed no precise meaning to them, as a designation of the mulberry bast cloth of the Japanese. And, at first, the Japanese in their turn copied the Wei when they adopted 木綿 as an ideographic notation of their own native word *yufu*. It was only at a later stage that they read it *momen* and used it in the sense of «cotton».

We might have expected to find something about *mu-mien* in the *Nan-fang ts'ao-mu chuang*, «Description of plants and trees of the southern regions», due to 嵇含 Hsi Han (264-307). In fact, the term does not even occur in it. But we must not forget that the transmission of the work attributed to Hsi Han is far from satisfactory; the text, as we have it, contains interpolations, and there are also omissions (cf. *BEFEO*, XIV, ix, 10).

The 羅浮山記 *Lo-fou-shan chi*, or «Description of the Lo-fou Mountain» (in Kuang-tung), by 袁宏 Yüan Hung (328-376; cf. *Sui ching-chi-chih k'ao-chêng*, 6, 19 a; Wu Shih-chien's *Pu Chin shu ching-chi-chih*, 2, 20 a) is lost, but the following passage has been preserved in the *T'ai-p'ing yü-lan* (960, 17 a; cf. also *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, 1 b, and, for a very incomplete version, *shih-huo tien*, 312, *tsa-lu*, 2 a): «The *mu-mien* gives in the first month flowers which are as great as [those of] the *fu-jung* ('nelumbium', or, if taken in the sense of 木芙蓉 *mu fu-jung*, 'hibiscus'). When the flowers fall, it forms carpels (子方 *tzü-*

*fang* = 子房 *tzü-fang*) which produce floss (*mien*). In the 'ears' of the carpel partitions there is floss which is very white. When... is formed, [the floss] is ripe (蠶成則熟 *ts'an ch'êng tsê shu*; *ts'an* seems to be corrupt; I believe we ought to read 繭 *chien*, 'cocoon', 'when the cocoon is formed'; the comparison with a cocoon occurs in *Liang shu*, 54, 13 b; these four characters are omitted in the *T'u-shu chi-ch'êng*, 303, 1 b). Southerners make cotton wool (*wên-hsü*) out of it.»

The *T'ai-p'ing yü-lan* (960, 17 a; cf. the fragments in *Yü-han-shan-fang chi i-shu*, ch. 2, 2 b) has also preserved an interesting passage of the *Kuang-chih*: «The *mu-mien* tree (*shu*) has red flowers which form carpels in great quantities, closely pressed against one another; [these carpels] produce a floss which is very soft. [The *mu-mien* tree] grows in Chiao-chou and Yung-ch'ang.» The date of the *Kuang-chih* is not well ascertained, and above (cf. p. 451) I have put it in the 4th or 5th cent. (cf. also *BEFEO*, IV, 412). The extracts in the *Shuo fu* (ch. 61) mark its author Kuo I-kung as living under the Chin, which were overthrown in 420, but this is in itself without much weight, and the compiler of the *Yü-han-shan-fang chi i-shu*, who agrees, does not seem to have had any authority on which to rely; the various extracts would rather suggest a later date, perhaps not before the 6th cent. But, in any case, we have here one more early mention of *mu-mien* in the ancient country of the Ai-lao.

A man of the first Sung dynasty (420-479), 顧微 Ku Wei, has written a work on the province of Canton entitled 廣州記 *Kuang-chou chi* (cf. *Sui ching-chi-chih k'ao-chêng*, 6, 31 a; MASPERO, in *BEFEO*, XVIII, III, 26), some fragments of which are collected in the *Shuo fu* in 120 chs., ch. 61. One of them says: «The southern Barbarians have no silkworms; they pluck *mu-mien* and make cotton-wool (絮 *hsü*) with it.» The same passage is quoted in the *Nung shu* (21, 16 a), but as coming from another lost *Kuang-chou chi*, that of 裴源 P'ei Yüan, the date of which is uncertain, except that it is already quoted by the author of the *Shui ching chu*, who died in 527 (cf. *Sui ching-chi-chih k'ao-chêng*, 6, 30 b; MASPERO, *ibid.*). The second attribution probably is the correct one, since it is already given in what must have been the source used by the author of the *Nung shu*, i. e. the *I-wên lei-chü* (85, 29 a). But there the quotation goes on with a further portion, which seems in fact to be the result of some erroneous amalgamation with another text (cf. *supra*, p. 458). A different quotation from a *Kuang-chou chi*, without indication of the author's name, is given in the *T'ai-p'ing yü-lan* (960, 17 a): «The branches [of the *mu-mien*] are like the branches of the 桐 *t'ung* (*Paulownia imperialis*, or = *wu-t'ung*, *Sterculia platanifolia* ?); its leaves are like [those of] the walnut-tree (*hu-t'ao*), but somewhat bigger. It grows in Chiao-chou and Kuang-chou.»

In the 5th cent. too, Shên Huai-yüan, in his *Nan-yüeh chih*, wrote: «The 'blue-green *t'ung*' (青桐 *ch'ing-t'ung*; on *t'ung* trees, cf. *infra*, p. 476) much resembles *mu-mien*, but passes it in brilliancy and fragrance» (cf. *T'ai-p'ing yü-lan*, 957, 9 b; the name of the author is corrupt in *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 237, 3 b).

As a mark of the austere life of the devout Buddhist Emperor Wu of the Liang dynasty (502-549), the historians note (*Liang shu*, 3, 13 a; *Pei shih*, 7, 8 a; *Tzü-chih t'ung-chien*, 159, 47-48) that he made only one meal a day, without meat or fish, «wore a dress of common cloth, [had] a black tent of *mu-mien*» (身衣布衣木絲皂帳), and used the same hat three years

and the same blanket two years. Thus *mu-mien* was clearly not a luxury (the Imperial tents were usually made of black felt outside, but lined and decorated with pieces of vermilion silk; cf. *T'ang liu-tien*, 11, 9-10). CIBOT (*Mém. conc. les Chinois*, II, 603) has spoken of the « robe de coton » of Wu-ti, and the same has been done by MAYERS, W. Williams (*The Middle Kingdom*, II, 36-37), PARKER (*China Review*, XIX, 192), and Dyer BALL (*Things Chinese*<sup>3</sup>, 149); but I can find no authority for such a meaning of *chang* (the only alternative meaning, instead of « tent », would be « curtain »). Though without a verb, the sentence *mu-mien tsao-chang* is independent from the preceding one. The *K'ang-hsi tzü-tien* (s. v. 棉 *mien*), which cites the *Tzü-chih t'ung-chien*, suppresses the first sentence and then, wanting a verb, gives « Liang Wu-ti sent (送 *sung*) a black tent of *mu-mien* », which is an alteration of the text. This, however, like the misquotation from Shih Chao which follows (cf. *infra*, p. 501), was merely copied by the authors of the *K'ang-hsi tzü-tien* from an earlier dictionary or encyclopaedia, since it is already quoted, in the same terms, by the *Ch'ün-fang p'u* of 1630 (*Mien p'u* section, 5 b) from the *Wu-Hsün tsa-p'ei*, then a recent work (cf. *supra*, p. 438).

I shall not dwell on the mere mentions of *mu-mien* in the T'ang period (the term occurs in the works of at least half a dozen T'ang poets), and still less under the Sung and Yüan, when cotton gradually spread over central and north-western China, but I wish to draw attention to a fact which is not without interest for the use of cotton, of whatever sort it may have been, among the aboriginal tribes of Yün-nan. These tribes were often designated from some particular characteristic of their dress or of the products of their country. Now one of them, belonging to the group of the 濮 *Pu* (which included the Tattooed *Pu*, the Red-mouth *Pu*, etc.), was known as the *Mu-mien Pu*, « the Cotton *Pu* » (cf. also *BEFEO*, VIII, 367, where the name is left untranslated). This information occurs in a group of five quotations expressly said to be drawn from Kuo I-kung's *Kuang chih*; they have been preserved in the *T'ai-p'ing yü-lan*, 192, 3 b, and are found also, without indication of source in the *T'ung tien*, 187, 8 b-9 a, and in the *T'ai-p'ing huan-yü chi*, 179, 16 b-17 a; I do not know why they have been left out in the *Yü-han-shan-fang chi i-shu* (perhaps because the compiler thought that the attribution was erroneous). The text is as follows: « In the country of the *Mu-mien Pu*, there is the *mu-mien* tree, which has abundant leaves, and very numerous carpels; in the carpels, the floss is similar to that made by silkworms; [the pod] is a big as the fist ».

We have seen that the word used for « cotton », *mu-mien* was made with the word which meant silk-floss, to which *mu* had been added to mark that it was a vegetal floss. But when cotton became of more common use in China, a new graphic specification was made, and 綿 *mien* began to be written 棉 *mien*. Although the *K'ang-hsi tzü-tien* gives only the latter form in *mu-mien*, even for examples of pre-T'ang and T'ang times, and although *P'ei-wên yün-fu* arbitrarily gives some of these examples under one form and some under the other, there is no indication that the new character was ever used before the Sung dynasty; it has not been traced before the *Kuang yün* (A. D. 1007-1011), and it is given as a new form by Yüan Wên (12th cent.) in his *Wêng-yu hsien-p'ing* (4, 3 a; cf. also *Kuei-ssü lei-kao*, 7, 21 a-b; 14, 6 a). But it is then of frequent occurrence, and, if we can trust the texts established by Ch'ien-lung's Commissioners for the *Ssü-k'u ch'üan-shu*, it is regularly used by Chou Ch'ü-fei (1178; 10, 9 a), Chao Ju-kua (1225), Wang Chêng (1313, with some inconsistencies in the present editions), the authors of the *Nung-sang chi-yao* (1273;

cf. *infra*, p. 499), the *Nung-sang i-shih ts'o-yao* (1330; cf. *infra*, p. 504) and the *Tao-i chih-liao* (1350). Even after these dates, however, its adoption was far from being general: the *Pên-ts'ao kang-mu* always employs the old form, and so does the *T'u-shu chi-ch'êng*.

The now usual name of cotton, 棉花 *mien-hua*, does not often occur, either as such or with the earlier form of *mien*, before the second half of the 17th cent.; Li Shih-chên and even the *T'u-shu chi-ch'êng* ignore it. It is, however, fairly ancient. In his *Hsi-yü fan-kuo chih* (*Pei-p'ing T'u-shu-kuan shan-pên ts'ung-shu* ed., 19 b), Ch'ên Ch'êng, who, in 1414, was sent on a mission to various countries of Central Asia, says of Lükčün (east of Turfan) that «it produces 綿花 *mien-hua* with which one can make cloth (*pu*)». In his 圖書編 *T'u-shu pien* (89, 30 b), written from 1562 to 1577, CHANG Huang (1527-1608) says that the *so-lo pu* (cf. *infra*, p. 478) is made of 棉花 *mien-hua*; the same form also occurs in the *Min-pu shu*, written in 1585 (cf. *infra*, p. 480).

As to 草棉 *ts'ao-mien*, «plant cotton», it is a botanical name of fairly late origin. Up to now, I have not traced it earlier than 趙學敏 CHAO Hsüeh-min's 本草綱目拾遺 *Pên-ts'ao kang-mu shih-i* which was completed in 1765 (5, 10 a-b); CHAO employs *ts'ao-mien* in his own composition, and also in a quotation from a 藥性考 *Yao-hsing k'ao* which is unknown to me, which seems to have been then a recent work. I must add a few words to vindicate the date 1765 which I have assigned to the *Pên-ts'ao kang-mu shi-i*. LAUFER (*Sino-Iranica*, 229) says that the work was published in 1650, and only reprinted in 1765. This is in agreement with Mo Yu-chih's Catalogue (Jap. ed., 8, 11 a), where we read that the *kêng-yin* of the preface of CHAO's collected medical works corresponds to 1650. On the other hand, YANG Shou-ching says, in a contradictory way (*Ts'ung-shu chü-yao*, 12, 31), first that the collective edition of CHAO's twelve medical works engraved under K'ang-hsi (1662-1722) is scarce, and secondly that only the *Pên-ts'ao kang-mu shih-i* has been engraved. The date 1650 is impossible, since the *Pên-ts'ao kang-mu shih-i* quotes abundantly from PIÑUELA's *Pên-ts'ao pu*, which appeared only in 1697 (cf. COURANT, *Catalogue*, 5332). CHAO Hsüeh-min's own preface to the *Pên-ts'ao kang-mu shih-i* is dated 1765, and I have no doubt that there has never been an earlier edition, either in 1650, or under K'ang-hsi. After his first work had been engraved, CHAO thought of publishing his other medical works in a collective edition with the present one, and wrote for the collection a preface dated *kêng-yin*; this *kêng-yin* corresponds neither to 1650, nor to 1710, but to 1770. As no work of CHAO has survived except the *Pên-ts'ao kang-mu shih-i*, it seems that the scheme of the collective edition was never carried out.

A term 花衣 *hua-i*, lit. «flower-garment», is known as a designation of «cotton cleansed of seeds». Curiously enough, it seems to go back to the beginning of the 5th cent.; *hua-i* occurs in ch. 25 of the *Shih-sung lü*, translated in A. D. 404 (NANJIÖ, No. 1115; 張, iv, 58 b), and is translated «cotton» by CHAVANNES, *500 Contes*, II, 260. Another term 子花 *tzü-hua*, «seed flower» is mentioned in the *Tung-hsi yang k'ao* (5, 6 a; cf. also 9 b, 10 b) among the products of Lü-sung (Luçon = the Philippines), with the gloss «it is *chi-pei* flowers (*chi-pei hua*)»; so it must have been a trade name of cotton, used, at least locally, at the beginning of the 17th cent. Such must have been the case also in the 6th cent. with 南布 *nan-pu*, «southern cloth» of *Ch'ên shu*, 27, 4 b, and, under the Yüan, 南綿 *nan-mien*, «southern floss», of YS, 90, 8 a. In 1617-1618, 東京布 *Tung-ching pu*, «cloth of the Eastern Capital», was the designation of a cotton fabric in narrow strips

from Tongking and Annam (*Tung-hsi yang k'ao*, 1, 11 b). In my opinion, the 花蕊布 *hua-jui pu*, «flower-bud cloth» (or «flower-stamen cloth»), from Samarkand, mentioned in the *Kuang-yü chi*, 24, 15 a (and earlier in *Ming i-t'ung chih*, 89, 23 a; also in Khotan, *ibid.* 89, 25 a), was not an «ornamented cloth», as translated by LAUFER, *Sino-Iranica*, 251, but a «cotton cloth».

Employed alone, the combination 花綿 *hua-mien*, «flower floss», in the Chinese translation of the *Mahāvvyutpatti* (Nos. 5870 [where 花線 *hua-hsien* is a misreading or a misprint] and 5871) is a late, and unusual form. Moreover, it shows once more that the translators of the *Mahāvvyutpatti* from Tibetan into Chinese often adopted arbitrary interpretations: *hua-mien*, «cotton», is given as a translation of Skr. *vakkali*, Tib. *bag-le-ba*. But the would-be Skr. *vakkali* can be nothing else than a Prākṛit form of Skr. *valkala*, «bark garment» (cf. Pali *vakkala* and *vakkali*), and Tib. *bag-le-ba* seems to be an adjectival form of *bag-le*, itself based on a Prākṛit form similar to Beng. *bāklā*, «bark» (on which cf. J. BLOCH, *La formation de la langue marathe*, 404; but *bag-le-ba* may have been contaminated by *Bag-le-pa* or *Bag-le-ba*, «of Balkh»).

I may add that the change of 綿 *mien* to 棉 *mien*, in order to show that the product was vegetal and not «silk» (*i. e.* animal) floss, had been anticipated by the creation of another character. The 杜仲 *tu-chung*, *Eucommia ulmoides*, is sometimes called *mu-mien*, on account of the silky fibres which can be drawn out when breaking its bark (cf. STUART, *Materia Medica*, 166; *mu-mien* is already given as an alternative name of *tu-chung* in ch. 12 of the *Hsin-hsiu pên-ts'ao*, written in the 7th cent. [*Chuan-hsi-lu ts'ung-shu* ed.]). In the early Middle Ages, this tree was merely called *mien* in what corresponds to the modern provinces Chê-chiang and Chiang-su. For writing that *mien*, a character 棉 *mien* was created, which is obtained through the addition of the «tree» radical to the character *mien* meaning «silk floss»; this character already occurs in the *Yü p'ien*, the author of which died in A. D. 581, and the tree, with its name written in this way, was described under the Sung dynasty by Su Sung (cf. *K'ang-hsi tzü-tien*). The more modern 棉 *mien* of *mu-mien*, «cotton», was coined in the same spirit, and in a way it may be said to be but a simplified form of 綿 *mien*, *Eucommia ulmoides*.

CH'Ü-SHUN. — Other ancient names of cotton, given in our dictionaries, remain to be examined. One is 屈胸 *ch'ü-shun* (\**k'juət-šjuěn*). It is said to be a Sanskrit word meaning «great fine cloth» (*ta hsi pu*), and is the name of the material of the robe which Bodhidharma had inherited from the patriarch Sipa and which he transmitted to his successors. It was blue-black, and had been made from cotton plucked at «the heart of the flowers». Hsü Kuang-ch'i had actually seen it, or its substitute (*Nung-chêng ch'üan-shu*, 35, 2). The earliest mention I know of it occurs in the *Fan-i ming-i chi*, dated 1143 (not 1151 as in NANJIÖ, *Catalogue*, No. 1640; 兩, XI, 87 b; cf. also FUJII's, *Bongo jiten*, 145; ODA Tokuno, 301). The *Ko-chih ching-yüan* (27, 22 b) quotes the 南華志 *Nan-hua chih*, a work which I do not know, as stating that «the ritual garment (信衣 *hsin-i*) of the sixth patriarch was made of seven strips (*t'iao*) of [cloth of] gold thread (*chin lü*); it is the *ch'ü-shun* cloth; in the Western Countries (Hsi-yü), they consider the *ch'ü-shun* as 'fine cloth' (*hsi-pu*)». One can never vouchsafe for the accuracy of the quotations in the *Ko-chih ching-yüan*; but the «gold threads» are impossible, since they could not be «cotton», or even be confused with «cotton». *Ch'ü-shun* seems to be a term of the Dhyāna or Zen sect, and like everything regarding the person-

ality and the activities of Bodhidharma, it is open to grave suspicion. No plausible Sanskrit original of *ch'ü-shun* can be suggested. Apart from the texts connected with Bodhidharma's robe, the only examples of *ch'ü-shun* I have come across occur in the works of Yeh-lü Ch'u-ts'ai (1189-1243), a scholar of Ch'i-tan Imperial descent, who was one of Chinghiz-khan's advisers and a minister of Ögädäi. He accompanied Chinghiz in his expedition to the West in 1218-1224, and in the short account of his journey, entitled *Hsi-yü lu*, we read that the people of the region of Samarkand all wore garments of *ch'ü-shun* (*Br*, I, 21). In the collection of his literary productions, entitled *Chan-jan chü-shih chi*, the term occurs twice (*Ssü-pu ts'ung-k'an* ed., 5, 5 a; 6, 19 b), the first time (but only in the *Chien-hsi-ts'un-shê* ed., 5, 3 b) with a note « *Ch'ü-shun* is a cloth (*pu*) of the Western countries (*hsi-yü*) ». BRETSCHNEIDER (*Br*, I, 21) proposed to connect *ch'ü-shun* with Arabic *قاسم qaśm*, and remarked that cotton was now called « *guza* » in Russian Turkestan. But *qaśm* means « old cotton » (LECLERC, *Traité des simples*, in *Not. et Extr.*, XXVI, I, 93), and neither semantically nor phonetically agrees with *ch'ü-shun*. As to « *guza* », or better *gōzä*, *γōzä* (> Turkī *yoza*), it is the Persian designation of the « capsule » of the poppy, flax, lily, and particularly cotton; it is out of the question here, and BRETSCHNEIDER would never have thought of it if he had known that Yeh-lü Ch'u-ts'ai's *ch'ü-shun* did not transcribe a local word heard in Turkestan, but was a term which had previously existed in Chinese. Modern Chinese commentators, unaware of the use of *ch'ü-shun* in Buddhist texts, have erroneously connected Yeh-lü Ch'u-ts'ai's term with 只孫 *Chih-sun* or 質孫 *chih-sun*, used in the Mongol period as the designation of a robe of honour (cf. *Hsi-yü lu liao-chu pu*, in *Chü-hsüeh-hsüan ts'ung-shu*, 4th *chi*, 38); but *chih-sun* transcribes Mong. *jisun*, « colour », and has nothing to do with *ch'ü-shun*. I do not think, however, that the occurrence of the term *ch'ü-shun* in Yeh-lü Ch'u-ts'ai's works would countenance the view that it was then current in any part of China. Yeh-lü Ch'u-ts'ai was a scholar more or less prone to the use of pedantic terms. He may not have been very familiar with cotton and its more ancient names in Chinese; he knew the term *mu-mien*, however, and used it in one of his poems (cf. *infra*, p. 514).

SĀLMALI. — Of more importance is another word, 睽婆 *shan-p'o* (\**śjäm-b'uā*). It is given as a Sanskrit name of cotton in LI Shih-chên's *Pên-ts'ao kang-mu* (36, 71 b), and from there it has passed into the *T'u-shu chi-ch'êng* (*ts'ao-mu tien*, 303, 1 a, 10 b), and into STUART, *Materia Medica*, 198, as an equivalent of the cotton plant, *Gossypium herbaceum*. In his turn, LI Shih-chên had taken it from the *Fan-i ming-i chi* (*loc. cit.* 87 b). This time we have to deal with a true, though truncated Indian word, but one which is not a designation of the cotton plant. As already stated in 1889 by WATTERS (*Essays*, 435), the word meant is Skr. *sālmali*, which is *Bombax malabaricum*, the silk-cotton tree. But it escaped WATTERS that, as a matter of fact, a Chinese form *shan-p'o* never existed. The author of the *Fan-i ming-i chi*, Fa-yün, took it from Hsüan-ying's *I-ch'ieh ching yin-i*. In this *yin-i*, *shan-p'o* occurs twice as a catchword. Once (ch. 21), it is used merely to indicate the pronunciation of *shan* (*Tripit.*, 爲, VII, 81 a, copied into Hui-lin's ch. 43, *ibid.* IX, 64 b); the text in which the term appears ought to be in ch. 4 of NANJIO, No. 363, but the arrangement of the present No. 363, in 13 chs., is different from that of the collection in 10 chs. commented upon by Hsüan-ying and Hui-lin, and I have failed to trace

the original passage where *shan-p'o* occurs; it looks as if the two characters followed each other in a *dhāraṇī*, and anyhow this has no bearing on the sense of the pseudo-*shan-p'o* as meaning *mu-mien*. Fa-yün's source is the other passage (ch. 2), where Hsüan-ying uses *shan-p'o* as a catchword and, after having given the pronunciation of *shan*, adds that « in [Chinese] translation, [the term] would be *mu-mien* ('cotton') » (*Tripit.*, *ibid.* VII, 5 b). But the work commented upon is the *Parinirvāṇasūtra*, ch. 1 (NANJIŌ, No. 113), in which the text (*Tripit.*, 盈, v, 6 a) speaks not of *shan-p'o*, but of a king of the *Asura* called 睽婆利 *Shan-p'o-li* (\*Śjām-b'uā-lji); the same form occurs in the so-called « southern » version (NANJIŌ, No. 114; *Tripit.*, *ibid.* VII, 7 b), and Hui-lin, in his *yin-i* of that « southern » version (ch. 25; *Tripit.*, 爲, VIII, 158 a) correctly gives *Shan-p'o-li*, with the explanation: « It is the God of cotton » (木綿神也; the 大 *ta* of our editions is a graphic corruption of 木 *mu*). So Hsüan-ying, who was mainly concerned with the pronunciation of *shan* and generally eschewed lengthy catchwords, was content, in writing the name of the king of the *Asura* *Shan-p'o-li*, with quoting only the first two characters of his name; Fa-yün blindly copied his note; and, as a consequence, the apocopate name of a king of the *Asura* has unduly become a pseudo-designation of cotton in Chinese and European lore.

Moreover, Hsüan-ying's and Hui-lin's explanations are most probably wrong, and *Shan-p'o-li* represents not a name Śālmali, but a Prākṛit form \*Śambari of Śambara, the name of a well-known king of the *Asura* (cf. *TP*, 1921, 78). It is nevertheless clear that the error was only possible if the Chinese scholiasts knew the *śālmali* under a form of the name which was more or less like *Shan-p'o-li*. I have discussed at some length the transcriptions of *śālmali* in a paper (*TP*, 1921, 79-81) to which I shall now have much to add; in particular, I have collected a number of Buddhist texts which associate the *garuḍa* with the *śālmali* or *kuṭaśālmali*, whereas in 1921 I could quote on this point from the *Rāmāyaṇa* only (in CHAVANNES, 500 *Contes*, II, 288, « çambara? » is to be corrected « śālmali »). Without entering into a detailed examination of the problem, I may now state with certainty that the « cave of 苦婆羅 *Shan-p'o-lo* (\*Śjām-b'uā-lā), the places 睽婆梨 *Shan-p'o-li* and 苦摩羅 *Shan-mo-lo* (\*Śjām-muā-lā), and the tree 苦末羅 *shan-mo-lo* (\*Śjām-muāt-lā) of my paper all represent, if not *śālmali*, at least a word the correspondent of which, in a Sanskrit text, would be *śālmali*. To these I can add 設拉末梨 *shē-la-mo-li* (\*śjāt-lāt-muāt-lji; *Bongo jiten*, 251), 舍摩利 *shē-mo-li* (\*śja-muā-lji; *ibid.* 223), 舍婆利 *shē-p'o-li* (\*śja-b'uā-lji; *ibid.* 223), 睽摩羅 *shan-mo-lo* (*Tripit.*, 昃, IX 103 a), 賒摩利 *shē-mo-li* (\*śja-muā-lji; *ibid.*, 辰, I, 30 a, 74 a), 奢摩離 *shē-mo-li* (\*śja-muā-lji; *ibid.* 46 b, 90 b). In Sanskrit, apart from *śālmali*, there is also a Vedic word *śimbalā*, said to mean the flower of the *śālmali*; the Prākṛit forms of the latter word are *sāmalī*, *sāmarī*; Sanskrit scholars are agreed to attach to Vedic *śimbalā* the Pali name of the *śālmali*, *simbali* (cf. UHLENBECK, *Kurzgefasstes Etymol. Wörterbuch*, 306, 310; PISCHEL, *Grammatik der Prākṛit-Sprachen*, § 109; T. W. RHYS DAVIDS, *Pali-English Dictionary*, 170). The modern Hindustani form is *sāmal* or *sāmbhal*, hence the Anglo-Indian *seemul* (cf. YULE, *Hobson-Jobson* 2, 807; for other forms of modern Indian dialects, cf. J. BLOCH, *La formation de la langue marathe*, 420, 426; contrary to SCHLEGEL, *Siamese Studies*, Supplement to *TP*, 1902, 62, 83, 84, I doubt that Siamese *sāmlī*, « carded cotton », is traceable to *śālmali*, because the change in meaning would be hard to explain). Apart from *shē-la-mo-li*, it is clear that the Chinese transcriptions are based neither on *śālmali* itself, nor on



dialects in which the ś- had passed to s- as in the known Prākritis and in Pali, but on forms \*śambala, \*śambali, \*śammala (?), \*śāmalī, \*śābalī (EITEL's restitutions of *shan-p'o-lo* as «djambalā» [a mistake instead of *jambīra*] and of *shan-mo-lo* as *cāmara*, the first of which has passed into STUART, *Materia Medica*, 117, are of course wrong, and I have already corrected them in *TP*, 1921, 76; nevertheless, they have since been repeated in SOOTHILL and HODOUS, *A Dictionary of Chinese Buddhist terms*, 313 a). My own impression would be that these forms provide the necessary intermediates between Skr. *śālmālī* and Pali *simbali*, and that perhaps it is on account of forms like Pali *simbali* that mediaeval Vedic scholars have explained the Vedic *simbalā* as meaning «flower of the *śālmālī*»; but this is a point which I have no authority to decide (I may remark, however, that J. BLOCH [*loc. cit.*] says nothing of Vedic *simbalā*, and connects Marathi *śāṃvar* and *śemvri*, as well as Pali *simbali* and all modern dialectical forms, directly with Skr. *śālmālī*). What I wish to establish is that all the transcriptions occurring in Chinese translations begin with ś-, not with s-, and that, except in one case, they are based not on *śālmālī* itself, but on forms without an -l- at the end of the first syllable.

SO-LO. — This last double characteristic has some bearing on the question of the interpretation of 娑羅 *so-lo* (\*śā-lā), which occurs with four or five different meanings in Chinese texts. Its most ancient and frequent use is to render Skr. *sāla* or *sāla*, *Shorea robusta*, a lofty hard wood tree famous in Buddhism because Śākyamuni attained *parinirvāṇa* between a pair of them (cf. *Bongo jiten*, 217; ODA Tokuno, 428, 812; YULE, *Hobson-Jobson*<sup>2</sup>, s. v. «saul-wood»). SOOTHILL and HODOUS (pp. 242, 323, 363) give alternative forms 沙羅 *sha-lo* (\*śa-lā) and 莎羅 *so-lo* (\*suā-lā), and the latter also occurs in TARANZANO, by the side of the regular one (*Vocabulaire*, II, 575, 576). But these forms have no authority. *Sha-lo* has been taken over from EITEL's *Handbook*, p. 139, where it was a misprint or an error; it would suppose \*śāla (\*śāla) rather than *śāla*, and moreover all the Chinese transcriptions are based on *sāla*. The second *so-lo*, as far as I am aware, occurs only once in ancient texts, as a rendering of Skr. *sāla*, in Tuan Ch'êng-shih's *Yu-yang tsa-tsu* (c. A.D. 860; *Chin-tai pi-shu* ed., *hsü-chi*, 6, 12 a); but our texts of the *Yu-yang tsa-tsu* are often corrupt, and since Tuan Ch'êng-shih employs the usual form elsewhere (18, 4 b), there is little doubt that he had also done so in the present case. The only exceptions to the first form indicated above are the cases when, as is so common in Chinese texts, the 娑 *so* of the transcription has been graphically corrupted to 婆 *p'o*. For *so-lo* in non-Buddhist works, cf. *Ch'i-min yao-shu*, 10, 47 a; *T'ai-p'ing yü-lan*, 961, 3 b; *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 309 (but no distinction is made there between the different values of *so-lo*, and even *to-lo*, *tāla* [cf. *supra*, p. 432], is thrown in with *so-lo*). It may be that the earliest extant occurrence of *so-lo* in lay works is in the fragments of the 魏王草木志 *Wei-wang ts'ao-mu chih* («Botanical notes by the prince of Wei», 3rd cent. [?]; cf. BRETSCHNEIDER, *Botanicon Sinicum*, I, 39); but the quotation in the *T'ai-p'ing yü-lan*, 961, 3 a (it is not included among the fragments of the *Wei-wang ts'ao-mu chih* in *T'u-shu chi-ch'êng*, *ts'ao-mu-tien*, 5, 7-8), which is extremely corrupt, clearly amalgamates a quotation beginning with «*so-lo* tree» and another from another source, referring to quite another sort of tree or bush (the 緜 *hsiang*), this second text being altered from that of the *Kuang-chou chi* quoted in *Ch'i-min yao-shu*, 10, 46 b. In fact, the first text of importance comes

from the lost 荊州記 *Ching chou chi* of 盛弘之 Shêng Hung-chih, who lived under the Liu Sung (420-479; cf. *Sui ching-chi chih k'ao-chêng*, 6, 13 b); it concerns a tree which in A.D. 300 appeared all of a sudden and grew to gigantic proportions in a temple of Pa-ling (Yo-chou-fu, now Yo-yang, in Hu-nan) and which a foreign monk stated to be a *so-lo* tree, the *sāla* species of Buddha's *parinirvāṇa*; the tree long gave quite ordinary small white flowers, and then, in 434, a flower of the shape and the colour of the nelumbium (? or hibiscus; *fu-jung*) appeared on it (I combine the more or less corrupt versions of the *Ch'i-min yao-shu*, 10, 47 a, *Yu-yang tsa-tsu*, 18, 4 b, and *T'ai-p'ing yü-lan*, 961, 3 b; the *T'ai-p'ing kuang-chi*, 406, 4 b, in fact copies Tuan Ch'êng-chih, but our editions now have *p'o-lo* instead of *so-lo*; cf. also *T'u-shu chi-ch'êng*, *ts'ao-mu-tien*, 309, *i-wên*, 3 b; *tsa-lu*, 2 a). The main interest of this tale of *mirabilia* is to establish that the *so-lo* tree was almost unknown in China. In the same way, when, in the 8th cent., the Chinese «protector» of Kučā forwarded to the Court 200 twigs of *so-lo* which he said came from Ferghāna, it created quite a sensation, and we possess various official and private accounts of this event (cf. *Yu-yang tsa-tsu*, 18, 4 b-5 a, and the texts in *T'u-shu chi-ch'êng*, *ibid.* by Chang Wei and Li Yung). A poem of Mei Yao-ch'ên (1002-1060) speaks of 紗羅 *so-lo* (*T'u-shu chi-ch'êng*, *ibid.*, *i-wên*, 3 a), which is more exactly the name of another tree, but his reference to Li Yung's text proves that he actually means our first *so-lo*, the *sāla* tree. Hung Mai (1123-1202) speaks of the *so-lo* tree of Li Yung's tablet both in his *Jung-chai ssü-pi* and in his *I-chien chih* (end of ch. 45; but, in some editions, *so-lo* is altered into *p'o-lo*). On the ultimate fate of that *so-lo* tree and the younger ones which took its place, cf. Yü Yieh's *Ch'a-hsiang-shih hsü-ch'ao*, 25, § *so-lo shu*. In the Hung-chi-ssü, a Buddhist temple located 45 *li* north-east of Nanking, two *so-lo* trees were said to have been brought back from abroad by the famous eunuch Chêng Ho in the first quarter of the 15th cent. (cf. Lang Ying's *Ch'i-hsiu lei-kao*, 40, 4 a; 談遷 T'an Ch'ien's 棗林雜俎 *Tsao-lin tsa-tsu*, *chung-chi* section [*Hsiao-shuo pi-chi ta-huan* ed.], 36 a); but, from the description, they seem to have been not *Shorea robusta*, but an *Aesculus* (cf. *infra*).

Of the other possible meanings of *so-lo*, one will not detain us. In the account of Chu-lien, *i. e.* the Čola (Coromandel coast), the *Sung shih* (489, 9 a), in a long list of local products, speaks of flowers called «blue-green (*ch'ing*), yellow and green-blue (*pi*) 娑羅 *so-lo*» (?), and PARKER (*China Review*, XIX, 193) has stated that *so-lo* was there the designation of «a sort of cotton». This is possible, but no more than possible, since we have no indication of what these *so-lo* of different colours could have been, nor even are we certain that we must read *so-lo*; in the preceding line, the *Sung shih* gives a wrong reading *so-lo-mi*, instead of *po-lo-mi*, «jack-fruit»; the reverse error is possible here, and, as a matter of fact, we find *p'o-lo* in the corresponding list of Chao Ju-kua (*HR*, 96). Nor shall I dwell on the identification of the *so-lo* tree (*shu*) with the 優曇花 *yu-t'an hua*, «*yu-t'an* flower», which seems to have been known in some parts of Yün-nan in the 17th cent. *Yu-t'an* is a shortened transcription of Skr. *udumbara* (not «*udambara*» as in LAUFER, *Sino-Iranica*, 411), *Ficus glomerata*, the fig-tree, to which many legends attached. I do not know the origin of this mistaken identification (on which cf. Yü Yüeh's *Ch'a-hsiang-shih hsü-ch'ao*, 25, § *so-lo shu*). It may have arisen from a confusion between *so-lo* and *yu-t'an-p'o-lo* (one of the transcriptions of *udumbara*), with the common misreading of the latter part of the transcription as *so-lo* instead of *p'o-lo*. Under the name of «seeds of the *so-lo* tree» (*so-lo shu tzü*), Hung

Mai (*I-chien chih*, end of ch. 45) describes what he himself shows to be seeds of *tsao-chia*, i. e. *Gleditschia chinensis*, which have certainly nothing to do with the true *so-lo* tree.

More definite is the use of 娑羅 *so-lo* as a designation of the 天師栗 *t'ien-shi-li*, « Celestial Master's chestnut » (by allusion to Chang *t'ien-shih* or Chang Tao-ling, the reputed founder of the Taoist church; BRETSCHNEIDER's explanation of *t'ien-shih* as « Buddha », *Botanicon Sinicum*, I, 65, is erroneous), i. e. the horse-chestnut, *Aesculus chinensis* (cf. *Pên-ts'ao kang-mu*, 29, 32-33; *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 222, 10 a [with a wrong reading *p'o-lo* instead of *so-lo*]; STUART, *Materia Medica*, 19; TARANZANO, *Vocabulaire*, II, 575). In the sense of horse-chestnut, *so-lo* is perhaps more regularly written 杪羅 *so-lo*, but this is not certain; 娑羅子 *so-lo-tzū* is a common form of the same name, and all quotations mentioning a 娑羅 *so-lo* tree are referred to the *Aesculus* in the *Pên-ts'ao kang-mu shih-i*, 7, 30-31. GILES in his *Chinese-English Dictionary* (No. 10.193) states that it is the name of the Indian *sāla* tree which has been transferred in China to the horse-chestnut. I think he is right. We have seen that Mei Yao-ch'ên used the last given form 杪羅 *so-lo* for what he intended to be the Indian *sāla* tree, *Shorea robusta*. As a matter of fact, I have little doubt that the passage of the name from the *Shorea robusta* to the *Aesculus chinensis* occurred very early, the confusion being due to the fact that the *Shorea robusta* was unknown in China, and at first the *Aesculus chinensis* was found only in a few districts of Ssü-ch'uan (cf. also the verse on the *so-lo* flower in the *I-pu fang-wu lio-chi*, by Sung Ch'i [998-1061], *Chin-tai pi-shu* ed., 6 b; here the *so-lo* is different from the *t'ien-shih-li*, *ibid.* 3 a). Absurd as it is, the story in Shêng Hung-chih's *Ching chou chi* seems already to betray the confusion. This is established, anyhow, by another consideration. STUART (p. 19) remarks that another *Aesculus*, the *Aesculus turbinata*, is known to the Japanese as *tochi-no-ki* (Ch. 七葉樹 *ch'i-yeh shu*, « tree with seven leaves »), but that it is not mentioned in the *Pên-ts'ao kang-mu* and may not exist in China. This is only partly true. Though the *Pên-ts'ao kang-mu* does not speak of the *ch'i-yeh* tree, it is described in the *Lo-yang ming-yüan chi*, a work of the end of the 11th cent. (*Chin-tai pi-shu* ed., 6 a; cf. *Ssü-k'u...*, 70, 13), and the *T'u-shu chi-ch'êng* (*loc. cit.* 309, *chi-shih*, 2a) duly cites the passage in its chapter on the *so-lo* tree. A poem of Ou-yang Hsiu (1007-1072) is entitled « The 'tree with seven leaves' (*ch'i-yeh shu*) of the Ting-li-yüan » (at Lo-yang), but in the poem itself the tree is called *so-lo* (cf. *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 309, *i-wên*, 3 a). The old *Chiang-ning fu-chih* speaks also of *ch'i-yeh shu* in temples of the Nanking region and considers them as *so-lo* trees (*ibid.* 309, *tsa-lu*, 2); two more quotations are given in *Pên-ts'ao kang-mu shih-i*, 7, 71 b. It is also an *Aesculus* which is described as « flower of the *so-lo* tree » in the *Tsao-lin tsa-tsu, chung chi*, 54 a, with the alternative name 鶴翎 *hao-ling*, « crane feathers ». In his *Ch'i-hsiu lei-kao* (40, 3 b-4 a), Lang Ying opposes the common belief that the *so-lo* tree was the 月中桂 *yüeh-chung kuei* (« moon cinnamon », *Litsea glauca*), since it was in fact the *ch'i-yeh* tree (*mu*). This identification of the *so-lo* tree with the *Aesculus chinensis* also occurs in a notice in which *so-lo* trees are mentioned on the T'ien-t'ai Mountain in Chê-chiang, and these trees had only « six or seven leaves » at the end of each branch (*T'u-shu chi-ch'êng*, *ibid.* 309, *chi-shih*, 2 a).

We are now prepared to approach the problem of *so-lo* as a designation of a « cotton tree ». WATTERS was first, I believe, to say that, in the latter sense, *so-lo* represented *śālmali*, the Indian name of the silk-cotton tree (*Essays on the Chinese language*, 435). He even went as

far as to say that the transcription was 娑羅木 *so-lo-mu*, but *mu* (\**muk*) of course means «tree» and is purely Chinese; moreover, it is not regularly attached to *so-lo*, even when the latter term is a designation of a «cotton tree». WATTERS's explanation long remained unchallenged, and, apart from the final *mu*, I formerly accepted it (*BEFEO*, IV, 173). LAUFER (*Sino-Iranica*, 491-492) then proposed another derivation, from Lolo *sala*, «cotton» (P'u-p'a *sala*, Čöko *sölö*). In *TP*, 1921, 83, I gave my reasons for renouncing *šālmali*, and felt inclined to accept LAUFER's view. I still hold *šālmali* to be impossible, because, as we have seen above, all the Chinese transcriptions of this word suppose a *š*-initial, not *s*- as in *so-lo* (< *sāla*, as well attested as *šāla*), and because too much of the original word would thus be omitted. But the derivation from Lolo *sala* is not evident, and I now think that this too may have to be abandoned.

LAUFER started from a quotation he found in the *Pên-ts'ao kang-mu* (36, 72 *b*): «The various Nan-chao Man do not breed silkworms. They merely collect the white floss inside the seeds of the 娑羅 *so-lo* tree (*mu*), which they make into threads and weave into strips called 娑羅籠段 *so-lo lung tuan*, 'so-lo covering damask'» (段 *tuan* is the character which is now, though irregularly, written 緞 *tuan*, and means «satin», but also «cotton damask»). In the *Pên-ts'ao kang-mu*, this was given as occurring in the *Nan-yüeh chih*, and LAUFER accepted it. We have already been confronted with an anachronistic quotation from the *Nan-yüeh chih*; the case is the same here, and LAUFER might have noticed that the *Nan-yüeh chih*, written in the 5th cent., could not have mentioned the name Nan-chao which did not exist until the 7th cent. But, in the present case, the quotation is genuine, and only the source is erroneously indicated. The same text, without indication of origin, occurs in the *Hsü po-wu chih* (*Tzū-shu po-chung* ed., 7, 2 *b*; WATTERS refers to this passage), a work of the middle of the 12th cent. (cf. *Ssü-k'u*..., 142, 47), but there it begins with «The various Man of the kingdom of P'iao» (= Pyü, the ancient name of Burma). It is also given, at greater length, in the *T'ai-p'ing yü-lan* (961, 3 *a*), but under a corrupt rubric 牧娑羅 *mu-p'o-lo*, due to the usual confusion of *so* and *p'o*, and to a graphic corruption of 收 *shou*, «to collect», into 牧 *mu*, «to tend cattle»; as it no longer made sense, *mu* was taken as part of the name of the tree. The text is as follows: «The 南夷志 *Nan-i chih* says: The Nan-chao abundantly collect (*mu*; read *shou*) the seeds of the *p'o-lo* (read *so-lo*) tree (*shu*), break their husk, the inside of which is white like willow catkins (*liu-hsü*), make this into threads and weave it into square strips, which they cut to make 'covering damask' (*lung tuan*). Men and women all wear it. The kingdom of P'iao, the Mich'ên and the [Mi-]no (cf. *BEFEO*, IV, 171, 172) all throw on (披 *p'i*) 'p'o-lo (read *so-lo*) covering damask'.» Although the quotation in the *Pên-ts'ao kang-mu* is not directly taken from the *T'ai-p'ing yü-lan*, it seems probable that it is the title *Nan-i chih* which has been altered to *Nan-yüeh chih*. As to *Nan-i chih* itself, it is the name under which the *T'ai-p'ing yü-lan* cites the *Man shu*, written in 864 (cf. *BEFEO*, IV, 132, 172; *Man shu*, 49 *a*). Our text of the *Man shu*, recovered from the *Yung-lo ta-tien*, is often corrupt, but the work is of first-rate importance. We read in it (*Chien-hsi-ts'un-shê* ed., 31 *a-b*): «West of the city (*ch'êng*) of Yin-shêng, the city of Chih-nan, Hsün-chuan and Chi-hsien (these are all places or regions in western Yün-nan), the Fan and Man tribes do not breed any silkworms. They merely collect the seeds of the *p'o-lo* (read *so-lo*) tree (*shu*), break their husk, the inside of which is white like willow catkins, and

weave [these] into square strips which they cut to cover the head (籠頭 *lung t'ou*; read *lung tuan*, ' [to make] « covering damask » '). Men and women all wear it. The kingdom of P'iao, the Mi-ch'ên and the [Mi-]no all throw on *lo-tuan* (read *so-lo lung tuan*, ' *so-lo* covering damask '). » Part of this text has passed into the *Hsin T'ang shu* where it is said (222 A, 1 b) : « West of Ta-ho (the Nan-chao capital on the eastern side of the Ta-li Lake) and Ch'i-hsien, people do not breed silkworms; they break the fruit (*shih*) of the 波羅 *po-lo* (娑 *so* > 婆 *p'o* > 波 *po*; read *so-lo*) tree (*shu*), the appearance [of which] is like floss (*hsü*), twist this into threads and make strips of it. » The alteration of *so-lo* into *po-lo* was made easier by the fact that the same texts give another term, 波羅 *po-lo* (*Man shu*, 31 a), or 金波羅 *chin-po-lo* (*Hsin T'ang shu*, 222 A, 2 b), as being the Nan-chao word either for « tiger » or for a « tiger skin » (cf. also *BEFEO*, IV, 483; the wrong form of the name of the tree in the *Hsin T'ang shu* misled WANG Ch'ang in *Chin-shih ts'ui-pien*, lith. ed., 160, 5 a; as to the 波羅 *po-lo* tree similar to the *po-yang*, *Populus alba*, which was offered by Magadha to the Chinese Emperor in 647 according to *Hsin T'ang shu*, 221 A, 11 b, and which CHAVANNES has adduced in *Doc. sur les Tou-kiue*, 119, it seems to be a *palāśa*, *Butea frondosa*, as in CHAVANNES, 500 *Contes*, II, 245, 247, more probably than a jack-fruit tree as surmised in *HR*, 213, and in any case can have nothing to do with the *so-lo* > *p'o-lo* > *po-lo* tree of Nan-chao). That the erroneous reading *po-lo* for *so-lo* is ancient, and probably original, in the *Hsin T'ang shu* is established by the fact that it also occurs in the *Wên-hsien t'ung-k'ao*, 329, 5 a, where there is a word for word repetition of the passage in the *Hsin T'ang shu*. The *so-lo* cloth is mentioned elsewhere in the *Man shu*. Of the 撲子 P'u-tzü *Man*, we are told (18 a) that « with blue (*ch'ing*) *p'o-lo* (read *so-lo*) damask (*tuan*) they make full trousers (通身袴 *t'ung-shên-k'u*; on this term see « Çardandan », p. 604). » Of various tribes akin to the Mang *Man*, we learn (21 a) that « the women throw on *so-lo* coverings (*lung*) of different colours (*wu-sê*) » (the text is wrongly punctuated in the edition); in the *Hsin T'ang shu* (222 A, 4 b), this has become : « The women let *p'o-lo* coverings of different colours hang down behind. » The information of f. 31 a-b partly recurs in the notices of the various countries; thus we are told (f. 43 a) that « in the kingdom of Mi-no and the kingdom of Mi-ch'ên, ... all throw on *p'o-lo* (read *so-lo*) coverings (*lung*) » and (f. 43 a-b) that « in the kingdom of P'iao..., the women... all wear skirts of blue (*ch'ing*) *p'o-lo* (read *so-lo*), and moreover throw on *lo-tuan* (read *so-lo lung tuan*, ' *so-lo* covering damask ') ». This too passed into the *Hsin T'ang shu* (222 B, 5 a), where it is said that the women of that country « put on a blue-green (*ch'ing*) skirt of *p'o* (婆裙 *p'o-ch'ün*; read *so-lo ch'ün*; this would-be *p'o-ch'ün* can have nothing to do with the *p'o-ch'ün* spoken of in *Ling-wa-tai-ta*, 6, 13-14), and throw on a *lo-tuan* (read ' a *so-lo lung-tuan* ') ». In 795-804, the king of Pi'ao sent a band of musicians to the Chinese court, and the titles of the tunes they played have been preserved in *Hsin T'ang shu*, 222 B, 7 b. One of them was called « Praising the *so-lo* flower » (讚娑羅花), and the text adds : « In P'iao (Pyū) language, [the title] was 囉莽第 *lung-mang-ti* (\**li'ong-mâng-d'iei*); the people of that country make clothes with that flower (*i. e.* with cloth woven with its floss), [which clothes] can keep their bodies clean. » Unfortunately the Pyū language is still practically unknown; it may be worth noticing, however, that no word resembling *so-lo* can possibly occur in the Pyū title.

There can be no doubt that, in all these texts, *so-lo* is the designation of some sort of

«cotton», perhaps from the cotton plant in the case of the Burmese countries of P'iao, Mi-ch'ên and Mi-no, but probably from the cotton tree in its original application to the tribes of western Yün-nan, particularly in the region of Yung-ch'ang. But, in such a case, «*so-lo* tree» was not the earliest designation of that «cotton tree» in Chinese literature. When the Chinese reached Indo-China and Yün-nan, they were particularly struck by two remarkable species: a tree which yielded flour, and another from the flowers of which cloth was woven. The tree which yielded flour was at first designated under the purely Chinese name 桃榔 *kuang-lang* (perhaps an enlargement of a popular name 光郎 *kuang-lang*, «bright boy»), but fairly soon a kindred species became known as 莎木 *so-mu*, «*so* [\**suâ*]-tree», so written in the *Ch'i-min yao-shu* (10, 46 a), the *T'ang yün* (cf. *Pên-ts'ao kang-mu*, 31, 24 b, and *K'ang-hsi tzü-tien, pu-i*, rad. 140 and the *T'ai-p'ing yü-lan* (960, 16 b), although LI Shih-chên always retains in his text and his quotations an unauthorized form 紗 *so* (\**suâ*). According to LI Shih-chên, the *so* of *so-mu* would be a phonetic alteration of 穰 *hsiang-mu*, «*hsiang* (\**sjang*)-tree», which designated the same tree in the geographical section of the *Wu lu* (third quarter of the 3rd cent.; cf. *supra*, p. 460). I am of a different opinion. *So-mu* too appears at an early date, since it occurs in the 蜀志記 *Shu chih chi* (cf. *Ch'i-min yao-shu*, 10, 46 a), or 蜀記 *Shu chi* (cf. *Pên-ts'ao kang-mu*, 31, 24 b), or *Shu chih* (cf. *T'ai-p'ing yü-lan*, 960, 16 b); the *Shu chi* would probably be that of 李膺 Li Ying (cf. *Sui ching-chi-chih k'ao-chêng*, 6, 16 a; *Ch'i-min yao-shu*, 10, 24 b; but this cannot be the Li Ying who died A. D. 169), and date from the 5th cent. at the latest. If the title be *Shu chih*, it would carry us back to the 3rd cent. (cf. *ibid.* 6, 23 b). In any case, we have to deal with a sago-yielding tree (cf. STUART, *Materia Medica*, 389-390), and I believe that the *so* (\**suâ*) of *so-mu*, «*so* tree», is nothing but a transcription of «sago» itself, Jav., Mal., Sund., Battak, Mak., and Bug. *sagu*, Dayak and Tagal *sago* (cf. FAVRE, *Dict. malais-français*, II, 599). If *hsiang* (\**sjang*), which, apart from the *Wu lu*, occurs in the famous *Wu-tu fu*, «Ode on the capital of Wu», written by Tso Ssü (3rd cent.), is phonetically connected with *so*, I would regard it as a more literary and less accurate representation of the same foreign word. On other later Chinese transcriptions of Mal. *sagu*, cf. TP, 1933, 391, and, for European transcriptions YULE, *Hobson-Jobson*<sup>2</sup>, 780-781; Polo describes *sago*, and even brought some of it back to Venice (cf. Vol. I, 377), but, like Odoric (*Wy*, 448), does not give its name.

The other remarkable tree found in the south by the Chinese was the one with the flowers of which cloth was made by the native tribes of western Yün-nan. I have translated above (p. 444) the passage from the *Hou-Han shu* (116, 8 a), according to which the people of Ai-lao, i.e. of Yung-ch'ang in western Yün-nan, «have the 梧桐 *wu-t'ung* tree (*mu*), the flowers (*hua*) of which they spin to make cloth (*pu*)». Yet BRETSCHNEIDER (*Botanicon Sinicum*, II, 349, 351) says that cloth was made from the bark of the tree, and the same information is given by STUART, *Materia Medica*, 423. It is true that Polo speaks of cloth made of the «bark» of trees at «Cuigu» (*q. v.*; = Hsü-chou-fu in Ssü-ch'uan, «Sui-fu» of our maps; cf. Vol. I, 298-299, and Y, II, 124, 127); but this cannot refer to the fabric under discussion here. A great deal of confusion has long prevailed among Chinese scholars on the subject of the various sorts of *t'ung*, which, in principle, is the designation of the *Paulownia* (cf. the numerous texts in *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 237-239). LI Shih-chên was the first to give *t'ung* and *wu-t'ung* under two

different rubrics; he has been followed by BRETSCHNEIDER (*Botanicon Sinicum*, II, 348, 350) and STUART (*Materia Medica*, 312, 423); both agree to see in the *t'ung* the *Paulownia imperialis* and in the *wu-t'ung* the *Sterculia platanifolia*, and to state that the tree from the bark of which cloth was woven was the *Sterculia*. I am convinced that the latter opinion is a double mistake. First, I cannot discover any ancient text which speaks of cloth made from the bark of the *wu-t'ung* tree. Kuo I-kung's *Kuang chih*, as cited in the *T'ai-p'ing yü-lan* (966, 9 b) says: «There is white *wu-t'ung*. The kingdom of P'iao (= Burma) has the white *t'ung* tree (*po-t'ung-mu*); its leaves (葉 *yeh*) have a white down (毳 *ts'ui*); the people take this down, soak it, and make it into threads with which they weave cloth.» Apart from the first sentence, the quotation also occurs earlier in the T'ang encyclopaedia *I-wên lei-chü* (85, 28 b). The *T'u-shu chi-ch'êng* (*ts'ao-mu tien*, 239, *chi-shih*, 2 a) gives it in the same form as the *T'ai-p'ing yü-lan*. Ch'ên Chu's *T'ung p'u*, «Monograph of *t'ung*», dated 1049, cites it in identical terms (*T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 237, 11 a). I have no doubt, however, that 葉 *yeh*, «leaves», is an old graphic corruption of 華 *hua* or 花 *hua*, «flower». As a matter of fact, the passage of the *Kuang chih*, as cited in the commentary of the *Hou-Han shu* (116, 8 a; cf. BEFEO, IV, 173), does not give *yeh*, but *hua* (the compiler of the fragments of the *Kuang chih* in the *Yü-han-shan-fang chi i-shu*, 2, 1 b, seems to have used a bad edition of the *Hou-Han shu*). Although the main compiler of the *I-wên lei-chü*, Ou-yang Hsün, died A. D. 641, whereas the commentary of the *Hou-Han shu* was completed only in 676-678, we have no edition of the *I-wên lei-chü* earlier than the one revised under the Ming dynasty, and cannot be sure that Ou-yang Hsün's original text already gave the faulty reading.

However that may be, the fact that *hua*, and not *yeh*, is the only possible reading can be established by another series of arguments. As we have seen, the *Hou-Han shu* says that cloth was made from the «flowers» of the *wu-t'ung* tree, and so does the *Hua-yang kuo-chih* (4, 18 a-b), which says in like terms: «In Yung-ch'ang, there is the *wu-t'ung* tree (*mu*), the flowers of which are soft like silk threads (絲 *ssü*). The people spin them to make cloth, every strip of which is about five feet broad; it is clean and white and does not get soiled; its popular name is 桐華布 *t'ung-hua pu* («*t'ung*-flower cloth»). They cover the dead with it, and afterwards wear it and sell it to others» (the quotation in *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 339, *chi-shih*, 2 b, copied from *T'ai-p'ing yü-lan*, 957, 9 b, is most inaccurate). In the same way, Tso Ssü (3rd cent.), in his «Ode on Ch'êng-tu» (*Shu-tu fu*), too famous not to have been genuinely preserved, says (*Wen-hsüan*, ch. 4): «For cloth (*pu*), they have the flowers of *t'ung* (種華 *t'ung-hua*); for flour, they have the *kuang-lang* (cf. *supra*, p. 473)». The rare character used by Tso Ssü, *t'ung*, not discussed in botanical works, is historically homophonous of the *t'ung* of *wu-t'ung*, and is certainly to be regarded as its equivalent. The commentary adds: «The tree is called *t'ung*; its flowers have a soft down which can be spun to make cloth. It grows in Yung-ch'ang». In speaking of cloth made from the «bark» of the *wu-t'ung*, BRETSCHNEIDER expressly refers to Lu Chi's «Memoir on natural history in the Book of Odes» (*Mao-Shih ts'ao-mu niao-shou ch'ung-yü shu*). This is a well-known work by 陸璣 Lu Chi, *tzü* 元恪 Yüan-k'o, a man of the Wu kingdom who lived in the middle of the 3rd cent. (LEGGE, *Chin. Classics*, IV, Proleg., 178, and BRETSCHNEIDER, *Botanicon Sinicum*, I, 33, still confuse this Lu Chi of the Three Kingdoms with the celebrated author

陸機 Lu Chi, *tzū* 士衡 Shih-hêng, 261-303, of the Chin dynasty; on Lu Chi of the Three Kingdoms, cf. *Ssü-k'u* . . . , 15, 8 a). The original work is lost, and the present text, in two chapters, is a *rifacimento* made up of early quotations, first published c. 1360 in the *Shuo fu* (ch. 4 of the *Shuo fu* in 120 chs.), and republished by MAO Chin, with an abundant commentary, in the *Chin-tai pi-shu*. Of modern critical editions, I do not have at my disposal the one published by Lo Chên-yü, and can use only that of CHAO Yu, dated 1779, republished in *Chü-hsüeh-hsüan ts'ung-shu*, first *chi*. The text says (ch. 1, 17 a) : « Of *t'ung*, there are the 'blue-green *t'ung*' (*ch'ing-t'ung*), the 'white *t'ung*' (*po-t'ung*) and the 'red *t'ung*' (*ch'ih-t'ung*). [The *po-t'ung*] is good for making psalteries (*ch'in-shê*). At present the people of Yün-nan and Tsang-ko (in Kuei-chou) make it into threads to make cloth; it is like a woollen cloth (*mao-pu*). » The words « *po-t'ung* » which I have put between square brackets are not given in CHAO Yu's text, copied from the *Shuo fu*, nor had they been added in MAO Chin's text or in his commentary; they do not occur either in the quotation made by *T'ai-p'ing yü-lan*, 956, 9 a (nor in the almost identical quotation which has passed into *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 237, 4 a). In CHAO Yu's commentary, it is said that this second « *po-t'ung* » may have been dropped accidentally, and a more recent note adds that it occurs in another edition. In this text, nothing is said of the floss or its origin; but even a superficial examination reveals that the text is incomplete, since we are not told what the Yün-nan and Tsang-ko people make into threads. As a matter of fact, we possess the complete passage, quoted by Su Sung (1020-1101; cf. BRETSCHNEIDER, *Botanicon Sinicum*, 1, 47), from whom it has passed into *Pên-ts'ao kang-mu* (35 A, 25 b) and the *T'u-shu chi-ch'êng* (*ts'ao-mu tien*, 237, 15 a); it is not to the credit of MAO Chin or of CHAO Yu that they should both have ignored the quotation in Su Sung, which says : « The *po-t'ung* is good for making psalteries (*ch'in-shê*). The people of Yün-nan and Tsang-ko take the white down [which is] inside its flowers (*hua chung*), soak it (淹漬 *yen-tzū*) and make it into threads (績 *chi*) to make cloth (*pu*); [this cloth] is like a woollen garment (毛服 *mao-fu*) and it is called 'flower cloth' (*hua-pu*). » This is clearly the text which BRETSCHNEIDER knew from the *Pên-ts'ao kang-mu*, and it speaks of « flowers », not of « bark ». According to the *T'ai-p'ing yü-lan* (359, 14), the « *t'ung* flower cloth » (*t'ung-hua-pu*) was listed in the *Kuang-chih* among the products of the kingdom of P'iao-jên (read P'iao, Burma). As late as 1461, the *Ming i-t'ung chih* (ch. 87, 26 b, under *Yung-ch'ang chün-min fu*) mentions the « *t'ung* flower cloth » of the Golden Teeth (Chin-ch'ih; see « Çardandan »), perhaps in an antiquarian mood; this has passed into the *Kuang-yü chi* 21, 20 b, which speaks of 細布 *hsi-pu*, « fine cloth », as a designation of a fabric made of *t'ung* flowers at Yung-ch'ang (= Chin-ch'ih). I find a mention of « bark » only in a text of the end of the 10th cent., the *T'ai-p'ing huan-yü chi*, which will be discussed *infra*, p. 478.

Despite all these quotations, I suspect that the designation of the tree as *wu-t'ung* is a misnomer, and that we have to deal either with a silk-cotton tree (*Bombax malabaricum*), or a cotton tree (*Gossypium arboreum*). Lu Chi's text, in which the tree from whose flowers cloth was made is called *po-t'ung*, says that the wood is good for making psalteries, and this would exclude *Gossypium arboreum*. But I think it would also exclude *Bombax malabaricum*. Lu Chi must have combined data which concerned two different trees. There is no question of « psalteries » in the text of the *Hua-yang kuo chih* and the *Hou-Han shu*, and the name is given there as *wu-t'ung*, not *po-t'ung*. Moreover, a simple look at chs. 237-239 of the *ts'ao-mu tien* section of the *T'u-shu*



*chi-ch'êng* is enough to show that Chinese scholars could never form a clear theory as to the identification of the various *t'ung* species mentioned in ancient works. As to the present *wu-t'ung*, alias *po-t'ung*, the «soaking» (*yen-tzũ*) is perplexing; as the same term occurs in the quotation from the *Kuang-chih*, it is not probable that 漬 *tzũ* should be regarded as a corrupt reduplication, subsequently enlarged into *yen-tzũ*, of the 績 *chi* that follows. It may be, after all, that, in the 3rd cent., the natives of south-western China had a special method of preparing the cotton floss for weaving. But, since cloth was made out of it, I hold that the silk-cotton tree is practically excluded; on the other hand, the designation *wu-t'ung* cannot refer to a plant, but at least to a small tree. My conclusion is that the so-called *wu-t'ung* tree of the *Hua-yang kuo chih* and the *Hou-Han shu* may be no other than the cotton tree, *Gossypium arboreum*, and that the *po-tieh* of the *Ai-lao* may be the cloth which was woven from its floss.

I have said that the 幢 *t'ung* of the *Shu-tu fu* was certainly the same as the 桐 *t'ung* of the *Hua-yang kuo chih* and the *Hou-Han shu*, so that it must also be a designation of the cotton tree. Curiously enough, this identification seems to have been made by some mediaeval scholars, since an author of the Mongol period, 陳高 Ch'ên Kao (a native of Wên-chou in Chê-chiang, doctor in 1354, † 1356; cf. *Ssũ-k'u* . . . , 168, 6 a), has written a poem entitled 種幢花 *Chung t'ung-hua*, «Sowing *t'ung* flowers» (cf. *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *i-wên*, 2 a), which unmistakably refers to *Gossypium arboreum*: its location in the south (炎方 *yen-fang*), its cultivation in the place of silkworms and mulberries, the care with which the young plants are attended, their full size of three feet, the yellow flowers and the white floss, finally the weaving into cloth for garments, all leave no doubt as to the identity. For once, it was a fortunate inspiration on the part of the compilers of the *T'u-shu chi-ch'êng* to include this poem not in the chapters on the 桐 *t'ung* trees like the passages concerning the *wu-t'ung* of the *Hua-yang kuo-chih* and *Hou-Han shu*, but among the notices on cotton.

To account for the use of 幢 *t'ung*, or 桐 *t'ung*, or 梧桐 *wu-t'ung* as early designations of the cotton tree, we must remember that, when the Chinese, in their advance through new countries, found unknown products, they had either to borrow their foreign names, or to use, as an equivalent, a Chinese term already referred to something more or less similar. This was how, for instance, they gave the name 胡桐 *hu-t'ung*, «*t'ung* of the Hu», and even sometimes *wu-t'ung*, to the balsampoplar, *Populus balsamifera*, of Central Asia (cf. LAUFER, *Sino-Iranica*, 339). I am convinced that they did the same for the cotton tree of Yün-nan, the more so since one at least of the *t'ung* trees was said to resemble *mu-mien* (cf. *supra*, p. 462), and that such a loose approximation accounts to a great extent for the difficulties of identification they vainly tried to overcome later on. On the other hand, the «*t'ung* flowers» and «*t'ung* flower cloth» of Yüan and Ming times were merely archaistic survivals. From T'ang times, a new name for the cotton tree of Yün-nan had been adopted; it was called the «*so-lo* tree».

We have found this use of *so-lo* in the *Man shu* and, in a corrupt form, in the *Hsin T'ang shu*. But there are other ancient examples of it. The *T'ai-p'ing kuang-chi* (406, 3 a) has preserved a long extract (the *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 312, 1 a-b, gives only the first part) from a T'ang work, the *Li-chou T'ung-wang hsien t'u-ching*, or «Monograph of the district of T'ung-wang of Li-chou» (south-west of the modern Ya-chou in Ssũ-ch'uan); this extract is

devoted to a 娑羅綿樹 *so-lo mien-shu*, or « *so-lo* cotton tree », which existed west of the district city of T'ung-wang. It required the extended arms of three or four men to embrace it; it first gave flowers, and later gave leaves; the flowers opened only when there was a warm summer. The pistils (葉 *juì*) of the flowers had a floss (綿 *mien*) which was called 娑羅棉 *so-lo mien*, « *so-lo* cotton » (no stress can be laid upon the use of the two forms of *mien* in our very faulty editions of the *T'ai-p'ing kuang-chi*). It is to be remarked that this tree was brought into a miraculous connection with a famous deceased Buddhist monk, and that some of the particulars given about the tree (its size, and the flowers preceding the leaves) recur in descriptions of *so-lo* trees in texts which are supposed to refer to *sāla* trees. In fact, the tree of Li-chou may have been a *Bombax*.

The *Pên-ts'ao kang-mu* (36, 72 b) cites an interesting text which it says occurs in 祝穆 *Chu Mu*'s 方輿志 *Fang-yü chih* (« Geography ») : « 平緬 P'ing-mien produces 娑羅 *so-lo* trees (*shu*); the big ones are thirty or fifty feet high. They form seeds (*tzü*) which have floss (*mien*). This floss is made into threads and woven to make white felt (白氈 *po-chan*; perhaps a misreading of 白疊 *po-tieh*) [and] *tou-lo-mien*. » LAUFER (*Sino-Iranica*, 491), when using this important text, said that *Chu Mu* was an author of the Sung dynasty. It is true that *Chu Mu*, of the Sung dynasty, published about 1239 a geographical work entitled 方輿勝覽 *Fang-yü shêng-lan* (cf. *Ssü-k'u* . . . , 68, 10 a), but the circuit (*lu*) of P'ing-mien (« Pacified Burma »), in the region of the present T'êng-yüeh, was established only in 1276 (*YS*, 61, 13 a). So there can be no doubt that the quotation really comes not from *Chu Mu*'s *Fang-yü shêng-lan*, but from the [聖朝混一] *Shêng-ch'ao hun-i fang-yü shêng-lan* which was published under Qubilai's reign (1260-1294; cf. MIAO Ch'üan-sun's *I-fêng-t'ang tu-shu hsü-chi*, 3, 1 a) and incorporated (in 1307?; cf. *Kuan-ku-t'ang ts'ang-shu mu*, 3, 27 b, where « 31th » *ta-tê* year must be a slip for « 11th ») into the [事文類聚翰] 墨大全 [*Shih-wên lei-chü*] *Han-mo ta-ch'üan*; unfortunately I have at present no access to the latter collection. It is, at any rate, clear that the *so-lo* tree of this text is either a cotton tree or a silk-cotton tree. The size would suggest a silk-cotton tree, but it is only the floss of the cotton tree which could be used for weaving *tou-lo-mien*.

The same may be said of the passage from the *Ko-ku yao-lun* also adduced by LAUFER (*ibid.*). The *Ko-ku yao-lun* was published in 1387 and again in 1388, and added to in 1456-1459. Among the passages which belong to the original redaction, we read (*Hsi-yin-hsüan ts'ung-shu* ed., 8, 4 b) : « 兜羅錦 *Tou-lo-chin*. The *tou-lo-chin* is a product of the Southern Barbarians (Nan-Fan), of the Western Barbarians (Hsi-Fan) and of Yün-nan. It is woven with the 錦 *chin* inside the seeds of the 莎羅 *so-lo* tree (*shu*), and is similar to velvet (剪絨 *chien-jung*). It is five or six feet in breadth, and is much used for making blankets (*pei*) and also clothes can be made of it. » *Chin* means « brocade », but *tou-lo chin* must be a corrupt reading for *tou-lo*-[綿]*mien*, as already said by LAUFER. As we have seen above (cf. *supra*, p. 431), *tou-lo-mien* was under the Ming dynasty the designation of a cotton velvet; moreover, it is *mien*, « floss », not *chin*, « brocade », which is found « inside the seeds » of the *so-lo* tree, and this *so-lo* tree must be the cotton tree, *Gossypium arboreum*.

This conclusion is fully borne out by the fact that *so-lo*, as a designation of the cotton tree, has survived down to the present day, mainly in the modernized form already used in the *Ko-ku yao-lun*. According to the *Ming i-t'ung chih* (87, 17 b), completed in 1461, the « 梭羅 *so-lo* tree

(*mu*) » grew at Wu-ting (north-west of Yün-nan-fu; cf. also *T'u-shu pien*, 89, 30 *a*, and the *Ta-Ch'ing i-t'ung chih* in 356 chs., 309, 9 *b*), and « 莎羅 *so-lo* cloth (*pu*) » was made at Ta-yao (N.N.E. of Yao-chou, now Yao-hsien, between Yün-nan-fu and Ta-li; 87, 13 *a*; this has passed into the *Kuang-yü chi*, 21, 16 *a*), at Chien-shui (= Lin-an, north-west of Mêng-tzŭ; I have not found this indication in the *Ming i-t'ung chih*, but only in the *Ta-Ch'ing i-t'ung chih* in 356 chs., ch. 307, 26 *b*), and in the native districts (*ch'ang-kuan-ssŭ*) of the territories (*tien*) of Ma-lung and T'a-lang (north-east of Yün-nan-fu; 87, 25 *a*); in the latter case, we are told that « it is made of cotton (*mien-hua*), scarcely eight inches in breadth; every year, some is paid [as tribute] to the authorities » (this has passed into *T'u-shu pien*, 89, 30 *b*). In the *Tien hsi*, completed in 1807, « 梭羅 *so-lo* cloth (*pu*) » is mentioned as worn by a Lolo tribe (cf. *BEFEO*, VIII, 343), but the more usual way of writing the term is 莎羅 *so-lo* (*ibid.* 359, 360 [where it is used by a Thai tribe to enshroud the dead, which reminds us of the text of the *Hou-Han shu*; cf. *supra*, p. 444], 363 [for the skirts of P'iao women, identical as to the name with the ancient P'iao kingdom of Burma]); this latter form is also used in the modern *Hsü Yün-nan t'ung-chih kao* (160, 27 *b*; 162, 29 *b*).

In its section on the products of Yün-nan, the *Hsü Yün-nan t'ung-chih kao* (58, 5 *a*) has a paragraph on « *so-lo* cloth » (莎羅布 *so-lo pu*) : « According to the *I-t'ung chih* (= *Ch'ing i-t'ung chih*; cf. above), it is made at Chien-shui (= Lin-an, north-west of Mêng-tzŭ). In the *T'ai-p'ing huan-yü chi* (published in 976-984; 79, 12 *b*), there is in Yao-chou (about half-way between Yün-nan-fu [now K'un-ming] and Ta-li) the 樟木 *t'ung-mu* ('*t'ung* tree'), from the bark (皮 *p'i*) of which one can make cloth. The *Yao-an fu chih* ('Description of Yao-an fu'; this was the name of Yao-chou, now Yao-hsien, under the Ming dynasty) says that this is the 梭羅布 *so-lo pu* ('*so-lo* cloth'); according to the old 'Monograph (*chih*) [of Yün-nan province]', it is the 樟布 *t'ung pu* ('*t'ung* cloth'; cf. the analogous text in *Tien hsi*, 4, 23 *b*, where [木] *mu-lo pu* is an error instead of *so-lo pu*). The *T'u-shu-pien* (89, 30 *b*; on this Ming work, cf. *supra*, p. 464) [says] that it is produced in the native districts (*ch'ang-kuan-ssŭ*) of the territories (*tien*) of Ma-lung and T'a-lang (N.E. of Yün-nan-fu); it is made of cotton (*mien-hua*), scarcely eight inches in breadth; every year, some is paid [as tribute] to the authorities. The *I-t'ung chih* (= *Ming i-t'ung chih*; cf. above) says that [the *t'ung-pu*] is produced at Chên-yüan (north of P'u-êrh; I have not found the original passage; the *Tien hsi*, 4, 25 *b*, mentions *so-lo pu* at Chên-yüan). » These texts show that, under the Ming dynasty, the « *so-lo* cloth » was correctly understood to be identical with the old « *t'ung* cloth », which was, as we have seen, made of the floss of *Gossypium arboreum*. As to the « bark » of the *T'ai-p'ing huan-yü chi*, in view of all the concordant texts adduced above, it is no doubt an error (less serious than for instance the one in PURCHAS, who gives as made of the « bark » of a certain tree the *mŭgā* cloth of Assam, manufactured from the threads of a sort of wild silkworm; cf. YULE, *Hobson-Jobson*<sup>2</sup>, s. v. « moonga, mooga »), and the *Ming i-t'ung chih* was right in speaking of the material as *mien-hua*, real cotton. The places mentioned are scattered all over the province, which is not surprising in view of the area of cotton cultivation in Yün-nan during the Ming dynasty. Although most of the cotton now used in Yün-nan is imported, the plant is still grown in the south-western part of the province and north of Ta-li, and the *Ta-Ch'ing i-t'ung chih* in 356 chs. (306, 23 *a*) expressly gives it (*mien-hua*) as a product of Lo-p'ing (south-east of Ch'ü-ching) in the extreme east of Yün-nan.

The word *so-lo* of these texts is clearly the same as the *so-lo* of the T'ang period, and we have now to explain it. Since we have dismissed WATTERS' etymology from Skr. *śālmali*, the only solution proposed hitherto is the ingenious one of LAUFER, according to which *so-lo* would render Lolo *sala* (P'u-p'a *sala*, Ččko *sölö*), «cotton». As we are actually dealing with a cotton tree, this sounds very tempting. Yet, after a more careful examination of the problem, I am far from being convinced. It may be an objection of no great account that the etymology should be sought in Lolo, while the term first occurs about the Nan-chao, who are supposed to have been Thai. Nor would I lay great stress on the fact that Lolo is really a monosyllabic language, in which the constituents of the polysyllabic terms generally lend themselves to a satisfactory analysis, which is not the case with *sala*. But the main point is that I am almost convinced, as a result of the inquiry conducted above, that *so-lo* is nothing else than the very transcription of Skr. *śāla*, *sāla*. The name, famous in Buddhism, of that lofty Indian tree, practically unknown in China, had already been occasionally transferred to the horse-chestnut; but a name was required for the cotton tree of Yün-nan, since that of *wu-t'ung* or *t'ung* was decidedly too misleading; *so-lo*, *sāla*, *śāla*, was available, and, adopted under the T'ang dynasty, did such good service that it is still in use nowadays. But then what about the Lolo *sala*? I believe that it is really connected with *so-lo*, *sāla*, but that the position is the reverse of what LAUFER thought it to be. There are a number of Chinese words in Lolo; in my opinion *sala* is probably one of them, and was borrowed by the Lolo at a time when they did not yet cultivate cotton, as the Thai tribes had done for many centuries; they called the cotton tree *sala*, because it was called *so-lo* (\**sā-lā*) by the Chinese around them.

Since *so-lo*, *sāla*, *Shorea robusta*, was confused with the cotton tree, and occasionally with the silk-cotton tree, and gave them its name, we need not be too much surprised that the reverse should have taken place. TARANZANO (*Vocabulaire*, II, 690) gives *tou-lo-chin* (read *tou-lo-mien*; cf. *supra*, p. 477, and *TP*, 1933, 431) as a designation of the «fruit of the *Shorea robusta*». Whatever be the source of the information, it certainly rests on an error, in consequence of which one of the names of «cotton», *tou-lo-mien*, was in its turn given to the *sāla* tree.

PAN-CHIH-HUA. — Another name seems to have passed through the same vicissitudes as *mu-mien* and *so-lo*, that is to say to have been referred now to the *Gossypium* and now to the *Bombax*. It is that of 斑 [or 班] 枝花 *pan-chih hua*, «flower with mottled branches», or, by corruption, 攀枝花 *p'an-chih hua*, «flower grasping the branches». The *Pên-ts'ao kang-mu* gives it as a designation of «the *mu-mien* which is like a tree». In TARANZANO (*Vocabulaire*, II, 489), *p'an-chih hua* is identical first to *Ceiba pentandra*, Gaertn. (*i. e.* *Bombax malabaricum*), and secondly, with a question mark, to *Gossypium arboreum*.

The earliest example I have found of this term occurs in a song (曲 *ch'ü*) entitled «Song of the flower with mottled branches» (*pan-chih hua ch'ü*). Its author, 汪廣洋 Wang Kuang-yang († 1380; cf. *Ming shih*, 127, 3 a) had become a doctor under the Yüan dynasty, but entered the service of the Ming; he was a Chiang-su man, but lived for some time in Hai-nan as an exile. I know the text only from the *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303; *i-wên*, 2 a-b. The song locates the *pan-chih hua* in Chiao-chou, *i. e.* in Tongking. The flowers, of red or reddish colour, open in the second or third moon. When they wither and the floss appears,

it is eagerly collected, women sit at their looms, and *chi-pei* is woven for the new white clothes of the husbands. *Chi-pei* has never been the name of *Bombax malabaricum*, and the latter was never and could never have been used for making cloth, but only as stuffing for mattresses, etc. So Wang Kuang-yang's *pan-chih hua* ought to be *Gossypium arboreum*; but Wang may have misapplied the term, and I must admit that the colour of the flowers and the time of their opening would rather suggest the *Bombax*.

In speaking of errors in the names of plants, Lu Jung, a doctor of 1466, has the following passage in his *Shu-yüan tsa-chi* (*Shou-shan-ko ts'ung-shu* ed., 12, 10 a) : «The tree-cotton-flower (*mu-mien-hua*) grows in Nan-yüeh (= Kuang-tung and Kuang-hsi); it is a tree four or five *chang* high (= 40 to 50 feet); the flower is red like the camelia (山茶 *shan-ch'a*, *Camelia japonica*); the seeds (*tzü*) are like the fruit (*shih*) of the *ch'u* (*Broussonetia papyrifera*); floss (*mien*) is produced in the seeds, and can be used to stuff cushions; it is what the people of Su-chou (in Chiang-su) call *p'an-chih-hua*. What is spun and woven to make cloth should be called 'cotton-flower' (*mien-hua*) only; the 雲間通志 *Yün-chien t'ung-chih* (a monograph on Sung-chiang in Chiang-su) calls it *mu-mien-hua*; it must be that it follows the error of Master 蔡 Ts'ai (= Ts'ai Ch'ên, the commentator of the *Shu ching*, on whom cf. *infra*, p. 487).»

In his *Tan-ch'ien hsü lu* (*Pao-yen-t'ang pi-chi* lith. ed., 8, 2 b), YANG Shên (1488-1559) has a brief note on *mu-mien*, more or less similar to that of Lu Jung. He first quotes two poems of the 9th cent. in which the expression «*mu-mien* flowers» occurs, then describes the *mu-mien* tree of the south, an armful in girth, with red flowers like the camelia and yellow pistils, states that it was not planted in Chiang-nan, and maintains that it is the «*mu-mien* tree» of the *Wu lu* (cf. *supra*, p. 460). He goes on to say : «This is the *pan-chih hua* of our days. It grows in A-mi-chou in Yün-nan (N. N.W. of Mêng-tzü), and is extremely abundant in Ling-nan (= Kuang-tung and Kuang-hsi).» We have seen that the «*mu-mien* tree» of the *Wu-lu* was *Gossypium arboreum*. But Lu Jung and YANG Shên's description is that of *mu-mien* in the sense in which *mu-mien* was used in Kuang-tung, where it was the name of *Bombax malabaricum*. Consequently YANG Shên's identification of the «*mu-mien* tree» of the *Wu lu* is erroneous. At the same time, Lu Jung and YANG Shên must have known what was meant by *pan-chih hua* in their own days, at least in some parts of China, and they clearly understood *Bombax malabaricum*. In spite of the fact that a lofty tree like the *Bombax* can hardly have been from the first known under the misleading name of «flower», and also that the *pan-chih hua* in Wang Kuang-yang's song ought to be *Gossypium arboreum*, we must admit that the same change of meaning which obtained in Kuang-tung for *mu-mien* also took place in the case of *pan-chih hua*, and that what may originally have been a name of *Gossypium arboreum* was, at a slightly later date, used for the *Bombax malabaricum*.

The 閩部疏 *Min-pu shu* is a short miscellany devoted to Fu-chien, and mainly to its products; it was published in 1585 by 王世懋 WANG Shih-mou, a native of T'ai-ts'ang in Chiang-su, and contains a curious passage on the cultivation of cotton (*Chieh-yüeh-shan-fang* ed., 7 b; cf. also *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *tsa-lu*, 1-2) : «I had formerly heard from old men that the people of Kuang[tung] planted cotton (棉花 *mien-hua*) which reached six or seven feet in height and that there were some [bushes] which were not changed (易 *i*) for four or

five years; at first I did not believe it. Passing Ch'üan-chou (in Fu-chien), I arrived between T'ung-an and Lung-ch'i (both S.W. of Ch'üan-chou). Swaying by the side of the road, [there was something] having the appearance of a hazel tree or Judas tree; when I came near and looked at it, it was cotton (*mien-hua*). The time was just *ch'ing-ho* (= the fourth moon); the old boughs already showed miserable yellow flowers. But this cannot be called *mu-mien*. The *mu-mien hua* ('*mu-mien* flower') is a lofty tree, with red (*tan*) flowers like [those of] the camelia (*ch'a*; often = *shan-ch'a*, 'camelia'); the fruit (*shih*) it bears is abundant; it is what in Wu (= Chiang-su) is called *p'an-chih hua*. YANG Yung-hsiu (= YANG Shên) has spoken of it at length in the *Tan-ch'ien* (= *Tan-ch'ien hsü-lu*), and has marvelled at it, saying that it occurred in Chan-i chou in Yün-nan (Chan-i chou, now Chan-i hsien, is somewhat north-east of Ch'ü-ching in Yün-nan; but YANG Shên really refers to A-mi chou, much more to the south [cf. *supra*], and it is A-mi chou which is correctly given here by the *T'u-shu chi-ch'êng*), and that he had heard that in the Ling (= Wu-ling mountains) and in Kuang[-tung] (but in fact YANG's text gives 'Ling-nan', not 'Ling Kuang') it was extremely abundant. [This shows that] he did not know that the *Hui-an chih* ('Description of Hui-an'; Hui-an is a *hsien* east of Ch'üan-chou in Fu-chien) had already mentioned that tree and called it 攀桂花 *p'an-kuei hua* ('flower of grasped cinnamon' ?); Yang says *pan-chih hua*; with the *p'an-chih hua* used in Wu (= Chiang-su), it makes three names for one and the same product. The quality (*p'in*) of the flower (of the *p'an-chih hua*) is inferior to that of the *mien-hua* (true cotton), and it is only good for stuffing mattresses » (this last sentence is omitted in the *T'u-shu chi-ch'êng*). On the question of names, I rather think that *p'an-kuei hua* is a graphic corruption of *p'an-chih hua* and that we have only two forms. What is more important is the astonishment of the Chiang-su man, who in his own province knew only *Gossypium herbaceum*, when he saw in Fu-chien the *Gossypium arboreum* for the first time. Moreover, his text leaves no doubt that, in his time and his province of Chiang-su, *mu-mien* and *p'an-chih hua* were synonymous and had become designations no longer of the cotton tree, *Gossypium arboreum*, but of the silk-cotton tree, *Bombax malabaricum*.

The *Wu-Hsün tsa-p'ei* (cf. *supra*, p. 438) also speaks of the *p'an-chih hua* (cf. *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *tsa-lu*, 2 a) : « Our 松 Sung (= Sung-chiang; the author was a native of the region of Shanghai) covers the world with its garments of cotton cloth (*mien-pu*), but the beginnings of the introduction of cotton (*mien-hua*) [into China] are not known. The tradition is that the plant came from the Hsi-Fan (a term loosely used in late Ming times for the 'Western Countries' of earlier and later texts) and for the first time entered China under the Yüan . . . » The author goes on with quoting in full a long text on cotton cultivation which he attributes to Shih Chao, who lived c. 1100, and concludes : « What Shih Chao speaks of is, without any doubt, cotton (*mien-hua*); the only thing is that now the bow to card cotton is made of wood and more than six feet long (the text attributed to Shih Chao spoke of a bamboo bow one foot and four or five inches long), which merely makes a small difference with the ancient [practice]; so it is wrong to say that [cotton cultivation] began under the Mongols (as will be shown in the last part of the present memoir, the text attributed to Shih Chao has nothing to do with him, and dates from the end of the 13th cent.). But Shih Chao speaks of the plant as *mu-mien*, which is not correct. The *mu-mien* was produced in Chiao[-chou] and Kuang[-chou]; this tree was an armful in girth;

its fruit was like a wine cup; in the 'mouth' of the [fruit] there was floss with which one could make cloth; see Chang Po's *Wu lu*. [The *mu-mien*] is the *pan-chih hua* of our days; YANG Yung-hsiu (= YANG Shên) has discussed [the question], and he was right.» So, for the author of the *Wu-Hsün tsa-p'ei* as for YANG Shên, WANG Shih-mou and LI Shih-chên, the *mu-mien* was *Bombax malabaricum*. At the same time, we can see from his text that, in his time, *mu-mien* was a more or less obsolete term, for the meaning of which he had to fall back on the ancient description given in Chang Po's *Wu lu*. But he was mistaken in speaking of the «armful in girth», the only feature which could not be applied to *Gossypium arboreum*, since there is no such sentence in the *Wu lu*. Above all, he has neglected an important point: cloth was woven with *mu-mien*, and as WANG Shih-mou justly remarked after other writers, the floss of the *Bombax*, our kapok, can only be used for stuffing mattresses; moreover, the author of the *Wu-Hsün tsa-p'ei* almost says so himself in another paragraph.

It is in the light of what has just been said that I am tempted to interpret this other paragraph on *mu-mien* of the *Wu-hsün tsa-p'ei* (cf. *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *hui-k'ao*, 2a): «*Mu-mien* is also called 瓊枝 *Ch'üing-chih*, 'Precious branch'. It is several ten-foot measures high. The tree [itself] resembles *wu-t'ung*; the leaves are like [those of] the peach-tree, but somewhat larger; the flowers are of a deep red colour and like the camelia (*shan-ch'a*). In spring and summer, the flowers open and cover the tree, which from afar looks like a gay brocade. When the flowers wither, they form a fruit (*tzü*) which is like a wine-cup; floss spouts out of its orifice (*k'ou*, lit. 'mouth'), abundant like a fine down. Anciently it was said that the Barbarians (Man) of Hai-nan wove [*mu-mien*] into a cloth which was called *chi-pei*; now [*mu-mien*] is used only to stuff mattresses, because it is soft and warm, and nobody employs it to make cloth. In Hsün[-chou] and Wu[-chou] (both in Kuang-hsi), there is also *mu-mien*; but local people do not collect [the floss], which is merely blown about and made to fall by the wind.» This is the only text in which I have found *ch'üing-chih* as an equivalent of *mu-mien*. This fancy name, taken from the fabulous *ch'üing-chih* tree in *Chuang-tzü*, has also been given to «coral», and *ch'üing-chih* [菜] *ts'ai*, «*ch'üing-chih* vegetable», is one of the names of the seaweed which we know as agar-agar (cf. *HR*, 176, 186). It does not seem to have ever been a true equivalent of *mu-mien*, except perhaps locally, among scholars, and perhaps for a short period. But, in view of the other text of the *Wu-Hsün tsa-p'ei*, it is just possible that *ch'üing-chih* was a misreading for *p'an-chih* [*hua*], this misreading being due to the attraction of *Chuang-tzü*'s *ch'üing-chih*. What the author gives is a description of the *Bombax malabaricum*, which we know from the other text to have been familiar to him as *pan-chih hua*. As to *mu-mien*, it would come in only as an antiquarian and scholarly — but mistakenly scholarly — equivalent of *pan-chih hua*.

In 1583, the Burmese army was defeated by the Chinese generals Liu T'ing and Têng Tzü-lung at a place called P'an-chih-hua (south-east of Yung-ch'ang in Yün-nan; cf. *Ming shih*, 315, 3b). Another P'an-chih-hua still exists at the extreme south-eastern corner of Yün-nan province. It is certain that these place-names were given under the Ming, probably by Chinese officials who were not natives of Yün-nan, on account of the presence of *Bombax* trees. Moreover, in Têng Tzü-lung's biography (*Ming shih*, 247, 10a), the battle of 1583 is located not «at P'an-chih-hua», but «under *p'an-chih* trees» 攀枝樹下, which proves the equivalence.

The texts already quoted show that the *p'an-chih hua* grew in the south-eastern (cf. also *Tien hsi*, 4, 26 a) and south-western parts of Yün-nan; a last text shows it in the northernmost districts of the province. It occurs in the *Ming i-t'ung chih* (87, 23 a), where it is said that the *p'an-chih hua* grows in Pei-shêng chou, i. e. the modern Yung-pei, north of the Yang-tzū and east of Li-chiang. The text adds that it looks like cotton (*mien-hua*), that cushions can be made of it, and that «it can also be used to make cloth»; I think that the last words rest on a confusion with *Gossypium arboreum*.

The same confusion occurs in other works. In the *Chao-ch'ing fu chih*, completed in 1830-1833 (re-edition of 1876; COURANT, *Catalogue*, No. 1758; 3, 43 a), there is a notice on *mu-mien*, beginning as follows: «It is also called *p'an-chih hua*. *Chi-pei* sprouts are grafted on a 烏柏 *wu-po* root (*kên*; here = trunk), and they produce flowers which give cotton (棉 *mien*).» I do not know any such term as *wu-po*, and I think that it is a misprint for 烏柏 *wu-chiu*, the tallow-tree, *Stillingia sebifera*, which is given in the quotation of the present passage made at an earlier date in *Kuei-ssü lei-kao*, 14, 5 a. The author of the latter work mentions this as an example of a «cotton plant» which becomes a «cotton tree». I have found no other allusion to this grafting process, which to a layman seems rather extraordinary. But the main point is that the notice of the *mu-mien* or *p'an-chih hua* proceeds with quotations from an older redaction of the *Chao-ch'ing fu chih* and from the *Kuang-tung t'ung-chih* which indubitably refer in principle to the *Bombax* and yet mix it up with the true cotton plant. From the text, it is clear that one of the sources, perhaps ultimately going back to the *Ming i-t'ung chih*, had said that the floss of the *mu-mien* could be used not only to stuff cushions, but also to make cloth; this is why the text adds: «On inquiry from Cantonese people, there is nobody who weaves it.» A similar remark occurs in the 植物名實圖考 *Chih-wu ming-shih t'u-k'ao* (completed in 1848; 30, 3-4), the author of which, while reproaching LI Shih-chên with having confused *mien-hua* (*Gossypium*) with *mu-mien* (*Bombax*), gives under *mu-mien* quotations which can only refer to tree cotton.

YÜEH-NO. — Of the terms dealt with by HIRTH and ROCKHILL (*HR*, 220) as probable designations of «cotton», only one remains to be examined, 越諾 *yüeh-no* (\**j<sub>k</sub><sup>w</sup>at-nâk*), which they thought was perhaps the name of a country in T'ang times, and later the name of «a light cotton gauze, or muslin». *Yüeh-no* was never the name of a country, but always the designation of a textile (cf. *BEFEO*, IV, 483). The term first occurs in the *Sui shu*, 83, 7 b (in the notice on Persia), and remained in use until the end of the Sung; LAUFER has discussed it in his *Sino-Iranica*, 493-496. But while stating quite correctly that the normal restitution of *yüeh-no* would be \**varnak*, LAUFER sought in it a double designation, *yüeh-* representing Pers. *barnū*, or *vālā*, or *bālās*, all names of textiles, and *-no* being the Pers. *naḥ*, Polo's «nac» (*q. v.*), a sort of brocade. There is a fundamental error in this theory: *yuēh-no* clearly renders one word, not two. On the other hand, the fact that *yuēh-no* first occurs in a notice on Persia is a mere accident (I must admit, however, that most mentions occur in connection with Persia and Mussulman countries west of India), and we have to deal with a trade-name then current in Central Asia and the Indian Sea. In my opinion, the simplest solution would be to regard *yüeh-no* (\**j<sub>k</sub><sup>w</sup>at-nâk*) as representing some Prākṛit form of Skr. *varṇakā*, a term derived from *varṇā* (usually «colour») and



known as the designation of a textile. *Varṇakambala* (formed with *varṇa*, «colour» and *kambala*, «woollen blanket») renders Ch. 氍毹 前 *ch'ü-shu* (or *ch'ü-yü*), a sort of woollen rug, in BACCHI, *Deux lexiques sanskrit-chinois*, 195, 326 (the Sanskrit characters give \**varṇakamula*, which the editor has corrected to \**varṇakamūla*; this would make the term consist precisely of *varṇaka* + *mūla*; but the Sanskrit characters are an early addition, not always correct, and the Chinese transcription, in my opinion, leaves no doubt that *varṇakambala* is meant, as is moreover supposed by the Japanese phonetic transcription added to the right; cf. also my remarks in my review of LÜDERS's *Textilien*, in *Oriental. Literaturzeitung*, 1938, No. 3, 184). But, if *yüeh-no* be *varṇakā*, I am at a loss to tell what sort of fabric was known under that trade name.

THE INTRODUCTION OF COTTON CULTIVATION INTO CHINA. — The history of this introduction has been traced in the 18th century by the Jesuit CIBOT, in *Mémoires concernant les Chinois*, II, 602-622; CIBOT's memoir is interesting for modern times, and clearly marks the difference between the «cotton tree», thriving in the south, and the «cotton plant», better fitted for more northern conditions, but it is more or less romantic for the earlier period; it may be responsible for some of the statements made in EDKINS's *Modern China*. The now prevalent view, as expressed in HIRTH and ROCKHILL's *Chau Ju-kua* (HR, 219) and in COULING's *Encyclopaedia Sinica* (I, 134), is that the introduction of cotton cultivation and spinning into China proper, and more precisely into Chiang-su, took place in the 14th cent., and was due to a woman from Hai-nan called 黃道婆 Huang Tao-p'o; GILES (*Biogr. Dict.* No. 870), following Wells WILLIAMS (*The Middle Kingdom*, II, 37) and MAYERS (*Chinese Reader's Manual*, p. 71), adds that cotton itself had been «introduced from Turkestan», which will be shown farther on to be an error.

The source of the information about «Huang Tao-p'o» is T'ao Tsung-i's *Cho-kêng lu*, dated 1366 (24, 12-13), and although T'ao's text, in an abbreviated form, has been made available in various works, for instance in the *Chinese Repository*, XIX, 458 (because of the quotation made from it in Hsü Kuang-ch'i's *Nung-chêng ch'üan-shu*, 35, 10 b-11), and in DORÉ, *Recherches sur les superstitions*, XI, 1040-1041, it is worthy of a complete and more accurate translation. The text says: «In Min (= Fu-chien) and Kuang (= Kuang-tung), many people plant (種 *chung*) cotton (*mu-mien*), and spin it to make cloth, which is called *chi-pei*. About 50 *li* east of Sung-chiang fu (south-west of Shanghai), there is what is called 烏泥涇 Wu-ni-ching ('Black mud Ching', 26 *li* south-west of Shanghai; cf. *Ti-ming ta tz'ü-tien*, 731). In that region, the soil is gravelly and poor, and does not yield grain for feeding the people; so they planned to plant trees in order to make a living out of it, and then they sought seeds (of *mu-mien*) there (*i. e.* in Fu-chien and Kuang-tung). They were absolutely without any such devices as 踏車 *t'a-ch'é* (another name for *chiao-ch'é*, or seed-cleansing stand, described in the *Nung shu*; cf. *supra*, p. 458) and 椎弓 *ch'ui-kung* (lit. 'hammer bow', another name for the 彈弓 *t'an-kung*, 'carding bow', described and depicted in *Nung shu*, 21, 17 a); they opened and separated the seeds only by hand, and with a string cord and a bamboo bow, the floss was 'flocked' by being beaten on a table (the text in the *Chin-tai pi-shu* ed., 按問振掉, makes no sense, and I follow the reading adopted in the *Nung-chêng ch'üan-shu*, 案間振擗); it was a wearisome task. At the beginning of the [now reigning] dynasty, there was an old woman (媪 *yü*), whose name was Huang tao-p'o (*tao-p'o* is

not really a name, but means literally 'old taoist woman', with perhaps some implication of sorcery as in 道 姬 *tao-yü*; moreover, Huang *tao-p'o* is sometimes called 'Huang *p'o*' only), who came from 崖 州 Yai-chou (or Ya-chou; in Hai-nan) and taught [the people] how to make the implements for cleansing, carding, spinning, and weaving [cotton]; rules were even laid down for mixing up reels (紗 *sha*) so as to combine colours and arrange threads for making designs (my translation is different from DORÉ's). So they wove blankets, cushions, waistbands, and handkerchiefs, on which twigs plucked off, round phoenixes, chess-boards, and characters shone out as if drawn by hand. When the people had been so instructed, they rivalled each other in making goods which they sold to other districts (*chün*). The families grew wealthy; but, after a short time, the old woman died. There was not one who was not grateful to her, and, shedding tears, all attended her burial. Moreover, they raised her a shrine, and sacrificed to her at the season each year. Thirty years later, the shrine was in ruins. A man of the village, 趙 愚 軒 Chao Yü-hsüan (this seems to be a *hao*, and to imply that the man was not a peasant), had it rebuilt; but now it has fallen into ruin a second time, and nobody has cared to rebuild it. Little by little, the name of the *tao-p'o* has faded away and sunk into oblivion.»

An author of the 14th cent., 王 逢 Wang Fêng (1319-1388), who, after the fall of the Yüan dynasty in 1368, lived in retirement in the very village of Wu-ni-ching, has devoted to Huang *tao-p'o* a poem which has been preserved in his literary collection entitled 梧 溪 集 *Wu-ch'i chi*. It does not add anything of importance to T'ao Tsung-i's account, except the indication that the shrine erected by Chao was burnt down by soldiers and rebuilt by a Mr 張 Chang, whom the authors of the *Sung-chiang fu chih* (18, 16 b) have been at a loss to identify.

According to the *Kuei-ssü lei-kao*, 14, 6 b, the *Chiang-nan t'ung-chih*, in the section on «altars and temples» (*t'an-miao*), devotes a notice to the shrine of Huang *tao-p'o*; but there is no *t'an-miao* section in the *Chiang-nan t'ung-chih* of 1684, the only edition to which I have access, and the section on «shrines» says nothing of the lady. Whatever its origin, here is the notice as given in the *Kuei-ssü lei-kao*: «The shrine of Huang *tao-p'o* was formerly at Wu-ni-ching of Shang-hai. Under T'ien-ch'i of the Ming (1621-1627), 張 所 望 CHANG So-wang (cf. *infra*, p. 438) removed it to 張 家 浜 Chang-chia-ping ('Creek of the Chang family'); 張 之 象 CHANG Chih-hsiang (1496-1577) wrote an account [of it] which said: 'In the *yüan-chêng* period of the Yüan (1295-1296), a person who had first migrated to Yai-chou came back to Wu-ni-ching, and taught the methods of sowing and planting, cleansing and carding, spinning and weaving, arranging the threads and sorting the colours, which [were in use] in Min (= Fu-chien) and Kuang (= Kuang-tung); on that account people raised a shrine to that [person]'.» The chronological order here given for CHANG So-wang and CHANG Chih-hsiang is in fact erroneous and must be reversed. As to CHANG Chih-hsiang's text, it is an adaptation, which is not always accurate, from the *Cho-kêng lu*.

About 1700, 高 不 蹇 Kao Pu-chien wrote a poem entitled «Poem on a night visit to the old site of the shrine of Mother Huang (Huang *mu*) at Wu[ni]-ching» (cf. *Sung-chiang fu chih*, 18, 16 b), which begins: «*Chi-pei* was a product of the southern Barbarians (Man); now it is much planted in this region. By weaving it one helps the 'young children' (= the people); the process has been learnt from Huang *p'o*.»

The *Sung-chiang fu chih* compiled under Chia-ch'ing (1796-1820; 18, 15-16; cf. DORÉ, *loc. cit.*) has a long paragraph on Huang *tao-p'o* and her shrine; it reproduces both the inscription of CHANG Chih-hsiang and that of CHANG So-wang. Chang Chih-hsiang's text expressly says that Huang *tao-p'o* was a native of the 鎮 *chên* to which Wu-ni-ching belonged, and that she came back in 1295-1296; but it adds that she «carried [back] cotton of Min (= Fu-chien) and Kuang (= Kuang-tung) and sowed it», which is a very loose and almost misleading way of speaking of Yai-chou in Hai-nan, which formed part of Kuang-tung. According to the *Sung-chiang fu chih*, the man who first built the shrine was the village headman 趙如珪 Chao Ju-kuei. After it had been destroyed by soldiers under the Yüan dynasty, it was rebuilt in 1465-1487 by the district magistrate 劉琬 Liu Wan. In 1573-1619 (in fact, before 1577), CHANG Chih-hsiang transferred it to Chang-chia-ping; less than thirty years later, this too was in ruins, and in 1626 Chang So-wang erected an independent shrine (*pieh-tz'ü*) west of the Ning-kuo-ssü. Another «independent shrine», at 梅溪街 Mei-ch'i-hung, in Shanghai itself, and south-west of the seat of the Shanghai magistrate, is the one spoken of in the *Mu-mien p'u* (15*b*); it may originally have been dedicated to another nun with the Huang surname; it was restored in 1784, and expressly dedicated to Huang *tao-p'o* in 1813.

A scholar, native of the Tui-shan, in the district of Shanghai, 毛祥麟 MAO Hsiang-lin, published in 1870 a miscellaneous work in 4 chs., entitled 對山書屋墨餘錄 *Tui-shan-shu-wu mo-yü lu*, in which a paragraph entitled «Huang *tao-p'o* tz'ü», «the Shrine of Huang *tao-p'o*», gives some additional information for more recent times (*Pi-chi hsiao-shuo ta-kuan* ed., 1st *chi*, 2, 21). According to MAO Hsiang-lin, the shrine at Mei-ch'i-hung in Shanghai was erected by the Shanghai people after the original shrine had been removed first to Chang-chia-ping and afterwards west of the Ning-kuo-ssü. The weavers of Shanghai used to assemble for a fair at the Mei-ch'i-hung shrine. Shortly after 1821, the provincial governor T'ao Chu restored it and enlarged its area with land which originally belonged to the family of MAO Hsiang-lin's grandmother. MAO Hsiang-lin gives details on the sights and the pavilions of this shrine, at which women used to come in crowds for theatricals every year in the fourth moon, on Huang *tao-p'o*'s birthday. But this beautiful site was laid waste in the T'ai-p'ing rebellion in 1853.

As may be seen from the above texts, the memory of Huang *tao-p'o*, this «public-spirited woman» as W. WILLIAMS calls her, has not sunk into such complete oblivion as T'ao Tsung-i's text would imply; DORÉ even reproduces a popular coloured picture of the old lady. But it may be that this popularity is a more or less artificial revival, of bookish origin. Late traditions which speak of Tämür's reign (1294-1307), or more precisely of 1295-1296, are not necessarily better informed than the almost contemporary T'ao Tsung-i, who merely says «at the beginning of the [now reigning] dynasty», and Chao Yü-hsüan who, according to T'ao Tsung-i, rebuilt the shrine, is very probably the same as Chao Ju-kuei who, according to the *Sung-chiang fu chih*, built it in the first instance. It is by no means certain either that there was any authoritative tradition about the birthday of Huang *tao-p'o*. Even if there may be some inaccuracies in T'ao's account, it is from it that we must start. From this account it emerges very clearly that cotton (*i. e.* true cotton) had been extensively grown in Fu-chien and Kuang-tung (of which Hai-nan always formed part) for an indeterminate length of time before the advent of the

Mongol dynasty. On the other hand, T'ao Tsung-i does not say that Huang *tao-p'ò* introduced cotton cultivation into Wu-ni-ching, but that, at the beginning of the Mongol dynasty, she taught the Wu-ni-ching cotton growers how to make the necessary gins for a simpler treatment of the floss and afterwards a better use of the threads. Interesting as it is, T'ao Tsung-i's text has no direct bearing on the introduction of cotton cultivation into China.

For a number of centuries, the opinion has been generally held by Chinese scholars that cotton was already known in almost prehistoric China, because the «Tribute of Yü», in the *Shu ching*, contains the following sentence 島夷卉服 *tao-i hui fu*, «the island barbarians [brought] garments of grass» (cf. LEGGE, *Chin. Classics*, III, 111); *hui*, «grass», would be a designation of various vegetable textiles, including «cotton» (*mu-mien*). This explanation originated with Ts'ai Ch'ên (1167-1230; cf. GILES, *Biogr. Dict.* No. 1968), whose famous commentary on the *Shu ching* was completed in 1210, and it has been accepted, with more or less qualification, by men like Ch'iu Hsün (1418-1495; cf. GILES, *Biogr. Dict.* No. 407) in his *Ta-hsüeh yen-i pu* (cf. *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, 6 b; *Chin. Repository*, XIX, 458), WANG Hsiang-chin (cf. *T'u-shu chi-ch'êng*, *ibid.*, *i-wên*, 2 a; on WANG Hsiang-chin, cf. *supra*, p. 438), T'AN Ch'ien (in his *Tsao-lin tsa-tsu*, *chung-chi*, 57 b), and even Hsü Kuang-ch'i (cf. *infra*, p. 488); the Emperor K'ang-hsi gave his sanction to it in the introduction to his «Ode on cotton», *Mu-mien fu* (reproduced in *Sung-chiang fu chih*, 6, 8 a). The sentence in question in the «Tribute of Yü» occurs in the section devoted to Yang-chou, *i. e.* the region of the lower Yang-tzû; *tao-i*, «island barbarians» is a vague term; at a later date, the Northern Wei used it as the designation of the dominions of the Liang, that is of the whole of south-eastern China. Of course there was no «cotton» in China before the Christian era; its occurrence in STEELE's *I li*, II, 94, is a bad anachronism. Ts'ai Ch'ên's explanation is untenable (just as much as his attempted connection between 貝 *pei*, which comes at the end of the sentence, with *chi-pei*, whereas this *pei* can only mean «shell», «cowry»), but its interest lies in the fact that, to have proposed it, Ts'ai Ch'ên must have been aware of the cultivation of cotton in some part of China. Now Ts'ai Ch'ên was a native of Chien-yang in Fu-chien, and we may conclude from his mistaken commentary that cotton was already largely cultivated in Fu-chien in 1210.

Other texts point also to times prior to the end of the 13th cent. (not to speak of the «14th» indicated by MAYERS, HIRTH and ROCKHILL, and COULING).

We must leave out the «*ta* (or *t'a*) cloth» of Han times, since we are not yet in a position to guarantee that it was cotton, and provisionally the slightly later *po-tieh* of the *Hua-yang kuo-chih* and the *Hou-Han shu*. But the question of the «*mu-mien tree*» mentioned by the two parallel passages of Chang Po's *Wu lu* as growing in Tongking and western Yün-nan, can be settled. LI Shih-chên had placed them in the same category as the «*p'an-chih hua*», which in his time was indubitably *Bombax*; Hsü Kuang-ch'i opposed him with very sound reasons in his *Nung-chêng ch'üan-shu* (35, 2).

I have already referred to the *Nung-chêng ch'üan-shu*, without however saying anything definite on the history of this agricultural encyclopaedia, the history of which is far from being satisfactorily explained. The date «1619» given by LAUFER, *Sino-Iranica*, 336, is an error or a misprint for «1639»; and the book was not printed «by Imperial command», as said by WYLIE

(Notes on Chinese literature, 76), whose 陳之龍 CH'ÊN Chih-lung, moreover, is a mistake for the name of the well-known Sung-chiang author 陳子龍 CH'ÊN Tzū-lung (he is the man called «a certain Tsz' lung» in BRETSCHNEIDER, *Botanicon Sinicum*, I, 83). There are two states of the *Nung-chêng ch'üan-shu*, one in 60, the other in 46 chs.; but, if we had only the notices in the *Ssü-k'u* . . . , 102, 8-9 and 15-16, we could not suspect that both were posthumous compilations, and that CH'ÊN Tzū-lung was to a great extent responsible for both. As a matter of fact, CH'ÊN Tzū-lung acquired HSÜ's mss. from his heirs, and in 1639, with the co-operation of 張國維 CHANG Kuo-wei and 方岳貢 FANG Yo-kung, arranged and printed them in 60 chs.; at a later date, he submitted the whole to a fresh revision, and produced the work in 46 chs., but the Ch'ien-lung Commissioners knew it only from a ms. copy, and I am not certain that it was ever printed; all the known copies of the *Nung-chêng ch'üan-shu* seem to represent the work in 60 chs. (cf. COURANT, *Catalogue*, Nos. 5369-5392), the preliminaries (*fan-li*) of which are signed by CH'ÊN Tzū-lung. It is difficult to say to what extent these 60 chs. have been edited by CHANG, FANG, and CH'ÊN. There is no doubt that several chapters, for instance chs. 19 and 20 on European hydraulics, had already been published separately in HSÜ Kuang-chi's lifetime, *i. e.* before 1633. Such must also have been the case for [part at least of the chapter on cotton, since HSÜ's «report to the throne» (*shu*) on the cultivation of cotton was already known to WANG Hsiang-chin, whose *Ch'ün-fang p'u* was published in 1630 (cf. the preface to the *Mien p'u* section; cf. *supra*, p. 438); probably it was included in the 農漬雜疏 *Nung-i tsa shu*, «Various reports on agriculture», in 5 chs., which are mentioned separately in the *Ming shih*, 98, 3a. On the other hand, I doubt whether the *Ming shih* is right when it registers separately a *Nung-chêng ch'üan-shu* in 60 chs. as the work of HSÜ Kuang-ch'i, and another in 8 chs. as that of CHANG Kuo-wei. In discussing the statements of earlier authors, HSÜ's remarks are invariably introduced with the words 玄扈先生曰, «Master Hsüan-hu says»; Hsüan-hu (not «元扈 Yüan-hu», as given by BRETSCHNEIDER on account of a taboo) is HSÜ's *hao*, and it may be that this designation was not used by HSÜ Kuang-ch'i himself, but added by the editors; at any rate, HSÜ could not have used it in a report to the throne. [So in *Pên-ts'ao kang-mu*, another posthumous work, the author's comments begin with 時珍曰 «Shih-chên says».] Most of the section on cotton has been translated, fairly accurately, by C. SHAW in the *Chinese Repository*, XVIII (not «XIV» as in BRETSCHNEIDER) [1849], 449-469, but SHAW did not always distinguish between quotations from earlier works and original remarks, nor was he aware that «Hsüan-hu» was HSÜ Kuang-ch'i himself.

Here is HSÜ's refutation of LI Shih-chên: [«The name *chi-pei* is first found in the *Nan-shih* (this is a double error; *chi-pei* was known before the *Nan-shih*, and the *Nan-shih* always gives *ku-pei*, except in one passage which is not the one alluded to by HSÜ Kuang-ch'i; cf. *supra*, p. 440), and has been transmitted down to our days. The meaning is not known, but I suppose it is a foreign word; what the popular writers call *mu-mien*. The fabrics which are made, called *ch'êng*, *wên-ju*, *wu-lin*, *pan-pu* (on all these names cf. *supra*, p. 457), *po-tieh*, 縹 *hsieh* (but with the value of *tieh*; cf. *supra*, p. 449), and *ch'ü-shun* (cf. *supra*, p. 465) are all this. It is certainly a plant; and if the *wu-lu* calls it *mu-mien* (*tree-floss*), it is because in the south the land is warm so that once sown it flowers and bears fruit for a number of years afterwards, just like the tree

hibiscus (木芙蓉 *mu fu-jung*, *hibiscus mutabilis*). It is different from the annual sowing in China, and so when he says that it lives for ten or more years it is clearly not a tree (cf. *Ciba Review*, 95, Dec. 1952, p. 340, where it is said that the cotton plant, indigenous in the tropics where it is perennial, is killed by frost and has become an annual in « the chief cotton-growing areas »). If *chi-pei* is called a tree, it is just as the *Yü-kung* uses the word herb (卉 *hui*; cf. *supra*, p. 487; the *hui* of the *Yü-kung* certainly does not refer to cotton), a word chosen to distinguish it from silkworm floss. If it is not called *mu-mien* in Min (Fu-chien) and Kuang[-tung], that is because there they call the *p'an-chih-hua* (*bombax*, cf. *supra*, p. 480) *mu-mien*. The *p'an-chih-hua* can be used to make mattresses and cushions; but though it is soft and glossy it is not tough, and cannot possibly be spun. How can it be made into cloth? If one suspects that *mu-mien* is this (i. e. *bombax*), and says that it can be made into cloth but the method is forgotten, he is mistaken; the *mu-mien* of which the *Wu-lu* speaks is precisely *chi-pei*. If one supposes that because it says that the tree is ten feet high it must be the *p'an-chih*, he forgets that the *p'an-chih* is more than 100 feet high. Since the *chi-pei* of the south will live for several years, that it should be more than ten feet high is after all no reason for surprise. So the *chi-pei* of Lin-i spoken of in the *Nan-shih* and the *mu-mien* of Yung-ch'ang in the *Wu-lu* both mean herbaceous *mu-mien* which will make cloth. I suppose that they are the *so-lo* tree (cf. p. 470 sqq.), but absolutely unrelated to the *p'an-chih-hua*. Moreover the cotton cloth (*mien pu*) woven in China differs from the Indian muslin (西洋布 *Hsi-yang pu*; cf. *TP*, 1933, 328) in fineness and is not brilliant at all. But when I saw the robe handed down from Shih Hui-nêng of Ts'ao-ch'i (曹溪釋惠能; 陸 Lu Hui-nêng, 638-713, was one of the Patriarchs of the school of Bodhidharma), said to be of *ch'ü-shun* cloth, that is to say *po-tieh* cloth, and described as woven from the heart (心 *hsin*) of the *mu-mien* of the western countries, it looked as glossy as threads of silkworm silk. Was this the *so-lo lung tuan* (娑羅籠段, « *so-lo* covering damask »)? Or are there still other sorts of *chi-pei* in the western lands? Moreover I had suspected that the fineness of the foreign cloth (洋布 *yang pu*) could not have been made from the *chi-pei* of our country; and when I saw *chi-pei* from Bengal, that the seeds were extremely small and the floss extremely soft, quite different from the Chinese sort, then I knew that the *chi-pei* hitherto transmitted was not the best.»<sup>(1)</sup>

This is a remarkable text, and I can only concur with almost every point of Hsü's argument. It is evident that a tree which is but ten feet high cannot be the *Bombax*, a great forest tree. Moreover, as Hsü says, the very mention that the *mu-mien* of the *Wu lu* could last ten years without being sowed again precludes the possibility that this *mu-mien* should be a long-lived tree like the *Bombax*. Without the slightest hesitation, we must conclude that it was a variety of *Gossypium arboreum*. The particulars given are in striking agreement with Polo's statements when the traveller describes in Guzerat « trees which make cotton, . . . six paces high . . . and these have quite twenty years » (cf. Vol. I, 420). Moreover YULE (*Y*, II, 394) has quoted extracts from Mohammedan authors who mention in India « cotton plants » which « grow as large as trees » and « yield produce ten years running ». Another reference may be added, because it happens to give the same number of years as Polo : « Some Arabs of the tribe of Kälb, informed me

<sup>(1)</sup> Paragraphe écrit à nouveau par A. C. MOULE.

(= Abū Ḥanifa) that, in their place, cotton grew so as to reach the height of an apricot tree, and lasted twenty years»; Abū Ḥanifa continues by saying that « the best (cotton) was the one which was recent and had been sowed in the course of the year » (cf. LECLERC, *Traité des simples*, III, 92-93). Polo says the same.

So we know from the *Wu lu* that a true cotton, the *Gossypium arboreum*, was cultivated in Yung-ch'ang in the last quarter of the 3rd cent. Now, it is precisely in the region of Yung-ch'ang that the *Hua-yang kuo-chih* and the *Hou-Han shu* mention the *po-tieh* cloth of the Ai-lao. The *Hua-yang kuo-chih* is slightly later in date than the *Wu lu*, so that the interpretation of *po-tieh* as « white [true] cotton cloth » would make no difficulty. As to the *Hou-Han shu*, it is true that, in principle, it deals with a period earlier than the *Wu lu*; but we have seen that, in the present case, it seems in fact to be indebted to the *Hua-yang kuo-chih*. Even if it were not so and if both works had copied from a lost and unknown source of the Han period, we should have to go back only to the 2nd cent. A. D., one century before the *Wu lu*, and there is no reason to suppose that the cultivation of cotton had just begun in western Yün-nan when the *Wu lu* was written. I have long thought of devoting a special memoir to the route between Burma and Yün-nan, a route which was already known to Greek geographers and which has been of much greater moment in the history of civilization than is generally believed. The early introduction of cotton cultivation into western Yün-nan is but a link in a long chain.

From the *Wu lu* again, we know that, in the 3rd cent., true cotton, represented by the *Gossypium arboreum*, was cultivated not only in western Yün-nan, but also in North Annam and Tongking. Both regions formed part of the Chinese Empire, but they were border provinces, and cotton was still unknown in China proper, except as an imported ware. The same location of *mu-mien* in western Yün-nan and Tongking occurs in the extant fragments of the *Kuang chih* (cf. *supra*, p. 462). In the 4th cent., the *Lo-fou shan chi*, devoted in theory to what was to be found on that mountain of Kuang-tung, speaks of *mu-mien*, which, in the first month, produced flowers similar to those of the *fu-jung* (« nelumbium », or « hibiscus »; cf. *supra*, p. 461), and a floss out of which Southerners made cotton wool. The early flowering and the fact that the text speaks of the floss as being used not for making cloth, but for stuffing, would favour the hypothesis that we find already in this early text the confusion, well attested in Ming times, which transferred the designation *mu-mien* from the *Gossypium* to the *Bombax*. The same doubt may be entertained as to the value of *mu-mien* in a passage of the *Nan-yüeh chih* (cf. *supra*, p. 462) and in an anonymous *Kuang-chou chi*, probably pre-T'ang in date, which says that the *mu-mien* occurs in Tongking and Kuang-tung (cf. *supra*, p. 462). But the fact remains that, already before the T'ang dynasty, there was some *mu-mien* growing in Kuang-tung; and we must not forget that Hai-nan was part of Kuang-tung province.

That *Gossypium arboreum*, and not *Gossypium herbaceum*, was the species mainly grown in eastern Indo-China in the 6th cent. seems to be established by the description given in *Liang shu*, 54, 1 b (copied also into *Nan shih*, 78, 1 b). The text speaks of the products of Lin-i (= Champa), which included *chi-pei* (*ku-pei* in *Nan shih*), and adds : « *Chi-pei* is the name of a tree (*shu*; not *ts'ao*, 'plant'). When its flowers are completely formed, they are like goose down. [The people] pull out the filaments (緒 *hsü*) and spin them so as to make a cloth which

is pure and white, not different from the cloth made with ramie (= 'grass cloth'). It is also dyed in different colours and [then] woven to make 'variegated cloth' (*pan-pu*).

Even during the T'ang dynasty, we have no texts referring to cotton cultivation in China proper. The authors of the Buddhist *yin-i* repeatedly say that *tieh* is a cloth made of *mu-mien*, which is a « plant » (*ts'ao*) of the Western countries. In the middle of the 7th cent., Hsüan-ying knew that, in Chi-pin (probably still Kashmir for him, not Kāpiśī) and south of it, that plant grew to become a tree (ch. 1; *Tripit.*, 爲, VII, 2 b). Hui-lin adds (ch. 68; *ibid.* ix, 182 a) that « now, Chiao-chih in the south (= Tongking) has it too »; he does not even speak of Kuang-tung. In 736, Chang Shou-chieh declares unambiguously : « *Po-tieh* is woven from *mu-mien* (cotton), which does not exist in China » (cf. *supra*, p. 446). The *Tz'ü yüan* (*s. v. po-tieh*) says that cotton first « entered » China under the T'ang dynasty; this can only refer to China proper, excluding Yün-nan and Hai-nan; but such a date is too late for cotton importation, and too early for cotton cultivation.

This name *po-tieh*, which goes back almost to Han times and is regularly used in Buddhist translations, became specially familiar in mediaeval China, from the beginning of the 6th cent., on account of the cotton which was grown in the Turfan region, which was called *tieh* or *po-tieh* by the local Chinese colonists. But even then no cotton cultivation can be traced which might have passed from the Turfan region into Kan-su, Shan-hsi, or Shàn-hsi. The *Hsin T'ang shu* (40, 8 b; the quotation in *K'ang-hsi tzü-tien*, *s. v. tieh*, is inaccurate) mentions « *tieh* cloth », as an article of tribute, only for Hsi-chou, *i. e.* for the Turfan region; from the *Yüan-ho chün-hsien t'u-chih* (40, 3 a, where *tieh*-[毛] *mao* is a wrong reading for *tieh pu* or *tieh-mao pu*), we know that this item figured among the articles of tribute required from Hsi-chou in the statutes laid down in 713-741 (the *P'ei-wên yün-fu*, *s. v. po-tieh*, gives a similar sentence as coming from the 唐六典 *T'ang liu-tien*; this would also take us back to the first half of the 8th cent.; but I have not found any such passage in the *T'ang liu-tien*, and the title may be misquoted).

The date at which cotton cultivation began in Chinese Turkestan is a most difficult problem. After mentioning that cotton was grown in the Turfan region in the first half of the 6th cent., HIRTH and ROCKHILL add (*HR*, 218) that « its use was not so general in Turkestan in the sixth century but that we find in Yen-ki [= Yen-ch'i, Qarāšahr] . . . the people using silk cocoons as wadding for clothes »; and they give a reference to the *Wei shu*, 102, 3 b. The original text is not very satisfactory, because it says that the people of Yen-ch'i used to « breed » silkworms, without making silk threads with the cocoons; this would rather suggest wild silkworms. What is more important is that the whole paragraph bears not on the 6th cent., but on the second quarter of the 5th cent., when it is quite possible that cotton was not yet cultivated even in Turfan.

I have said above that, according to LÜDERS, no name of a cotton textile occurs in the Kharoṣṭhī documents of the first centuries of our era discovered in Chinese Turkestan, but that I should have to discuss this point (cf. *supra*, p. 433). Among the textiles of Ta-Ch'in mentioned in the *Wei lio*, one is called « 溫色 Wên-sê cloth (*pu*) »; cf. HIRTH, *China and the Roman Orient*, 74, 113. But, instead of Wên-sê, the *T'ai-p'ing yü-lan* (820, 19 a) gives 溫宿 Wên-su, also adopted in the Ch'ien-lung edition of the *Wei lio* (*San-kuo chih*, 30, 13 a), and this is



probably the proper reading, the *sê* being due to the contamination of the next name, in which *sê* occurs. Now, Wên-su is the ancient name of Uč-Turfan (north-west of Aqsu), and it may be that the cloth manufactured in the Mediterranean Orient was called « Wên-su cloth » by analogy with the one really coming from Wên-su. The *T'ai-p'ing yü-lan* readings are not to be neglected; for instance, for the name preceding that of « Wên-sê cloth » or « Wên-su cloth », our text of the *Wei lio* gives « 度代 *tu-tai* cloth » (or, in the Ch'ien-lung edition, 度伐 *tu-fa*), but, in the *T'ai-p'ing yü-lan*, it is « 鹿代 *lu-tai* cloth », and *lu-tai* (\**luk-d'âi*) closely resembles the red brocade *lohtai* (or *luhtai*) of Kāšyari (cf. *Brockelmann*, 119, where SCHINDLER's restoration into 絡帶 *lo-tai*, \**lâk-tâi*, is valueless; *lo-tai* exists, but its meaning is quite different; cf. *TP*, 1929, 144). But, even if we read « Wên-su cloth » instead of « Wên-sê cloth », that would only bear evidence to the early renown of the textiles of Chinese Turkestan, without providing any indication as to their nature, wool or cotton.

Another case is more intricate. One of the textiles most often mentioned in the Kharoṣṭhi documents is *koj'ava* or *košava*. LÜDERS (*Textilien im alten Turkistan*, 3-11) pronounced himself in favour of a Skr. original \**kaucapa*, itself of foreign origin, and rejected a possible, but difficult derivation from Kuči, the ancient name of Kučā; \**kaucapa* would be the designation of a woollen blanket or carpet. In my review of LÜDERS's paper (*Oriental. Literaturzeitung*, 1938, 184-185), I suggested that Ch. 毳毼 *ch'ü-shu* (\**k'iu-šizu* or \**g'iu-šizu*) might be a transcription of the same word (Hui-lin [爲, VIII, 93 b] says that *ch'ü-shu* was a foreign word of Central Asia [胡 Hu], and that the fabric was popularly known as 毛錦 *mao-chin*, « woollen brocade »), and remarked that the derivative form of \**kaucapa*, *kaucapaka*, occurred in ch. 44 of the *Vinaya* of the Mūlasarvāstivādin translated by I-ching, but in a form 孤咕薄迦 *ku-ch'ê-po-chia* which would suppose \**kochavaka*. I added that I-ching, however, was not a very strict phonetician in his transcriptions. As a matter of fact, I have since found, in another *Vinaya* translation by I-ching (寒, v, 72 b), the same word transcribed 高禱婆 *kao-chê-p'o* (\**kâu-t'šjâp-b'uâ*), i. e. *kaucava*. A text of the *Kuang chih*, quoted by the *K'ang-hsi tzü-tien*, s. v. 遠 (equivalent of 毼 *shu*), seems to connect *ch'ü-shu* with *po-tieh*, i. e. in principle « cotton »; but the text is of doubtful origin, and need not detain us. More important is Hui-lin's gloss on I-ching's \**kochavaka* (*kaucapaka*), which is said to be 細妙好白疊布名, « the name of a fine good *po-tieh* cloth » (such is the text in 爲, IX, 156 a, and in *Tōkyō Tripit.* of Taishō, 54, 713<sup>3</sup>; the omission of *tieh* in SAKAKI's *Mahāvryutpatti*, No. 5861, is an error). Now, a note by I-ching himself, following his transcription *kao-chê-p'o* of *kaucava*, says that it was the name of a « carpet » (毯 *t'an*; I do not think that the variant 綵 *t'an* can be adopted here, except as a graphic equivalent of *t'an*, « carpet »). As a matter of fact, I-ching must have written on the authority of some dictionary; *kaucava* was the designation both of a blanket worn as a garment and of a carpet; in the text translated by I-ching, it could not be a carpet, since it was the first of the five garments allowed to the monks by the Buddha. But the word *t'an* used by I-ching implies that it was a woollen stuff. We should gain nothing by supposing that a confusion was made between \**kaucapa* (*kaucava*, \**kaucapaka*, etc.) and 高咕薄迦 *kao-t'an-po-chia* of Ch. 18 of the same *Vinaya* (張, VIII, 18 a), probably a transcription of \**kauṭumbaka* (cf. *Divyāvādāna*, 559<sup>10</sup> : *kauṭumba*; *Mahāvryutpatti*, No. 9163; *koṭambakam*; Pali *koṭumbara*), since the definition of \**kauṭumbaka*

in the Chinese text (上毛縑 *shang mao-t'an*, «superior woollen blanket [?]») would not refer to cotton either. On the other hand, Hui-lin, in all his glosses on *tieh* and *po-tieh*, maintained that both terms applied to a cotton, not a woollen, stuff. The natural conclusion is that he must have made a mistake in the present case, and that the *kaucava* was not a «*po-tieh* cloth», *i. e.* cotton stuff; we cannot deduce from his gloss that the *koj'ava* or *košava* of the Kharoṣṭhī documents bears evidence to the cultivation of cotton in Chinese Turkestan at the beginning of our era.

But there is another difficulty. In *TP*, 1923, 129, I have already alluded to another passage of Hui-lin, who, commenting on the name Pa-lu-chia, Bharuka (= Aqsu) of Hsüan-tsang, says (爲, x, 46 b) : «This country produces fine good *po-tieh* [and?] extremely fine woollen blankets (*mao-chi*), which are appreciated in the neighbouring kingdoms and in China; at the [present] time, people (lit. the people of the time) call them 'Mo-lu *tieh*'; in fact, they are woollen cloth. See what is said in the *K'uo-ti chih*» (此國出細好白氈上細毛屬爲隣國中華所重。時人號爲末祿疊。其實毛布也。見括地志說). When I briefly referred to this text in 1923, I accepted it at its face value, and supposed that 末祿 Mo-lu (\*Muât-luk) was another transcription of Bharuka; at the same time, I noted that Hsüan-tsang and the *Hsin T'ang shu* spoke of the excellent textiles of Bharuka, and that the *Hsin T'ang shu* mentioned a «small kingdom» of Mo-lu east of the Arabs (who had conquered Persia). But the question is not so simple, and we must go into it in greater detail.

The *K'uo-ti chih*, a comprehensive geographical work in 550 chs. and 5 chs. of prefaces and tables, was prepared by order, and published in 642 under supervision of a member of the Imperial House, 李泰 Li T'ai, prince of 魏 Wei (cf. *Hsin T'ang shu*, 80, 3 a-b; 58, 14 a). It has long been lost. The extant fragments were collected and published in 1797 by SUN Hsing-yen in the *Tai-nan-ko ts'ung-shu* (cf. CHAVANNES, *Mém. historiques*, I, ccxvi; *JA*, 1902, II, 144; *BEFEO*, IV, 131); there is no mention in them of Pa-lu-chia or Mo-lu (SUN Hsing-yen did not know Hui-lin's work, which was recovered from Corea only in the 19th cent.), so that we cannot say positively what elements in Hui-lin's gloss are traceable to the *K'uo-ti chih*. But we have some indirect means of reaching a solution.

In the *Hsin T'ang shu* (221 A, 9 a), we are told, among other things, that Pa-lu-chia (Bharuka), a «small kingdom», produced 細氈 襪 *hsi chan ho*, «fine felt and serge». The whole notice is taken in fact from Hsüan-tsang, who says (Kyōto University ed., I, 17) that Bharuka has «fine felt and fine serge, which are appreciated in the neighbouring kingdoms» (細氈細襪鄰國所重; instead of «felt», JULIEN, *Mém.*, I, 10, translates «cotton», and WATERS, *On Yuan Chwang's Travels*, I, 64, «cloth»; this is due to a misreading 氈 *tieh* in a late Ming edition, but all the ancient editions and mss. give 氈 *chan*, which is confirmed by the *Hsin T'ang shu*). Evidently we have here the source of part of Hui-lin's gloss, which is precisely a comment on Hsüan-tsang's text. On the other hand, it could not have occurred in the *K'uo-ti chih*, since the form Pa-lu-chia of the name of Aqsu is Hsüan-tsang's own transcription, and the pilgrim had not returned from India when the *K'uo-ti chih* was published. The mention of the woollen textiles of Pa-lu-chia having been taken by Hui-lin from Hsüan-tsang, his indebtedness to the *K'uo-ti chih* must be limited to the «fine good *po-tieh*», which were called «Mo-lu *tieh*».

In 1923, because of Hui-lin's remark that these «*Mo-lu tieh*» were «in fact woollen cloth», I referred to them as «*Mo-lu fine woollen cloth*». But it is clear that Hui-lin himself felt that there was there a contradiction in terms, since Hsüan-tsang spoke of woollen textiles, and he, Hui-lin, knew that *tieh* or *po-tieh* was cotton; hence his remark, which would imply that «*Mo-lu tieh*» was a misnomer. I think that the solution lies in another direction, and that the «*Mo-lu* (\*Muât-luk) *tieh*» have nothing to do with Bharuka (Aqsu); if Hui-lin brought them together, it was merely because he wrongly identified the two names on account of some phonetic analogy.

Already in Han times, the Chinese knew of the existence on the eastern part of An-hsi (= the Parthian Empire), of a city called 木鹿 *Mu-lu*, which has already been identified with Merw by HIRTH (*China and the Roman Orient*, 142-143) and VON GUTSCHMID (*Geschichte Irans*, 66); cf. CHAVANNES (*TP*, 1907, 177). LAUFER (*Sino-Iranica*, 187) objected that *Mu-lu* was \**Muk-luk*, and could not regularly represent Mouru, the form in which Merw occurs in the *Avesta*. But Mōuru, or Mōurv — rests on old-Persian Margav —, young-Avestik Maryav- (cf. BARTHOLOMAE, *Altiran. Wörterbuch*, 1147, 1169), to which we find corresponding forms in «*Margiana*» and in Armen. «*Marg*» (cf. HÜBSCHMANN, *Armen. Grammatik*, I, 51). The geographical equivalence *Mu-lu* = Merw can hardly be doubted, and, to make it phonetically acceptable, we have only to suppose that 木 *mu* (\**muk*) is corrupt for 末 *mo* (\**muât*), as already suggested by SCHLEGEL (*TP*, 1901, 6). This corruption seems to have occurred in pre-T'ang times since, when the old geographical names were artificially used in 656-660 for a new administrative organization of the Western countries, the region of Buḥara became the government of *Mu-lu* (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 138, 273, 347). Yet it may be that the *Mu-lu* form in the *Hsin T'ang shu* is itself not original. We read in the section devoted to the Arabs in the *Hsin T'ang shu* (221 B, 8 b) that «a man of 木鹿 *Mu-lu* in Ḥorasān, called 並波悉林 *Ping-po-hsi-lin*, rose against the Khalif Märwān (this passage had been misunderstood by BRETSCHEIDER, *On the knowledge possessed by the ancient Chinese of the Arabs*, 9). From the *T'ang hui-yao* (100, 12 b) and the *T'ai-p'ing huan-yü chi* (186, 13 b), we are certain that in this case the source of the *Hsin T'ang shu* is the lost 四夷述 *Ssü-i shu*, «Memoir on the four Barbarians (= on all foreign countries)», by 賈耽 *Chia Tan* (730-805). There can be no doubt that «*Ping-po-hsi-lin*», in which the first two characters are corrupt (the *T'ai-p'ing huan-yü chi*, 186, 14 a, gives Li[立]-*po-hsi-lin*, which is no better), transcribes the name of the well-known Abū Muslim. As to his place of origin, it is written 木麤 *Mu-ts'u* in the *T'ai-p'ing huan-yü chi* (186, 14 a), but 末麤 *Mo-ts'u* in the *T'ang hui-yao* (100, 13 a). It is quite certain that *ts'u* is corrupt for *lu* (the intermediary form being the 麤 variant of *ts'u*); but the *mo* reading of the *T'ang hui-yao* may be correct. Even if it is not, it shows how easily a confusion can take place between 木 *mu* and 末 *mo*.

But 木鹿 *Mu-lu*, or 末鹿 *Mo-lu*, is not the only transcription of the name of Merw in T'ang times. The *Hsin T'ang shu* (221 B, 8 b-9 a) has a notice on the «small kingdom» 末祿 *Mo-lu* (\**Muât-luk*), located east of the Ta-shih (Arabs), in which many people have the surname 木 *Mu* (cf. LAUFER, *Sino-Iranica*, 381, 399, 402, without identification). Here again, the source of the information is not doubtful; it is the 經行記 *Ching-hsing chi*, i. e. the account written by 杜環 *Tu Huan*, who had been made a prisoner by the Arabs at the battle of Talas in 751,

and came back to China in 752 (cf. CHAVANNES, *Doc. sur les Tou-kiue*, 298); important fragments of his account have survived because they have been quoted by his kinsman Tu Yu in the *T'ung tien*, completed in 801. But, in our texts of the *T'ung tien* (193, 9 a), the name of the kingdom of Mo-lu, the notice of which is much more detailed than in the *Hsin T'ang shu*, has been altered to 朱祿 Chu-lu, and the surname of the local people occurs as 朱 Chu; the *T'ung chih*, 196, 32 a, gives Mo-lu and Mo; the *Wên-hsien t'ung-k'ao*, 339, 7 a, 米祿 Mi-lu and Mi, respectively. The true form of the «surname» occurring as 木 Mu, or 朱 Chu, or 末 Mo (in the *T'ai-p'ing huan-yü chi*, 186, 16 a), or even as 米 Mi, must clearly be the same as the first part of the name of the kingdom, and the true form of the name of the kingdom is certainly Mo-lu as in the *Hsin T'ang shu*, the *T'ai-p'ing huan-yü chi* and the *T'ung chih*, that is to say is identical with the «Mo-lu» of Hui-lin's «Mo-lu tieh»; and in point of fact Tu Huan says that, in that kingdom, there is «fine soft tieh cloth» (細軟疊布 *hsi-juan tieh-pu*). We certainly have to deal here with the Merw cotton stuffs which were famous in the Middle Ages; at the Congress of Persian Art in London in 1931, WIET spoke of the praise bestowed on the Merw cotton stuffs in early Arabic works.

We are now in a position to pass judgement on Hui-lin's would-be «Mo-lu tieh» of Bharuka (Aqsu). What Hui-lin really knew as Bharuka textiles were the woollen stuffs mentioned by Hsüan-tsang. He also had some knowledge of cotton stuffs from Mo-lu, and, out of phonetic analogy, attributed them to Bharuka. But this was a mistake, and the would-be Bharuka cotton stuffs actually were of Merw manufacture. The only point which remains uncertain is about the *K'uo-ti chih*. Hui-lin wrote in 817, that is to say after Tu Yu had published his *T'ung tien*; moreover, Hui-lin may have known Tu Huan's original account, which perhaps expressly mentioned the «Mo-lu tieh», and the *K'uo-ti chih* might be a misquotation. But it is no less possible that Tu Huan was not the first to use 末祿 Mo-lu to render the name of Merw, and that the *K'uo-ti chih* really mentioned the «Mo-lu tieh». The only point of interest for the present inquiry is that we must definitely dissociate the woollen stuffs mentioned by Hsüan-tsang at Aqsu from the «Mo-lu tieh». These were really cotton stuffs, as their name implies, but they were manufactured at Merw, not in Chinese Turkestan. There is nothing in Hui-lin's gloss, once it is corrected, which would tend to show that Aqsu was famous for the production of refined cotton stuffs in the first half of the 7th cent. The cotton cultivation attested in the first half of the 6th cent. in the region of Turfan by the *Liang shu* has hitherto found no counterpart in any other oasis of Chinese Turkestan.

Returning to China, it is also under the T'ang dynasty that we hear of the «so-lo tree» as yielding cotton, and of the *so-lo lung tuan*, which must have been cotton damasks; as Hsü Kuang-ch'i surmised, this *so-lo*, the name of which has remained in use until now, can be no other than *Gossypium arboreum*. The name was known outside of Yün-nan, since a text of that time mentions a «so-lo floss tree» at Ya-chou in Ssü-ch'uan, but this «so-lo floss tree», from its description, was not a *Gossypium*, but a *Bombax* (cf. *supra*, pp. 470-477); cotton cultivation was thus still restricted to Yün-nan and Tongking, or at least its beginnings in other regions remained unnoticed in the sources now extant.

In all likelihood, it had already begun in Hai-nan under the T'ang dynasty, perhaps among

the Li aborigines, of Thai descent, who may have played there a part analogous to that played by their far-away Ai-lao cousins in western Yün-nan. The *Kuei-hai yü-hêng chih*, written in 1175 (cf. *supra*, p. 454) says (14 a) : «The 'Li curtains' (黎幕 *Li-mu*) are made 'south of the Sea' (*hai-nan*, i. e. in Hai-nan; on this equivalence, see 'Cheynam'). The people of the Li districts (峒 *tung*, a technical name for the territorial divisions or the villages of the southern aborigines) acquire Chinese variegated silks, undo the coloured threads and, mixing them with *mu-mien*, weave the whole into [*Li-mu*]. A *mu* ('curtain') is made by joining four strips (*fu*).» And then : «The 'Li sheets' (黎單 *Li-tan*; *tan* is the same as the modern 禪 *tan*; *tan-mu* in Chao Ju-kua is a combination of *Li-tan* and *Li-mu*; the translation 'single curtains' in *HR*, 183, is erroneous) are also woven by the Li people; it is a cotton cloth (*mu-mien pu*) with blue (*ch'ing*) and red lines (*tao*). Kuei-lin people (in Kuang-hsi) buy them wholesale as bedding articles (*wo-chü*).»

In 1178, Chou Ch'ü-fei mentions *chi-pei* («cotton») among the products of the region occupied by the Li clans, and says that Li men «wind their waist with *chi-pei*», and that Li women «always wear skirts (*ch'ün*) made of *chi-pei*, the variegated colours (*wu-sé*) of which are glittering» (*Ling-wai tai-ta*, 2, 8 b, 9 a). But, of the Man, who, in principle, are aborigines of the mainland in southern China («Man» may refer to the aborigines of Hai-nan in other texts, but not in the *Ling-wai tai-ta* which always calls them Li-jên, unless Chou Ch'ü-fei is here drawing from another undetermined source), he says also (*ibid.* 10, 9 a) : «In the winter, they plait (編) 'goose hair' (鵝毛 *é-mao* = goose down) and *mu-mien*, and in the summer they twist (緝 *ch'i*) [the fibres of] banana [leaves], [the fibres of] bamboo, and [also] hemp and ramie, to make garments.» The word «to plait» is certainly to be understood here as meaning «to weave»; the difference in the terminology is probably due to the fact that Chou Ch'ü-fei here copies some earlier unidentified work.

Finally, there is in Chou Ch'ü-fei a special paragraph entitled «*Chi-pei*», which is worthy of a complete translation (*Ling-wai tai-ta*, 6, 12-13) : «The *chi-pei* tree (*mu*) is like a low small mulberry bush; the calyx (枝萼 *chih-o*) is like that of the *fu-jung* (probably = *mu fu-jung*, hibiscus); the carpels (心葉 *hsin yeh*) of the flower all have a fine downy floss about half an inch long, just like willow down, and several tens of black seeds. The southerners collect the downy floss, roll out the seeds with iron pins, and then tease out the down by hand and spin it. They make cloth of it without the trouble of joining and twisting the threads, and it is extremely strong and good. The T'ang histories make it *ku-pei*, and also make it a herbaceous plant (*ts'ao*). If we regard 古 *ku* as a corruption of the character 吉 *chi*, still a plant and a tree are different things. Can it be that there is a distinct herbaceous *ku-pei* which is not the tree *chi-pei*? Or if we grant that a low bush (*mu*) is like a plant (*ts'ao*), still must the writing of the characters (*ku-chi*) make us leave the problem unsolved<sup>(1)</sup>? In Tei-, Hua-, and Lien-

<sup>(1)</sup> *Chiu t'ang shu*, 197, 1 b. Pelliot confessed himself unable to make sense of this passage; and Gustav Haloun could do little better. The present version or paraphrase is made with the very kind help of Dr Chêng Tê-k'un. It is easy, the author seems to mean, to say that *ku* is a misprint for *chi*, but still the difference between *mu* and *ts'ao* must be explained. Were there two different things with different names? We do not know. Alternatively it is easy to say that *ts'ao* might be loosely used for a small bush (*mu*), but then the difference between *ku* and *chi* becomes a difficulty again. Shall we leave it unsolved? [A. C. M.]

chou (three *chou* in Kuang-tung on the mainland) and among the Li districts (黎峒 *Li tung*) of Hai-nan, some of the rich use it in place of silk and ramie. In Lei-, Hua-, and Lien-chou it is woven into strips. When these are long, broad, and clean and white, fine and close, they are called 慢吉貝 *man chi-pei* (*HR*, 219, translate 'soft *chi-pei*', evidently on account of the following name; but *man* is 'lax', almost the contrary of what we should expect for a close texture; it may be that *man* was adopted on account of the other 縵 *man*, 賸 *man*, etc., which have been used as designations of the cotton sampot or sarong; cf. *supra*, p. 453). The narrow strips, which are coarse and lax and of a dull colour, are called 麤吉貝 *ts'u chi-pei*. Some *chi-pei* stuffs are extremely fine as well as light and soft, clean and white, and, when worn, last long. Those woven in Hai-nan are of many qualities. When the strips are very broad and do not make pieces of a regular size (端匹 *tuán-p'i*), by joining two strips one can make bed sheets (臥單 *wo-tan*) which are called 黎 *Li* sheets. By taking strips of different colours, the strange designs of which are conspicuously bright, and joining four strips one can make a curtain (幕 *mu*), called 黎飾 *Li-shih* (*shih* 'ornament' is probably a corrupt reading for *mu* 'curtain', which is given in the *Kuei-hai yü-hêng chih* and also in *HR*, 176). That with the various colours fresh and bright and fit to be used as a cover for a writing table (文書几案 *wên-shu chi-an* 'a table for documents') is called 鞍搭 *an-ta* (I do not know this term; *an* is perhaps a mistake for *an* 'table', and if so *an-ta* might mean 'table-cloth'). The long strips are used by the Li people to wind round their waists. That woven by the Nan-chao is still more delicate. The white is *chao-hsia*. The king wears *po-tieh*, the queen wears *chao-hsia*. These are what the T'ang histories call *po-tieh chi-pei* and *chao-hsia chi-pei*. »

Apart from the fact that they testify to the renown of Yün-nan cotton goods, Chou Ch'ü-fei's last sentences are valueless. Their antiquarian character is revealed by the use of the obsolete name Nan-chao instead of Ta-li. The *chao-hsia* could not be white (cf. *supra*, p. 453). The T'ang histories speak of *ku-pei*, not of *chi-pei*. Moreover their *po-tieh ku-pei* and *chao-hsia ku-pei* are not mentioned in connection with Nan-chao, but with Champa. All that precedes this last paragraph, however, is very important. Chou Ch'ü-fei's hesitation in identifying the *ku-pei* of the T'ang with the *chi-pei* of his own time, apart from the graphic corruption from *ku* to *chi*, is due to the fact that the «plant» of the T'ang histories was *Gossypium herbaceum*, while the «tree» he knew was *Gossypium arboreum*; but both gave true cotton. It also comes out very clearly from his account that, in 1178, most of the cotton cultivation in Hai-nan was in the hands of the Li aborigines. But cotton, either indigenous or imported from Hai-nan, was also already being woven in different parts of Kuang-tung. Nothing is yet said, however, of the cultivation or manufacture of cotton in Fu-chien or in any part of China proper, apart from the southern coast of Kuang-tung.

The importance of cotton weaving in Hai-nan under the Southern Sung is confirmed by the *Sung shih* (406, 1 a-b). A Cantonese, 崔與之 *Ts'ui Yü-chih* (c. 1160-1240), who held an office in his native province, made a tour of inspection in Hai-nan about A. D. 1200: «The people of Ch'üung (= of Hai-nan) [used to] weave with *chi-pei* (= cotton) clothes and coverlets; this work was entirely entrusted to women, to the point that some would abandon their young children all the year round or neglect their old men; the people suffered greatly from this... »

Chao Ju-kua's work is dated 1225. In its section on «cotton» (*chi-pei*), it gives no indication of the places where cotton was cultivated. In the description of the various Indo-Chinese countries, cotton is mentioned as being grown in Chiao-chih (=Tongking, *HR*, 46), Champa (*HR*, 48), and probably Cambodia, which produced *mien* cloth (lit. «floss cloth»; *HR*, 53; «probably Siam» in *HR*, 219, seems to be a slip, since Siam is not included among Chao's notices). More is said in the long paragraph on Hai-nan. Not only is *chi-pei* included among the local products (*HR*, 176), but Chao has a paragraph on [*Li*]-*tan* and [*Li*]-*mu* (*HR*, 183, ll. 4-7), analogous to, but not identical with, that of Chou Ch'ü-fei. This paragraph, however, occurs in exactly the same terms in Li Hsin-ch'uan's *Chien-yen i-lai hsi-nien yao-lu* (187, 14-15), which must antedate Chao Ju-kua's work by a few years (cf. *Ssü-k'u* . . . , 47, 37-38); it is probable that both Li and Chao here draw from an undetermined source, perhaps the monograph on Hai-nan which had been written c. 1210 by one of Chao Ju-kua's ancestors (cf. *HR*, 178, 186; we must not lose sight of the fact, however, that the source may eventually have been the *Kuei-hai yü-hêng chih*, since the present editions of this work are only fragments of the original). What is more important, Chao Ju-kua ends his notice on the island with the following words : «The other products are mostly the same as in the Barbarian lands, with the exception of areca nuts and cotton (*chi-pei*) which are there extraordinarily plentiful; the Ch'üan[-chou] traders look principally to these for making profit.» So, in 1225, Hai-nan was the principal source of the export of cotton towards Fu-chien.

Although Chou Ch'ü-fei only mentions «cotton» as grown in Hai-nan, and as grown or woven in some districts of the southern coast of Kuang-tung, and although Chao Ju-kua is silent even on the latter point, several texts prove that, at dates prior to both authors, cotton was already grown not only in Kuang-tung, but in Fu-chien as well.

The earliest is P'ang Yüan-ying's *Wên-ch'ang tsa-lu*, dated 1085 (cf. *supra*, p. 437), in which we read : «Beginning with Min (=Fu-chien) and Kuang[-tung] and more to the south, there is much cotton (*mu-mien*). The local peasants (*t'u-jên*) emulate each other in planting it, and collect its flowers to make a cloth which is called *chi-pei*.» P'ang Yüan-ying goes on to say that this is the same plant as the one described as «*ku-pei* tree» in the *Nan shih* (cf. *supra*, p. 437).

Then comes Fang Cho's *Po-chai pien*, published c. 1125 (cf. WYLIE, *Notes on Chinese literature*, 157; *JA*, 1913, 1, 350; *supra*, p. 438; *Po hai* ed., 2, 5 b; *Tu-hua-chai ts'ung-shu* ed., redaction in 10 chs., 3, 3 b; redaction in 3 chs., 2, 5 a-b). It says : «In Min (=Fu-chien) and Kuang-tung, much *mu-mien* ('cotton') is sowed. The tree (*shu*) is seven or eight feet high; its leaves are like [those of] the 柞 *tso* (an oak, *Quercus serrata* or *Quercus dentata*); it forms a fruit which is like a large water-chestnut (大菱 *ta-ling*; I do not think that *ta-ling* is the name of a distinct species; the *Po-hai* ed. and the *T'u-shu chi-ch'êng*, *ts'ao-mu tien*, 303, *tsa-lu*, 1 b, give 大麥 *ta-mai*, 'barley'; improbable in itself, this reading is rejected by the editor of the *Tu-hua-chai ts'ung-shu*, and it is also *ta-ling* which is given in 1285 in the quotation from the *Po-chai pien* made in Hu San-hsing's commentary on *Tzū-chih t'ung-chien*, 159, 48 a), but of blue-green (*ch'ing*) colour. When autumn is well advanced, it opens and discloses a white floss which is quite downy. The local peasants pick it up, discard the husk, get entirely rid of the black seeds

with an iron staff and gently card [the floss] with a small bow to make it mix and rise; then they spin and weave it into a cloth which is called *chi-pei*. At present, that which the trade appreciates in *mu-mien* is the sort which is fine and dense (緊 *chin*). That must be considered superior which has most flowers; in counting (cross-wise) [the *mu-mien*] which reaches up to 120 flowers is of the best quality. The southern Barbarians (Man-jên) of Hai-nan (or 'south of the Sea') weave the [*mu-mien*] into napkins (帨 *chin*), on which they draw small characters or various flowers, with the most clever art; this is what was anciently called 'po-tieh napkins' (*po-tieh chin*) . . . » Fang Cho's *mu-mien*, seven or eight feet high, is clearly the *Gossypium arboreum*. I feel inclined to believe that his « Hai-nan Man-jên » are not some undetermined Barbarians « south of the Sea », *i. e.* of the South Seas, but really the Li aborigines of Hai-nan, and that he was right in attributing to them the manufacture of the « *po-tieh napkins* », mentioned more than once by T'ang poets.

The *Tun-chai hsien-lan* was written at the beginning of the 12th cent. (cf. *supra*, p. 438). The passage on *mu-mien* is too similar to that in the *Wên-ch'ang tsa-lu* not to have been copied from it. Yet it contains a sentence which is not in the original, unless it has been omitted in the quotation made of it in the *Shuo fu*. The *Tun-chai hsien-lan* says : « Beginning with Min (= Fu-chien) and Kuang[tung] and more to the south, there is much cotton (*mu-mien*). The local peasants emulate one another in planting it, and some have as much as several thousand stalks. They collect its flowers to make a cloth which is called *chi-pei* cloth (*chi-pei-pu*). » Then the *Tun-chai hsien-lan* speaks of the *ku-pei* of the *Nan shih* in the same terms as the *Wên-ch'ang tsa-lu*.

Yü Chêng-hsieh (*Kuei-ssü lei-kao*, 7, 21 a, and 14, 4 b) has mentioned a passage on the forms *chi-pei* and *ku-pei*, which occurs in what he calls « 彭乘 P'êng Shêng's 墨客揮犀 *Mo-k'o hui-hsi* »; in fact, it is to be found in the *Hsü* [續] *Mo-k'o hui-hsi* (*Han-fên-lou ts'ung-shu* ed., 1, 6-7), and is identical with the one just translated from the *Tun-chai hsien-lan*. But, although the *Ssü-k'u* . . . Commissioners accepted the ascription of the work to P'êng Shêng and were forced to suppose that he was homonymous with, but different from, the well-known P'êng Shêng of the Northern Sung (*Ssü-k'u* . . . , 141, 9-10), it is now well established that both the *Mo-k'o hui-hsi* and the *Hsü Mo-k'o hui-hsi* are anonymous compilations made up in late Sung times of extracts from the works of Sung authors, one of these works being precisely the *Tun-chai hsien-lan* (cf. WANG Kuo-wei's *Kuan-t'ang wei-chi*, 1, 3, and *TP*, 1929, 156).

Even if we discard the last two texts as not being original, those of the *Wên-ch'ang tsa-lu* and the *Po-chai pien* leave no doubt that cotton cultivation was already extensively practised in Kuang-tung and Fu-chien at the end of the 11th century.

The 農桑輯要 *Nung-sang chi-yao*, « Manual of agriculture and silkworm breeding », in 7 chs. was prepared by order of Qubilai, and has a preface dated 1273 by the well-known scholar, 王磐 Wang P'an (on whom, cf. *YS*, 160, 1-3; not 王盤 Wang P'an, as in BRETSCHNEIDER, *Botanicon Sinicum*, 1, 82). It went through several editions, in 1314 (?), 1320, perhaps 1322, and by Imperial order was largely distributed throughout the country in 1329, 1332 and 1342; it was really the official manual of husbandry of the Mongol period (cf. PELLLOT, *A propos du Keng tche t'ou*, in *Mém. conc. l'Asie Orientale*, 1, 112; *Tu-shu min-ch'iu chi chiao-chêng*, 3 B,



2-3). The work is given as an anonymous production of the Board of Agriculture (*ssü-nung-ssü*) in LU Wên-ch'ao's *Pu Liao Chin Yüan i-wên chih* (*Pa-shih ching-chi chih* ed., 39 b) and in CH'ÏEN Ta-hsin's *Yüan shih i-wên chih* (same ed., 3, 5 b), but attributed by CHIN-MÊN Chao (*Pu san-chih i-wên chih*, same ed., 20 b; the *Chung-kuo jên-ming ta tz'ü-tien*) to 暢師文 Ch'ang Shih-wên (1247-1317). This attribution clearly rests on a passage in Ch'ang Shih-wên's biography (YS, 170, 5 b), which says that, in 1286, he presented to the Emperor the *Nung-sang chi-yao* [書] *shu* which he had compiled; but this is irreconcilable with the text of Wang P'an's preface, dated 1273; moreover Ch'ang Shih-wên does not seem to have ever been connected with the Board of Agriculture. In the Catalogue of the library of 錢謙益 CH'ÏEN Ch'ien-i (1582-1664), entitled 絳雲樓書目 *Chiang-yün-lou shu-mu*, the *Nung-sang chi-yao* is given as the work of 羅文振 Lo Wên-chên (cf. *Tu-shu min-ch'iu chi chiao-chêng*, 3 B, 3 a) and LU Wên-ch'ao (1717-1795) certainly had the *Nung-sang chi-yao* in mind when, in his *Pu Liao Chin Yüan i-wên chih* (*Pa-shih ching-chi-chih* ed., 40 b), he mentioned the «*Nung-sang* [撮] *ts'o-yao*, in 7 chs., by Lo Wên-chên»; the number of chapters shows that this incorrect title cannot be an abbreviation of that of the *Nung-sang i-shih ts'o-yao*, in 2 chs., by Lu Ming-shan, which will be spoken of farther on (the *Tsao-lin tsa-tsu*, *chung-chi*, 57 b, speaks of Wang P'an's preface of the *Nung-sang ts'o-yao*; so this erroneous form, instead of *Nung-sang chi-yao*, seems to have been current in Ming times). But I do not know who Lo Wên-chên was, nor what the authority may be for such an ascription. No bibliographer seems to have noticed that Hsü Kuang-ch'i cites the *Nung-sang chi-yao* as the work of 孟祺 Mêng Ch'i (cf. *Nung-chêng ch'üan-shu*, 35, 3 a, 14 b; *Chinese Repository*, xviii, 463; the mention of Mêng Ch'i in *Mu-mien p'u*, 2 b, 3 b, is merely taken from Hsü Kuang-ch'i). Mêng Ch'i was really connected in 1273 with the Board of Agriculture (cf. YS, 160, 9 a-b), but nowhere can I find any indication that he had anything to do with the *Nung-sang chi-yao*. Whoever the real author may be, we are sure that the passage on cotton (*mu-mien*) belongs to the original redaction of 1273, since there was no other edition before 1314, and this passage already appears, though with some omissions, in Wang Chêng's *Nung shu* (10, 5 b), dated 1313.

The text occurs in ch. 2, 19-20, and is as follows : «The rule for planting cotton (木棉 *mu-mien*) is to choose a soil with the double harmony (兩和 *liang-ho*), not a soil [which is too] damp and rich. In the first moon, when the breath of the earth gets through, [one should] deeply till [the ground] three times, exposing and covering to adjust its warmth, and afterwards divide it into ridges (畦畛 *kuai-chên*); each ridge is eight paces (*pu*; the Chinese 'pace' is a double pace) long and one pace broad; of this half a pace is the 'face' (*mien*) of the ridge, and half a pace is the 'back' (*pei*) of the ridge. Without hoeing, [the ground] is twice smoothed with a rake, and the surface earth obtained is put in heaps on the 'back' of the ridge. About 穀雨 *ku-yü* (20th of April), a fine day is selected to put in the seeds. One day before, the ridges already made are watered three times. The seeds are washed with water, and piled up on the damp soil. For one night, they are covered with an earthenware jar, which is taken off the next day. They are then gently rubbed with a little ash (灰 *hui*), and with due regard to even distribution they are scattered inside the watered ridges. They are then covered one finger deep with the surface earth which had been taken out. Without any

more watering, one waits six or seven days, till the sprouts come up together. Then, if the weather be dry, one waters them. Hoeing is practised, so that [the soil] should always be clean. If the [young plants] are dense, [some] are transplanted elsewhere; if they are sparse, this is not necessary. For each pace, only two plants are left. If they were [more] dense, they would not bear fruit. When the plant reaches more than two feet in height, one takes away the leading shoot. When a lateral branch reaches one foot and a half, the 'heart' (*hsin*) is also taken away, [so that] no leaves shall be barren (*k'ung*). When [the plant] flowers and bears fruit, one simply waits until the floss being about to fall is ripe; as soon as it is ripe, it is plucked and spread out on frames, exposed to the sun by day and to the dew at night. One waits until the seeds are dry to take them out [of the frames]. Then an iron staff is used, two feet long and one finger thick, the two ends gradually tapering, like a rolling-pin (趕餅杖 *kan-ping chang*), and also a pear-wood board, three feet long, five inches broad, and two inches thick, which serves as a 'bed' (*ch'uang*). The floss is taken in turn and placed on the board; the iron staff is then 'rolled' to drive out the seeds, and one gets 'pure cotton' (*ching-mien*, lit. 'pure floss'), which is spun and woven into 毛絲 *mao-ssü* (muslins?; cf. *TP*, 1933, 437-438), or into cotton garments extremely light and warm.»

The *K'ang-hsi tzü-tien* (s. v. 棉 *mien*) cites a long text on *mu-mien* as coming from 史炤 Shih Chao's «Commentary on the *Tzü-chih t'ung-chien*» (資治通鑑釋文 *Tzü-chih t'ung-chien shih-wên*) and being a gloss on Ssü-ma Kuang's mention of the «black tent of *mu-mien*» of the Liang Emperor Wu-ti. Shih Chao lived c. 1060-1140 (I have now reason to believe that the approximate dates 1100-1160, which I proposed in *BEFEO*, ix, 214, are too late; Shih Chao could not really have been a man of the southern Sung, as is said in *Ssü-k'u* . . . , 47, 15-16; but GILES's «11th century» [*Biogr. Dict.* No 1704] may be too early), and his commentary has a later preface, by another man and dated 1160. In view of its contents, the gloss would be of particular interest if it really were by Shih Chao. But it does not occur in his work (*Shih-wan-chuän-lou ts'ung-shu* ed., 17, 5 a), and is, in fact, by a later commentator, 胡三省 Hu San-hsing. The wrong attribution to Shih Chao occurred already in the *Wu-Hsün tsa-p'ei* (cf. *T'u-shu chi-ch'êng, ts'ao-mu tien*, 303, *tsa-lu*, 2 a, and *supra*, p. 463), so that it must have been made in some Ming dictionary or encyclopaedia from which the *K'ang-hsi tzü-tien* copied it; the error is also repeated by Yü Chêng-hsieh (*Kuei-ssü lei-kao*, 14, 4 b). Hu San-hsing lived from 1230 to 1287 (the date 1307, given for his death in DES ROTOURS, *Traité des examens*, 76, is an error), and his commentary on Ssü-ma Kuang's work was published in December 1285. The gloss says (*Tzü-chih t'ung-chien*, 159, 48 a) : «There is much *mu-mien* ('cotton') in Chiang-nan (i. e. in Chiang-su and Chê-chiang). To sow it, the seeds are put into place on the last day of the second or the third moon. When they come up, [the ground] must be hoed on the four sides [of the seeds] three times a month. If the hoeing is not done in time, [the ground] becomes overgrown with weeds, and [the young plants] wither and die. On entering summer, it becomes progressively luxuriant. On reaching autumn, it bears yellow flowers and forms fruit. When this is ripe, the skin splits into four, and from the slits in it (綻 *chan*, i. e. the carpels) protrudes something like [silks] floss (*mien*). The peasants (*t'u-jên*) roll out the kernels with an iron bar (錠 *t'ing*), and take what is like [silk] floss. They make a small bow of bamboo, one foot and

four or five inches long, and draw the cord to card the floss and make it even and fine. They roll it into small tubes and take it to the spinning-wheel to spin it. It runs on spontaneously in the same manner as a cocoon which is being unwound, without giving the trouble of twisting and connecting. Cloth is made with this. That which comes from Min (= Fu-chien) and Kuang [-tung] is the most refined and close. . . » Hu San-hsing ends up with copying in its integrity Fang Cho's text already translated above (cf. *supra*, p. 498). This gloss stands quite apart in Hu San-hsing's work, which, as a rule, is almost devoid of original information. But Hu was a Chê-chiang man, and for once could not resist the temptation of describing at some length a recent form of cultivation which was practised in his own province. The interest of this text lies in the fact that it must have been written in 1285 at the latest, several years before the date assigned by later tradition to Huang *tao-p'o's* directions. Already in 1285 there was « much cotton » grown, with an elaborate technique, in Chiang-nan. It does not sound probable that the cultivation of a new plant should have been developed in the baleful years which culminated in the conquest of the Sung capital Hang-chou by the Mongols (1276). In all likelihood, the introduction of cotton cultivation into Chiang-nan goes back at least to the first half of the 12th century.

Not many years after the composition of Hu San-hsing's note, we find further information in Wang Chêng. BRETSCHNEIDER (*Botanicon Sinicum*, I, 81) attributes to Wang Chêng a special monograph on the cotton plant, entitled 木棉圖譜 *Mu-mien t'u-p'u*; but it never existed as an independant work. BRETSCHNEIDER may have inferred its existence from the « preface to the *Mu-mien t'u-p'u* » copied in *T'u-shu chi-ch'êng, ts'ao-mu tien*, 303, *i-wên*, 2 a; this, however, is merely the introduction to the illustrated notices on cotton cultivation, which constitute the second part of ch. 21 (ff. 16-19) of Wang Chêng's comprehensive treatise on agriculture, the *Nung shu*, the general preface of which is dated 1313. Wang Chêng was the inventor of the « revolving case » for moveable type, which he actually devised to print his *Nung shu* (cf. T. F. CARTER, *The invention of printing in China*, 161-166; I do not know why CARTER always dates the *Nung shu* « 1314 »).

There are two sections on « cotton » (*mu-mien*) in the *Nung shu*, one in the general descriptions of plants (10, 5-6), and the other explaining the machines needed for cleansing and carding the floss. The first text begins as follows : « *Mu-mien* is also called *chi-pei*. It is sowed about *ku-yü* (20th of April), and progressively collected at *li-ch'iu* (7th of August). The flower is yellow like the 葵 *k'uei* ('mallow', a generic name of malvaceous plants, some of which have small yellow flowers); the 'root' (*kên*) is single and straight; the 'tree' (*shu*) is not valued for its height, but for the number of 'branches' (*kan*). It does not grow from the old root, but is produced by sowing seeds. As to the seeds to be sowed, those which are first collected are not full, and those [which ripen] near hoar-frost time cannot be used; the best are only those collected in the months between. [The fruit] must be thoroughly dried in the sun, and stored with the floss. When the time of sowing approaches it is sundried again, then rolled and planted. This plant (*chung*) was originally produced in the kingdoms of the southern sea (*nan-hai chu-kuo*). Later, all the districts (*hsien*) of Fu-chien had it. Recently (*chin*), it has also been much sowed in 江東 Chiang-tung (lit. 'East of the Chiang', *i. e.* Chiang-su and Chê-chiang) and 陝右 Shàn-yü (lit. 'Right

of Shàn', *i. e.* Shàn-hsi). It is there prosperous and abundant, without any difference from its country of origin. Those who sow it derive great profit from it. . . » Wang Chêng then quotes almost the whole of the passage in the *Nung-sang chi-yao* translated above, and concludes : « Cotton (*mu-mien*) is something the planting of which does not tell on the peasant's time; its production does not require much human effort. In regular succession it flowers and bears fruit; one may say it is floss without silkworms and cloth without hemp; moreover it can replace felt and rugs, and help to save the cost of serge garments (衣襦 *i-ho*, probably a wrong reading instead of 裘襦 *ch'iu-ho*, 'furs and serge', as in 21, 16 a). One may say it combines the advantages of north and south. » It will be noticed that, while Wang Chêng marks cotton cultivation as a « recent » acquisition, it was already in general practice in Fu-chien, and had been introduced not only into Chiang-su, but also into Shàn-hsi. North-western China is here mentioned for the first time as a cotton-growing region. Although Wang Chêng's text would almost imply that, even to Shàn-hsi, the seeds had come from the south, it may well be that, in fact, the Shàn-hsi cotton growers were indebted to their near neighbours of Turfan.

Wang Chêng's illustrated notices on the method of preparing cotton after the « fruit » has been taken from the plant (each of which ends with a mnemotechnic verse) are mainly interesting in showing that an elaborate technique was already in full swing in 1313. But the introduction to these notices adds some information of historical value (21, 16 a) : « After 'the mulberry land had silkworms' (桑土既蠶, a quotation from the *Shu ching*; cf. LEGGE, *Chinese Classics*, III, 99), people have cared only for cocoons and their floss silk, and have known nothing about the use of cotton (*mu-mien*). Cotton was produced south of the sea (海南 *hai-nan*; or 'in Hai-nan'); the method of planting and manufacturing it came north gradually. Chiang and Huai (= Chê-chiang, Chiang-su, and An-hui) as well as 川蜀 Ch'uan and Shu (= Ssü-ch'uan) had profited from it, and when south and north were united (*i. e.* when the Mongols had conquered the Sung dominions), traders sold it in the north, so that the wearing of it gradually developed. It is called 吉布 *chi-pu* ('*chi* cloth', abbreviated from *chi-pei pu*) and *mien-pu* ('*mien* cloth' = '*mu-mien* cloth'). [Here Wang Chêng quotes in a note the passage from the pseudo-*I-wu chih* I have discussed above; (cf. *supra*, p. 439); he then goes on :] In the making of [cotton] strips, there are fixed sizes in length and breadth (as there are for silk). In softness, denseness, lightness, and warmth, [cotton] can equal silken fabrics (*tsêng-po*); it is also used for plush garments, rugs, and satins, which can take the place of the real articles. P'ei Yüan's *Kuang chou chi* says (cf. *supra*, p. 462) : 'The southern Barbarians (*Man-i*) have no silkworms, but pluck *mu-mien* to make floss (*hsü*)'. Moreover, according to the 諸番雜誌 *Chu-fan tsa-chih* (this refers to Chao Ju-kua's *Chu-fan chih*, but it is a summary of Chao's information, not a word for word quotation), *mu-mien* is produced by the *chi-pei* tree (*mu*); such kingdoms like Chan-ch'êng (= Champa) and Shê-p'o (= Java) all have it. At the present time it has become already one of the valued things of China, but there is not enough of it, apart from what comes from its place of origin. If we compare it with mulberry and silkworms, one does not have the trouble of plucking [the leaves] and breeding [the worm], and there is certainty of reaping; if compared with hemp and ramie, it spares the work of twisting and connecting [the threads], and has the advantage of warding off the cold. One may say that it is cloth (*pu*) without hemp, and floss (*hsü*) without cocoons. Although it

is called 'southern product' (*nan-ch'an*), we can speak of its real usefulness, since, the northern regions being very cold, when the cocoons and their floss are not always enough one has [to meet] the cost of furs and serge; but this is very economical. . . » In this introduction, we see that, already before the unification of China in 1276, the cultivation of cotton had spread not only to the whole of the region of the lower Yang-tzū, but also to Ssü-ch'uan, whether it had come to the latter province from Chiang-su, or from Turfan *via* Shàn-hsi.

One other work on agriculture written under the Mongol dynasty has a paragraph on cotton cultivation; it is the 農桑衣食撮要 *Nung-sang i-shih ts'o-yao*, « Compendium on clothing and food [as obtained] by agriculture and silkworm breeding (*lit.* 'mulberry') », in 2 chs. As we have it, the work, recovered from the *Yung-lo ta-tien*, begins with the author's preface, dated 1330, but his name is not given there, and occurs only at the beginning of the first chapter, as 魯明善 Lu Ming-shan. Ch'ien-lung's Commissioners (*Ssü-k'u* . . . , 102, 6*a*) knew another text, beginning with a preface by 張鼎 Chang Li; in this preface, which the Commissioners have not included in the *Ssü-k'u ch'uän-shu*, Chang Li said that Lu Ming-shan first published his work in 1314, and that he was the son of a Uighur who had settled in China; he had taken his name of «Lu» from the appellation (*tzü*) of his father, and was himself properly called 魯鐵柱 Lu T'ieh-chu, though he was commonly known by his *tzü* as Lu Ming-shan. The *Nung-sang i-shih ts'o-yao* is regarded as having been written to supplement the *Nung-sang chi-yao*; it does not add much to the latter in the following passage on cotton (*Mo-hai chin-hu* ed., ch. I, 11-12) : « Sowing *mu-mien*. — One must first take the seeds and soak them in water and ashes in equal proportions, then wait until they bud; [thereupon,] a hole is dug in each foot of manured soil, and five to seven seeds put into it. One [then] waits for the sprouts to come up; if they are [too] dense, some of them are taken out and only two or three vigorous sprouts are left. [The ground] is carefully hoed. The ends of the shoots are regularly plucked, so as not to have high shoots; if the shoots are too vigorous, they will not form fruit. Cotton is reaped in the eighth moon ».

The YS has preserved a text on the cotton tribute which was required from the cotton growing provinces (15, 8*a*) : on May 16, 1289, « cotton inspectorates (*mu-mien t'i-chü-ssü*) were established for Chê-tung (= Chê-chiang), Chiang-tung (= Chiang-su), Chiang-hsi, Hu-kuang (= Hu-pei and Hu-nan) and Fu-chien; every year, the people had to deliver [to the inspectors] 100,000 'pieces' (*p'i*); an Inspector General (*tu t'i-chü-ssü*) had the general control ». This is the first mention of cotton cultivation in central China. An edict of 1295-1296 mentions cotton (*mu-mien*) among the goods accepted in payment of State taxes (cf. WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 4, 43*b*). All this antedates Huang *tao-p'o*'s exaggerated activities.

According to the *Chieh-ch'êng yü* (3*a*; cf. *supra*, p. 438), the Emperor Tämür (1295-1307), on a spring evening, ordered the Palace ladies to spread in a hall all the flowers that had fallen, and then to play and wrestle on the flowers, being dressed themselves in «*po-tieh* trousers» and wearing turbans of *chi-pei* brocade (*chi-pei chin*); I do not know what distinction the 14th cent. author made between these two designations of cotton.

On June 21, 1323, the Emperor Ying-tsung repaired to a pavilion where the old clothes of Chinghiz-khan and Qubilai-khan were kept. «All were made of plain silk (縑素 *chien-su*) or

cotton (*mu-mien*), and had been repeatedly patched and repaired. [Ying-tsung] drew a long sigh and told his attendants : 'To such a point did Our Ancestors suffer hardships in founding Our dynasty and were thrifty in their dress ! How can I forget this for one moment ? ' » (YS, 28 6 a). It is quite possible that Chinghiz had taken to wearing cotton of Turkish manufacture. Ying-tsung's successor T'ai-ting-ti was presented in 1325 or 1326 (cf. WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 32, 15 a) with a « great field tent of cotton » (*mu-mien ta hsing-chang*), the earliest counterpart I have found hitherto to the « black tent of cotton » of the Liang Emperor Wu in the first part of the 6th cent. (cf. *supra*, p. 462).

All these texts show that, from its earliest days, the Mongol dynasty, less hampered than the purely Chinese dynasties by the old traditions which regarded silk on the one hand and hemp and ramie on the other as the only proper materials for Chinese clothing, was fully aware of the great advantages to be derived from the new plant of foreign origin and did all in its power to favour its cultivation and sale throughout the Empire. Nor can there be any doubt that the cotton which was cultivated in Chiang-nan from the 12th cent. was not the cotton tree, *Gossypium arbo-reum*, then cultivated in Fu-chien, but the cotton plant, *Gossypium herbaceum*.

I know of no text before the 17th cent. to support CIBOT's contention (*Mém. concern. les Chinois*, II, 604) that the cotton plant was introduced into China from the Hsi-Fan (*i. e.* from Central Asia), and none of any date in favour of the statement in Wells WILLIAMS, *The Middle Kingdom*, 1883 ed., II, 37 (copied by DYER BALL, *Things Chinese*<sup>3</sup>, 150), that, early in the 11th cent., the plant was brought over and cultivated in the north-western provinces by persons from Khotan (« Khotan » is perhaps a mistake for « Kao-ch'ang » = Turfan). The same may be said of MAYERS's view (*Chinese Reader's Manual*, p. 71) that Huang tao-p'o's activities in Chiang-nan took place « about the commencement of the 14th century, after the cotton plant had been introduced from Turkestan » (cf. also GILES, *Biogr. Dict.* No. 870; STUART, *Materia Medica*, 197-198). Apart from the *Wu-Hsün tsa-p'ei*, written about the middle of the 17th cent., the earliest work which may seemingly lend colour to a Central Asian origin is the *Tai-tsui pien*, or more completely 瑯琊代醉編 *Lang-yeh tai-tsui pien*, by 張鼎思 CHANG Ting-ssü. CHANG Ting-ssü was a doctor of 1577; his *Lan-yeh tai-tsui pien*, in 40 chs., was written when he was well advanced in life, presumably c. 1600 (cf. *Ssü-k'u...*, 126, 12 b; 132, 2 b). I have had no access to the complete work, and the passage on cotton is not included among those copied in ch. 8 of the *Shuo fu hsü*; so I can only give it as it is quoted in the *Pên-ts'ao kang-mu shih-i* (5, 10 a) : « According to the [*Lang-yeh*] *Tai-tsui pien*, cotton seeds (*mien-hua chung*) were brought over by the Barbarian envoy (*fan-shih*) 黃始 Huang Shih; towards the end of the Sung, [cotton cultivation] was for the first time (始 *shih*) introduced into Chiang-nan » (the quotation is somewhat different in the *Kuei-ssü lei-kao*, 14, 6 b, which speaks of « Kuang-tung » instead of « Chiang-nan »). In itself, the word 番 *fan*, « Barbarian », may just as well refer to a southern foreigner as to a man of Central Asia, a Hsi-Fan; but the main point is that the text seems to be devoid of any authority. The very name of the « Barbarian envoy », with his purely Chinese surname, at once raises suspicion. Moreover, it would be most extraordinary if that foreign envoy not only had a Chinese surname but happened to have the same surname as Huang tao-p'o. My view is that, in all likelihood, we have here to deal with a distorted version of Huang tao-p'o's story. I believe it is probable that

the 始 *shih* of Huang Shih is corrupt either for the 嫗 *yü* of «Huang *yü*», «the old woman Huang», or more probably for 姑 *ku*, «nun», as surmised by the *Kuei-sü lei-kao*, 14, 6 b, and is due to the attraction of the *shih* which follows. The words «Barbarian envoy» seem to be an arbitrary addition, made because cotton was known to have been imported from abroad. The fact that, for cleansing and carding cotton the Chinese first used an iron bar, a board and a bow, and that these are also the instruments used for the same purposes in Persia mentioned in the tetraglot vocabulary, the Mongolian and Turkish parts of which have been published by POPPE (p. 218), provides no evidence in any direction, since the early processes were probably the same in Central Asia and in Indo-China or Indonesia. In spite of all that has been said to the contrary, the probabilities are that the cotton plant came to Chiang-nan from Hai-nan. Nor can I see anything more than a personal romantic view of the problem in CIBOT's detailed account (pp. 605-607) of the opposition which cotton cultivation met under the Yüan at the hands of the traditionalists and of peasants and traders whose vested interests were threatened by the new textile.

In any case, one century after Polo, the use of cotton was universally adopted. When Hung-wu established his dynasty (1368), «an order was issued that in all private fields from five to ten *mou* in extent, half a *mou* each was to be planted with mulberry, hemp, and cotton (*mu-mien*); from ten *mou* and above, the quantity had to be doubled. For hemp, eight ounces were levied per *mou*; for cotton, four ounces per *mou*; when mulberries were planted, the tax began in the fourth year...» (*Ming shih*, 78, 1 b). The rates at which cotton (*mien-hua*) was accepted in 1385 in payment of taxes has been preserved in WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao* (4, 44 b). We have seen that, in the first half of the 17th cent., WANG Hsiang-chin and Hsü Kuang-ch'i had devoted much attention to cotton cultivation; other sources of the Ming dynasty might easily be added. For instance, we are told that, in Ming times, Ying-chou (in An-hui) produced «red cotton» (*hung mien-hua*) and «bluish-green cotton» (*ch'ing mien-hua*; cf. *Tsao-lin tsa-tsu, chung-chi*, 57 b). The Manchu dynasty too did much to promote the growing and weaving of cotton. Just as there had been, from Sung times, famous pictures of silkworm breeding and agriculture which were entitled *Kêng-chih t'u*, pictures of cotton cultivation were published in 1765 by Imperial order under the title of 棉花圖 *Mien-hua t'u* (cf. COURANT, *Catalogue*, No. 5415; I. HEDDE, *Description méthodique des produits divers recueillis dans un voyage en Chine*, Saint-Étienne, 1848, 8 vo, 307). Towards the end of the 18th cent., 褚華 CH'ü Hua, a native of Shanghai, published a description of the various kinds of cotton, the 木棉譜 *Mu-mien p'u* (cf. WYLIE, *Notes on Chinese literature*, 77); it is found, without preface or date, in the *I-hai chu-chên* (this is the edition I have used) and in the *Chao-tai ts'ung-shu*; cf. also *Sung-chiang fu-chih*, 6, 8 b-11 b. Much of it is copied from LI Shih-chên and Hsü Kuang-ch'i, but there is also a good deal of original information. The author is well known, and it is certainly an error of Yü Chêng-hsieh, who quotes it at second hand and ascribes to him a distinction between «hill cotton» (山花 *shan-hua*) and «field cotton» (田花 *t'ien-hua*) which is not in the text, to write the name 儲華 CH'ü Hua. Cotton is now one of the staple products of Chinese husbandry and the word 布 *pu*, «cloth», which had long been referred to hemp or ramie cloth («grass cloth»), now designates cotton goods par excellence.

Chinese cotton stuffs even gained a reputation abroad. The name of the Ming southern

capital, Nan-ching, our «Nanking», was first used by Europeans as the designations of certain silks : cf. for instance «xopas (= 手帕 *shou-p'a*, 'handkerchiefs') de nanquim» in 1534 (DALGADO, *Glossário Luso-Asiático*, II, 535), and «raw Lankine silk» in 1615 (YULE, *Hobson-Jobson*<sup>2</sup>, 616). But, in the 18th cent., «nankeen» was used for a kind of cotton stuff. In this sense, YULE's earliest quotation is dated 1793-1794; the term occurs, however, in *Paul et Virginie* (1787), and BLOCH (*Dict. étymologique*, II, 85) traces it back to 1766; it may even be older. The curious fact is that the best «nankeens» do not seem to have always been woven with cotton grown in China. Hsü Kuang-ch'i already remarked that foreign cotton was superior to the Chinese product (cf. *supra*, p. 489), and, according to a statement which seems to rest on good authority, the best Chinese «nankeens» were made with cotton imported from Pegu (cf. CORDIER, *La France en Chine au XVIII<sup>e</sup> siècle*, XXXVI). But this was no longer the case in the 19th cent., as is shown by R. FORTUNE's *Three years wanderings in the Northern Provinces of China*, London, 1847, 264-265. According to HEDDE (*Description méthodique*, 306), the cotton known as Nanking cotton was the yellow one, a fact confirmed by FORTUNE. The three main colours of cotton, white, yellow and bluish, had come to the knowledge of the Chinese, but they are merely accidental varieties of the same species, and often occur together in the same fields (cf. FORTUNE, *loc. cit.* 265). CHAO Hsüeh-min (*Pên-ts'ao kang-mu shih-i*, 5, 10a) quotes a certain 沈焯 SHÊN Chao, whom I cannot identify, as saying : «Among the Barbarians, there are three kinds [of cotton], blue-green, yellow, and white; at present, it is the only white which is particularly current.»

THE AGNUS SCYTHICUS. — Much has been written about the lamb-plant, or Tartarian lamb, or *Agnus scythicus*; the main elements of information may be found in *Br*, I, 154-155; HIRTH, *China and the Roman Orient*, 54, 260-263; H. LEE, *The Vegetable lamb of Tartary*, London, 1887; SCHLEGEL, *The Shui-yang... and the Agnus scythicus...* (in *Actes du VIII<sup>e</sup> Congrès international des Orientalistes*, 4<sup>e</sup> partie, 4<sup>e</sup> section, 17-32); CORDIER, *Odoric de Pordenone*, 425-432; CHAVANNES, in *TP*, 1907, 183-184; LAUFER, *The story of the pinna and the Syrian lamb* (in *The Journal of American Folk-Lore*, XXVIII [1915], 103-128); Ed. BRÜCKNER, *Das Pflanzenschaf (Baranetz)* [in *Russische Revue*, XXI (1882), 131-146; not mentioned in CORDIER, *Bibl. Sinica*<sup>2</sup>, 1879, 3981-3982]. I have to examine the problem here because, in some respects, it is closely connected with the history of cotton in Asia.

There are two series of texts, one which speaks of the 水羊 *shui-yang*, or «water sheep», the other of the 種羊 *chung-yang*, or «sowed sheep».

It has always been said that the earliest text to mention the *shui-yang* occurred in the *Hou-Han shu*; but in fact, as in the case of *po-tieh* (cf. *supra*, p. 445), the *Hou-Han shu* really draws from a post-Han source, which is here certainly the *Wei lio* (second third of the 3rd cent.). In its section on Ta-Ch'in (= Mediterranean Orient), the *Wei lio* says (cf. HIRTH, *China and the Roman Orient*, 71, 112): 有織成細布。言用水羊毳。名曰海西布。此國六畜皆出水。或云非獨用羊毛也。亦用木皮或野繭絲作織成。氈毳毼毼屬帳之屬皆好。其色又鮮於海東諸國所作也。 This passage is rendered by HIRTH : «...They weave fine cloth, and say they use the down of water-sheep in making it; it is called Hai-hsi-pu [cloth from the



west of the sea]. In this country all the domestic animals come out of the water. Some say that they do not only use sheep's wool, but also the bark of trees [vegetable fibre?] and the silk of wild silk-worms in weaving cloth, and the Ch'ü-shu, the T'a-têng, and Chi-chang class of goods [serge or plush rugs?] of their looms are all good; their colours are of brighter appearance than are the colours of those manufactured in the countries on the east of the sea.» Except for «tree-bast» for «the bark of trees» and «[that is, evidently, flax]» instead of «[vegetable fibre?]», LAUFER's translation (*loc. cit.* 103) is practically identical. Yet such a rendering is open to various objections. First of all, HIRTH ought to have written «cloth of West of the Sea». Hai-hsi, «West of the Sea», as expressly stated in the *Wei lio*, was the popular designation of Ta-Ch'in, and it is used in contradistinction to Hai-tung, «East of the Sea», the «Sea» being the Indian Ocean, and particularly the Persian Gulf. On the other hand, *ch'ü-shu* and *t'a-têng* are names of rugs which are to be retained (cf. *supra*, pp. 492 and 447), but there is no such term as *chi-chang*; *chi* is the designation of a woollen carpet, and *chang* means «curtain» or «tent». But, above all, none of the former translators seems to have suspected that 織成 *chih-ch'êng* has two meanings in Chinese; in many cases, it merely means «to weave» (as for instance in the passage on the *lan-kan* cloth of the Ai-lao, *supra*, p. 444). But in ancient times *chih-ch'êng* was also the designation of a particular sort of brocade (cf. the section *chih-ch'êng* in *T'ai-p'ing yü-lan*, 816, 11 a, and the *Tz'ü-yüan*, s. v. *chih-ch'êng*; also *supra*, p. 452). The very construction of the first sentence quoted above implies that *chih-ch'êng* is to be taken there in the latter sense. I think that we must translate it in the same way when it occurs a second time, and it is so understood in the *T'ai-p'ing yü-lan*, which moreover punctuates the passage as I have done. The quotation from the *I-wu chih* which HIRTH (p. 255) found in the *K'ang-hsi tzü-tien* and which would seem to favour another punctuation, with *chih-ch'êng* in its ordinary sense of «to weave», is a distorted and arbitrary combination of part of the passage of the *Wei lio* with a text from the *Nan-chou i-wu chih*, both given independently in the *T'ai-p'ing yü-lan*, 708, 12 b (but this is not the direct source of the *K'ang-hsi tzü-tien*, in which the last sentence ends differently). The whole section of the *Wei lio* on the products of Ta-Ch'in has been in fact very badly translated by HIRTH, and even CHAVANNES has not been too successful in his rendering of the corresponding passage in the *Hou-Han shu* (TP, 1907, 183). I would propose the following interpretation of the Chinese text quoted above: «They have a brocaded (*chih-ch'êng*) fine cloth, for which it is said they use the down (*ts'ui*) of the water sheep (*shui-yang*), and which is called 'Hai-hsi cloth' ('cloth of the West of the Sea [kingdom]'). In that kingdom, the six domestic animals all come out of the water. Some say that they not only use sheep's wool (*mao*), [but] also use the bark of trees or the silk of wild silkworms to make [this] brocade (*chih-ch'êng*). Their *ch'ü-shu*, *t'a-têng* and [other textiles] of the class of woollen rugs (*chi*) and curtains (*chang*) are all good; moreover their colours are brighter than [the colours] of those manufactured in the kingdoms of the East of the Sea (Hai-tung).»

The parallel passage in the *Hou-Han shu* reads as follows (118, 4 b): 又有細布或言水羊毳野蠶繭所作也。HIRTH's translation (p. 41) is: «They further have 'fine cloth', also called *Shui-yang-ts'ui* [*i. e.* down of the water-sheep]; it is made from the cocoons of wild silk-worms.» CHAVANNES (TP, 1907, 183) renders it: «They have moreover a light cloth which some

say is made from the down of the water sheep, but which in fact is manufactured with the cocoons of wild silk-worms.» LAUFER (p. 103) maintained that the true meaning was : «Further they have a fine cloth said by some to originate from the down of a water-sheep, and they have also a stuff made from wild-silkworm cocoons.» According to LAUFER, «it is clear that the above Chinese clause consists of two separate and co-ordinated parts. A stuff made from wild-silkworm cocoons is not capable of eliciting a tradition pertaining to a water-sheep.» But what is «clear» is that the text of the *Hou-Han shu* is merely abbreviated from that of the *Wei lio*, and should be translated : «They also have a fine cloth, which some say is made from the down of the water sheep or the cocoons of wild silkworms.» SCHLEGEL (*The Shui-yang*, 26) has adduced from the *Ko-chih ching-yüan* (27, 23 a) a quotation from the 孔六帖 *K'ung liu-t'ieh*, which says that «in the kingdom of Po-ssü (Persia), people weave the hair of the water sheep into a cloth which is called 'cloth of the West of the Sea'». The *K'ung liu-t'ieh*, a work written in Sung times by K'ung Ch'uan [傳] to supplement the *Liu-t'ieh* written under the T'ang by Po Chü-i, was never published in a separate edition, and, in the combined *Po K'ung liu-t'ieh* (ch. 96, § «sheep»), I cannot find the text quoted in 1736 by the author of the *Ko chih ching-yüan*. [In the *Po K'ung liu-t'ieh* with preface by 韓駒 Han Chü (c. 1550, 8, 27 a, s. v. 布 pu), we find, 海西布 : 波西織水羊毛爲布曰海西布 «West of the waves (or : in Po-hsi) they weave the wool of the water sheep into cloth called cloth of the West of the Sea.» PELLIoT, who had not seen this passage of the *K'ung liu-t'ieh*, had written : «There can be no doubt that the name 'Persia' (in *Ko-chih ching-yüan*) is due to an arbitrary change, and that the text is derived either from the *Wei lio* or from the *Hou-Han shu*.» It is in fact copied word for word from the *Hsin T'ang shu*, 221 B, 8 a, s. v. Fu-lin, with Po-hsi prefixed. If the author of the *Ko-chih ching-yüan* used this same edition of the *Po K'ung liu-t'ieh* it is evident that he corrected Po-hsi, which may be a variant of Po-ssü or perhaps a misprint for Hai-shi, to Po-ssü kuo. A. C. M.]

The same may be said of the mentions of the *shui-yang*, and the cloth made from its down, in all other T'ang or Sung works : *T'ung tien*, 193, 4 a; *T'ai-p'ing huan-yü chi*, 184, 4; *Hsin T'ang shu*, 221 B, 8 a; *T'ung chih*, 196, 25 a; *Wên-hsien t'ung-k'ao*, 339, 1 a. All are abbreviated from the text of the *Wei lio*, which, with the exception of the *T'ung chih*, the various authors have wrongly punctuated. The text translated (often mistranslated) by PAUTHIER (*De l'authenticité de l'inscription nestorienne*, 39) as from the [Hou] *Wei shu* is a late *rifacimento* in which passages of the *Wei lio* have been combined with information from later sources (cf. *infra*, p. 512). HIRTH (pp. 80, 115), SCHLEGEL (*The Shui-yang*, 23) and LAUFER (*The Story of the Pinna*, 104) attribute to the *Wên-hsien t'ung-k'ao* a reading 海中布 *hai-chung pu*, «cloth [from] within the sea», instead of «Hai-hsi pu», «cloth of the West of the Sea», and LAUFER thinks that Ma Tuan-lin, the author of the *Wên-hsien t'ung-k'ao*, effected the change under the influence of the *šūf al-bahr*, «wool of the sea», of mediaeval Arabic authors. Even if the reading *hai-chung pu* were correct, it is entirely out of the question that Ma Tuan-lin should have shown so much originality, and the change would merely be one of the many examples of the carelessness of that mediocre compiler. But, in fact, *hai-chung pu* is a faulty reading, and the standard editions of the *Wên-hsien t'ung-k'ao* give «Hai-hsi pu», like the sources which it copied.

The conclusion is that, in the whole of Chinese literature, there is only one mention of the

*shui-yang*, that found in the *Wei lio*, in the middle of the 3rd cent. Later texts have been copied or abbreviated from it, and do not represent any independent tradition. In the *Wei lio* itself, this «water sheep» occurs only in connection with a certain textile, which was woven with threads of variegated colours without a monochrome ground (地 *ti*; this was the main differentiation between *chih-ch'êng*, which had no «ground», and the 錦 *chin*, which had one; but it was not always strictly adhered to in the practical use of the two terms); and even then, the author of the *Wei lio* had heard conflicting reports, some saying that the fabric was made of tree bark (or bast), others of the silk of wild silkworms. Moreover, there is a disquieting sentence in the text: «In that kingdom, the six domestic animals all come out of the water», to which former inquirers did not devote a single word of comment. It sounds as though Ta-Ch'in being a maritime kingdom, the «West of the Sea Kingdom», a rumour had reached China that Ta-Ch'in was indebted to the sea not only for its «water sheep», but for its oxen, horses, dogs, etc. So far, nothing can be made of 秦牛 *ch'in-niu* or 犍牛 *ch'in-niu*, which theoretically might be construed as meaning «[Ta-]Ch'in oxen» (on the legend of oxen said to have been offered as tribute by Ta-Ch'in, cf. *T'ai-p'ing yü-lan*, 900, 15 b). The late gloss mentioned in CHAVANNES (500 *Contes*, I, 65) says these were «oxen [or cows] of the north», but the *I-ch'ieh ching yin-i*, which are earlier, merely quote the 字略 *Tzū lio* [c. A. D. 500?], which defines *ch'in* as «the name of an ox»; and in ch. 6 of the *Li-shih a-p'i-t'an lun* [NANJIO, No. 1297], the *ch'in-niu* are assigned not to the north, but to the Apāragodaniya, i. e. to the mythical continent of the West. The 秦水名牛 *ch'in-shui ming-niu* of the *Liu-tu chi-ching* (3rd cent A. D.) are certainly not «ses bufflèses célèbres du Nord», as translated by CHAVANNES (500 *Contes*, I, 65), but «his famous cows of the Ch'in river». I am not in a position, however, to decide whether the author of the text had in mind the old Ch'in river in Shàn-hsi, which could hardly have been mentioned in a «translation» from the Sanskrit, or the «[Ta-]Ch'in water», which would possibly provide a «water ox» parallel to the «water sheep». But the interpretation remains doubtful, and the connection would be extremely slender. In any case, since all domestic animals in Ta-Ch'in are in the same plight, the *shui-yang* is merely the equivalent of *yang* alone, and, as a matter of fact, it is the word *yang* («sheep») alone, not «water sheep», which is used when the *Wei lio* speaks a second time of the wool of the same animal. Under such conditions, while admitting that there must have been in China, in the early 3rd cent., a tradition about some special sort of «sheep's down» of Ta-Chin, I think that we must be careful not to lay too much stress on the statement that this sheep was a «water sheep».

By the side of the single mention of the «water sheep», we have a number of texts concerning «sheep that were planted».

Standing quite apart, and hitherto unnoticed except in the *Yüan-chien lei-han* (436, 15 b) and the *Tz'ü-yüan hsü-pien* (s. v. *chung-yang*, «sowed sheep»), there is a puzzling passage occurring in 劉子 *Liu-tzū* (*Tzū-shu po-chung* ed. in 2 chs., 2, 19 a [§ 44]). *Liu-tzū* is a work of uncertain date and authorship. Its attribution either to 劉歆 Liu Hsin (1st cent. B. C. and A. D.), or to 劉勰 Liu Hsieh (early 6th cent.) is certainly to be rejected. 劉孝標 Liu Hsiao-piao's authorship (= Liu Hsün, 462-521) is improbable. A T'ang preface, by the commentator 袁孝政 Yüan Hsiao-chêng, gives, as author of the *Liu-tzū*, 劉晝 Liu Chou of the Northern Ch'i. Liu

Chou's notice in the *Pei-Ch'i shu* (44, 4*b*; cf. also *Pei shih*, 81, 11*a-b*) does not seem much to countenance this attribution, and the Commissioners of the *Ssü-k'u...* (117, 20-22; they used a copy divided into 10 chs.) do not discard the possibility that the *Liu-tzŭ* may be a forgery of T'ang times due to Yüan Hsiao-chêng himself. This is certainly an error. *Liu-tzŭ's* remark on a name given by Chou people to a dead rat (§ 16) is quoted in an original note to ch. 6 of Fa-lin's *Pien-chêng lun* (露, VIII, 55*b*), completed c. 626, and clearly as taken from an authoritative work already of some age. Moreover, as remarked by LU Wên-ch'ao, who knew Yüan Hsiao-chêng's work (*Ch'ün-shu shih-pu*, 29, 1*a*), the difference in style and value between the text and the commentary precludes the possibility that both should be the work of one and the same man.

The passage in § 44 of *Liu-tzŭ* reads as follows: « Wên of Chin sowed husked rice (*mi*), and Tsêng-tzŭ planted sheep (晉文種米 [菜 *ts'ai*, 'vegetables', given in the *Yüan-chien lei-han*, 436, 15*b*, followed by the *Tz'ü-yüan hsü-pien*, is a wrong reading] 曾子植羊); it was not because they were stupid by nature, but they did not discriminate between regions (方隅); being men with a great destiny, they were not trained in petty matters. » Wên of Chin is probably the famous Duke Wên of Chin, 重耳 Ch'ung-êrh, who lived in the 7th cent. B. C. (cf. GILES, *Biogr. Dict.*, No. 523), and whose biography in the *Shih chi* (ch. 39; cf. CHAVANNES, *Mém. histor.*, IV, 267-308) presents all the characteristics of a historical novel. Tsêng-tzŭ is certainly 曾參 Tsêng Shên, a disciple of Confucius (GILES, *Biogr. Dict.*, No. 2022), and one of the paragons of filial piety. But, in the sources available to me, I find no trace of the tradition alluded to by the *Liu-tzŭ* for either Duke Wên of Chin or Tsêng Shên. At any rate, this « planting of sheep » has no direct bearing on the story of the « ground born sheep » which we have next to investigate.

Before passing on to it, I must say something, however, of a text which may serve as a link between the two traditions. Yüan Hsiao-chêng had not only written a preface to *Liu-tzŭ*, but also a commentary. Ch'ien-lung's Commissioners say that both have been lost since Ming times, but, since they mentioned that *Liu-tzŭ* was included in the *Tao-tsang*, they might have known better: not only the text, but the commentary is there (cf. WIEGER, *Canon taoïste*, No. 1018), though without any preface; the Sung edition described in the *Ch'in-ting t'ien-lu lin-lang shu-mu hou-pien*, 5, 19-20, which gives the commentary, also lacks the preface. On the passage translated above, Yüan Hsiao-chêng's commentary says (*Tao-tsang*, Commercial Press ed., 9, 4*b*): « Wên of Chin, studying [the methods of] foreign countries, sowed husked rice; although the seeds did not grow, [the text means] to say that his designs were great... Tsêng Shên of the Lu kingdom, studying [the methods of] men of foreign countries, cut into small pieces (剉 *ts'o*) the skin of sheep, and sowed them with some earth; although they did not grow, his designs were great. » However unsatisfactory, Yüan Hsiao-chêng's gloss implies that there was an early tradition, which I cannot trace, about the sowing of pieces of sheep skin.

The earliest account in the long series of parallel texts concerning the « ground born sheep » occurs in Chang Shou-chieh's commentary on the *Shih chi*, ch. 123 (TAKIGAWA ed., 123, 12; in his reconstruction of the extant fragments of the *K'uo-ti chih* of 642, SUN Hsing-yen [*Tai-nan-ko ts'ung-shu* ed., 8, 13*a*] holds that the quotation is not taken by Chang Shou-chieh directly from the *I-wu chih*, but is part of a passage of the *K'uo-ti chih* in which the quotation was already included; I do not see, however, any serious ground for such an opinion): « 宋鷹 Sung Ying's »

異物志 *I-wu chih* says : 'North of 秦 Ch'in, in a small borough (邑 *i*) in its dependency, there are lambs (*yang-kao*) who are born spontaneously in the ground. [People] wait until they are about to shoot forth (萌 *mêng*), and [then] build enclosures around them, for fear that beasts [might] eat them. The navel (臍 *ch'i*) of [these lambs] is attached to the ground; if it were separated by cutting, they would die. Instruments are beaten to frighten them; they then shriek in terror, and the navel then breaks [of itself]. Thereupon [the lambs] set out in search of water, and form herds.' » Chang Chou-chieh's commentary is dated 736, but Sung Ying's *I-wu chih* is of course more ancient. CHAVANNES, who first drew attention to the passage (*TP*, 1907, 183-184), remarked that, if the name Sung Ying was to be corrected to 朱應 Chu Ying, we should have then a quotation from Chu Ying's 扶南異物志 *Fu-nan i-wu chih* (« Account of the *mirabilia* of Fu-nan [= Cambodia] »), written c. 250 A. D., but that I had shown (*BEFEO*, III, 276-277) that such a correction was not beyond dispute. It is true that, in 1903, I expressed surprise at the fact that the few extant quotations from a work written on the return from a mission to Cambodia should all deal with Central Asia (Ta-Yüan = Ferghâna; the Great Yüeh-chih) and the Mediterranean Orient. But it is fact that both the *Sui shu* and the *T'ang shu* speak only of the work of Chu Ying, while the quotations give the name of the author as Sung Ying; one of the forms may easily be accounted for as being a graphic corruption of the other, and there is no likelihood that there should have been two different works, one by Chu Ying, the other by Sung Ying. On the other hand, the companion work to Chu Ying's, written by the chief envoy K'ang T'ai, is often quoted for passages relating to India, Central Asia, and the Mediterranean Orient. Finally, all the quotations attributed to the work of Sung Ying actually refer to conditions which are those of the 3rd cent. We know, moreover, that K'ang T'ai and Chu Ying met in Fu-nan an envoy from the Ta Yüeh-chih (cf. *BEFEO*, III, 271; *Études asiatiques... de l'EFEO*, II, 250). So I think that the author of the *Sui ching-chi chih k'ao-chêng* (6, 22 b) was right in regarding Chu Ying and Sung Ying as one and the same man. I have little hesitation in attributing to the middle of the 3rd cent. the text on the « ground born lambs » quoted by Chang Shou-chieh. On the other hand, Chang Shou-chieh quotes the passage when commenting on Ta-Ch'in, and there is no doubt that we must read, at the beginning of the text, « North of Ta-Ch'in » instead of « North of Ch'in ».

Although without mention of the source, the same text, sometimes abridged, sometimes more complete, has been made use of by several authors of the T'ang period.

In ch. 238 of the *Yüan-chien lei-han*, an encyclopaedia of the beginning of the 18th cent., a notice on Ta-Ch'in includes a text said to be taken from the [*Hou*] *Wei shu*, which contains, among others, the passage on the *shui-yang* and the text on the « ground born lambs »; it has been translated by PAUTHIER (*De l'authenticité de l'inscription nestorienne*, 39), and is alluded to by HIRTH (*China and the Roman Orient*, 54). But there is nothing of the kind in the corresponding section of the *Wei shu* (ch. 102). It is true that the original redaction of ch. 102 of the *Wei shu* is lost, and has been replaced by the corresponding section in the *Pei shih* (with the suppression of the passage which, in the *Pei shih*, concerned the Sui dynasty). But we know, from the quotations of the original *Wei shu* in the *T'ai-p'ing yü-lan*, that from the outset the redaction of the two chapters was practically identical. Even if we should suppose it to have been different in some paragraphs, the compilers of the *Yüan-chien lei-han* had no more access to the original

ch. 102 of the *Wei shu* than we have ourselves, and there is no earlier encyclopaedia known which might have preserved such a section and from which the *Yüan-chien lei-han* might have quoted. There is nothing in that would-be quotation from the [*Hou*] *Wei shu* which we do not find in earlier or later sources. I consider that it is a *rifacimento*, a more or less conscious forgery, which cannot be adduced as being a quotation of the legend of the «ground born lambs» of a date earlier than Chang Shou-chieh.

In its section on Ta-Ch'in (193, 4a), the *T'ung tien*, completed in 801, gives the same text as Chang Shou-chieh, except that, the whole paragraph being devoted to Ta-Ch'in, it simply begins here with «To the north...» (without «Ch'in» or «Ta-Ch'in»). More important is another discrepancy: instead of the final 爲群 *wei ch'ün*, «they form herds», the *T'ung tien* gives 無群 *wu ch'ün*, «they have no herds».

The *Pei-hu lu* was completed c. 875 (cf. *BEFEO*, ix, 223). We read there (*Shih-wan-chüan-lou ts'ung-shu* ed., I, 21 b): «Moreover, the Fu-lin kingdom has lambs which are born in the ground. The people of the country wait until they are about to shoot forth (*mêng*), and [then] build enclosures to hold them, so as to prevent outside beasts from devouring them. The navel of these [lambs] is attached to the ground; if it were cut, they would die. But men don cuirasses, mount on horseback and beat drums to frighten them. The lambs shriek in terror, and the navel breaks [of itself]. Thereupon they set out in search of water and pasture.» There is here no question of the herds.

Next comes the *Chiu T'ang shu* (198, 12a), which gives, in the section on Fu-lin (< \*Frōm = Rōm) or Ta-Ch'in, a text identical with that of the *Pei-hu lu* (it is mistranslated by HIRTH, *China and the Roman Orient*, 54).

The *T'ai-p'ing huan-yü chi* (184, 4a) copies the text of the *T'ung-tien*, and, like the *T'ung tien*, says that the lambs do not form herds.

The text in the *T'ang hui-yao* (99, 23a) is the same as in the *Pei-hu lu* and the *Chiu T'ang shu*.

The *Hsin T'ang shu* (221 B, 8a) would represent the same text as is found in the *Chiu T'ang shu* in terser literary style, except that it ends like the *T'ung tien* with 不能群 *pu-nêng ch'ün*, «they cannot form herds» (介馬 *chieh-ma*, «in armour and on horseback», has been misunderstood by HIRTH, *loc. cit.* 60).

The *T'ung chih* (196, 25a) copies the *T'ung tien*, and the *Wên-hsien t'ung-k'ao* (339, 1a) copies the *T'ung chih* (LAUFER, *The Story of the Pinna*, 116, was mistaken when he believed that the author of the *Wên-hsien t'ung-k'ao* was indebted to Chang Shou-chieh's quotation). A much abridged version of the same text occurs in the *K'ung liu t'ieh* (*Po K'ung liu t'ieh*, 96, 24a).

So far for the story of the «ground born lambs». All the texts go back ultimately to one original, that which was incompletely quoted by Chang Shou-chieh, and which, unless we suppose that Chang Shou-chieh made an error in indicating his source, must belong to a work written as early as the middle of the 3rd cent. So this legend is practically contemporaneous with that of the «water sheep». It has two remarkable features: one is the armour donned by men on horseback to frighten the lambs, the other that the lambs did not form herds, since the isolated statement to the contrary in Chang Shou-chieh's quotation is almost certainly the result of a corruption.

But, while the texts concerning the « water sheep » and the « ground born lamb » were transmitted from author to author in a purely bookish and stereotyped form, the second tradition was revived in the Mongol period, probably by an impulse from abroad, and is then attested from various sources.

On returning from an embassy to Chinghiz-khan in the beginning of 1222, the Chin envoy Wu-ku-sun Chung-tuan gave an account of his journey which has been preserved in a work by 劉祁 Liu Ch'i, the preface of which is dated 1235 (Liu Ch'i lived from 1203 to 1250; the date « 1295 » given in *Br*, I, 25, is wrong by a whole cycle). He says of the Mussulmans that « their clothes, coverlets, cushions and curtains are all made of sheep's down; this down is planted into the earth » (其衣衾茵幕悉羊毳也。其毳植於地; cf. WANG Kuo-wei's, *Ku hsing-chi chiao-wei*, 6 b; *Br*, I, 31). Of course, cotton is meant.

Yeh-lü Ch'u-ts'ai, who spoke of « cotton » as *ch'ü-shun* (cf. *supra*, p. 466), says in one of his poems on Russian Turkestan (*Chan-jan chü-shih chi*, *Ssü-pu ts'ung-k'an* ed., 6, 5 b) : « They do not dress with *lung-chung-yang* » (? « sheep planted on hillocks »; 無衣攏種羊 *wu i lung-chung-yang*); and elsewhere (12, 13 b) : « The western lands have good conditions; [but] in general they have no silkworms or mulberries. Every family plants *mu-mien* (cotton); this is the *lung-chung-yang*. » Shêng Ju-tzü, the author of the *Shu-chai lao-hsieh ts'ung-t'an* (*Chih-pu-tsu chai ts'ung-shu* ed., I, 5 b), who quotes the first passage after his extracts from Yeh-lü Ch'u-ts'ai's Diary, adds that the term *lung-chung-yang* also occurs in the *T'ang hui-yao*; but this is a mistake; the *T'ang hui-yao* merely speaks of lambs born in the ground (cf. *supra*, p. 513).

The Taoist Ch'iu Ch'ang-ch'un also has an interesting text on the textiles of the Mussulman countries (*Hsi-yu chi*, WANG Kuo-wei ed., ch. I, 24 b-25 a) : « They produce silks (帛 *po*), which are called 禿鹿麻 *t'u-lu-ma*; this is what is vulgarly said to be woven with the 'wool of sowed sheep' (種羊毛 *chung-yang mao*). » BRETSCHNEIDER's translation (*Br*, I, 70), « vegetable wool », does not do justice to the text, nor can I agree with WALEY's (*The Travels of an Alchemist*, 86) « sheep's wool planted in the ground »; it is the sheep, not the wool, which is said to be planted. *T'u-lu-ma* occurs a second time (ch. 2, 5 a), written 禿鹿馬 *t'u-lu-ma*; this time, it has been omitted by BRETSCHNEIDER (*Br*, I, 95), and rendered « cotton stuff » by WALEY (p. 114). Elsewhere, Ch'iu Ch'ang-ch'un refers twice to *po-tieh* (ch. 2, 1 b, 2 b), the second time in a poem which has been left out by the translators; in the first case, BRETSCHNEIDER (*Br*, I, 89) translated *po-tieh* « woollen stuff, of a white colour », and WALEY (p. 107) « cotton ». Unless Ch'iu Ch'ang-ch'un misused a term which was then no longer familiar, « cotton » only can be right. As to the 帛 *po* which is called *t'u-lu-ma*, BRETSCHNEIDER was content with calling it « a kind of stuff », and WALEY, who elsewhere always renders *po* as « silk » or « plain silk », has also dodged the difficulty by speaking of « stuff ». Both translators eschewed « silk », because they knew that the « wool of sowed sheep » could only be cotton. *T'u-lu-ma* does not provide a ready solution. The phonetic analogy of *t'u-lu-ma* with *to-lo-ma* of the modern « *to-lo-ma* of Kuang-tung » (cf. *supra*, p. 431) is a mere coincidence. WANG Kuo-wei's opinion (I, 24 b) that *t'u-lu-ma* is identical with *tou-lo-mien* or *tu-lo-mien*, « *tula* floss », is of course valueless. On the other hand, even for the sake of rhythm, Hsü Sung (*Hsin-chiang fu*, 22 a) ought not to have divided the term as if it were *t'u-lu* + *ma* (« hemp »); *t'u-lu-ma* is clearly a transcription. BRETSCHNEIDER (*Br*, I, 70) referring to

TROTTER's report on his journey to Khotan (1874, p. 155), says that *tölma* is still the name of a stuff in Chinese Turkestan. But the word cannot have been correctly noted by TROTTER. I have no doubt that what is referred to by Ch'iu Ch'ang-ch'un is the stuff which RADLOV's Dictionary (III, 1190, 1259, 1566) variously transcribes *torma*, *tormai*, *törmä*, and *türmä*, clearly because he had no data as to the true vocalization of this Čayatai word, written تورمه or تورما; I should think that *turma* is the correct form. RADLOV adds that the word is borrowed from Pers. تورمه, for which I can find no such meaning (cf. *TP*, 1931, 422-423). But the main point is that all the Čayatai dictionaries speak of *turma* neither as silk, nor as cotton, but as a very fine woollen stuff. So it seems that there is a double inaccuracy in Ch'iu Ch'ang-ch'un's statement, first when he speaks of the *t'u-lu-ma* as *po*, «silk» and secondly when he identifies it with the «wool of the sowed sheep», which certainly is cotton.

In 1259, 常德 Ch'ang Tê set out as an envoy from Qubilai to Hülägü in Persia. The account of his journey was written down by 劉郁 Liu Yü in 1263 and entitled 西使記 *Hsi-shih chi*. Though there is a remote possibility that Liu Yü may have added some particulars among the *mirabilia* at the end, as Shêng Ju-tzü did when copying Yeh-lü Ch'u-ts'ai's *Hsi-yu lu*, we may on the whole consider the *Hsi-shih chi* as a fairly faithful account of what Ch'ang Tê had seen or heard in the West. In the *Hsi-shih chi* the following passage occurs (WANG Kuo-wei ed., 10 a; cf. *Br*, I, 154; LAUFER, *The Story of the Pinna*, 123) : «The 壠種羊 *lung-chung-yang* is produced in the Western Sea (Hsi-hai). The navel (臍 *ch'i*) of the sheep is sown (種 *chung*) in the ground and watered. On hearing thunder, [the sheep] comes out (lit. is born), the navel [remaining] attached to the ground. When full-grown, [the sheep] is frightened by [the striking of] wood (i.e. wooden instruments); the navel (i.e. the umbilical cord) breaks off, and [the sheep starts] browsing grass. On reaching autumn, [the sheep] can be eaten. In its navel, there are again seeds [to be planted].» BRETSCHEIDER's rendering «the flesh of the navel» instead of «in its navel» is due to a faulty reading 臍肉 *ch'i-jou* instead of 臍內 *ch'i-nei*.

In the above texts we find the expression *chung-yang*, «sowed sheep», which is clear, and another, 壠種羊 *lung-chung-yang*, which translators have been content to render as «sheep planted on hillocks», without comment on the term itself. The character 壠 *lung* is a variant of 壟 *lung*, which means «hillock», «mound»; but we never find this second, and more common form in the texts speaking of the *lung-chung-yang*. The term seems to have baffled LI SHIH-CHÊN, who, in the *Pên-ts'ao kang-mu* (50 A, 34 b), writes it 瓏種羊 *lung-chung-yang*; from the *Pên-tsao kang-mu*, this had passed into the *T'u-shu chi-ch'êng*, *ch'in-ch'ung-tien*, 112, 16 b (the account in the *T'u-shu chi-ch'êng*, mentioned by LAUFER, *The Story of the Pinna*, 116, 123, as an independent source, is merely a *verbatim* copy of the whole *Pên-ts'ao kang-mu* paragraph). But 瓏 *lung*, which is the designation of a gem, makes still less sense than *lung*, «hillock». My impression is that the usual *lung-chung-yang* is the outcome of some confusion. *Chung* means «seed», «to sow», but also «race», «breed». Now, there has been in China more than one product which was said to be 龍種 *lung-chung*, of the «dragon-breed». Just before the *lung-chung-yang*, the *Hsi-shih chi* speaks of the 龍種馬 *lung-chung-ma*, «horses of the dragon breed» (*Br*, I, 153). The *lung-chung-yang* comes out of the ground («is born») when it hears thunder, and thunder is connected with the dragon. In the earlier texts on the «ground born sheep», the armoured men



on horse-back, beating drums to frighten the sheep, may be related to the scaly dragon, which, on the other hand, is associated with water, and this would provide a link with the « water sheep » of the *Wei lio*. According to the *Hsi-shih chi*, the sheep are frightened by beating wood; but « wood » is the element of the east, and the east is the quarter assigned to the « blue-green dragon ». So I think that there is a fair chance that the term *lung-chung-yang*, when first created, meant « dragon-breed sheep ». It is more difficult to say why this *lung*, « dragon », was changed to *lung*, « hillock ». Perhaps this was due to the fact that the sheep was « sowed », and that, as a consequence, *chung* came to be understood in its other meaning « to sow »; hence the shortened form *chung-yang*, « sowed sheep », occurring more than once in the Mongol period. With such an interpretation, *lung* had become meaningless, since the sheep were not « dragon-sowed sheep »; *lung*, « hillock », was then adopted as a makeshift. The reason for its choice may be that it could be accounted for by the raised ridges (cf. *supra*) in the cotton fields. But I readily admit that my tentative explanation would be safer if we could trace at least one example of *lung-chung-yang* written in such a way as actually to mean « dragon-breed sheep ».

A well-known author of the Mongol period, 吳萊 Wu Lai (*tzü* 立夫 Li-fu; 1297-1340), has written (*Yüan-ying chi*, *Ssü-pu ts'ung-k'an* ed., 4, 6 a-b; cf. CH'EN Yen's *Yüan-shih chi-shih* 6, 18-19) a poem entitled 西域種羊皮書褥歌寄李仲羽 « Song of a pillow, written on the skin of a sown sheep of the Western Countries, sent to Li Chung-yü »; Chung-yü is the *tzü* of 李翼 Li I. The poem says: « In Persia in the valleys spirits speak at night. In Persia sheep are kept for various purposes. Stabbed with a knife on the right spot the sheep may be eaten; but within a vallum the shin-bones of the sheep are left as seed. All round them a fence is built, and when the sound of the pounding is heard lambs are born again from the shin-bones. Green grass springs up densely, but the navels are not yet severed [so that the lambs cannot reach the grass(?)]. But when the iron-shod horse hoofs gallop round the fence, the lambs rear up on their hind legs [breaking the umbilical cords], and fall back on the grass... » The rest of the poem is of no interest for the present inquiry. LAUFER's « vertebra of the neck » (*The Story of the Pinna*, 123) is due to a misprint, 頸 *ching* for 脛 *hsing*, in the *T'u-shu chi-ch'êng*.

Wu Lai's poem is alluded to in a curious text of 姚桐壽 Yao T'ung-shou (first half of the 14th cent.) which is included in his 樂郊私語 *Lo-chiao ssü-yü* (*Pao-yen-t'ang pi-chi* lith. ed., f. 13; part of it has been quoted at second-hand, and poorly translated by SCHLEGEL, *The Shui-yang*, 25). Yao T'ung-shou says: « The great master (大師 *ta-shih*) 楚石 Ch'u-shih is a venerable old monk (*sha-mên*); when he followed the Imperial cart to Shang-tu (see 'Ciandu'), he wrote the poems 'North of the Desert' (漠北 *Mo-peï*), 'Cherishing the past' (懷古 *Huai-ku*; or the poem 'North of the desert, cherishing the past'), etc. In them (or 'in it') I read 'They say themselves that sheep can be sowed, [but] do not believe that cocoons can give silk'. I was then in doubt whether he meant that sheep could be sowed. So I questioned the master on the point. The master said: 'West of the Great Desert (大漠迤西 *Ta-mo i hsi*), people can sow sheep. Every time they slaughter a sheep, they use its skin and flesh, but keep its bones, which they bury in the ground on the first 未 *wei* day of the winter (*wei* is the cyclical sign corresponding to « sheep » in the duodenary cycle). When reaching the first *wei* day of the last month of spring, they blow flageolets and pronounce spells, and then lambs (子羊 *tzü-yang*) come out of the ground; for

each set of bones buried, they can get several lambs; this is a transformation outside the pale of the four kinds of birth; it is not to be wondered at; it is because China does not have it that doubts have arisen.' Later I read the 'Song of the book cushion [made] of the skin of a sowed sheep of the Western Countries' by Wu Li-fu (= Wu Lai), which says... [Yao T'ung-shou here copies the whole poem, and adds :] (Wu Lai) says that it is the shin-bone which is sowed, which is not in agreement with what the Master [Fan]-ch'i saw with his own eyes. It must be that in Persia there is another method of sowing, in agreement with what was heard in Wu's poem.» Ch'u-shih is the *tzū* of 梵琦 Fan-ch'i, a well-known Buddhist priest who lived from 1296 to 1370; Fan-ch'i seems to have lived in northern China only in 1321-1323; his information would thus go back to that period. But it will be noticed that he ascribes the «sowed sheep» to regions west of the Great Desert, and that, contrary to Yao T'ung-shou's statement, he does not say that he saw it himself.

Another poet of the Mongol period, 白璣 Po T'ing, *tzū* 廷玉 T'ing-yü (1248-1328), has written an obscure poem entitled 續演雅十詩 *Hsü Yen-ya shih-shih*, «Ten verses in continuation of the *Yen-ya*», which is preserved in his 湛淵集 *Chan-yüan chi* (edited as 湛淵遺稿 *Chan-yüan i-kao* in *Chih-pu-tsu-chai ts'ung-shu*; the poem is in ch. 2, 5-6; the original *Chan-yüan chi* is lost, and the present text has been re-compiled from the fragments which have survived; the poem was copied in 1366 by T'ao Tsung-i into his *Cho-k'eng lu*, 9, 3 b). The fourth verse is 嬰啼聞木枝 羝乳見茅茹 何如百年身 反爾無根據 «The wailing of the baby is heard on the branch of the tree; the breast of the ram is seen among the blades of grass. How is it that a body which is to last a hundred years has on the contrary no root to which to be attached?». A commentary, almost certainly due to Po T'ing himself, accompanies every verse, and the gloss on the fourth one is as follows: «North of the Desert (*mo-pei*), they sow sheep's horns and [these] can give birth to sheep, which are as big as hares. They are fat and very good to eat. As to 'the baby-girl wailing on the branch of the tree', see what is reported in the *Shan-hai ching*.»

I do not know to which passage of the *Shan-hai ching* Po T'ing may allude. More than once, in its descriptions of fabulous animals, the *Shan-hai ching* says that their «voice» is like that of a baby, but there is nothing in the whole work which recalls the sentence used in the *Hsü Yen-ya shih-shih*. It may be a misquotation altogether, or occur only in some commentary where it has escaped me; at any rate, it is not to be found in Kuo P'o's *Shan-hai ching t'u-tsan* (*Ch'ün-shu shih-pu* ed., ch. 19). If I have properly understood Po T'ing's meaning, he opposes creatures whose lives are short, in spite of the fact that they remain tied to the place of their birth, to man, whose span of life can extend to a hundred years, although he has no «root» from which he would always receive strength and food.

The wailing baby attached to the branch of the tree seems to be a variant of the famous story of the *wāqwāq* tree, mainly known from Mussulman and Chinese sources. For China, it had been quoted in 1883-1886 by SCHLEGEL from the *Wên-hsien t'ung-k'ao* (in DE GOEJE's Appendix to DEVIC, *Merveilles de l'Inde*, 303), then by TERRIEN DE LACOUPERIE (*Transactions of the Ninth Congress of Orientalists*, II [1893], 901-902), who traced it to the *Chiu T'ang shu* (in a combined edition of both *T'ang shu*), and supposed that the tale had been brought to China by the Arab embassy of 713. In a review in *TP*, 1904, 486-487, CHAVANNES, unaware of TERRIEN's

paper, quoted in his turn the same story from the *T'ung tien*, and since Tu Yu, the author of the *T'ung tien*, quotes various passages of the lost account written by his kinsman Tu Huan after having returned from the Arab countries in 762, CHAVANNES supposed that Tu Huan had been the propagator of the legend in China; FERRAND (*JA*, 1932, I, 199) sided with CHAVANNES. In fact, both dates, 713 as well as 762, are too late. The passage is not included among those which Tu Yu quotes from Tu Huan's work, but, on the contrary, Tu Yu states most definitely that the story was told by the Arab envoys who came in *yung-hui* (650-655), in fact in 651; these envoys were those who said that their kingdom was then in its 34th (lunar) year, and was ruled by its third ruler. So the story of the *wāqwāq* tree in China must be dated 651 A.D., and is more ancient than any Arabic account.

It occurs in fact in other Chinese works than the three works quoted hitherto, but always in the same form. The texts are : (1) 述異記 *Shu-i chi* (ch. I, 13 a), attributed to 任昉 *Jên Fang* (460-508). But that a work of c. 500 should speak of the Ta-shih (Arabs), known to the Chinese only from the middle of the 7th cent., is of course a blatant anachronism. In fact, the *Shu-i chi* is a spurious work which is not earlier than the 8th or the 9th cent. (cf. *Ssü-k'u* . . . , 142, 43-44). (2) The *T'ung tien* (193, 8 b), completed in 801. (3) The *T'ang hui-yao* of 804 (on which cf. *JA*, 1913, I, 262), as quoted in the *Pei-hu lu* of c. 875 (*Shih-wan-chüan-lou ts'ung-shu* ed., I, 14 b). (4) The *Chiu T'ang shu* (198, 12 b), and the early quotations from it in *T'ai-p'ing yü-lan* (796, 14 a, and 961, 3 b). (5) The *T'ang hui-yao* of 961 (100, 12 a; cf. *JA*, 1913, I, 262.) (6) The *T'ai-p'ing huan-yü chi* (186, 13 b). (7) The *T'ung-chih* (196, 31 b). (8) The *Wên-hsien t'ung-k'ao*, 339, 7 a. All are derived from one and the same original. The most complete of the ancient texts is that of the *T'ung tien* : « [The envoys of 651] moreover said : 'Their king had sent (常 *ch'ang* = 嘗 *ch'ang*, given in the other texts) a man on a boat, with clothing and provisions; [the man] went to sea, and sailed across it for eight years without reaching the western coast. In the middle of the sea, he saw a square rock; on the rock there was a tree, with red branches and blue-green (*ch'ing*) leaves. On the tree small children (小兒 *hsiao-êrh*) had been born all over, six or seven inches high. On seeing men, they did not speak, but all could laugh and wave their hands and feet (I think that CHAVANNES mispunctuated the text here, and that TERRIEN DE LACOUPERIE, 902, was right; but the author of the *Tung-hsi yang-k'ao*, 12, 14 b, read the text as CHAVANNES did). Their heads were attached to the branches of the tree. When people plucked [some of them] and took them into their hands, they immediately became dry and black. The envoy took a branch [of the tree] and went back; now [the branch] is kept at the residence of the king of the Ta-shih.» SCHLEGEL (in DE GOEJE, *loc. cit.* 303) added that «the name of the tree was *ie-mie*». As CHAVANNES remarked (*TP*, 1904, 487), there is nothing of the kind either in the *Wên-hsien t'ung-k'ao*, or in the *T'ung tien*, or, I may add, in any of the parallel texts relating the story. The curious fact is that TERRIEN DE LACOUPERIE too (p. 902) gives the name, though as 椰木 *yeh-mu*, «coconut-tree»; but he knew SCHLEGEL's note, and seems to have tacitly corrected it so as to fit his own identification of the tree. Although the tree is given no name in Chinese texts, it is clear that it is the same as the *wāqwāq* tree of Arabic texts; but it is no less certain that *wāqwāq* can be neither 越國 *Yüeh-kuo* (\*J<sup>h</sup>et-k<sup>w</sup>ək), «South China and northern Indo-China», as thought by TERRIEN DE LACOUPERIE,

nor 倭國 *Wo-kuo* (\*·Uâ-k<sup>w</sup>ək), Japan, as maintained by DE GOEJE. FERRAND was right (*JA*, 1932, 196-243) in identifying it with Madagascar (with a second *Wāqwāq* probably in Sumatra). Most of the Arabic texts speak of «women» instead of «small children», and none mentions the laughter, but only the particular sound «*wāq, wāq*», uttered by the fruit in human shape. Of the minor discrepancies in the Chinese texts, one only is of some interest. While most texts say that the «small children» had the «head» (頭 *t'ou*) attached to the tree, the *T'ang hui-yao* of 961 merely says «since» (既 *chi*) they are attached, and the earlier *T'ang hui-yao* of 804, as quoted by the *Pei-hu lu*, says that they were attached to the tree by the «coccyx» (尻 *k'ao*). «Since» is certainly corrupt. In view of the text of Al-Jahiz, which says (FERRAND, 202) that «the *Wāqwāq*» hung on the tree «by their hair», «head» is probably correct. [Children (*wa-wa*) attached to branches is a familiar subject of decoration on Sung pottery, particularly Ting-chou ware; see the *T'ao-shuo*]. I do not think that the still earlier legend of the «head of the king of Yüeh», which is the coconut, is really the prototype of the story as TERRIEN DE LACOUPERIE tried to show, but some contamination between the two legends is not excluded. Moreover, there are parallel Indian stories which FERRAND, I do not know why, has ignored: the *ṛṣi Vālakhilyas*, one inch high, who hung upside down on a tree (cf. TERRIEN DE LACOUPERIE, 899); Al-Birūnī's «*Khastha*, i. e. people who are born from the trees, hanging on them by the navel-strings» (SACHAU, *Alberuni's India*, I, 302); the men born of trees, whose lives lasted but one day, of the *Saddharmasmṛtyupasthāna*, in whom S. LÉVI, perhaps too rashly, saw the prototype of the Arabic *wāqwāq* (cf. *JA*, 1918, I, 33, 144-145); finally, in Malabar, according to Odoric's Italian ms. in Florence (not in *Wy*), the «trees which bear men and women like fruit upon them. They are about a cubit in measurement, and are fixed to the tree up to the navel, and there they be; and when the wind blows they be fresh, but when it does not blow they are all dried up. . .» (*Y*<sup>1</sup>, II, 138-139, 343; I suspect that *insino al bellico*, «up to the navel», is due to some misunderstanding, and that the original statement was that the creatures were attached to the tree «by the navel», as in Al-Birūnī).

It seems to me probable that Po T'ing's babies whose wailing was heard on the branches of the tree are *wāqwāq*. But it will be noticed that the only Chinese source on the subject, passed on from one author to the other, makes them laugh, not wail. The laughing does not appear in Mussulman sources, and unless there is in the commentaries of the *Shan-hai ching* some corresponding passage which I have failed to discover, I am led to the conclusion that Po T'ing is alluding to a new form of the legend which had reached China under the Mongols. His comment on the «sowed sheep» has also some features which are not found elsewhere in Chinese sources: the statements that it was the horns which were sowed, and that the size of the sheep was that of a hare. On the second point, I have no parallel to quote, but, for the «horns», I may mention that, according to an Arabic alchemist of the early 12th cent., «seeds are produced by planting the horns of hoofed animals» (LAUFER, *The Story of the Pinna*, 118). That Po T'ing should have here original information of western origin is not a matter of surprise, since there is at least one other similar case. In *TP*, 1917, 1-30, LAUFER has devoted an important paper to the mandrake, and shown that the 押不蘆 *ya-pu-lu* described by Chou Mi (1232-1308) in two parallel passages represented the Arabo-Persian name *yabrūḥ* of that plant. But the fact that

Chou Mi's informant, 白廷玉 Po T'ing-yü, was no other than our Po T'ing seems to have escaped him, and also that a verse, with a commentary, was devoted to the *ya-pu-lu* in the *Hsiü Yen-ya shih shih* (verse and commentary have both passed into the *Cho-kêng lu*; the commentary begins with *mo-pei*, «north of the Desert», as in the case of the «sowed sheep»; so in the case of the *ya-pu-lu*, this *mo-pei* cannot be an arbitrary change in the quotation made from the passage of Chou Mi by LI Shih-chên, as was thought by LAUFER, p. 4, but must come from a third unknown parallel passage in which it really occurred). Po T'ing had named to Chou Mi his own informant, a certain 盧 Lu, *tzü* 松匡 Sung-yai, who unfortunately remains unidentified.

In the miscellaneous works of Ming writers, I have noted only one passage relating to the «sowed sheep» (*chung-yang*); it occurs in the 敵帚齋餘談 *Pi-chou-chai yü-t'an*, by 沈德符 Shên Tê-fu (1578-1642; *Yen-yün i-pien* ed., 6 a-b) : «An ancient saying is : 'Northerners do not believe that the southerners have junks (*chu-lu*) of 10,000 石 *shih*; southerners do not believe that the northerners have tents of 10,000 men; foreigners do not believe that in China there are worms which spit threads, which form cocoons which are unwound to make silk.' True indeed is this saying. The same thing occurs when it is said that people of the Western Countries sow sheep. Whenever this is said to people, many also do not believe it. Their custom, as to the method of sowing, is to take a sheep, flay off his skin and take off his meat, but not to break his bones, which are buried in the earth with the five viscera. The next year, after the spring rains, bubbles (*p'ao*) appear at the place of sowing and spread close to one another. Monks, holding charms, blow conchs and beat drums. When the sound is heard in the earth, countless young sheep immediately jump out, but their umbilical cord (*ch'i-tai*) remains attached to the belly of the dead sheep. The monks again, with a rite of psalmodic recitation, cut it off, and then each sheep runs away. People wait until they have grown up and then eat them. The next year, they again sow them in the same manner, and it never comes to an end. Such is the invariable account given by men of the Western Countries. Many men from China who entered that country have also seen the thing; it is only among those who have not witnessed it with their own eyes that many entertain doubts...» Shên concludes by citing, as very similar to his account, the passage from the *Hsin T'ang shu* and Po T'ing's poem. Although his tale, in which he has absolute faith, has much in common with the other sources, it offers certain details which mark it as of independent origin.

The same cannot be said, however, of LI Shih-chên's paragraph in his *Pên-ts'ao kang-mu* (50 A, 34 b; this has been copied, with all its serious mistakes, into the *T'u-shu chi-ch'êng*, *ch'in-ch'ung tien*, 112, 16 b, and, with the most obvious ones corrected, into the *Tz'ü-yüan hsü-pien*, *ch'ou*, 71). But the very title of the paragraph, 地生羊 *ti-shêng-yang*, «earth-born sheep», is not, as LAUFER thought (p. 116), «an interesting feature», but a mistake. LI Shih-chên took it from his quotation from the *Pei-hu lu*, which he begins thus : «In the kingdom of Ta-Ch'in, there is the 'earth-born sheep'; but the actual text of the *Pei-hu lu* is «In the kingdom of Fu-lin, there are lambs which are born in the earth»; there is no specific name of the animal. It may be that LI Shih-chên devised this name under the influence of 地生毛 *ti shêng mao*, which occurs in the *Shu-i chi* (2, 8 a; cf. also *Sui shu*, 22, 13 a); but this also is no name, and really means «the earth-produced wool». After citing the various texts which speak of the sheep

as being «sowed» by means of the navel, or the shin-bone, or the horns, LI Shih-chên adds : «The three versions are slightly different, and one does not know what was actually sowed; but the truth must be what is said by Liu [Yü] (in the *Hsi-shih chi*). That too is, however, mysterious; such are the wonders of nature!»

Wu Lai's sheep, whose shin-bone was sowed, might be said to be a 骨種羊 *ku-chung yang*, a «bone sowed sheep», and Hsü Sung, who had heard of the tradition, inquired in vain about this «bone sowed sheep» when travelling in Chinese Turkestan from 1812 to 1816 (cf. his *Hsin-chiang fu*, 22 a). HUNG Liang-chi (1746-1809), the author of the 曉讀書齋雜錄 *Hsiao-tu-shu-chai tsa-lu* (*Hung Pei-chiang ch'üan-chi* ed., 1st series, I, 19-20) has been more successful, or more credulous. After repeating Chang Shou-chieh's quotation from the *I-wu chih*, which gives «Ch'in» instead of «Ta-Ch'in», Hung Liang-chi adds on his own account : «This is what is now called *ku-chung yang* ('bone sowed sheep'). When formerly I was in Ch'in (= Shàn-hsi), I asked Ordos tribute envoys, and they said the same (as in Chang Shou-chieh's quotation). They also said that this species was produced only by sowing sheep bones. I suspect that such was also the case in ancient times.»

But this *ku-chung yang*, «bone sowed sheep», had also been popularly altered to 骨重羊 *ku-chung yang*, «sheep whose bones are heavy». A paragraph is devoted to the latter name in the 茶餘客話 *Ch'a-yü k'o-hua* of 阮葵生 JUAN K'uei-shêng (1731-1789; *I-hai chu-ch'ên* ed., 8, 2) : «The *ku-chung yang* ('sheep whose bones are heavy') is produced among the Mussulmans (Hui-tzŭ) outside the boundaries in Pu-ha-la (Bokhara), which can be reached by going some twenty days west of Yeh-êrh-ch'iang (Yarkänd). The sheep produced are of small size and poor in flesh; but their bones are extremely heavy. The Mussulmans did not think much of them, but, after the Great (= Imperial) Army had subdued Yeh-êrh-ch'iang (i.e. after 1758), these [sheep] by degrees penetrated into China. At the beginning, caps were made of their [fur], and they would cost twenty to thirty taels; in recent days they do not cost more than ten taels, and fur gowns are also made of them. Recently I heard that the whole range of mountains to the south-west of An-chi-yen (Andijan) was all [full] of such [sheep]. The black ones, however, are extremely numerous; of grey ones, there is not one out of ten, and the price [of these] is ten times higher. I say that only a few years will pass before these ['heavy bone sheep'] will cost the same as ordinary sheep. After a greater lapse of time Shàn-[hsi] and Kan[-su] will also produce them everywhere. Small boys in the streets speak of 古董 *ku-tung*, or 古銅 *ku-t'ung*, which shows how hearsay tradition can be inaccurate . . . » It is hard to see what JUAN K'uei-shêng means by the last sentence. *Ku-tung*, more usually written 骨董 *ku-tung*, «work of art», «curio», is often said, though without proof, to have been altered from 古銅 *ku-t'ung*, «old bronze» (see the Introduction to the *Ku-t'ung chih* by Li T'iao-yüan), but that offers no relation to the *ku-chung yang*, the name of which does not seem to have ever popularly become *ku-tung* or *ku-t'ung*. And it would be absurd to imagine that *ku-tung*, which goes back at least to Sung times, might have been altered from the *ku-chung* of *ku-chung yang*.

Apart from the final sentence on *ku-tung* or *ku-t'ung*, the same passage, in a somewhat shorter form, occurs in the 西域聞見錄 *Hsi-yü wên-chien lu*, a work on Central Asia by 七十一 Ch'i-shih-i, tzŭ 春園 Ch'un-yüan, which exists also, in more or less different redactions, under

the titles 西域總志 *Hsi-yü tsung-chih* and 西域瑣談 *Hsi-yü so-t'an*. In the ms. copy of the Bibliothèque Nationale (COURANT, *Catalogue*, Nos. 1830-1831, where it is given as anonymous, and cf. No. 707) the passage occurs in ch. 8 (it has been omitted in GUELUY's translation, *Description de la Chine occidentale*, in *Muséon*, 1886-1887). Ch'i-shih-i's work is dated 1778 and is based to a great extent on personal information; but I am not certain that, in the present case, he is not indebted to JUAN K'uei-shêng. A similar passage in 趙慎畛 CHAO Shên-chên's 榆巢雜識 *Yü-ch'ao tsa-chih* (2, 30 a-b) is expressly stated to have been derived from the *Hsi-yü wên-chien-lu*, but CHAO Shên-chên introduces the quotation with the following remark: « Formerly I had heard that in the Western Countries (Hsi-yü) they sowed bones to produce sheep. The method was to sow [the bones] the preceding year; the next year, at the vernal equinox, people would recite spells and beat drums, and the sheep would jump out by themselves. The accounts in the books of stories (*shuo pu*) say the same. . . » At the end of the passage, we read that the « sheep with heavy bones » which were of grey colour were popularly known as 草上霜 *ts'ao-shang shuang*, « frost on grass ». The latter term has remained in use down to our own day as the designation of « grey astrakhan » (cf. GILES's Dictionary, *s. v. shuang*; KIONG and COURTOIS, *Quelques mots sur la politesse chinoise* [Var. Sin. No. 25], 24, 102, and *Tz'ü-yüan*). In the modern name of another fur, 青種羊 *ch'ing chung-yang* (*ibid.* 102), lit. « blue-green sowed sheep », I can see but a survival of the old tale. No modern Chinese author seems to have doubted the existence of this vegetable animal, and an author of the middle of the last century 梁紹壬 LIANG Shao-jên, who speaks of some (very doubtful) freaks of nature in his 兩般秋雨金隨筆 *Liang-pan ch'iu-yü-an sui-pi*, completed in 1837 (*Hsiao-shuo pi-chi tu-kuan* ed., 8, 25 b), is content with the remark that « as to the 'sowed sheep' (*chung-yang*) of the Western Countries, it is still more difficult to explain ».

After quoting the Chinese texts, we must now interpret them, and this will only be possible by making use also of Western documents.

The chief authority is Odoric (*Y<sup>1</sup>*, II, 240-241; *Wy*, 482-483): « Another really marvellous thing may be related, which however I did not see, but heard from trustworthy persons. For it is said that Cadeli (var. Cadelis, Cadili, Calle, Cauli, Caoli, Caloy, Kaloy, Chadli; Maundeville: Cadilla, Caldilhe; cf. also HALLBERG, 89) is a great kingdom, in which there are the mountains which are called Caspei (var. Capesei, Capesci; 'Capei' in *Wy*, 482, is a misprint). And it is said that very large melons (*pepones*) grow there, which, when they be ripe, open of themselves, and a little beast is found inside like a small lamb, so that they have both the melons and the meat that is there. And although that may seem incredible, yet it may be quite true; just as it is true that there be in Ireland trees which produce birds. »

LEE (*The Vegetable Lamb*, 11) says that « Caspius » (in fact, this form does not occur) is « probably an error of transcription for 'Caspian'. The mountain of Caspius (now Kasbin) is about eighty miles due south to the Caspian Sea, and in Persian territory, near Teheran ». LAUFER (*The Story of the Pinna*, 125) repeats and adopts this identification, which he attributes to YULE; but YULE said nothing of the kind, and CORDIER (in *Y<sup>1</sup>*, II, 243) expressly stated that LEE was « here mistaken ». LEE's « Kasbin » is of course Qazvin (see « Casvin »), which is the name of a city, not of a mountain. YULE's own opinion (*Y<sup>1</sup>*, II, 242) was that « the Caspian mountains are of

course the Caucasus, or some part of it, whilst the kingdom of Cadeli is the country on the Ethil, Adil, or Herdil, *i. e.* the Volga ». To support this identification of « Cadeli », YULE adds that « the *c* is constantly substituted for an aspirate by the Italian travellers (*e. g.* Polo's Cormos for Hormuz)... The 'Caspian Hills' and the Volga are at some little distance, but that distance does not subtend a great angle from China where Odoric heard the story ! » LAUFER (124-125) insists on the fact that exactly the same tradition occurs in Odoric, who started on his journey between 1316 or 1318 and returned in 1330, and in Sir John Maundeville, « who travelled in Asia from 1322 to 1356 », and on the other hand that Odoric's text is so near the one found in Ch'ang Tê's *Hsi-shih chi* that their agreement « proves that both have reproduced with tolerable correctness a bit of folk-lore picked up by them on Persian soil ». It is astonishing that as well-read a scholar as LAUFER should have still referred to Sir John Maundeville's forgery as the account of a real journey; here, Maundeville merely copies Odoric. As to YULE's identifications of « Cadeli » and the « Caspian mountains », they are mainly due to the remarkable coincidence between Odoric's text and various legends of the 16th cent. which I shall soon discuss. But the equation of « Cadili » with a name of the Volga is far from being evident. YULE's comparison with Hormuz > Cormos is misleading. There is really an *h*- at the beginning of Hormuz, but there is none at the beginning of Atil or Etil, the Turkish name of the Volga. YULE's variant « Herdil », taken from RAMUSIO (*cf.* vol. I, 994), can only be an accidental spelling, probably in some corrupt text. But the true mediaeval Turkish form is *Ātil* or *Ätil* (so in Kāšyari's ms.; BROCKELMANN's « Ytyl » [p. 244], *i. e.* *Ītil*, is a mistake), in some later dialects « Ädil » and « Edil » (*cf.* RADLOV's Dictionary, I, 842, 857); *Ātil*, in the *Hudūd al-Ālam* (*cf.* MINORSKY's Index, *s. v.* « Ātil » and « İtil »), seems to represent an older not palatalized pronunciation (*cf.* MARQUART, in *Ungar. Jahrbücher*, IX, 96). The form is confirmed by the early Byzantine transcriptions 'Αττῖλας, 'Ατῖλ, 'Ετῖλ, 'Ατᾶλ, and also by Rubrouck's « Etilia » (*cf.* ROCKHILL, *Rubrouck*, 107) and by the « Edil » of the Catalan Map and of a Genoese document of 1374 (*cf.* Y<sup>1</sup>, I, 307; II, 105). The incorrect forms « Erdir » and « Erdil » occur in Josafa Barbaro (RAMUSIO, II, 98 v<sup>o</sup>). So YULE's explanation of the *c*- of « Cadeli » falls to the ground. No less erroneous is LAUFER's view that Odoric had picked up the story « on Persian soil ». Odoric narrates the story when he speaks of his stay at « Cambalec », *i. e.*, Peking, and YULE was right in stating that Odoric had heard the story in China. There is no wonder in this, since we have seen that it was current in northern China in the first half of the 14th cent. Finally « Cadeli » is probably a misreading. CORDIER (*Odoric de Pordenone*, 432) has derided L. DE BACKER for saying that the « great country » meant was Corea. As a rule I do not think highly of DE BACKER's scholarship, but it is a fact that « Caoli » or « Cauli » is very nearly the form which would best account for the various readings, and it is the very name which was given to Corea in Odoric's time (see « Cauli »). All the mediaeval accounts of the « sowed sheep » locate it outside of China proper, north or west of the Desert. If the name used by Odoric was really the one then generally employed for Corea, it is true that Corea lies neither « north » nor « west » of the Desert, but it is a far away country to the north-east of China, which Odoric knew only by name, where he might have misplaced the legend. But how are we to account for the « montes Caspei », which, as YULE said, certainly are « Caspian mountains » ? Well, they are « Caspian mountains », but that does not imply that they are « of course Caucasus », as YULE believed them to be. ODORIC



mentions the « montes Caspei » in the present passage only; but, from the *Libro del conocimiento* (Wy, 567), we see that, in the 14th cent., the « Caspian mountains » were located both north and south of Mongolia and included at least the Altai and the mountains east of it. I wish to add, however, that I am far from stating that the true form of the name is « Caoli » or « Cauli ». Moreover, Maundeville, while he is indebted to Odoric for the story of the lambs, locates « Caldilhe » or « Cadilla » on the way from Cathay towards « Upper India » and « Bacharia », and, although he mentions Bokhara under another name, his description of « Bacharia » has some features which could fit Bokhara (see « Bucara »). But if so, his source for this geographical location could not have been Odoric, who does not mention « Bacharia ».

When commenting on Odoric's « melons » and « lambs », neither YULE nor CORDIER has referred to certain similar Talmudic accounts, which have been discussed by L. LEWYSOHN (*Zoologie des Talmuds*, Frankfurt, 1858, pp. 65, 356-358), H. LEE (*The Vegetable Lamb*, 6-8) and LAUFER (*The Story of the Pinna*, 120). The Talmudic story seems to date in the first half of the 13th cent. An animal, called *yedua*, with the bones of which witchcraft was practised, was said to issue from the earth like the stem of a plant. As far as the stem (or umbilical cord) stretches, it devours all the herbage round it. LAUFER insisted on the fact that the *yedua* had « human form in face, body, hands and feet ». But these same Talmudic texts had already been adduced c. 1600 in connection with the « borametz » of Tartary, and there the *yedua* is said to have been in the shape of a lamb (cf. Claude DURET, *Histoire admirable des plantes*, Paris, 1605, 8vo, 322-341; A. COLIN's translation, *Histoire des simples medicamens apportés de l'Amerique*, of the work of Nic. MONARDÈS of Seville, 2nd ed., Lyon, 1619, 8vo, 248-262, which is here for the most part a word for word copy from DURET). The close connection of the two stories, in Odoric and in the Talmudic commentary, is unmistakable.

Odoric's story recurs only two centuries later. BRETSCHNEIDER (*Br*, I, 154) alludes to the accounts of the *Agnus scythicus* given by « Hieron. Cardanus, and after him J. C. Scaliger (both in the first half of the sixteenth century) », but refrains from any bibliographical reference. Girolamo CARDANO, who lived from 1501 to 1576, speaks of the *Agnus scythicus* in *De Rerum Varietate* (L. 6, § 22), published in 1557. Nor was SCALIGER's work, entitled *Exotericarum Exercitationum Liber quintus decimus de Subtilitate* (Exercitatio, CLXXXI, § 29), which is always quoted on the subject (cf. Y<sup>1</sup>, II, 241; CORDIER, *Odoric de Pordenone*, 427-428), published « in the first half of the sixteenth century », but in 1557 (not « 1537 » as in HIRTH, *China and the Roman Orient*, 262), and this date allows us to ascertain the source of both authors, to wit Sigismund (or Siegmund) VON HERBERSTEIN's *Rerum Moscoviticarum Commentarii*, which was published in Vienna in 1549 (reprinted in 1551 and 1556; cf. the Italian translation in RAMUSIO, II, 175 v<sup>o</sup>, copied by CORDIER, *Odoric de Pordenone*, 428; see also R. H. MAJOR's translation, *Notes upon Russia*, Hakluyt Soc. 1852, II, 74-75). HERBERSTEIN relates what he had heard from a trustworthy man, Dmitrii Daniilovič (no surname is given), who had accompanied his (Dmitrii's) father on a legation to the Tartar king of « Zavolha » (i.e. beyond the Volga), between the Volga and the « Iaïch » (Yaïq = Ural River). Dmitrii had seen there a plant in the shape of a rounded melon, in which something was born, five « palms » high, similar to new-born lamb; in the local language, it was called « boranetz, i. e. lamb ». It had a very soft fur, with which people used to make caps. This « plant » had

blood, but no flesh, and, instead of flesh, a certain stuff which reminded one of crawfish. It was rooted into the ground by the navel, and pastured all around the root, and when there was no more grass to eat, it would die. Wolves were very fond of [eating] it.

All this has passed into SCALIGER (§ Agnus Scythica) and CARDANO, who, however, altered the name to « borametz » and « barometz », respectively. It is clear that the word meant by HERBERSTEIN is not *barančik*, as said by CORDIER (*Odoric de Pordenone*, 426, and *Y<sup>1</sup>*, II, 242) but *baranec* (*c = ts*), another diminutive form of *baran*, « sheep » (HERBERSTEIN's « borametz » was not a mistake, since *baran* occurs as *boran* in old Russian texts; cf. BERNEKER, *Slav. Etymol. Wörterbuch*, I, 43); by some misunderstanding, HERBERSTEIN attributed to the Tartars the Russian designation used by Dmitrii Daniilovič. In 1607, MARGERET (*L'Etat de la Russie*, 1855 ed. by H. CHEVREUL, xv, 2) describes the « animal plant », without giving its name. This was, however, well known in his time, but SCALIGER's erroneous form *borametz*, or rather CARDANO's *barometz*, had prevailed. In DURET's *Hist. admirable des plantes* and in COLIN's translation of Monardès, a figure of the *barometz* is followed by the opinions expressed on the subject by SCALIGER, CARDANO (1501-1576), G. POSTEL (1510-1581; adduced also by HERBERSTEIN as having heard a similar story from Michael, official interpreter of Turkish and Arabic in Venice; RAMUSIO's translation does not contain this paragraph, in which fur caps made from the « plant » are said to be brought from Samarkand), J. B. PORTA (1550-1615), DU BARTAS (1544-1590), and Blaise DE VIGENÈRE (1523-1596). Although largely indebted to SCALIGER, OLEARIUS (in 1636) correctly gives *boranez* like HERBERSTEIN; and so does J. J. STRUYSS (1669; Dutch ed. of 1686, in-4, 191; the « bonnaret » quoted by CORDIER, *Odoric de Pordenone*, 430, is a mistake of the French edition of 1718). John BELL of Antermomy (*Travels from St Petersburg*, 4to ed., Glasgow, 1763, I, 43-44) comes to a negative conclusion, and so does CHAPPE d'AUTEROCHE in his *Voyage en Sibérie* (1768, fol., I, 346).

Even in the case of travellers who speak partly from personal experience, all the accounts can be traced back to HERBERSTEIN, either directly or through SCALIGER. As to HERBERSTEIN himself, I entertain a suspicion that he may have known either Odoric (perhaps in the French edition of 1529), or the more popular Maundeville, and is indebted to one of them for the « melon »; but I must admit that there is no mention in Odoric or Maundeville of the sheep's navel being rooted in the ground, which finds a counterpart in mediaeval Chinese accounts. Since it also occurs in Talmudic texts, I wonder whether one of these had possibly come to the knowledge of HERBERSTEIN. BRÜCKNER (*Russische Revue*, XXI [1882], 131-132) says that SPRENGEL was the first to connect « erroneously » the Odoric and Maundeville tale with HERBERSTEIN's account and states that J. BAUHIN († 1613) had carefully distinguished the two, making the former vegetable lamb to be MONARDÈS's « dracunculus », i.e. « dragon's blood » (on which cf. *HR*, 197-198). This is an error, and BRÜCKNER must have read very superficially BAUHIN who, in his *Historia Plantarum* (1650), I, lib. III, 405-406, devotes ch. 213 to « dragon's blood », and ch. 214 to « Agnus Scythica, Borometz ». It is in the latter chapter that he speaks both of the Odoric-Maundeville tale, and of the « borometz », for which he is indebted to SCALIGER. According to BAUHIN, the plant « had wool, flesh, and blood, and resembled a lamb in every respect, growing, however, on a stalk a yard high. Turning about on its flexible stem, it consumed the herbage within its reach and then pined away with the failure of the food until it died. » For the mediaeval tale, the mention that the *animal-*

*culum* was *simile Agno sine lana* seems to imply that his source was not Odoric, but Maundeville, at least indirectly. Directly, his information was drawn from « Laurentius Servius in Chronica Germ. anni 1504 » and « Jo. Maria Bonard. de miner. », of which I can trace neither for the present.

As to the name *baranec* and the fur used to make caps, an obvious solution is that HERBERSTEIN, perhaps in agreement with some Turkish tradition, transferred the legend to a local product, in fact to the skin of the stillborn lamb or lamb obtained before birth by opening the belly of the mother, our astrakhan. In 1692, the clearheaded WITSEN (*Noord en Oost-Tartarye*, 1705 ed., I, 288), in a paragraph « De Agno Scyticâ, seu fructu Boronietz (Boranits) » had inserted with approval a Latin note which he had received from KAEMPFER, in which the latter stated that the « borometz seu barannetz » could be nothing else but « the placenta of a lamb taken from the womb of its mother ». This is already, almost word for word, the text which, a few years later (1712), KAEMPFER himself published in his *Amoenitates Exoticae*, fasc. III, 505-508. We have seen that, in the 18th cent., the « sheep with heavy bones » of the Chinese, which, through a mistake in the meaning of *ku-chung yang*, is the lineal descendant of the « sowed sheep », was no other than the astrakhan. Moreover the fur caps made from the lamb plant, according to POSTEL's Turkish informant, came from Samarkand, which is very near Bokhara, mentioned for the « sheep with heavy bones » by the Chinese.

But this was too simple. In 1725, according to BRETSCHNEIDER (*Br*, I, 154), Dr BREYN of Danzig (this must be Johann Philip BREYN, whose works are not at my disposal; from bibliographies I cannot trace any book of his which is dated 1725) declared that the pretended *Agnus scythicus* was nothing more than the rhizome of a large fern, which, once placed in an inverted position, had the appearance of the legs and horns of a quadruped; specimens of that fern, known as *penghawar*, were artificially made to have a still closer resemblance to the animal, and magical properties were attributed to it. The great LINNAEUS, having received in 1752 a certain fern from southern China, had no hesitation in declaring it to be the *Agnus scythicus*, and called it *Polypodium barometz*. Modern botanists have also used the term *Aspidium barometz*, but prefer *Cibotium barometz*. Such would be the *Agnus Scythicus*. « Adam and Eve were said to have been astonished on seeing this vegetable lamb in the Garden of Eden » (STUART, *Materia Medica*, 345). In one of the very spare footnotes added to his translation of HERBERSTEIN, MAJOR accepted the *Polypodium* theory. As to BOULLON's *Dictionnaire de botanique*, almost every word under the headings « Agneau de Scythie » and « Barometz » is a glaring error. So *barometz*, miscopied from HERBERSTEIN's « boranetz », became universally adopted in botanical nomenclature. The Russians corrected the form of the word, but not the value, and now *baranec* is the Russian designation of a *Polypodium*, just as *barometz* is in the rest of the world. No botanist paid attention to the fact that this fern, extensively found in south-eastern Asia and said to occur in the mountainous regions of Central Asia, did not grow in the very country from which the pseudo-name *barometz* had been imported.

LEE (pp. 24-44) has already exposed the absurdity of the fern story, but we have now to account for the whole legend in its various forms, without stopping at BRÜCKNER's hypothesis of a solar myth (p. 144).

In 1871, BRETSCHNEIDER (*On the knowledge possessed by the ancient Chinese*, 24) suggested

that the stuff woven from the hair of the «water sheep» might be the byssus, «a cloth stuff woven up to the present time . . . from the thread-like excrescences of several sea-shells (especially *Pinna squamosa*)». HIRTH (*China and the Roman Orient*, 260-263) adopted BRETSCHNEIDER's explanation not only for the «water sheep», but also for the lamb whose navel is attached to the ground according to texts of the T'ang period (and also probably pre-T'ang; cf. *supra*, p. 512); but he denied any connection between the latter and the lamb in ODORIC and SCALIGER (read HERBERSTEIN). ERMAN (*Reise um die Erde*, 1st part, Hist. Bericht, 1 [1833], 197-198) was the first to interpret the whole story of the *Agnus Scythicus*, to put it in YULE's terms, as «a mythical view of the cotton plant» (cf. Y<sup>1</sup>, II, 242). Others followed. Referring to J. O. HALLIWELL's edition of Maundeville (London, 1839; reprinted 1866 and 1883; cf. CORDIER, *Bibl. Sin.*<sup>2</sup>, 2024), where there is only, however, Maundeville's bare text (pp. 263-264) without comment, VINING, when publishing in 1885 his ill-fated book, *An inglorious Columbus* (p. 450), had no hesitation in stating that cotton bears «wool», «and hence may be considered as a vegetable-sheep». But the main exponent of the «cotton» theory was Henry LEE, throughout his *Vegetable Lamb of Tartary*. SCHLEGEL adopted it in part, but interpolated some notions referring to the camel, which it is needless to discuss. CHAVANNES (*TP*, 1907, 183), contrary to HIRTH, maintained that the «earth-born lamb» of T'ang texts had nothing to do with the «water sheep», but was the same as the *Agnus scythicus* of mediaeval folklore. LAUFER sided with CHAVANNES, and, above all, declared that LEE's «cotton» theory was a complete failure. «It is inconceivable», LAUFER says (124-125), «that in the fourteenth century, when cotton and the manner of its production were perfectly known in Asia and Europe, any such abstruse fable should have arisen in regard to cotton. The Indian cotton-plant became intimately familiar to the classical world, thanks to Alexander's campaign; and I do not know that it ever became the object of fables in India, China, Greece, or Rome, or in Syria, or among the Arabs. The Chinese of the sixth century, and assuredly of the T'ang period, knew very well what the cotton-plant and its products were; and neither is there in the Chinese documents regarding cotton any reference to lambs, nor is there the slightest allusion to cotton in the Ta-Ts'in or Fu-lin texts regarding the water-sheep and the earth-born lamb. The two groups of traditions are most clearly differentiated, and offer absolutely no point of contact.» This peremptory judgement has been summed up in *Wγ*, 483; yet it is already somewhat qualified in the very notes of LAUFER's paper, and I shall have to express more positive objections.

The best plan may be, perhaps, to take up the legend in reverse chronological order, and to start from its latest developments. There can hardly be any doubt that the *baranec*, «small sheep», of HERBERSTEIN is the still-born lamb, yielding «astrakhan». But this cannot be a purely western development of the legend, since JUAN K'uei-shêng also speaks of the caps made from the wool of the «heavy-bone sheep», and adds that there was no more than one which was grey for ten that were black, another distinctive feature of the «astrakhan» lamb. On the other hand, to judge from HUNG Liang-chi's text, the story of the «bone sowed sheep» seems not to have entirely died out among the Mongols as late as c. 1800.

We cannot tell how and when the story was transferred to the «astrakhan» lamb; but we may be certain that its meaning was quite different in the 13th and 14th cents., and here the

problem of cotton is again involved. LAUFER maintained that cotton was excluded because it was too well known, particularly in China, to be able to give rise to any such tales, and also because there was no reference to lambs in the Chinese documents regarding cotton. I venture to challenge both these lines of argument.

In China, there was so little knowledge in pre-T'ang and T'ang times of what cotton actually was that the Buddhist authors of the *I-ch'ieh ching yin-i* felt it necessary to state over and over again that *po-tieh* (« cotton ») was made of the down of a plant, not of the hair of an animal, as was commonly said in dictionaries. Few people realized that the *po-tieh* of the Turfan region was the same as the *ku-pei* or *chi-pei* of the South Seas. As late as 1461, the official Ming Geography, 明一統志 *Ming i-t'ung chih* (84, 20 a; cf. *Br*, II, 192), speaking of the products of the Turfan region, describes the «*po-tieh* cloth » in almost the same terms as the *Liang shu* and the *Hsin T'ang shu*, but adds that it was made « with cocoons which wild silkworms formed on 苦參 *k'u-shên* (*Sophora angustifolia?*) ». This has been copied afterwards into the « expanded » *Kuang-yü chi* of 1686 (24, 30 b; cf. LAUFER, *Sino-Iranica*, 492). Even the *Ta-Ch'ing i-t'ung chih* follows in their wake and speaks of the *po-tieh* of Ha-mi (Qomul; see « Camul ») as being made of the « webs » of wild silkworms (this is stated in the *Tz'ü-yüan*, but I cannot find it in the various redactions of the work which are at my disposal, neither COURANT, 1439, COURANT, 1492, nor the modern redaction; I suspect that the *Tz'ü-yüan* copied the *Ko-chih ching-yüan*, 27, 27 a, which merely speaks of the *I-t'ung chih*; this may be the *Ming i-t'ung chih*, with the substitution of Ha-mi for the Huo-chou [Qara-ḥoja near Turfan] of the original work). But this is an arbitrary combination of two distinct items of information. The *Liang shu* and subsequent works, including Wang Yen-tê's report of 984, had spoken of the *po-tieh* of Kao-ch'ang, *i. e.* cotton. On the other hand, Wang Yen-tê had stated that at I-chou, *i. e.* Qomul, there were wild silkworms which lived on *k'u-shên*, and from the cocoons of which floss and silk could be made (cf. *Sung shih*, 490, 4 a-b). But Wang Yen-tê never said that this silk was *po-tieh*; the error arose in Ming times. As a consequence of such misstatements, *po-tieh* is given as a silken cloth in the *Tz'ü-yüan*. This shows to what extent even late Chinese sources can go astray as to the true nature of cotton stuff.

When Odoric was in the region of the lower Yang-tzū, he heard a tale about Pygmies, three spans in height, who could do better work of «*goton*, id est *bombacis* (var. *bombicis*) » than any other people in the world. The whole passage remains practically unexplained (cf. CORDIER, *Odoric de Pordenone*, 345-355; *Y*<sup>1</sup>, II, 206-208; *W*<sub>γ</sub>, 468-469; HALLBERG, 418-420; also Maunde-ville, A. LAYARD ed., 1895, 259). Three spans, or, in the Chinese way of measurement, three « feet » is the usual height of Pygmies in both western and Chinese texts (cf. *TP*, 1905, 562). I feel inclined to believe that the tale, far from bearing evidence of the development of cotton cultivation in the region of the lower Yang-tzū, attributed the manufacture of the best cotton stuff to some legendary tribe located along the upper course of the river. And this also would tend to show that, even in the 14th cent., cotton cultivation was not as free from legend as LAUFER imagined.

So much for indirect evidence; but there is also the direct testimony provided by the texts. Even while belittling LEE's « cotton » theory of the *Agnus scythicus*, LAUFER admitted that Odoric's

tale had « a remote flavour of the cotton-plant » (p. 126), and thought that the confusion of « melon » and the « cotton-plant » might be due to the phonetic similarity of the Persian *harbuṣ*, « water-melon », and *kārbās* or *kīrbās* « cotton ». But the connection between the « sowed sheep » and cotton is not peculiar to Odoric; it was the common belief in northern China in the 13th and 14th cents. The equivalence is unambiguously expressed by both Yeh-lü Ch'u-ts'ai and Ch'iu Ch'ang-ch'un. WALEY (*The Travels of an Alchemist*, 86), commenting on Ch'iu Ch'ang-ch'un's text, almost hits the mark when he says : « The Chinese, being unfamiliar with cotton, could not believe that a stuff was obtained by cultivating a tree, and imagined that a lamb, being buried, produced a crop of fresh lambs next year. » If I qualify my approval of WALEY's note, it is because I agree with LAUFER in believing that the story of the « sowed sheep » did not originate in China. Nay, it may even be that the connection between the vegetable lamb and cotton was thought more or less possible in the West in the Middle Ages. Maundeville, in his motley forgery, mentions « Bacharia » in connection with the vegetable lamb, and elsewhere describes « Bacharia » as a country where cotton is sowed, so that « there is plenty of cotton at all times ». This is already symptomatic in itself, and, in view of the Chinese texts which locate the « sheep with heavy bones » in the region of Bokhara, it would be conclusive if it could be proved that Bokhara partly underlies the name and the description of « Bacharia ».

WALEY says that the story can be traced back to the 6th cent. in China; so we must assume that, like CHAVANNES, he connects the « sowed sheep » of the Mongol period with the « earth-born lamb » of T'ang and even pre-T'ang texts, but leaves the « water sheep » out of account. Like LAUFER (p. 117), I think, however, that there is an intimate interrelation of the « water sheep », the « earth-born lamb », and the « sowed sheep »; and, if I have been right in my dating of Ch'ang Shou-chieh's source, the « water sheep » and the « earth-born lamb » are practically contemporaneous. I have shown also that perhaps too much stress had been laid on the attribute « water » (cf. *supra*, p. 508-509). In LAUFER's learned monograph, much ingenuity has been expended to show that the « lamb » was a religious symbol brought over to China by Syrian Christians; I shall not discuss this opinion, for which I can find no support either in eastern or in western texts. Moreover, if the earliest mention of the « earth born lamb » goes back to the middle of the 3rd cent., this is too early a date for a Christian channel. On the other hand, the 3rd cent. of our era was a great epoch for the propagation of all sorts of legends between China and the West.

What is then at the base of this legend which, in the 3rd cent., appears under the double form of the « water sheep » and the « earth-born sheep »? LAUFER, like BRETSCHNEIDER, HIRTH and CHAVANNES, speaks of « byssus », but with much greater precision than his predecessors. BRETSCHNEIDER, followed by HIRTH and CHAVANNES, had said that « byssus » was the name of a textile woven from the excrescences of sea-shells, especially *Pinna squamosa*; but this view rested on a confusion. In classical Greek, *βύσσος* was the name of a costly textile, generally white, sometimes yellow (the only one produced in Greece proper, in the vicinity of Elis in Achaia, was yellow); opinions are still at variance whether it was « cotton » or « flax », the balance of opinion being at present in favour of the latter (cf. FRAZER, *Pausanias*, III, 470-472); in any case, it was a vegetable stuff, which had nothing to do with sea-shells. On the other hand, our zoological nomenclature gives the name of « byssi » (plural of « byssus ») to the filaments secreted by the

foot of the *Pinna* and other bivalve molluscs. From these filaments, textiles can be obtained they are mentioned in Greek texts from the 2nd cent. A. D. (perhaps already at the end of the 1st cent., if *πινικόν* of the *Periplus Maris Erythraei* really refers to them); but they are never called «byssus». These pinna textiles were famous in hellenistic times; as late as the 10th cent., Iṣṭaḥrī and Maqdisī attribute the pinna wool, «of silken hue and golden colour», to an animal which, at a certain period of the year, runs out of the sea and leaves the wool on stones against which he rubs himself (cf. *TP*, 1907, 183; LAUFER, *The Story of the Pinna*, 111). So there is a solid foundation for LAUFER's conclusion that the «water sheep» of the *Wei lio* is no other than the *Pinna* shell. I may add that the *Pinna* wool of the Mediterranean seems to have come again to the knowledge of the Chinese, but to have then been connected with a purely Chinese legend. Tso Ssū's «Ode on the Wu capital», *Wu tu fu* (second half of the 3rd cent.), already alludes to the mermaid who weaves silk (綃 *hsiao*) in the depth of the sea. In the *Shu-i chi* (6, 13 a; cf. *supra*, p. 518), the mermaid is called 鮫人 *chiao-jên* (see «Ambergris»), and the silk woven by her is 鮫綃 *chiao-hsiao*, «mermaid silk». Now, the *Ling-wai tai-ta* (3, 3 b) and Chao Ju-kua mention *chiao-hsiao* as one of the typical products of the Mediterranean (cf. *HR*, 141, 142; in fact, the *Han hai* ed. of Chao Ju-kua gives 絞綃 *chiao-hsiao*, a form which also occurs in Yeh-lü Ch'u-ts'ai's *Chan-jan chü-shih chi*, *Ssü-pu ts'ung-k'an* ed., 5, 5 a); apart from the inaccurate use of «byssus» as the designation of the textile meant, HIRTH and ROCKHILL seem to have been right in regarding this «mermaid silk» as a textile made from the byssi of the *Pinna*.

Yet I wonder whether this solution covers all the facts of the case. As we have seen, Chinese mediaeval accounts of the «sowed lamb» undoubtedly refer to cotton. Passage from an animal to a plant or a mineral, or *vice-versa*, is of common occurrence in hellenistic and mediaeval science. Even our modern nomenclature would favour such confusions. If we had no knowledge of the subject beyond the names, what legends could not arise from such modern terms as 羊毛樹 *yang-mao shu*, «sheep wool tree», now adopted in Chinese for an *Eriodendron*, or *srinbal*, lit. «worm wool», one of the names of cotton in Tibetan (cf. for instance, *Mahāvvyutpatti*, No. 5870), or «vegetable sheep», which is the English name of a white woolly plant of New Zealand! LAUFER thought that it took several centuries for the legend to develop from the «water sheep» to the «full-fledged ovine species equipped with phenomena of plant-growth». But, as a matter of fact, the western counterpart of the «water sheep» has not been traced, so far, to a text earlier than the Arabic accounts of the 10th cent.; while, on the other hand, the term «earth-born sheep» seems to occur in Chinese texts just as early as the «water sheep», *i. e.* in the 3rd cent. My impression is that both the «water sheep» and the «earth-born sheep» belong to a composite legend, the western prototype of which still escapes us, and for which the *Pinna* textiles furnish only some of the elements (but there is no reason to bring in also the legends concerning the big-tailed sheep, as was done by DE GOEJE in *TP*, v [1894], Supplement, 63). Another consideration makes me hesitate to follow LAUFER. If the famous «West of the Sea cloth» of Ta-Ch'in were merely a *Pinna* textile, «byssus», as the designation of a textile, ought to be left out of account entirely. Now, it is a striking fact that it is precisely *byssus* which has given the usual names of cotton stuff throughout northern Asia (cf. *supra*, p. 427). I have no authority to express a personal opinion on the true value of *βύσσοσ* or to decide

whether it was flax or cotton, although the two varieties, white and yellow, would almost speak in favour of the latter. But the main fact remains that βύσσος could not well have become the usual name of cotton stuff in nomadic Asia unless some similar value had attached to it in the West. So I believe that, from the start, cotton has as good a chance as the *Pinna* to have formed the basis of the twin legend of the «water sheep» and the «earth-born lamb».

One more point must be raised. The «earth-born lamb» was located in a small district which lay north of Ta-Ch'in and was dependent on it. The lamb was protected by an enclosure; armoured men on horseback had to frighten him by beating drums. The northern location and the noise occur even in late mediaeval stories of the «sowed sheep». LAUFER's explanation (p. 119) is as follows: «In the original animal fable these cuirassed men were crustacea, the shelly crusts of which were subsequently transmuted into cuirasses; they terrified the pinna, which, taken aback at the sight of the enemy, dropped its byssi. These byssi drifted ashore, where they were picked up by men for the purpose known to us.» Our information is so fragmentary that one cannot be positive *pro* or *contra* any such solution. So it is with great diffidence that I beg to dissent from LAUFER, and to suggest that, in the «earth-born lamb» located north of Ta-ch'in and the armoured men who strove to seize it, we may hear a faint echo of the story of the Argonauts and the Golden Fleece.

## 184. COWRIES

<i>krute borgil</i> ( <i>burgil</i> ), <i>krute</i>	<i>porcelane</i> LT, Z	<i>porzelane</i> V, VA
<i>portulaca borgil</i> (c. 157) G	<i>porcellana</i> (-ne) R	<i>porzellana</i> (-ne) V, VA, VB
<i>porcelaine</i> (-s) F	<i>porcellane</i> L, LT, Z	<i>porzellane de mar</i> (c. 157) VA
<i>porcelainnes</i> , <i>pourcelainnes</i> FA	<i>porciellane</i> TA <sup>1</sup> , TA <sup>3</sup>	<i>pourcelainnes</i> FA, FB

Polo speaks of cowries, *Cypraea moneta*, as «porcelainnes»; the origin and the different meanings of this word will be discussed under PORCELAIN.

Much has been written on the cowry and other shells used either as ornament or as currency. The most detailed monographs are those of Oskar SCHNEIDER, *Muschelgeld-Studien*, posthumous edition, Dresden, 1905, 8 v<sup>o</sup>, 191 pages, ill., and of J. W. JACKSON, *Shells as Evidence of the Migrations of early Culture*, Manchester, 1917, 8 v<sup>o</sup> (with an important Introduction by Elliot SMITH). Robert E. C. STEARN's *Ethno-Conchology: a study of primitive money* (in *Report of the National Museum 1886-1887*, 297-334, ill.) is important only for American shells. The best summary is a paper by F. A. SCHILDER, *Die ethnolog. Bedeutung der Porzellanschnecken*, in *Zeitschr. für Ethnologie*, 58 [1926], 313-327 (where, however, JACKSON's work is ignored). Interesting data have also been collected in R. BRIFFAULT, *The Mothers*, London, 1917, 8 v<sup>o</sup>, III, 265-278, and in G. ANDERSSON, *Children of the Yellow Earth*, London, 1936, 8 v<sup>o</sup>, 294-312 and 323-324.



Our word «cowry» ultimately goes back to Skr. *kaparda*, probably a pre-Aryan word, which occurs also under the derivative forms *kapardaka* and *kapardikā* (cf. *Mahāvīyutpatti*, SAKAKI ed., Nos 5994, 9374; not «*kapārda*» and «*kapārdika*» as in SCHILDER, 324) > Prākṛit *kavaḍḍa*, Maharati *kavḍā*, *kavḍī*, Hind. *kauṛī*, Gujarati *koḍī*, Singh. *kavaḍiya*. The pseudo-Skr. *kvaṇa* in BAGCHI, *Deux lexiques sanskrit-chinois*, 50 and 281, must be read \**kvaḍa* or even perhaps \**kavaḍa*; it is one of the many cases when the Brahmī form was in fact reconstructed from the Chinese transcription, and in the Chinese transcription the nasal often renders the corresponding unaspirated sonant (cf. the pseudo-Skr. forms *vaiṇuriya* and *nenāra* instead of *vaiḍuriya* [*vaiḍūrya*] and *denāra* [*dīnāra*], *ibid.*, 281). Our word cowry comes from Hind. *kauṛī* (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 269-271; DALGADO, *Glossário Luso-Asiático*, 241-243; *JA*, 1920, II, 290-291); the earliest European example of the word, in a Portuguese plural form *caurrys*, is dated 1520. For other European forms of «cowry» cf. SCHILDER, 324, and for African ones SCHILDER, *ibid.* and DELAFOSSE, in *JA*, 1926, I, 177-179, 183. Equally based on Hind. *kauṛī*, but earlier by a century, is the Chinese transcription 考嚙 (or 嚙) *k'ao-li*, in Ma Huan's *Ying-yai shêng-lan*, § of Bengal; Ma Huan may have heard the word before 1416, and certainly not later than 1433 (cf. *TP*, 1915, 437). The impossible derivation of cowry from the Greek *χοφίλη* ought not to have been repeated by JACKSON, 126, and ANDERSSON, *loc. cit.*, 305, even as an alternative solution.

The true Arabic name of the cowry is *وَدَعَةٌ* *wad'ah*, pl. *وَدَعٍ* *wada'*, or *وَدَعَةٌ* *wada'a*, pl. *وَدَعَاتٍ* *wada'āt*, which can easily be accounted for by deriving it from *wada'a* «to place», because the cowries were worn as amulets or ornaments; GRAY's opinion (*Pyrard of Laval*, II, 443) that it is «evidently» an Arabic corruption of «Sansk. *kavaḍi*» (read «Maharati *kavḍī*») is valueless. But Mohammedan authors also employ terms of which the foreign origin is not doubtful. The earliest one occurs in the *'Aḥbār aṣ-Ṣīn wa l-Hind*, which was written in 851. There it is said that, in the Maldive Islands, the cowries were called *كَنْج* *kastaḥ* by the natives (cf. J. SAUVAGET, *Relation de la Chine et de l'Inde*, 1948, 3, 36. The MS and REINAUD's text read *كَبْتَج* *kabtaḥ*, which FERRAND, *Voyage du marchand arabe Sulayman*, 33, accepts, but SAUVAGET changes it to *kastaḥ* (*kastag*) on the authority of *Le livre des merveilles de l'Inde*, 216-217, where it is the equivalent of *kanbār*, coco-nut fibre (Bīrūnī [Bērōnī], I, 210) which the author of the *Relation* mistook for the word of cowry, *kawḍah*, or *kūḍa*, Tamil *kavaḍi*); the word has been altered to *كَنْج* *kanḥ* in JAUBERT's *Géographie d'Édrisi*, I, 69; it has not survived in modern Maldivian, in which the cowry is called *boli* (often used by PYRARD of Laval as early as the beginning of the seventeenth century). [If SAUVAGET is right, all this passage is unnecessary, *kabtaḥ* a miswriting of *kastaḥ* = coco-fibre, *kastaḥ* mistaken by author for «cowry».] I hold it for practically certain that *kabtaḥ* goes back to a Prākṛit form of *kapardaka*; a final *-ka*, passing to *-ga*, is regularly rendered *-ḥ* in Arabic. But the median *-bt-* is hard to explain. The Arabic *-b-* seems to represent a native form in *-p-*, because a native *-b-* would have probably been rendered in Arabic with a *-w-*; but I do not see how the influence of this native *-p-* could make a surd *-t-* out of the sonant *-ḍ-* which must have resulted in Prākṛit from the *-rd-* of *kapardaka*. More frequent is *كَوْدَه* *kawḍah*, sometimes written *كَوْدِي* *kawḍah*, which is regularly entered in Arabic dictionaries. Al-Bīrūnī (A. D. 1030) speaks of the Maldives as *Dīwa-Kawḍah*, «Cowry Islands» (cf. SACHAU, *Alberuni's India*, I, 210), and *kawḍah* occurs in Persian in the *Ṭabaqāt-i Nāširi*, written in 1240 (the «*kauṛī*» adopted in RAVERTY's

translation, 1, 556, and from it in YULE's *Hobson-Jobson*<sup>2</sup>, 270, is misleading, since it is neither Persian nor English. My friend MASSÉ was good enough to check for me the original passage in the edition of the *Bibliotheca Indica*, 149). *Kawdah* was still understood and occasionally used in Egypt down to c. 1800 (cf. DE SACY, *Chrestomathie arabe*, 1, 253). Of the two forms the better one is of course *kawdah*, a correct rendering of Prâkrit *kavaḍḍa* > \**kavḍa*. *Kawdah* has not survived in modern Persian, which, according to MASSÉ, employs only a word كجی *kojī*, «small shell», «cowry», not given in the usual Persian dictionaries; its etymology and history are not known.

There are many species of *Cypræa*, but the one which comes *par excellence* under the designation of «cowry» is *Cypræa moneta*, the second in rank being *Cypræa annulus*, both sea-shells. From prehistoric times the *Cypræa moneta* has been carried across almost the whole of Europe and Asia, and was used as an ornament and as a means of exchange. There is no doubt moreover that because of its shape a sexual magic attached to the cowry (for China, cf. KARLGREN, *Some fecundity symbols*, in *The Museum of Far Eastern Antiquities*, Bull. No 2 [1930], 34-36, and ANDERSSON, *Children of the yellow Earth*). It also came to be used as regular currency in many parts of Asia and, at a later period, in Africa. Something similar occurred in America with the «seawan» and «wampam» shell beads of North American Indians.

The earliest mention of a shell currency comes from China. Since TERRIEN DE LACOUPERIE's days it has been customary to state that the cowry, adopted by the Chinese from prehistoric aborigines of the east and south, could be traced as early as in the «Tribute of Yü» section of the *Shu ching* «about the 14th cent. B. C.», that it was gradually superseded by metallic currency, and was finally suppressed in 338 B. C. by the prince of Ch'in owing to «irregular and insufficient supply» (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 269; TERRIEN DE LACOUPERIE, in *Coins and Medals, their Place in History and Art*, by the authors of the British Museum Official Catalogue, London, 1885, p. 190-197, 235, where the suppression of the shell currency is dated in the 2nd year of Hui-wên «i. e., 335 B. C.», an error for 336 B. C.; also in *JRAS*, 1888, 438-439; COULING, *Encyclopædia Sinica*, 136. In TERRIEN DE LACOUPERIE's *Catalogue of Chinese Coins ... in the British Museum*, London, 1892, IX, XII-XIV, no mention is made of the suppression of the shell currency, and the monetary reforms of the Ch'in state are dated not 338, but 336 B. C.). Unfortunately TERRIEN DE LACOUPERIE, whose monograph *On Barter and the Shell Currency of Ancient China*, referred to in his *Catalogue*, IX, was never published, was content with the bare affirmation of the suppression of shell currency in 338 B. C., and never mentioned his source; on the other hand it is well known that LACOUPERIE's biased treatment of Chinese texts was often tantamount to complete distortion. The whole problem must be approached anew in the light of recent discoveries.

Cowries have been found on the late neolithic sites of Yang-shao in Ho-nan and Chu-chia-chai in Kan-su, and they also form one of the typical motifs of prehistoric painted pottery in China (cf. J. G. ANDERSSON, *Archæolog. Studien in China* [*Mitteil. d. anthropolog. Gesellsch. in Wien*], 1924, 78; *Preliminary Report on Archæological Research in Kansu* [*Mem. of the Geolog. Survey of China*], 1925, 18; *Children of the Yellow Earth*, 323). These sites were inhabited by tribes which, from the anthropological point of view, may be termed proto-Chinese,

but which, even in Ho-nan, may have remained outside the pale of what was perhaps already moulding into shape the earliest Chinese community. No trace of writing yet occurs at Yang-shao or Chu-chia-chai, the sites of which may tentatively be dated early in the second millennium B. C.

On the other hand Chinese script is found already full-fledged at An-yang, the site of one of the Shang capitals in the second half of the same millennium. There too many cowries have been unearthed, together with imitations made of mussel-shell (玳瑁, *yao*), and already perhaps of bone and stone (cf. *TP*, 1923, 7-8; ANDERSSON, *Archæolog. Studien*, 78). Real cowries still occur, however, among the finds of the great Hsin-chêng tomb in Ho-nan and of the Sha-ching graves in the Chên-fan district of Kan-su; sites which are dated approximately in the middle of the first millennium B. C. (*Children of the Yellow Earth*, 323). ANDERSSON is of opinion that «the cowry seems to have come to China from India». This is not impossible, although cowries of various species are known to occur at the Liu-ch'iu Islands, at the Pescadores, and even on the shores of Shan-tung (cf. *JRAS*, 1888, 439). We must await the results of further research before passing a final judgement on a question which is of course of far-reaching consequence for the history of prehistoric and proto-historic intercourse in the Far East.

As pointed out by ANDERSSON, the early imitation of cowries in mussel-shell, bone, and stone, prompted probably by an inadequate supply of the true material, supports the view that a shell currency existed in China under the Shang dynasty. It was long ago observed, and by the Chinese themselves, that Chinese writing provided a strong argument to warrant the same conclusion. The character 貝 *pei* (\**puâi*), which means «cowry» and which, in its most ancient form, is a pictogram of a shell, enters as the radical into most of the Chinese characters meaning «precious», «trade», «riches», etc., and is down to our own day one of the components of the usual term for «a precious thing», 寶貝 *pao-pei*. And *pao-pei* is still used sometimes as a designation of the *Cypræa* (cf. TARANZANO, I, 372); but perhaps this is borrowed from Japan, where the characters *pao-pei*, read *takaragai*, are a current designation of this shell. This 貝 *pei*, however, is a substitute borrowed in the first millennium before our era to replace the original *pei* pictogram, which was then abandoned. As to the original *pei* pictogram, it may be worth noticing, from the point of view of symbolism, that, contrary to the use of the cowry motif on pottery and bronzes, it seldom shows the longitudinal slit of the lower part of the cowry, and as a rule seems to represent the back with its transverse streaks (cf. 汪仁壽 WANG Jên-shou, 金石大字典 *Chin-shih ta tzü-tien*, 27, 21-22; TAKATA, *Kojô-hen*, 99, 1-3). In the etymological vagaries, mostly based on phonetic puns, which started with Liu Hsi's *Shih ming* (end of Han) and found a fresh expression in Lu T'ien's *Pi ya* (second half of the 11th century), *pei* is derived from 背 *pei*, «back». There is no foundation in GILES's statement (*Glossary of Reference*, 211) that it was the pearl oyster which the ancient Chinese used as currency under the name of *pei*, and that the ancient form of this character was a picture of the open shell.

Our information on this early Chinese shell currency is scanty, and is marred by the fact that *pei* was the generic designation of all sorts of shells. The *Êrh ya*, an ancient dictionary which is now included among the «Thirteen Classics» and goes back to about the second century B. C., has a passage on *pei* (JUAN Yüan ed., 9, 21 b; cf. KLAPROTH's inaccurate translation in *JA*,

Febr., 1834, 148; the text, as translated here, is that of the T'ang «stone classics» : «The *pei* which lives on land is the 𧈧 *piao*; the one which is in water is the 𧈧 *han*. Great ones are 𧈧 *hang*; small ones are 𧈧 *chi*. The black (*hsüan*) is [called] 貽貝 *i-pei* (or «The *hsüan-pei* is a black *pei*[*i-pei*]»); the *hsüan-pei* is mentioned in the *Wang hui* chapter of the *I Chou shu*, and by Huan K'uan [cf. *infra*, p. 539]). The 餘貽 *yü-ch'ih* is yellow with white streaks; the 餘泉 *yü-ch'üan* is white with yellow streaks. The 𧈧 *pa* (or *p'a*) is wide [in the middle] and pointed at both ends (𧈧 *k'uei*); the 𧈧 *ch'un* is large and thin (? 險 *hsien*); the 𧈧 *tsé* is small and cylindrical (𧈧 *t'o*.)» This text is not always clear, nor even established with certainty : for instance 𧈧 *chi* and 𧈧 *tsé* are graphically similar, and similarly defined; Kuo P'o's commentary (beginning of the 4th century A. D.) and Hsing Ping's sub-commentary (beginning of the 11th century) are based on a text which gave one and the same character in both cases. On the other hand it is abnormal, though not unique, to have in the *Érh-ya* two definitions for the same word; the second mention and definition of *chi* or *tsé* may or may not be an early interpolation. The *i* of *i-pei* ought perhaps to be read *t'ai*, since it is certainly the same word as 𧈧 *t'ai* of identical meaning. There are other doubtful forms, and one may hesitate between 𧈧 *hang* and 𧈧 *hang*, 𧈧 *ch'ih* and 𧈧 *ch'ih*, etc. The *han*, given here as the name of a *pei* living in the water, occurs elsewhere (9, 20 b) as the designation of small spiral shells (𧈧 *lo*). What is clear, however, is that, for the author of the *Érh-ya*, the *pei* was not necessarily a sea-shell. Nor was it always a univalve (gastropod). The 尚書大傳 *Shang-shu ta-chuan*, written in Han times speaks of «great *pei*» (*ta-pei*) of the lower Chiang (= Yang-tzŭ-chiang) and Huai, which were as big as the fellow of a cart (車渠 *ch'é-ch'ü*), and *ch'é-ch'ü* (written 礮礮) has become in later times the regular designation of a large bivalve, the mother-of-pearl shell; Kuo P'o says that these «great *pei*» were of the *hang* class. And «great *pei*» are mentioned, on the same occasion, by *Huai-nan-tzŭ* and the *T'ai-kung liu-t'ao*. In Lu Chi's «Memoir on natural history in the Book of Odes» (middle of the 3rd century; see COTTON, p. 474; *Chin-tai pi-shu*, ed., 2 B, 42 a), we are told that large *pei* reached a diameter of one foot (another reading gives «one foot and six or seven inches»); the *Tz'ü-yüan* even speaks of *ch'é-ch'ü* measuring three feet in diameter. In the same *Tz'ü-yüan*, *pei* is given as the common designation of both gastropods and bivalves. These various shells were popular enough to have been made the subject of a special treatise, the 相貝經 *Hsiang pei ching*, or «Doctrinal book on the properties of *pei*». Its text however is poorly established and, as we have it, certainly incomplete. It is to be found in different encyclopaedias or *ts'ung-shu* (*T'ai-p'ing yü-lan*, 807, 14 b; *Shuo fu* in 120 ch., ch. 97; MAO Chin's commentary on Lu Chi's *Memoir*, 2 B, 44-45; *T'u-shu chi-ch'êng*, *ch'in-ch'ung-tien*, 156, 3-4; Kao Ssŭ-sun's *Wei lio*, *Shou-shan-ko ts'ung-shu* ed., 1, 5-6; also in one of the enlarged editions of the *Han-Wei ts'ung-shu*, etc.; cf. *Ts'ung-shu ta tz'ü-tien*, 4-189); yet none of the editions I have seen seems to have copied the somewhat different passage quoted in *T'ai-p'ing yü-lan*, 941, 1 a. KLAPROTH (*JA*, Febr., 1834, 149) and KARLGRÉN (*Some fecundity symbols*, 35) have translated part of its descriptions of *pei*, some of the names of which do not occur elsewhere, and may be imaginary (several of these names are now given as scientific designations of various species of *Cypræa*, for instance 委貝 *wei-pei*, 綬貝 *shou-pei*, 浮貝 *fou-pei* (cf. TARANZANO, *Vocabulaire*, I, 372), but they seem to have been merely culled from the *Hsiang pei ching* by modern scholars). KARLGRÉN

says that the *Hsiang pei ching* is by «Chu Chung from the earlier Han dynasty», a statement which it seems hard to accept. The work is not mentioned in the bibliographical section of the *Ch'ien-Han shu*; it occurs for the first time in the *Sui shu* (34, 12 a) as an anonymous work which existed in the first half of the 6th century, but was already lost c. A. D. 600; it is again listed, however, in *Chiu T'ang shu* (47, 4 b), *Hsin T'ang shu* (59, 8 a), and *Sung shih* (106, 10 b, where 貝 *pei* is altered to 貝 *chü*), always without name of author. The alleged authorship is deduced from the beginning of the treatise : «朱仲 Chu Chung received it from 琴高 Ch'in Kao. Ch'in Kao, riding a fish, floated on river and sea and would not fail to investigate what water produced. [Chu] Chung studied [the art of] the genii (*hsien*) with [Ch'in] Kao and acquired his method (*fa*); moreover he offered pearls to the Emperor Wu, and went no one knows where. When 嚴助 Yen Chu was prefect (*t'ai-shou*) of Kuei-chi (in Chê-chiang) [Chu] Chung came forth again, presented [Yen] Chu with a *pei* one foot in diameter, and also gave him a text which said. . . » Then follows the text itself. Ch'in Kao of Kuei-chi is a legendary figure of the time of the Fighting Kingdoms (cf. *T'u-shu chi-ch'êng, shên-i-tien*, 225, 11-12). Chu Chung is absurdly made to have lived both in the time of the Fighting Kingdoms, since he studied with Ch'in Kao, and under the Han, since he offered pearls to the Emperor Wu. The only notice of him occurs in the old *Lieh-hsien chuan* (cf. *T'u-shu chi-ch'êng, shên-i-tien*, 227, 6), where mention is made of enormous pearls offered by him to the Emperor Kao and the Emperor Ching, but nothing is said either of the Emperor Wu or of Yen Chu. Yen Chu alone is a real historical figure; he died c. 122 B. C. But the *Hsiang pei ching* is no doubt a forgery of post-Han times. It makes Wên-wang (11th century B. C.) receive a *pei*, four feet in diameter, from Ta-Ch'in; but Ta-Ch'in, a designation of the Mediterranean Orient, does not appear in Chinese texts before the Christian era. Even admitting, which may be already too much, that the present *Hsiang pei ching* is the same which existed in the first half of the 6th century, it cannot be older than the 4th or 5th century. Its indications, interesting though they may be from the point of view of folklore, can have no direct bearing on the ancient history of the *pei* in China. One point only may be noticed; the choice, in the apocryphal account of the origin of the treatise, of a man who held office in Chê-chiang may have been inspired by the prevalence at the mouth of the Yang-tzŭ of the «great *pei*» already mentioned in the *Shang-shu ta-chuan*.

Chinese classics do not throw much light on the problem of the cowry. The «Tribute of Yü» in the *Shu ching* is surely not as old as the fourteenth century B. C., as maintained by TERRIEN DE LACOUPERIE, and cannot in fact be dated before the first centuries of the first millennium before Christ. Moreover the «Tribute of Yü», describing the offerings of the «island barbarians» (*tao-i*) at the mouth of the Yang-tzŭ (the people who wore «grass clothes»; see COTTON, p. 487), merely says 厥筐織貝, which LEGGE translated (*Chinese Classics*, III, 111) : «The baskets were filled with woven ornamental silks». In the Index, however, LEGGE remarks (p. 719) : «I should almost be inclined to interpret the character of cowries». Personally, I believe that *chih pei* does not mean «woven ornamental silks», nor even «silk with woven cowry designs», but «woven fabrics ornamented with shells», *i. e.* which had real shells or parts of shells sewn on to the textile. But these shells need not necessarily have been cowries, nor have they anything to do with shell currency. The same may be said of two passages in the

*Shih ching* or *Book of Odes*, where 成是貝錦 and 貝冑朱纓 are translated by LEGGE, respectively «... may be made out to be shell embroideries (= looking like a beautiful shell)» (*Chin. Cl.*, IV, 346), and «with shells on vermilion-strings adorning their helmets» (*ibid.*, 626); I think that the «shell-embroideries», or, more literally, «shell brocades», were brocades adorned with real shells. In a chapter of the *Shu ching*, 文貝仍几 is translated by LEGGE (*Chin. Cl.*, III, 553) «and the usual bench adorned with tortoise-shell», and further on (III, 554) 大貝 *ta-pei* is rendered «the large tortoise-shell». Although Chêng Hsüan (A. D. 127-200) seems to have said somewhere «The *pei* is now called 瑋瑁 *tai-mei*» (cf. MAO Chin's commentary on Lu-Chi's *Memoir*, 2 B, 43 a), this is either an error, or Chêng Hsüan used *tai-mei* as an equivalent of 玳瑁螺 *tai-mei lo*, lit. «Tortoise spiral shell», a designation of the telline conical bivalve; the bench in the *Shu ching* must have been adorned not with «veined tortoise shell», but with real veined shell (*wên-pei*), and the *ta-pei* too was a sea-shell. Moreover, such is the explanation correctly given in K'ung Ying-ta's sub-commentary (cf. *Shih-san-ching chu-shu*, JUAN Yüan's ed., 18, 21 a, 23 a). The only instance of the use of the word *pei* in the *Shu ching* which is more precise is when officials are reproached with thinking only of hoarding 貝玉 *pei yü*; LEGGE (*Chin. Cl.*, III, 240) translates «cowries and gems», but the literal meaning is «cowries and jade» and we know that, under the Chou, jade too was used as a sort of currency.

Of greater importance are the old rituals, *I li* and *Li chi*. The *I li*, or ritual of the ordinary nobles (士 *shih*), shows that, in the course of the funerary ceremony, three cowries (*pei*) were put into the mouth of the dead nobleman together with some cooked rice (cf. STEELE, *I li*, II, 50, 97; DE GROOT, *The Religious System of China*, I, 275-277; and the discussion in *Shih-san-ching chu-shu*, 35, 15); the *I li* is perhaps the most ancient, and at any rate the most coherent and the most trustworthy of the three rituals. In the collection of treatises of different dates known as *Li chi*, ch. *T'an-kung*, mention is made of the rice and the cowries (*pei*) which were put into the mouth of the dead (cf. COUVREUR, *Li ki*, I, 200; in II, 184, COUVREUR refers to I, 235 and 247, of the same *T'an-kung*, for jade discs being put into the mouth of the dead, but these occur only in his translation, not in the text; the jade discs, and jade simply, are mentioned in another chapter, the *Tsa chi*, II, 145, and several times in the *Chou li*). According to the chapter *Tsa chi* of the *Li chi* (COUVREUR, II, 184), nine cowries were put into the mouth of the Emperor, seven into the mouth of the appanaged princes (*chu-hou*), five into the mouth of the great officers (*ta-fu*), and three into the mouth of the ordinary nobles. Early commentators have explained that this was the rite under the Hsia dynasty, but that, under the Chou, in the middle of the first millennium B. C., pearls were put into the mouth of the Emperor, jade (*yü*) into that of the appanaged princes, jade discs (*pi*) into those of the great officers, and cowries into that of the ordinary nobles (the version of a similar text from the *Po-hu t'ung*, in DE GROOT, *op. cit.*, I, 277, is to be rejected, because 璧 *pi* has been corrupted there into 來 *lai*; *lai*, «to come», makes no sense). Although I do not think that KARLGRÉN's discussion of these rites (*Some fecundity symbols*, 39-40) carries full conviction, I agree with him that many of the opinions expressed on the subject by Han commentators are mere guess-work. As they are, they seem, however, to betray an historically justified feeling that the importance of the cowry had decreased in China in the course of the two millennia before our era. In regard to

the meaning of these rites, and in spite of the almost formal statement in the *T'an-kung* (COUVREUR, I, 200; the passage is less precise in the text than in the translation), I feel almost inclined to think that, at least originally, rice was put into the mouth of the dead as food, and precious things (pearls, jade, and above all cowries) were put there both to prevent the decay of the body and to serve as a sort of currency to meet the wants of the deceased in the nether world.

The shells, or at least the cowries, were pierced to be strung together, sometimes at one or both ends, more commonly by grinding a hole in their back. Strung cowries were called 朋 *p'êng*, but the question is still debated as to how many shells made a *p'êng*; texts and opinions vary from two to ten; the example of Wang Mang's monetary reforms (cf. *infra*) cannot be adduced to any useful purpose, as it seems doubtful that scholars of Wang Mang's time, at the beginning of our era, still had any authorised tradition of the practice of the Shang and the Chou. However that may be, *p'êng* of cowries are often mentioned in the An-yang inscriptions on tortoise-shell or bone as well as in those on ancient bronzes, and the *Book of Odes* speaks of an Imperial gift of a hundred *p'êng* (cf. LEGGE, *Chin. Cl.*, IV, 280; WANG Kuo-wei, *Kuan-t'ang chi-lin*, 3, 17-18; 郭沫若 Kuo Mo-jo, 甲骨文字研究 *Chia-ku wên-tzũ yen-chiu*, I, § 10, 1-4). A problem analogous to that of the *p'êng* of shells is raised by the 珥 *chio* of jade. Moreover, these strung shells or jades were not necessarily intended for hoarding or counting purposes: they were often worn at the neck as ornaments. This is well shown by the character 珥 *ying*, «neck ornament», «necklace», which is made of two *pei* characters and represents the old and correct form, now replaced by the substitute 璣 *ying* (cf. Hsü Hao's *Shuo-wên chieh-tzũ chu chien*, 6 B, 38; Hui-lin, *I-ch'ieh ching yin-i*, ch. 40, in *Tökyö Tripit.* of Meiji, 爲, IX, 48 a).

I find no trace anywhere of a text which, even misunderstood, would countenance TERRIEN DE LACOUPERIE's statement regarding the suppression of the cowry currency by the prince of Ch'in in 338 or 336 B. C. (TERRIEN DE LACOUPERIE was perhaps misled by the fact that the first Ch'in coinage is said to have been in 318 B. C.; cf. MASPERO, *La Chine antique*, 92, 1<sup>re</sup> éd.). We are only told that, when Ch'in Shih-huang-ti had become the sole Lord of the Empire, he established a double «currency» (幣 *pi*), one of gold called «superior currency» (*shang-pi*), and the other of copper cash (*t'ung-ch'ien*) of the same sort as had been used under the Chou; «as to pearls, jade, tortoise [-shell], cowries (*pei*), silver, and tin, they were used to adorn objects and were preserved as precious things, but served no [longer] as currency» (*pi*; *Ch'ien-Han shu*, 24 B, 1-2; D. BODDE, *China's First Unifier*, Leyden, 1938, 172). The *Shuo wên* (c. A. D. 100) expresses the same opinion when it says (ch. 6, *pei* «radical»): «In ancient times [people] bargained (貨 *huo*) by means of cowries (*pei*) and used tortoise[-shell] as precious [currency] (寶 *pao*; this word was used by Wang Mang as a technical term for his tortoise-shell currency, and has survived in the legends on cash down to our day). Under the Chou there was copper currency (泉 *ch'üan*); when we come to [the time of] the Ch'in, they abolished the cowries (*pei*) and issued cash (*ch'ien*)». Ch'in is of course Ch'in Shih-huang-ti, the Emperor of the end of the third century B. C., and not a prince of the state of Ch'in a century earlier.

In all likelihood the shell currency had been gradually abandoned, and Ch'in Shih-huang-ti

merely gave the sanction of law to what was already the state of affairs prevailing in the empire. It is quite probable that cowries were too few and at the same time too cumbrous to meet the commercial needs of the growing Chinese community. There is a bare mention of the *pei* in Ssü-ma Ch'ien (cf. CHAVANNES, *Mémoires historiques*, III, 600). But the abandonment had been a slow process, and efforts had been made to prevent it. As we have seen, imitations of cowries in mussel-shell, stone, and bone had already been made at the end of the second millennium B. C., and the manufacture certainly went on during the first half of the first millennium. We have perhaps an echo of these imitations in stone when Huan K'uan (1st century B. C.) says that the Hsia used «black cowries» (*hsüan-pei*) and the Chou «purple stones» (*tzü-shih*; cf. GALE, *Discourses on Salt and Iron*, 27). Bronze imitations of cowries followed, both inscribed and uninscribed, at first quite realistic, and these I see no reason to regard as charms, as KARLGRÉN has suggested (*Some fecundity symbols*, 44). Since the middle of the twelfth century Chinese numismatists have described certain enigmatic bronze coins, of ovoid shape with a flat bottom and convex back, under the designation of 蟻鼻錢 *i-pi ch'ien*, «ant-nose coins» (in more recent works they are also termed 鬼頭 *kuei-t'ou*, «devil heads», 鬼臉 *kuei-lien*, «devil faces», and 貨貝錢 *huo-pei ch'ien*, «currency-shell coins»). In spite of amazing confusions and a fantastic chronology, it was TERRIEN DE LACOUPERIE's merit to have first expressly stated in *JRAS*, 1888, 428-439, that the «ant-nose coins» were nothing but debased bronze imitations of cowries (cf. also his *Catalogue of Chinese Coins*, pp. XII, 300). The same view has since been upheld independently by LO Chên-yü, who declared that the type of the characters engraved on these bronze cowries was «of the late Chou» (cf. *TP*, 1923, 8), and his opinion has been endorsed by KUO Mo-jo (*loc. cit.*, 2-3). But, to judge from the specimens I have seen reproduced, I do not think that by «late Chou» we ought to understand a date later than c. 400 B. C. (the information drawn by JACKSON, 177-178, from TERRIEN DE LACOUPERIE's *Western Origin of the Early Chinese Civilisation* is valueless). The bronze imitations of cowries were strung through a small round hole pierced at one end. This seems to dispose of the theory, still adopted by SCHILDER (p. 322), that the central square hole of the traditional Chinese cash is traceable to the stringing of cowries.

In A. D. 9 the usurper Wang Mang, under the influence of Confucian scholars, started a number of reforms which aimed in principle at reviving the practices of antiquity (cf. DUBS, in *TP*, 1939, 219-265); and one of them was a drastic change in currency. The «knife-money» and the «five *shu* coins» were abolished, and a new system of currency was devised which comprised items in «gold, silver, tortoise[-shell], shells (*pei*), bronze-coins (*ch'ien*), and «cloth» (*pu*) [coins], some of the items being called *pao*, «treasure», and others *huo*, «currency» (cf. *Ch'ien-Han shu*, 24 B, 8-9; the text is mispunctuated and mistranslated in VISSERING, *On Chinese Currency*, 1876, p. 51). The «bronze coins» or cash were divided into six classes according to their size and weight. That of one *shu* (= 1/24 of an «ounce») was called «small coin» (小錢 *hsiao-ch'ien*); of three *shu*, «tender coin» (小錢 *yao-ch'ien*); of five *shu*, «young coin» (幼錢 *yu-ch'ien*); of seven *shu*, «middle coin» (中錢 *chung-ch'ien*); of nine *shu*, «adult coin» (壯錢 *chuang-ch'ien*); this adult coin of «forty» replaced the former large coin of «fifty». Similar names were adopted for five items of «shell currency» (貝貨 *pei-huo*): shells (*pei*) under a diameter



of 1.2 inch could not be made into *p'êng*, and every one had an exchange value of 3 cash; the «small shell» (*hsiao-pei*) was at least 1.2 inch in diameter, and two of them made one *p'êng*, which was worth 10 cash; the «tender shell» (*yao-pei*) was at least 2.4 inches in diameter, and two of them made one *p'êng* which was worth 30 cash; the «adult shell» (*chuang-pei*) was at least 3.6 inches in diameter, and two of them made one *p'êng* which was worth 50 cash (not 150, as in JACKSON, 178, copying *T'ai-p'ing yü-lan*, 807); the «great shell» (*ta-pei*) was at least 4.8 inches in diameter, and two of them made one *p'êng* which was worth 216 cash. There were also four different «tortoise treasures» (*kuei-pao*), which were respectively valued at 100 cash or 10 *p'êng* of «small shell», 300 cash or 10 *p'êng* of «tender shell», 500 cash or 10 *p'êng* of «adult shell», and 2160 cash or 10 *p'êng* of «great shell». I see no reason to suppose, with JACKSON, 181, that these «tortoise-shells» were in fact cowries. The whole scheme was unpractical and does not seem to have been actually carried out; but it calls for some remarks. One is that it seems to have been devised from bookish recollections, apart from any contact with real things; and this would suggest that shell currency, no less than barter based on tortoise shells, had entirely died out in China long before Wang Mang's time. Another point to be noticed is the variety of sizes of the shells, from less than 1.2 inches to over 4.8 inches; this absolutely precludes the possibility that the shell currency imagined by Wang Mang's counsellors should have been merely a cowry currency, at least a currency of *Cypræa moneta*. Although it has no direct connection with the question of the cowry, it may also be observed that since the *p'êng* of two «great shells» was worth 216 cash, each of these shells was valued at 108 cash, *i. e.* the number which played so important a part in India and, at a later date, in Chinese Buddhism, and still remains the number of the beads in the Buddhist rosary, just as it was the number of the beads of the necklace worn by officials under the last Chinese dynasties (cf. my remarks in *TP*, 1927, 137; PENZER, *The Ocean of Story*, I, 242; VI, 14; IX, 145). My colleague MASPERO reminds me that the number 108 occurs in Wang Mang's time in another context: Wang Mang divided the day of twelve double hours (*shih*) successively into 96 and 108 刻 *k'o*. In my note in *TP*, I suggested a tentative explanation of the number 108 as representing the twelve months multiplied by the nine planets; and other hypotheses have been made by starting from 3 or 9 (cf. PENZER, IX, 145, «(3 + 3) (3 + 3) { 3», *i. e.*  $6 \times 6 \times 3$ , and PRZYLUKI, in *Roczn. Oriental.*, VII, 8, «99, 9»). My suggestion cannot hold, of course, for Wang Mang's time, since the Chinese did not then know of the «nine planets» system of India. It seems that Wang Mang's 96 or 108 *k'o* were the result of a desire to have a number of *k'o* which was at the same time a multiple of 12 and as near 100 as possible. But one does not see why the number 108 was also adopted as the value of the «great shell». Attention may be drawn to a last point: in Wang Mang's currency even the smallest shells were more highly valued than the copper cash, whereas, at a later time, in the countries where a true cowry currency was employed, it was the copper cash which was always exchanged for several cowries. This seems to be an indication that shells were comparatively scarce in China in Wang Mang's time.

In his valuable paper *Some fecundity symbols* (p. 34) KARLGRÉN, after stating that cowries were employed as coins in archaic China, adds: «Provincially, this custom lived down to the Mongol epoch, and even in Ming times it is reported from Yünnan». ANDERSSON (*Children of the*

*Yellow Earth*, 300) expresses a similar opinion, copying JACKSON, 183. We shall soon come to this use of cowries in Yün-nan, among populations which were outside the range of Chinese culture; but I have never met with any text which would suggest that cowries continued to be used by the Chinese as currency in any part of China proper since the beginning of our era; nay, if we except Wang Mang's abortive attempt at a revival of the ancient practice, they had fallen into complete disuse on or before the downfall of the Chou dynasty in the third century B. C.

But shells long continued to be worn as ornaments, just as we have seen them mentioned in several passages of the *Shih ching* and *Shu ching*. Many texts on the subject could be adduced, ranging from Han almost to T'ang times (cf. *T'u-shu chi-ch'êng*, *ch'in-ch'ung-tien*, 156, 5 a; *T'ai-p'ing yü-lan*, 807, 14; 841, 1). It is often difficult, however, to determine which texts refer to cowries, and which to other shells of various sizes, and including bivalves.

When later we find again in Chinese texts mentions of the use of a shell currency on Chinese soil, they refer to the aborigines of Hai-nan and Yün-nan.

The *Ling-piao lu-i* (c. A. D. 900; Wu-ying-tien ed., 3, 5-6; cf. *T'ai-p'ing yü-lan*, 941, 1 a) says: «The 紫貝 *tzü-pei* (lit. «purple shell») is [the same as] the 研螺 *ya-lo* (lit. «polishing gastropod»). The 黎 Li Barbarians of 儋 Tan[-chou] and 振 Chên[-chou] collect them on the sea-shore and use them as currency (*huo*)». The Li are the Thai aborigines of Hai-nan; Tan-chou and Chên-chou were, under the T'ang, the official names of the modern Tan-chou and Yai-chou in Hai-nan. For various *Cypræa* which have been similarly used for polishing purposes in Europe, Egypt and Indonesia, cf. SCHILDER, 315.

MAO Chin's Commentary on Lu Chi's 'Memoir on Natural History in the Book of Odes' (2 B, 43-44) has the following passage: «The *Pên-ts'ao* mentions only the *tzü-pei* (i. e. does not mention the cowry properly so called, nor the *ta-pei*). The 唐本注 *T'ang pên chu* says: 'Its shape is like [that of] the *pei*; its size is two or three inches; it is produced in the Eastern Sea (Tung-hai) and the Southern Sea (Nan-hai). Its upper side has purple spots on a white bone (*ku*; i. e. «ground»)! The 圖經 *T'u-ching* says: '蘇恭 Su Kung's Commentary (*chu*) says: The *tzü-pei* is [the same as] the *ya-lo*. Its shape is like [that of] the *pei*, but [more] round; its size is two or three inches. The Li Barbarians of Tan[-chou] and Chên[-chou] collect them and use them as currency goods (貨市 *huo-shih*). Among northerners, painters alone use them [and they use them] as polishers (研物 *ya-wu*). There are many species of *pei*. The ancients used them as currency (*pao-huo*), but the *tzü-pei* was the species particularly valued . . .'. This text, written in the more or less slipshod manner of late Ming writers, is not always clear, nor is it easy to decide where some of the quotations begin and end. To say that the *Pên-ts'ao* mentions only the *tzü-pei* is not correct, since the *Pên-ts'ao* of the T'ang spoke of the cowry properly so called (*pei-tzū*; cf. *infra*); on the other hand, Li Shih-chên's *Pên-ts'ao kang-mu* already existed in MAO Chin's days, and it contains two distinct paragraphs, one on *tzü-pei*, the other on *pei-tzū* (cowry). Perhaps MAO Chin, ignoring Li Shih-chên's work, had in view the 證類本草 *Chêng-lei pên-ts'ao* of 1108 (cf. BRETSCHNEIDER, *Botanicon Sinicum*, 1, 47), which still exists, but of which no copy is at my disposal. I do not know what MAO Chin means by *T'ang pên chu*; it may be a commentary either on the *Pên-ts'ao* of the T'ang or, less probably,

on the *Chêng-lei pên-ts'ao*, the author of which was 唐慎微 T'ang Shên-wei. The *T'u-ching* is certainly the *T'u-ching pên-ts'ao*, written after 1057 by 蘇頌 Su Sung (cf. BRETSCHNEIDER, *Bot. sin.*, 47). And the *T'u-ching pên-ts'ao* may have quoted Su Kung, the author of one of the redactions of the *T'ang pên-ts'ao*, about the middle of the seventh century. I am not certain where the quotation from Su Kung ends, nor even the one from Su Sung, since the last sentences may have been added by MAO Chin himself.

*Tzū-pei*, «purple shell», is in fact an ancient term in Chinese, though it is not certain that, from the beginning, it referred to the same shell as in works posterior to our era. From the «Nine songs» (*Chiu-ko*) of the *Elegies of Ch'u*, we know that, as early as the third century B. C., the *tzū-pei* were used for decoration. In 179 B. C. the King of Nan-yüeh (= Canton), Chao T'ò, sent 500 *tzū-pei* to the Han Emperor (*Ch'ien-Han shu*, 95, 5 b). Although the term does not occur in the *Êrh-ya*, Kuo P'ò added it in his commentary on this work. In his commentary on the *Shan-hai ching* (section *Ta-huang nan ching*) he gives it as a synonym of *wên-pei* (the same «veined *pei*» which we have already seen in the *Shu ching*), although in the section *Hsi-shan ching* he suggests that another shell is the *wên-pei*. Ch'in Shih-huang-ti's tomb is already described in legendary fashion by Ssü-ma Ch'ien; it was a representation of the world, with rivers of quicksilver (cf. CHAVANNES, *Mém. historiques*, II, 194). The *Yüan-chien lei-han* (364, 25 a) quotes a no less fantastic description from the lost 三輔故事 *San-fu ku-shih*: a spring (*ch'üan*) of quicksilver had been arranged; the moon was made of a «brilliant pearl» (*ming-chu*); and in the «water» (probably = *shui-yin*, «water silver», quicksilver), there were many «veined shells» (*wên-pei*). Whatever the truth may be with regard to these earlier mentions, the certain fact is that since the fourth or fifth century *tzū-pei* (sometimes *ta-pei*) has been the designation of a well-defined shell, which is not the cowry but a bigger *Cypræa*, the *Cypræa macula*, as indicated by TARANZANO, *Vocabulaire*, I, 372. TARANZANO adds, as alternative names, *wên-pei*, which may go back to Kuo P'ò's commentary on the *Shan-hai ching*, and 蚌螺 *hsia-lo*, which is a mistake for 研螺 *ya-lo*, «polishing shell». In the twelfth century there is an excellent description of the *ta-pei* or *tzū-pei* in Chou Ch'ü-fei's *Ling-wai tai-ta*, 7, 9 b.

For the true cowry, *Cypræa moneta*, TARANZANO gives only one designation 貨貝 *huo-pei*, «money shell», of which I know no literary example, but not 瑪瑙貝 *ma-nao pei*, «agate shell», which is said by KUO Mo-jo to be now the scientific name of the cowry. As a matter of fact specific designations of the true cowry have been in use at least since the early fifth, and perhaps since the fourth, century of our era. Fa-hsien, c. A. D. 400, mentions the use of 貝齒 *pei-ch'ih*, «shell teeth», in India (cf. LEGGE, *A Record of Buddhist Kingdoms*, 43). The *T'ai-p'ing yü-lan* (807, 14 b) has preserved the following passage of 劉欣期 Liu Hsin-ch'i's 交州記 *Chiao-chou chi*: «The great *pei* (*ta-pei*) are produced in Jih-nan (Annam); they are like wine-cups. The small *pei* (*hsiao-pei*) are the *pei-ch'ih* («shell teeth»). They are effective against poison; all (? i. e. both kinds) exist with purple colour». The *Chiao-chou chi* cannot be later than the first half of the fifth century; it is even believed to go back to the fourth (cf. MASPERO, in *BEFEO*, XVIII, III, 22; the only difficulty for such a date is that a fragment, occurring in the edition of the *Ling-nan i-shu*, 2, 1 b, mentions the «king of Po-sü», i. e. Persia, and, if the fragment be genuine, would antedate by more than half a century the earliest

known mention of Po-ssü in Chinese texts; see PERSIE). The same form *pei-ch'ih*, certainly referring to the cowry, occurred in the *Ming-i pieh-lu* by T'ao Hung-ching (452-536, or 456-540; cf. *Pên-ts'ao kang-mu*, 46, 22 a). The *Liang shu* (54, 1 a), followed by the *Nan shih* (78, 1 a), gives the *pei-ch'ih* among the products of Lin-i (= Champa). In a note to ch. 6 of the *Suvarṇaprabhāsa* (黃, ix, 23 b), I-ching, discussing the value of the *kārṣāpaṇa*, reckons its value in *pei-ch'ih*; *pei-ch'ih* also occurs in his *Nan-hai chi-kuei nei-fa chuan* (致, vii, 89 b; cf. TAKAKUSU, *A Record of the Buddhist religion*, 192), and is still employed by Hui-lin, ch. 29 (爲, viii, 180 a). The T'ang *pên-ts'ao* (= *Pên-ts'ao* of the T'ang, written in the 7th century) said (*T'ai-p'ing yü-lan*, 807, 14 a; this passage is not included in the sections of the [*Hsin-hsiu*] *pên-ts'ao* of 659 recovered from Japan and published in the *Chuan-hsi-lu ts'ung-shu*): «The 貝子 *pei-tzū* is called *pei-ch'ih*; it is produced in the Eastern Sea». As Su Sung says, the name *pei-ch'ih*, «shell teeth», was given to the cowry on account of the «teeth» on both sides of the slit in its flat underside. This term, which is not given by TARANZANO, also occurs more than once in early Buddhist commentaries quoted in the *Bonzo jiten*, 114-115 and 454-455. It is reversed as *ch'ih-pei*, «teeth shell», in *Chiu T'ang shu*, 198, 8 b, but correctly written *pei-ch'ih* in *Hsin T'ang shu*, 221 A, 10 b. As for *pei-tzū*, it occurs in ch. 2 of the *Aśokāvadāna* (NANJĪŌ, *Catalogue*, No. 1343), translated in 512 (cf. also Hui-lin, ch. 76, in 爲, x, 22 a). In a work of c. A. D. 970 the cowry was called 白貝 *po-pei*, «white shell», a term which never became common (*Pen-ts'ao kang-mu*, *ibid.*). In his general description of India Hsüan-tsang says that the inhabitants use as currency «gold coins, silver coins, 貝珠 *pei-chu*, and small pearls (*hsiao-chu*)». JULIEN (*Mémoires*, i, 94) proposed to read *chu-pei* instead of *pei-chu*, and translated «pearl shells». However, it was not pearl oyster shells, but pearls themselves, which were occasionally used as a sort of currency. Even if we read *chu-pei* in this passage, we should be entitled to translate it «cowries» («shells [used as if they were] pearls»), because this is the explanation adopted by Hui-lin (爲, viii, 150 b) when *chu-pei* occurs in ch. 76 of the *Avataṃsaka*. (As a matter of fact JULIEN, tacitly abandoning his «pearl shells», gives «cowries» in his *Index*, *Mémoires*, ii, 546). *Chu-pei*, «cowries» occurs in a passage of the *Kuang-chih* on Burma (cf. *infra*, p. 555). The T'u-yü-hun women of Central Asia used to tie cowries (*chu-pei*) to the end of their plaited hair (*Sui-shu*, 83, 1 b; *Chiu T'ang shu*, 198, 4 b [confirmed by *T'ai-p'ing yü-lan*, 941, 1 a]; *Hsin T'ang shu*, 221 A, 5 b). In the notice of Liu-ch'iu (probably Formosa) in *Sui shu*, 81, 5 a, we are told that the men adorned their caps with *chu-pei*, and that the women sewed «spiral shells» (*lo*) on their clothes, and tied «small shells» (*hsiao pei*) to the lower hem; here again, I think that *chu-pei* means «cowry». But in Hsüan-tsang's text we should probably retain *pei-chu* («pearls [which are] shells») and translate it «cowries» just the same, since *pei-chu* is given as a synonym of *pei-ch'ih* in two early Japanese Buddhist commentaries quoted in FUJII's *Bongo jiten*, 114 and 454 (cf. also ODA Tokuno, 210<sup>2</sup>). Since T'ang times a current name of the cowry has been *pei-tzū*. Su Sung (1020-1101) says of it (*Pên-ts'ao kang-mu*, 46, 22 a): «The *pei-tzū* is the smallest of the *pei*; its shape is like [that of] a snail; it is about one inch long. Its colour is slightly white and red, but there are some which are dark purple and black. At present, many are strung together to be given to children as playthings. Northerners sew them as ornaments on to clothes and felt hats. Barbers use them

as ornaments for mirrors, and painters as polishers». The second part of the last sentence may be due to a confusion with the *tzü-pei*. As to the first part, it has a counterpart in another work of Sung times, the 爾雅翼 *Ērh-ya i*, which says of cowries (cf. *T'u-shu chi-ch'êng, ch'in-ch'ung-tien*, 156, 5 a) : «At present they are only used by barbers, as an ornament for mirror-belts (鏡帶 *ching-tai*)». I suppose the custom of the barbers was to hang a mirror at the waist.

But these same cowries, for which China had so little use under the Sung, turn up again in Chinese texts during the Mongol period, as the common currency of Yün-nan; Chinese statements are here in full agreement with those of Polo. Curiously enough, the 蠻書 *Man shu*, written in 864, which provides so much valuable information on the customs of the aborigines of Yün-nan, is silent about shell currency. It merely speaks of the necklaces of conch-shells (珂貝 *k'o-pei*), cowries (巴齒 *pa-ch'ih*; it is certainly *pa-ch'ih* which has been altered to 己齒 *chi-ch'ih* in our editions of Hui-lin, ch. 29 [爲, VIII, 180 a, and *Bongo jiten*, 114]; on *pa-ch'ih*, cf. *infra*), and pearls (*chên-chu*) worn by the women of a tribe north-east of Yung-ch'ang (*Chien-hsi-ts'un-shê* ed., 20 a). Further on (38 a), when describing the customs of the Nan-chao kingdom which then ruled over the whole of Yün-nan, it says : «The country does not use any coins. Whenever they barter for silk fabrics (*tsêng-po*), felt, woollens, gold, silver, turquoise (*sê-sê*), oxen, or sheep, they value them in strips (罽 *mi*) of silk, and say 'Such a thing is worth so many strips'». The natural conclusion would be that in the ninth century there was still no shell currency in Yün-nan. Yet this conclusion is contradicted by a quotation in the section on *pei-tzū* in the *Pên-ts'ao kang-mu* (46, 22 a) : «[李] 珣 [Li] Hsün says : 'In Yün-nan, [cowries] are very extensively used as money (*ch'ien-huo*) in trade exchange (*chiao-i*)'». Li Hsün was the author of a *Materia medica of sea drugs* (海藥本草 *Hai-yao pên-ts'ao*), written in the second half of the eighth century (cf. BRETSCHNEIDER, *Botanicon Sinicum*, 1, 45). This book is not attributed to Li Hsün, a Persian in *Wên hsüeh chia ta tz'ü tien* No. 1733, or in *Fên ming ta tz'ü tien*, p. 414. Not being able to trace the quotation further back than the *Pên-ts'ao kang-mu* of 1578, first published in 1596, I must leave in abeyance the question of its authorship and authenticity. There are many misquotations in the *Pên-ts'ao kang-mu*.

Moreover no mention of a shell currency in Yün-nan is made in Chou Ch'ü-fei's *Ling-wai tai-ta*, written in 1178, in his paragraph entitled *ta-pei*, «great *pei*», (7, 9 b). The text says : «In Hai-nan there is the *ta-pei*; it has a round back with purple spots; its flat face has a deep slit; on both sides of the slit there are fine 'threads' (縷 *lü*) which sink into the slit; this is what the *Pên-ts'ao* calls *tzü-pei* («purple *pei*»; cf. *supra*, p. 541). There are also small [*pei*], the breadth of a finger in size, with a back slightly bluish. In the Ta-li kingdom there are used for the decoration of armour and helmets. Moreover, in ancient times, cowries (*pei-tzū*) were used as currency (*t'ung-huo*); precious vessels were also made of them, and used in temples and at Court; at present, in the south, they are looked upon like oysters and clams; the things which are valued in ancient and in modern times are certainly not the same.» LAUFER, who used the present text in his *Chinese clay figures*, 193, partly misunderstood the description of the *tzü-pei* (which is certainly a *Cypræa*, though not the cowry). Moreover, he believed that it was the *tzü-pei* which was used for the decoration of armour in Ta-li, while in the text it is the small

slightly bluish shell, *i. e.*, in my opinion, the true cowry. But the main proof is this. Ta-li is Yün-nan. Although Chou Ch'ü-fei did not travel to Yün-nan, he knew that cowries were used there for decoration; on the other hand, he alludes to the cowry currency in ancient China. If cowry currency had been widespread in Yün-nan in his time, it would seem that he must have heard of it, and, if he had, he could not have failed to mention it, even, and the more so, if he should have mistaken it for a survival from China's past. So it may be that, in his days, the people of Yün-nan still valued objects mainly in «strips of silk», as they did in the ninth century. At any rate, if cowries were already more or less in use in Yün-nan as currency, these must have come from Burma or Siam, and were perhaps somewhat different from those which were sent there from Hai-nan for decorative use.

Whatever the truth of this matter may be, cowries were the only currency actually used in Yün-nan when the Mongols seized that province in the second half of the thirteenth century. From that date we find various new designations for the cowry. The term 貝錢 *pei-ch'ien*, «shell cash», which was used in T'ang times in the *Fan-yü tsa-ming* (cf. BAGCHI, *Deux lexiques sanskrit-chinois*, 50), never occurs, although it may have survived in Japanese: SAKAKI's edition of the *Mahāvvyutpatti* uses it, No. 9374 (read *haisen?*), although the true Japanese word for *pei*, «shell», is *kai*; the *Cypræa* is 寶貝 (Ch. *pao-pei*), read *takaragai* (from *takara*, «precious», and *kai*, «shell»; «Wohlstands-Muschel» in SCHILDER, 325, is misleading), and the cowry properly so called is 貝子 (Ch. *pei-tzū*), read *hidakaragai* (from *ki*, «precious» [?], *takara*, «precious», and *kai*, «shell»). In Chinese texts of the Mongol period we find *pei* or *pei-tzū*, as in literary Chinese or 貝 *pa*, or 貝子 *pa-tzū* (cf. *infra*); in Ming times *pa* or 海貝 *hai-pa* «sea *pa*» (cf. *TP*, 1915, 102, 110, 437; 1933, 416), or 海肥 *hai-pa* (*Ming shih*, 313, 4 *a*; *Ming T'ai-tsung shih-lu*, 77, 3 *a*; WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 29, 20 *a*; *Tien i*, 3, 10; misread 海肥 *hai-fei* by KLAPROTH in *JA*, Febr., 1834, 155, hence «Meerfelt» in SCHNEIDER, 107, and SCHILDER, 325), or 海吧 *hai-pa* (*Pên-ts'ao kang-mu*, 46, 22 *a*), or 螺貝 *lo-pei* (*Tien i*, 3, 10), or 螺吧 *lo-pa* and 螺巴 *lo-pa* (*Tung-hsi yang k'ao*, *Hsi-yin-hsüan ts'ung-shu*, ed., 2, 14 *b*; 7, 10 *b*, 12 *a*); also under the Ming, but mainly under the Manchu dynasty, 海巴 *hai-pa* (WANG Ch'i, *Hsü Wên-hsien t'ung-k'ao*, 33, 10 *b*; *Ssü-t'i ho-pi wên-chien*, 32, 101 *a*, and Chinese version of the *Mahāvvyutpatti*, Nos 5994, 9015, 9264, 9265, 9374), and 海貝 *hai-pei* (*Tien hsi*, 4, 16-17; *Hsü Yün-nan t'ung-chih kao*, 160, 32 *a*, 37 *a*).

The common element *pa* of most of these terms must be accounted for. In spite of the variety of the characters used to write it, and contrary to PARKER's opinion (cf. *Y*, III, 82), I do not think that it can be the transcription of a foreign word connected with the Thai name of the cowry to be mentioned further on. Nor do I believe it to be identical with the name of the shell called 吧 in the *Êrh-ya*. The dictionaries give *p'a* as the correct pronunciation of the latter character; yet, when translating the text of the *Êrh-ya*, I have transcribed it *pa* in agreement with a phonetic note appended to the commentary. But I think that its use in *hai-pa* is of scholarly origin, to connote a popular pronunciation *pa* of 貝 *pei* (\**puâi*; *lo-pa* is but a modern form of the ancient 寶貝 or 螺貝 *lo-pei* (\**lâ-puâi* > Mong. *labai*, «horn», originally «conch»; on *lo-pei*, cf. Hui-lin in 爲, VIII, 54 *b*; *Hsin T'ang shu*, 222 B, 6 *b*). We should have expected \**puâi* to give \**pai*, but there are many other cases in which the final *-i* has been dropped, such as 卦 *kua*

(\*k'ái), 罷 *pa* (\*b'ái), etc. This form *pa* must go back to T'ang times, since the 巴齒 *pa-ch'ih* of the *Man shu* (cf. *supra*, p. 544) can be no other than *pei-ch'ih*, «cowry».

When the Mussulman Sayyid Ajall was sent by Qubilai to establish a regular administration in Yün-nan, he was soon confronted with the problem of the cowry currency. We read in his biography (*YS*, 125, 2*a*) : «The people of Yün-nan used cowries (*pei*) instead of cash (*ch'ien*). At this time the paper-money system (*ch'ao-fa*) was first introduced, and the people were not pleased with it. Sai-tien-ch'ih (Sayyid Ajall) reported to the Court, which gave permission [to the people] to revert to their [old] custom.» The exact date is given in the *pên-chi* (*YS*, 9, 2*a*) : «The 13th *chih-yüan* year, in the first month . . . , on the *ting-hai* day (Febr. 7, 1276), the «moving Grand Secretary» of Yün-nan, Sayyid Ajall, came to report on the change of the names of the prefectures (*lu*) of Yün-nan. Moreover, he said that [the methods of] trade were not the same in Yün-nan as in China; the people [there] really did not understand the paper-money system. The best plan would be to permit the circulation of the cowries (*pa-tzū*) of both official and private origin, which would be more convenient for the people. The proposals were adopted.»

Less than four months later was issued an edict which has been preserved in the 通制條格 *T'ung-chih t'iao-ko*, 18, 20-21, under the heading 錢私 *Ssü-pa*, «Private cowries»; it is dated February 7, 1276. The question had been raised in 1275 whether people trading with Yün-nan ought to be allowed the free private import of cowries (*pa-tzū*) from the stocks existing in the warehouses of the Commissariat for maritime foreign trade (*shih-po-ssü*) in Chiang-nan. Conflicting opinions had been expressed as to the advantage or disadvantage of this free import for the authorities and for the population of Yün-nan. Private import was prohibited. The interest of this edict lies in the fact that it shows that cowries were exported to Yün-nan from ports at the mouth of the Yang-tsu, and that these cowries were not collected off the coasts of China, but came by sea from foreign countries.

On November 21, 1305, «10,000 ingots (*ting*) in paper-money [currency] were given to the 'moving Grand Secretariat' (*hsing-shêng*) of Yün-nan to be used concurrently with cowries (*pei*). As to the cowries, those which were not native products were to be treated in the same manner as forged paper-money» (*YS*, 21, 9*b*). Cowries have never been a native product of Yün-nan, and the so-called «native» ones are clearly to be understood as those which had been imported from Siam and possibly Burma and, in principle, at a more or less ancient date (on the direct trade from Siam to Yün-nan at the beginning of the fifteenth century, cf. *TP*, 1933, 383-385).

The importation of «private cowries» seems to have remained for many years a sore point with the Imperial administration in Yün-nan. In the *Yüan tien-chang* (20, 31*a*), an edict dated in the eighth moon of 1301 (September 3-October 2) states that the cowry currency (*pa-huo*) of Yün-nan worked under the same conditions as obtained for other currency in the rest of China, and that it was the official number of existing cowries which determined the price of goods. But of late, officials and private people had vied with one another to smuggle quantities of «private cowries» across the customs barriers of the province, taking advantage of neglect of duty or using bribery, and a final stop had to be put to such an evil practice. All such «private cowries» were thenceforth to be confiscated and officials neglectful of their duty to be punished.

As to the exchange value of the cowries, we know from *YS*, 12, 4*a* (from which it passed

into WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 4, 43 b) that on October 15, 1282, the following decision had been reached: «It was decided that the taxes (*shui-fu*) levied in Yün-nan should be on a gold (*chin*) basis, but payable in cowries (*pei-tzū*), one 錢 *ch'ien* of gold being valued at 20 索 *so* of cowries.» The *ch'ien*, one tenth of a Chinese ounce, is the *mace* of early European trade in China. This is the only text of Mongol times which mentions the *so* as the unit for counting cowries, and it does not tell us how many cowries made a *so*; but from later texts it will be seen that 80 cowries formed a *so*. It is thus clear that, when Polo says (cf. Vol. I, 277) that, in Yün-nan, «eighty cowries are worth one *saggio* of silver», he is alluding to the *so*.

The use of cowries in Yün-nan continued during the Ming dynasty; unfortunately, the most detailed information on this point is said to occur in 謝肇淛 HsIEH Chao-chih's 滇略 *Tien lio*, a work written c. 1600 (cf. *Ssü-k'u* . . . , 68, 39-40; HsIEH Chao-chih was a doctor of 1592), no copy of which is available in Paris. In 1385 it was decreed that the «autumn taxes» of Yün-nan might be paid «in gold, silver, cowries (*pei*), cloth, lacquer, cinnabar, and quicksilver» (*Ming shih*, 78, 1 b). In the sixth moon of the ninth year of Yung-lo (June 21-July 20, 1411) «the head official (*ch'ang-kuan-ssü*) of the local district (*tien*) of 溪處 Ch'i-ch'u in Yün-nan (south-west of Lin-an), who paid annually [to the treasury] in cowries (海肥 *hai-pa*), asked for a change of the assessment into silver and paper-money (*yin-ch'ao*); this was sanctioned» (WANG Ch'i, *Hsü Wên-hsien t'ung-k'ao*, quoted by FUJITA, 33 a; I have mislaid the reference to the original text). More details, but without the indication of the month, are given in *Ming shih*, 313, 4 a: «The ninth year of Yung-lo (1411), the head official (*ch'ang-kuan*) of the local district of Ch'i-ch'u, 自恩 Tzū-ên, came to the Court and offered a tribute of horses and of vessels of gold and silver; he was rewarded in accordance with the statutes. Tzū-ên then said that his office paid 79,800 *so* of cowries (*hai-pa*) annually, that these were not a product of the country, and asked to be permitted [to pay instead] with paper money and silver (*ch'ao-yin*), which would be more convenient. The Board of Revenue objected that the amount [of the tribute] had been fixed in Hung-wu (1368-1398) and that it was difficult to change the assessment. [But] the Emperor said: 'To [wish to] take what one has not is in truth to oppress the people; these are far away barbarians, and should be shown so much the more compassion.' The request was granted.» This text provides a good illustration of the number of cowries which were imported into Yün-nan, since a single local district had to pay annually to the Imperial Treasury 79,800 *so*, i. e. 6,384,000 cowries.

The officials who, in 1411, had opposed the Emperor's benevolent dispositions must have had their own way in the end, since it was not until 1555 that the Yün-nan authorities were permitted to cast copper cash, and this permission was rescinded in 1565. In 1576, «the Governor [of Yün-nan] 郭延梧 Kuo Yen-wu said that Tien (= Yün-nan) produced copper which was left without melting, while on the other hand cowries (*hai-pei*) were bought at a high price, [and that such a proceeding] was not advantageous; as a result, a Mint [for cash] was opened» (滇繫 *Tien hsi*, written in 1807, 4, 16-17; cf. also 袁嘉穀 YÜAN Chia-ku's 滇釋 *Tien i*, published in 1923, 3, 10). Again the Mint was closed in 1580, and «the copper cash in store in the [Yün-nan] treasury were used for the expenses of the troops in Kuei-chou». The Yün-nan Mint was not formally reopened until 1660 (*Tien hsi*, *ibid.*).

Even during the existence of the Mint of 1576-1580, Yün-nan had continued to pay



government taxes partly with cowries. In a well-informed note which appeared in the *North-China Herald* of 1889, p. 534 (it is reprinted in *JNCB*, XXIV [1890], 130-133), there is the following passage : « We are told that in 1578 the Government received from Yünnan 13,764 taels in paper money, 944 piculs of grain and 5,769 strings of shells. » The origin of the text is not indicated, but in fact it comes from WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 29, 20 a, and represents the amount of the annual ordinary taxes (*k'o-ch'ao*) which were imposed upon the province; extra taxes, for instance the « fish-tax » (*yü k'o-ch'ao*), implied only a payment in paper money and grain, without cowries (*ibid.*, 21 a). The translation is fairly accurate (« grain » stands for « rice and wheat » in the text; « picul » is misleading, as 石 *shih* must here be the measure of capacity of 100 pints, not the « picul » of 100 pounds; moreover, I doubt whether *shih*, even as a weight, in principle 120 pounds, could then have been, as in more recent times, the equivalent of the *tan* or picul of 100 pounds). But the fractions are omitted : the true figures are 13,764.255 taels in paper money, 944.8885 *shih* of grain, and 5,769 strings (*so*) 20 𠄎 *shou* of cowries (*hai-pa*). Now, as will be shown further on, the *shou* consisted of 4 cowries; but 20 *shou*, *i. e.* 80 cowries, made a complete « string » (*so*), so that there must be some mistake in the figure. The passage is, however, of real interest because it is the only one known to me where a subdivision of the *so* is actually used in an account.

Since the decisions of 1411 and 1576 had hardly been enforced, the importation of cowries into China by sea went on until a much later date. Some may have come from the Liu-ch'iu, the regular tribute of which included cowries (*hai-pa*; cf. WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 33, 10 a); but it is possible that these remained at the Capital to be used as ornaments, just as did the « gastropod shells » (*lo-k'o*; these were probably conches) of the same origin. The cowries intended for currency in Yün-nan were imported by regular maritime trade. According to the tariff of 1589, imported cowries had to pay a duty of 0.02 ounce of silver per *shih*, and, according to that of 1615, of 0.017 ounce (cf. *Tung-hsi yang k'ao*, 7, 10 b, 12 b). Cowries fell into disuse only in the middle of the seventeenth century, as can be deduced from the final reopening of the Mint in 1660 and also from a passage in the early *Yün-nan t'ung chih* which is in the Bibliothèque Nationale (COURANT, *Catalogue*, Nos 1785-1790, 7, 5-6). COURANT says that this copy is a reprint, made in Ming times, of the edition published in compliance with an Imperial edict of 1454. But he is mistaken. The edition speaks of the Ming as « Ming », not as « Ta-Ming », and mentions not only the latest reigns of that dynasty, including Ch'ung-chêng (1628-1644), but also the succeeding Ch'ing dynasty; so it is clearly a redaction of early Ch'ing times. The passage on the cowries is as follows : « Formerly cowries (*pei*) were in great use; popularly they were called 𧄂子 *pa-tzŭ*. One was called 莊 *chuang* (etc.; cf. *infra*, p. 549); then follows a long discussion on the use of cowries in ancient China, which, the authors thought, the custom in Yün-nan continued) . . . From [T'ien-ch'ih (1621-1627) and [Ch'ung-chêng (1628-1644) of the Ming and afterwards, silver rose in value and cowries (*pa*) lost value; finally they were withdrawn and put out of use; the Barbarians adopted the currency system of Our dynasty (the Ch'ing), and for a long time they have not employed [the cowries] any more. But we give a full notice of them to preserve [the memory of] the ancient custom. »

In his *Pên-ts'ao kang-mu* (46, 22 a), LI Shih-chên, certainly drawing from an earlier Ming

work which he does not mention, says of the cowries : «At present they are used only in Yün-nan, where they are called *hai-pa*. One cowry is a 庄 *chuang*; four *chuang* make a 手 *shou*; four *shou* make a 苗 *miao*; five *miao* make a 索 *so*». LI Shih-chên's source is very probably the *Ming i-t'ung chih*, written in 1461, where the same text occurs (86, 4), with a final sentence : «Even for paying taxes they use them». KLAPROTH published this scale in *JA*, Febr., 1834, 155, probably from some encyclopaedia, and gave 莊 *chuang* for the first term. This form also occurs in the list, otherwise identical with that of the *Pên-ts'ao kang-mu*, which is given in the 湧幢小品 *Yung-ch'uang hsiao-p'in* (30, 10 b), a work written in the first half of the seventeenth century (its preface is not dated, but the author, Chu Kuo-chêng, was a doctor of 1589; the last date I have noticed in the work is 1616, in 30, 26 b). The *Yung-ch'uang hsiao-p'in* may have drawn from the Ming redaction of the *Yün-nan t'ung-chih*, prepared in compliance with an Imperial order of 1454, which perhaps also gave 莊 *chuang* (at least such is the form, as we have seen, in COURANT, *Catalogue*, N° 1785, 7, 5-6). In his *Hsü Wên-hsien t'ung-k'ao* 王圻 WANG Ch'i, whose preface is dated 1586, after quoting the text of 1282 on the value in gold of 20 «chains» (*so*) of cowries, added the following note (18, 4 b) : «In Yün-nan one cowry is a 庄 *chuang*; 4 *chuang* make a *shou*; 4 *shou* make a *miao*; 4 *miao* make a 索 *t'o*». FUJITA, in his commentary on the *Tao-i chih-liao* (33 a), expresses the opinion that *t'o* may here be a corruption of *so*. It is possible that he is right, and *so* occurs elsewhere in WANG Ch'i's work (cf. p. 547), but even then the text would not be satisfactory, since it required not four, but five, *miao* to make a *so*. There is no indication, however, that, in addition to the «string» or *so* of 80 cowries, there should have been in Yün-nan a «bag» or *t'o* of 64; and we ought probably to read «five» *miao*. The encyclopaedia *Ko-chih ching-yüan*, published in 1735, quotes (33, 26 b) from the 稗事類編 *Pai-shih lei-pien* a passage which mentions the 莊 *chuang*, the *shou*, and, omitting the *miao*, says that twenty *shou* make a *so*. This is not incorrect in itself, but one may never be sure that the quotations made in the *Ko-chih ching-yüan* are accurate. I have seen the *Pai-shih lei-pien* quoted more than once, and I even think I remember having formerly handled a modern reprint in small size; yet I can find no indication of a work of that title in the catalogues at my disposal. A natural inference would be that the work referred to, with a slight change in the title, is the 稗事彙編 *Pai-shih hui-pien* in 175 chapters, of which there is no recent edition available, at least in its original form (cf. *Ssü-k'u...*, 132, 14). But this too is the work of WANG Ch'i, and the information given in his *Hsü Wên-hsien t'ung-k'ao* is not in agreement with that in the *Pai-shih hui-pien*. This suggested confusion between *lei* and *hui* in the title of the book actually occurs in the *T'ao shuo* by CHU Yen.

From the texts quoted above it is clear that the *so*, which we have already found mentioned in a text of 1282, was a count of 80 cowries ( $4 \times 4 \times 5$ ). In Ming times, the people of Yün-nan also used another character to write the name of this unit, namely 𠄎 *sa*. The *K'ang-shi tzü-tien* quotes the 稗史 *Pai shih* as saying : «The people of T'ien (= Yün-nan) say that 80 cowries (*pei*) make one *sa*». KLAPROTH's source, too, mentioned this *sa*. Here again I am not in a position to say what work the *Pai shih* may be; it can hardly be the *Pai-shih lei-pien* (? *Pai-shih hui-pien*), since the latter speaks of *so*, not of *sa*. From YÜAN Chia-ku,

himself a native of Shih-p'ing (north-west of Lin-an in Yün-nan), we learn that an inscription existing in the Temple of the Earth Lord (T'u-chu-miao) at Shih-p'ing speaks of «so many *sa* of cowries (*pei*)» (*Tien i*, 3, 10). According to an unidentified text which was used by KLAPROTH, the *so* was worth 6 *li*, i. e. 0.006 of an ounce (of silver) or tael. I find the same information in the *Tien hsi* (12 a, 21 b; also quoted by YÜAN Chia-ku), in the course of the following text : «Nowadays in Tien (= Yün-nan) the use of cowries (*pei*) has dwindled to very little, but Barbarian women near the border collect them as head ornaments. Popularly they are called *pa*. In using them, one piece made a 欸 *chuang*; four *chuang* made a 首 *shou*; four *shou* made a 緡 *min*, also called a *miao*; five *min* made a 卉 *hui* (certainly a wrong reading for 𠄎 *sa*; it may not occur in the original edition of the *Tien hsi*; I have had to use a reprint of the Kuang-hsü period); *hui* (read *sa*) is 索 *so*. A *so* is worth 6 *li* of silver, but can be exchanged for several tens of minor things; that is why the Barbarians found it convenient. . . . » The author goes on to state that this was a survival of the old Chinese cowry currency, and that cowries formed the greater part of the currency used in Yün-nan under the Ming dynasty. This text of late date already betrays a corrupt tradition; the terms *chuang* and *shou* are written in a new manner, and I doubt whether *min*, which is itself in China one of the designations of a «string» of cash, was ever really used as a substitute for the *miao* of Ming texts. At any rate, the confusion shows that the terms *chuang* and *shou* conveyed no meaning to the author of the *Tien hsi*.

The statement in SCHILDER, 318, based on SCHLEGEL's *Nederl.-Chines. Woordenboek* (II, 408, s. v. *kauri*), that the cowry currency remained in use in Yün-nan down to the nineteenth century is not borne out by any authority. Nor can I find the source on which ANDERSSON relied (*Archaeolog. Studien in China*, 78) when he stated that the Lolo still used a cowry currency. On the contrary, VIAL is positive (*Dict. français-lolo*, p. [10]) that, «for a long time now this shell is only used to adorn the caps of young girls and little boys». This is also true of other tribes in Yün-nan, for instance the Li-su (cf. K. WARD, *The Land of the Blue Poppy*, 142). Sometimes cowries serve as ornaments for animals (cf. BOCK, *Im Reiche des weissen Elefanten*, 1885, p. 150). The cowries used in Yün-nan were white, according to Polo, and their Lolo name (Ñi dialect) is *žè-mà-šlu*, lit. «white fruit of the water (= the sea)» (water + fruit + white); but the Chinese cash, which replaced the cowry, inherited this name and is called *žè-mà* (cf. VIAL, *op. cit.*, 95, 297).

I have no explanation to proffer for the designations *chuang*, *shou*, and *miao* of Chinese Ming texts. Except *shou*, «hand», which might be interpreted as a «handful» of four cowries, they do not make much sense in Chinese, and, although they do not look like transcriptions, it may well be that, in fact, they transcribe native names, either Thai, or Lolo, or even Burmese. But something more may be said about *so* or *sa*. According to Polo, the rate of exchange in central Yün-nan between gold and silver was one to eight, and from the *YS* we know that 20 *so* were equivalent to 0.1 ounce of gold. Consequently, the silver value of the *so* in Polo's time ought to have been 0.04 tael, and, if we should take the rate of one to five, which is indicated by Polo for «Zardandan», i. e. south-western Yün-nan, the silver value of the *so* would be 0.025 tael. These are, however, irreconcilable either with Polo's statement that 80 cowries, i. e. a *so*, were worth one *saggio* of silver, since his *saggio* seems to have been the *ch'ien*, i. e. the *mace*, 0.1 tael, or with the later Chinese equivalence of the *so* with 0.006 tael. We lack sufficient data to enable

us to reach a final solution, but, as will be seen further on, Polo's information finds curious counterparts in other sources. As to *so*, it means «rope», «cord», «string», and would be a fitting designation for a «string» of cowries; but it may also, in principle, be a transcription, with a semantic adaptation in the choice of the transcribing character. It has been understood by Chinese scholars as meaning «string»: so the «*Pai-shih lei-pien*» says that, if twenty *shou* (= 80 cowries) made a *so*, it was «because, when reaching that number, [the cowries] could be strung (*kuan*)», and the *Yung-ch'uang hsiao-p'in*, that «*pei* were made into a *so* as cash is made into a 緡 *min*» (*min* is one of the terms used for a «string» of cash). On the other hand, *so* is \**sák*, and passed through a \**sâ* stage c. A. D. 1000-1100, before becoming the modern *so* c. 1100-1200; in such a case *so* could be a purely Chinese name, borrowed as *sâ* by one of the native languages c. A. D. 1000-1100, and from that native language retranscribed in Ming times as *sa* in Chinese. A somewhat similar case occurred with the Lolo word for «cotton», *sa-la*, which I take to represent Chinese *so-lo* (\**sâ-lâ*; see COTTON, p. 479). If this be right, the fact that the native word for a «string» of cowries was borrowed from the Chinese of c. A. D. 1000-1100 would support the deduction, suggested by the text of the *Man shu*, that in the second half of the ninth century there was not yet a cowny currency in Yün-nan.

The modern lolo language of Yün-nan is of no direct help, since it has taken over, in a modified form, the Chinese names of weights and measures. So in the Ñi dialect the ounce or tael is *ló* < Ch. *liang*; 0.1 of a tael is *ts'ó* < Ch. *ch'ien*; and 0.01 of a tael is *fá* < Ch. *fên*. On the other hand, starting from the old Chinese system of the «string» of 1000 cash which was equivalent to one tael of silver (in fact, it has varied in modern times between 700 and 800 cash), the Lolo use the same words *ló*, *ts'ó*, and *fá* for a «string» of cash, and 0.1, and 0.01 of a «string», respectively (cf. VIAL, p. [10]). As a consequence, *ts'ó* (< Ch. *ch'ien*) is not only 0.1 tael, i. e. one «mace», but also 100 cash theoretically (in fact 70 to 80). Now the *Hsü Yün-nan t'ung-chih kao* (ch. 166) contains a comparative vocabulary of the native languages, in which the Lolo words are quoted from the 彝雅 *Tsuan ya*, evidently a Lolo-Chinese Vocabulary; I do not know its date, nor do I think that it has ever been published independently; it is probably not earlier than the nineteenth century, but the words are often quite different from those given in VIAL's *Dictionnaire français-lolo*. In this chapter 166, 27-28, we read that, in Lolo, «pound» (斤 *chin*) is 己 *chi* (VIAL : *ce*), «ounce» or «tael» (兩 *liang*) is 來 *lai* (VIAL : *lo*), «0.1 ounce» or «mace» (錢 *ch'ien*) is 撒 *sa* (VIAL : *ts'ó*), «0.01 ounce» or «candareen» (分 *fên*) is 他分 *t'a-fên* (VIAL : *fái*; *t'a* is in fact «one», VIAL : *t'i*; cf. *ta*, «one», in Burmese). All these words are clearly borrowed from the Chinese, mostly in denasalized forms, with one exception : *sa* cannot represent *ch'ien*. But if we remember that, in modern Lolo, the Chinese copper cash has taken over the name *zè-mà* of the cowny, it will appear quite natural that for 100 cash (in fact 70 or 80), the Lolo should have retained the well known name *sa* of the «string» of 80 cowries. In this one case, and owing to the wide diffusion of the word *sa*, the Lolo was spared, at least for a time, from borrowing yet another Chinese term. Incidentally, it must be more than a fortuitous coincidence when Polo makes 80 cowries, i. e. one *so*, equal to one *saggio* of silver, i. e. one *ch'ien*, and in modern Lolo *sâ* (= *so*) is used with the same sense of *ch'ien*. If I am right in my contention, the word *sa*, used instead of *so* for a «string» of cowries in Ming times, probably represents the

form *sa* of the Lolo, and this Lolo *sa* may well itself have been borrowed from the Chinese *so* (\**sāk* > \**sā*) c. A. D. 1000-1100. Such a case, added to that of Lolo *sa-la*, «cotton», will have to be remembered in future attempts to determine the part played by Thai and Lolo elements, respectively, in the culture of the mediaeval Nan-chao and Ta-li kingdoms of Yün-nan.

At whatever date we should place the beginning of the cowry currency in Yün-nan, the origin of this currency is perfectly clear. We are always too prone to attribute to China the various elements of Yunnanese culture. As a matter of fact, much of it comes from the south (see COTTON), and the southern origin of the cowry currency is amply proved by the mere fact that the «string» was of 80 cowries. This leads us to examine the problem of the cowry currency in India and in the regions which have long been under Indian influence.

Apart from Yün-nan, Polo speaks of a cowry currency in «Toloman», Bengal, «Caugigu», and «Amu» (cf. Vol. I, 298); and in the description of «Lochac» (cf. Vol. I, 370) he says that from this kingdom go all the cowries used in the above-named provinces. «Toloman» was in the north-eastern part of Yün-nan, and may have shared in its cowry currency. Of Bengal, where cowries remained in use almost to our day, Polo speaks only from hearsay, repeating what he was told in Yün-nan. «Caugigu» (< Ch. Chiao-chih-kuo) and «Amu» (< \*Annam) are both Tongking, which he does not seem to have even visited; but he heard of Tongking as Chiao-chih-kuo in Yün-nan, and of Annam probably when travelling by sea from China to Champa. There is no indication that a cowry currency ever existed in Tongking itself, but it may have been used more or less on its north-western borders. As to «Lochac», this is certainly Siam, where a cowry currency obtained down to the middle of the last century (the identification of «Lochac» with the Pulo Condore islands in JACKSON, 183, and ANDERSSON, *Children of the Yellow Earth*, 296, is untenable). Siam, however, imported her cowries from the Maldives and later also from the Philippines. Polo never went to «Lochac», and what he really meant, and what, in my opinion, was probably quite true in his time was that the cowries used in Yün-nan mainly came from «Lochac», *i. e.* Siam. But they were not produced in Siam.

Polo's chapter on «Lochac» is the most ancient source which refers to cowries in Siam. Next come Chinese texts. Wang Ta-yüan's *Tao-i chih-liao*, dated 1350, says of 羅斛 Lo-hu (Polo's «Lochac», Siam; FUJITA's ed., in *Hsüeh-t'ang ts'ung-k'o*, 32 b; cf. ROCKHILL in *TP*, 1915, 110): «It is the rule to conduct their trade with cowries (*pa-tzū*) instead of coins. Ten thousand of them are equal in value to twenty-four taels (*liang*) in *Chung-t'ung* paper money. It is extremely convenient for the people.» In 1416 (? or shortly after 1433) Ma Huan is content with stating that the people of Hsien-lo (=Siam) «in trading use cowries just as [we do] copper cash» (*TP*, 1915, 102). Fei Hsin, writing in 1436, copies into his paragraph on Hsien-lo what the *Tao-i chih-liao* said of the use of cowries in Lo-hu, but gives the value of ten thousand cowries as «twenty» taels instead of «twenty-four» (*TP*, 1915, 105); but this seems to be a slip or a corrupt reading. With the same reading «twenty» (and with an additional change, *kuan*, «string» instead of *liang*, «tael»), Fei Hsin's text has passed into *Shu-yü chou-tzū lu*, 8, 12 a. It is difficult to determine the real value of the equivalence proffered by Wang Ta-yüan. In principle, «*chung-t'ung* paper money (*ch'ao*)» is the name of the banknotes issued by Qubilai in the *chung-t'ung* period (1260-1263), although they had been replaced in the course of Qubilai's reign by others,

the *chih-yüan* notes, the rate of exchange of which against silver was five times higher. These *chih-yüan* notes never met with general acceptance, and the *chung-t'ung* notes were revived and retained in fact until the end of the dynasty. More will be said on the subject under « Paper Money ». Suffice it to state here that, as a rule, the actual value of the *chung-t'ung* paper money in silver was only one tenth of its nominal value. So 10,000 cowries should have had in Siam, c. 1350, a value of taels 2.4; but this seems to be excessive if we remember that, in Yün-nan, there must have been more than 13,000 cowries to one tael. Cowries ought to have been cheaper in Siam than in Yün-nan, on account of the cost of transportation. A possible solution would be to imagine that silver was comparatively scarce in Siam. The question, however, is rendered still more intricate by the fact that, according to Wang Ta-yüan (13 a), the people of Chiao-chih (= Tongking) counted 67 cash instead of the official number of 70 in exchange for « one tael of *chung-t'ung* silver » (*chung-t'ung yin i liang*). But I think that *yin*, « silver », is here a corrupt form of *ch'ao*, « paper money »; in such a case the « *chung-t'ung* paper money tael », being one tenth of a silver tael, would exactly correspond to 70 cash of a « string » of 700 cash which was itself worth one silver tael. Wang Ta-yüan (114 a; cf. *TP*, 1915, 445) speaks also of « *chung-t'ung* paper money » in his description of Wu-tieh (Orissa); there was there a silver coin (the *ṭāṅga*) which was valued at « ten taels of *chung-t'ung* paper money » and exchanged for « 11,520 odd cowries »; this gives « 11,520 odd cowries » to a silver tael. We may doubt that the cowries can have fetched in Siam more than twice as much as in Yün-nan or in Orissa, and the more so in view of the information we have on the low value of cowries in Siam in the seventeenth to nineteenth centuries.

In CHÊNG Hsiao's *Huang-Ming ssü-i k'ao*, dated 1564 (*Kuo-hsüeh wên-k'u* ed., 1, 63), and in MAO Jui-chêng's *Huang-Ming hsiang-hsü lu*, dated 1629 (Nat. Libr. of Peiping ed., 4, 16 b), the Siamese are said to « use cowries (*hai-pa*) in trade ». Similarly, in WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao* (236, 2 b) and in the original redaction of the *Kuang-yü chi*, dated 1600, we read of Siam that « it makes use of cowries (*pa-tzũ*), instead of cash, as currency » (24, 11 a).

The *Tung-hsi yang k'ao*, written in 1617-1618, says in its paragraph on Siam (2, 14 b) : « Their custom is to use cowries (*hai-pa*) instead of cash (*ch'ien*). If one year they did not use cowries (*pa*), there would be a great epidemic in the country. So they go on [using cowries] and do not change [the currency] ». The same text, though in an abridged form, has passed into the *Ming shih*, 324, 8 b. The interest of this notice is to show that, at least popularly, cowries were supposed in Siam to have, beside their intrinsic value, a prophylactic use. This falls in with the earlier Chinese remark that shells are very efficient against poisoning. No such belief is mentioned by SCHILDER (p. 328), nor by JACKSON.

According to ANDERSSON (*Children of the Yellow Earth*, 301), « at the end of the seventeenth and as late as the middle of the eighteenth century the cowry was used as money in Siam, but in 1881 it was no longer in use in Bangkok ». The last statement is true, although cowries were then still used as money in the interior of the country (cf. BOCK, *Im Reiche des weissen Elefanten*, 289); but the abandonment of the cowry currency even in the region of Bangkok was recent, and even to foreigners in Siamese service salaries were still paid in cowries c. 1840 (cf. SCHNEIDER, 107); in 1863 BASTIAN still found them used throughout the country (cf. BASTIAN,

*Reisen in Siam im Jahre 1863*, III, 44, 213). Before disappearing, the cowry currency may have undergone a last development in the Shan states and perhaps in Siam. CORDIER (Y, II, 74) had quoted PARKER as saying in the *China Review*, XXIV (not XXVI), 106, that he had still seen « porcelain money » in the Shan states. To this Parker replied in 1904 : « The porcelain coins which . . . I myself saw current in the Shan states or Siam about ten years ago were of white China, with a blue figure, and about the size of a Keating's cough lozenge, but thicker. » So they were not real cowries, but imitations in porcelain, like the ancient Chinese imitations in musselshell, bone, stone, or metal. I am not certain, however, that they were really used as currency in « the Shan states or Siam », and not meant for instance for gambling, as is still the case with true cowries in India (cf. JACKSON, 170-171), in Nepal (cf. W. Brook NORTHEY, *The Land of the Gurkhas*, Cambridge [1938], 107-108), and in Tibet (cf. S. CHANDRA DAS, *Tibetan-English Dictionary*, 300). Stewart CULIN, *Chinese games with dice and dominoes (Report of the Smithsonian Institution for 1893)*, 501, has illustrated the use of cowries as counters in the Siamese backgammon. Moreover, *lén biá*, « to play cowries », is the usual Siamese term for « to gamble » (cf. PALLEGOIX, *Dictionarium linguae Thai*, 54). Another possible solution would be to see in these porcelain coins the direct descendants of the ceramic coins which, about the end of the seventeenth century, GERVAISE mentions as being used, with the sanction of the king, in distant parts of the country. But, in such a case, it would be a matter of surprise that no traveller should have noticed them in the intervening period.

The Siamese name of the cowry is *biá* (CUAZ, *Dict.-franç.-siamois*, 151), in Laotian *biá hói* (GUIGNARD, *Dictionnaire laotien-français*, 181; *hói* means « shell »; CUAZ, *Lexique français-laotien*, s. v. « cauris », gives « *ngön biá* » and « *ngön hói* », rather surprising since *ngön* means « silver »). This is certainly the word which the Chinese-Papai Vocabulary of the Ming dynasty (87 b) transcribes *pieh* = *bie*, *biä*, and perhaps the same as that which the Chinese-Pai-i Vocabulary (27 b) transcribes *mi*. It is not peculiar to the Thai languages. In Khmer, « shell » is *bié* (TANDART, *Dict. français-cambodgien*, I, 418), clearly the same as Siamese *biá*. But there is also a Khmer form *bier*, « cowry », with which corresponds Cham *biör* (cf. CABATON, *Dict. cham-français*, 358). I shall presently revert to these forms with a final -r.

The interesting point is that the Thai forms of the word for « cowry », such as Siamese *biá*, Laotian *biá hói*, etc., cannot be separated from Malay *biya*, « shell » in general, and especially « cowry » (many details of the various species of *biya* are given by RUMPH [« RUMPHIUS »] in his *D'Amboinsche Rariteit-kamer*, 1705, ed. 1741). Spelt in Arabic with the same letters, there is another Malay word pronounced *bēya*, which means « tax », « duties », and, in agreement with FAVRE's surmise (*Dict. mal.-franç.*, II, 185-186), it is probably a doublet of the former word, recalling a time when taxes were paid in cowries. I am not in a position to decide whether the Thai borrowed the word from the Malays, or the reverse; in principle, the first hypothesis seems more probable. As to the phonetic resemblance between *biá*, *biya* (*bēya*) and the Ch. *pei* (\**puâi*), I hold it to be merely accidental. But I would pass no judgment on another possible connection. We have seen that there are forms ending with -r (Khmer *bier*, Cham *biör*); SCHILDER, p. 324, quotes from QUOY and GAIMARD, *Voyage de l'Astrolabe*, Zool. III (Paris, 1834), a form *puré*, « cowry », in the Tonga islands. I am in some doubt as to the true value of this

word; in DUMONT D'URVILLE, *Voyage de l'Astrolabe*, Philologie, 1833, 8 v<sup>o</sup>, 2nd part, 162, «poure» is given as a word heard by GAIMARD at Tikopia, but is stated to mean «coquillage», i. e. «shell» in general. Now the Singhalese name of the cowry is *bella*, in Maldivian *boli*. I leave it to others to decide whether *bier*, *björ*, and *puré* (?) can be connected with *bella*, *boli*, and whether a liquid has possibly been dropped in *biâ*, *biya*. On the other hand, the Portuguese *buzio* (Span. *bucio*, > French *bouge*, Dutch plur. *boegies*, *bougies*, and, erroneously, *bon-gies*), which SCHILDER (p. 324) has tempted to derive from the Siamese *biâ* or Maldivian *boli*, can have nothing to do with either. The problem of the cowry is not touched in Ph. N. BOSE's *The Indian Colony of Siam*, Lahore, 1927.

From the Indian Ocean, cowries could have reached Yün-nan not only through Siam, but also through Burma. Unfortunately, I find but little information on the use of cowries in Burma. SCHILDER, 316, mentions Burma among the countries which used the cowry for ornament, but says nothing of a cowry currency. In the *Kuang chih* (c. A. D. 400?; see COTTON, p. 462) «conch» (𑖀𑖄 *k'o*) and «cowries» (*chu-pei*; on this term, cf. *supra*, p. 543) are listed among the «products» of the kingdom of P'iao, i. e. Lower Burma (cf. *T'ai-p'ing yü-lan*, 359, 15; the name of the country has been altered in our editions to P'iao-jên, but the original is not doubtful). A more precise statement occurs in Sulaymân (*Fe*, 44); in his notice of Rahmâ, i. e. Pegu, or Lower Burma, he states that «among the inhabitants barter is conducted by means of cowries; it is the currency of the country, i. e. its wealth». So there must have been a cowry currency in Pegu by the middle of the ninth century. But HARVEY, who cites the above text in his *History of Burma* (p. 10), gives no confirmation from any other source. The Burmese name of the cowry is *kriwé* > *kyiwé*.

Of the «salt money» of southern Ssü-ch'uan (see «Gaindu») Polo says that 80 «loaves» of it were worth one *saggio* of fine gold (cf. vol. I, 275, and Y, II, 54, 57-58), i. e. one *ch'ien* of gold, the very value assigned by the YS to 20 *so* of 80 cowries. As a consequence, 80 cowries ought to be worth 4 loaves of salt or, to express it otherwise, one loaf of salt was worth 20 cowries. I am too poorly informed on the subject of the history of the «salt money» in Ssü-ch'uan to be able to decide whether its value in multiples of 20 (i. e.  $4 \times 5$ ), with a higher unit of 80 (i. e.  $4 \times 20$ ), be accidental or may be connected with the system used in reckoning cowries. But, for the latter, there can be no doubt that the system is the one which obtained of old in the countries bordering on the Indian Ocean.

I lack all information on the ancient method of reckoning cowries in Indonesia, but more is available on the ancient practice in Siam. LA LOUBÈRE, who in 1687-1688 found the cowry currency used throughout the country, says (*Description du royaume de Siam*, Amsterdam reprint, II, 222-223) that the cowries came from the Maldives, and occasionally from the Philippine Islands, and that 6,400 were worth one silver tical (but, contrary to SCHNEIDER, 107, and JACKSON, 171, he does not mention Borneo, nor does he say that the cowries were carried by ships as ballast) The same rate was indicated in 1688 by GERVAISE (*loc. cit.*, 152), according to whom there were 800 cowries to one *fûâng* (i. e. 1/8 of a tical), and it still obtained in 1822 (cf. CRAWFURD, *Journal of an Embassy . . . to the Court of Siam . . .*<sup>2</sup>, London, 1830, II, 34). If we take YULE's figures (*Hobson-Jobson*<sup>2</sup>, 888, 918), the tical is 225.5 English grains, and the Chinese tael almost 580 grains;



this would give, in round numbers, 16,800 cowries for one tael. I am afraid, however, that such a calculation is misleading, since it does not take into account the indications of earlier travellers as to the scale of coins and weights which obtained in Siam at the end of the seventeenth century. According to LA LOUBÈRE (II, 48), GERVAISE (p. 152), and SPARR DE HOMBERG (in *JA*, 1920, II, 98), there were in Siam 20 taels to the catty or pound (and not 16 as in China). The tael itself was of four tical, and the tical of four mace («mas» in SPARR DE HOMBERG; «mayon» in LA LOUBÈRE, who says that it was the name used by foreigners for the Siamese «seling» [*sälüng*]; on «mayon», cf. *mayam* in *Hobson-Jobson*<sup>2</sup>, 530 b, and in *JA*, 1920, II, 127); as a result the Siamese tael was of 16 mace like the Malay one, not of 10 mace like the Chinese. One mace was of two *füâng*, as is still the case, since the *füâng* continues to be 1/8 of a tical. But the result is that there were 32 *füâng* to one tael, and, with 800 cowries to one *füâng*, the Siamese tael would be worth 25,600 cowries. On the other hand, LA LOUBÈRE is positive that the Siamese catty (of 20 taels) was worth only 8 Chinese taels (of 16 taels to the catty), which makes one Chinese catty worth two Siamese; consequently there must have been, in Siamese value, 51,200 cowries to one Chinese tael. This is irreconcilable with the earlier data in the *Tao-i chih-lio*, which would suppose for the cowries in Siam a value more than twelve times greater. Another indication in SPARR DE HOMBERG is no less puzzling. According to him, the Siamese mace («mas») was worth 800 cash («casjes»). Now, the mace being of two *füâng*, and the *füâng* exchanging for 800 cowries, this would make two cowries for one cash. If by cash SPARR DE HOMBERG meant the Chinese copper coin (no copper coins were then cast in Siam), cash can never have been so cheap in Siam as to exchange at the rate of more than 25,000 for one Chinese tael, while in China itself the rate was between 700 and 800. But if by «cash» SPARR DE HOMBERG meant the cowries, he overvalues them, since his rate would give only 400 cowries to the *füâng*, instead of the 800 which all sources state to have been the accepted value from the seventeenth to the first half of the nineteenth century.

Whatever the truth may be, the value of the cowry, which was still at the rate of 6,400 cowries to one silver tical in 1822, must have much declined in the following decades, since one tical actually exchanged for from 8,000 to 9,600 cowries according to BASTIAN, or even for 17,600 according to PALLEGOIX (cf. SCHNEIDER, 108). The drop in value may be due, to some extent, to a change in the kind of cowry which came to Siam. There can be no doubt, in view of the Chinese texts of the early fifteenth century, that, at that time, the bulk of the cowries came to Siam from the Maldives, and consequently were *Cypræa moneta*. But, towards the end of the seventeenth century, LA LOUBÈRE (*Description du royaume de Siam*, I, 222) has already mentioned the Philippine Islands as a small source of supply. Still earlier, in 1609, Antonio DE MORGA (*Sucesos de las Islas Filipinas*, RIZAL ed., 279; H. E. J. STANLEY's transl., Hakluyt Society, 285) had said that «siguei» (*i. e.* *sigay*, the Tagal word for «cowry») were shipped from the Philippines to Siam and Cambodia. In 1676 NAVARETTE (*Tratados historicos... de China*, 61) had spoken of the small shells, very beautiful, called «Sigueyes», which came from the «coast of India» and Manila and were used in Siam as minor currency; the same cowry currency obtained at Surat and came there from the «Baldivia», *i. e.* Maldives. GERVAISE (*Hist. nat. et polit. du royaume de Siam*, Paris, 1688, 4<sup>o</sup>, 152) says that ships brought the «shells» («coquilles») to Siam from the Moluccas

and Philippines. The cowry found in the Philippines, however, like that of the eastern coast of Africa, is not the *Cypræa moneta*, but the *Cypræa annulus*, and MARTENS states in 1872 (SCHNEIDER, 108) that all the cowries he still found on the market in Siam were *Cypræa annulus*. That the fall in the value of the cowry in Siam c. 1830-1850 should be connected in any way with the substitution on a large scale of the *Cypræa annulus* for the *Cypræa moneta* is, however, by no means certain, since a similar depreciation occurred about the same time in Orissa, where the *Cypræa moneta* alone seems to have circulated.

From GERVAISE's description (p. 151-152), « round on one side as a musket-ball, and on the other flat with a slit in the middle », as well as from the plate in LA LOUBÈRE, it is clear that the former silver coinage of Siam was but an imitation of the cowry, and even now the monetary nomenclature retains traces of the former cowry currency. In contemporary Siam, as in the seventeenth century, the money unit is the « tical » (this term, used by foreigners, is not Siamese; cf. YULE, *Hobson-Jobson*<sup>2</sup>, 918; Sir R. TEMPLE, in *Indian Antiquary*, XXVI, 253 sq.; FERRAND, in *JA*, 1920, II, 254), in Siamese *bāt*, or *ngõn bāt* (lit. « silver *bāt* »). But there are special words for four « tical », *ngõn tām̄lũng nũng*, lit. « one silver *tām̄lũng* » (*nũng* = one), and for eighty « tical », *ngõn xǎng nũng*, lit. « one silver *xǎng* »; while to express « twenty tical » or « forty tical », one does not say « twenty *bāt* » or « forty *bāt* », but « five *tām̄lũng* » or « ten *tām̄lũng* ». Although there is a word for 0.01 of a « tical », *sātang* (*sātang nũng*), so that for 0.25 of a « tical » one can say « 25 *sātang* », the classical term for 1/4 of a tical is *ngõn sǎlũng*, « [one] silver *sǎlũng* »; 1/8 of a tical is *ngõn fũang*, « [one] silver *fũang* »; 1/64 of a tical is *õt nũng*, « one *õt* ». But *ngõn fũang nũng*, « one silver *fũang* », is also the designation of « 800 cowries », as it was according to the scale used from the sixteenth century to the first part of the nineteenth. A « string » of cowries is *tǎb nũng*, « one *tǎb* », and, although I find no precise statement to this effect, I suppose it to have originally been 80 cowries. So in Siam the whole silver monetary system of the present day is on a basis of  $4 \times 20(4 \times 5 \text{ or } 5 \times 4) \times 10$ , and of sub-multiples of 4 and 8 ( $4 \times 2$ ). It still reflects, to a great extent, the scale known for the cowries in Yün-nan under the Ming dynasty, with its « strings » of 80, made up of  $4 \times 4 \times 5$ .

Polo mentions also the use of a cowry currency in Bengal, the part of India where, in fact, this currency has been mostly employed in modern times. Information on the cowry currency in India is multifarious and, for the modern period, precise; I shall limit my remarks to the discussion of Chinese sources and the elucidation of some questions connected with the scale and the value of this currency.

The usual names of the cowry in Sanskrit are *kapardā* (> *kapardaka*, *kapardikā*) and *varāṭa* (> *varāṭaka*), once *śvetikā*, « the white one » (cf. STEIN, *Rājataranginī*, II, 324), and the first two are certainly old, but they have not been traced hitherto in early texts of a certain date. Bhāskara (not « Blankara » as in SCHILDER, 324) and Daṇḍin are approximately dated in the sixth century of our era. Fa-hsien, who travelled in India at the beginning of the fifth century, antedates them when he says of the people of India proper (Madhya-deśa) that in trade they use cowries (*pei-ch'ih*; cf. LEGGE, *A Record of the Buddhist Kingdoms*, 43; on *pei-ch'ih*, cf. *supra*, p. 542).

In his general description of India Hsüan-tsang, who travelled there in the second quarter of the seventh century, says (cf. Stan. JULIEN, *Mémoires*, I, 94) : « In trade they use (*huo yung*),

in exchanging what they have for what they do not have, gold coins, silver coins, cowries (貝珠 *pei-chu*; cf. *supra*, p. 543), and small pearls.» But, in the notices of the various kingdoms, it is only in the one devoted to Kōngoda, a small country between Orissa and Kalinga on the eastern coast of India, that Hsüan-tsang makes the following remark (*Mémoires*, II, 91) : « Its currency consists of shells (*lo-pei*) and various pearls (*chu-chi*). » Drawing perhaps, as often, from Hsüan-tsang, but with a different wording, the *Chiu T'ang shu* (198, 8 b) says of India that « it employs cowries (*ch'ih-pei*; it may be a corrupt reading instead of the more usual *pei-ch'ih*, but it must have already been given in Sung editions) as currency ». This passage of the *Chiu T'ang shu* has been copied by Chao Ju-kua (*HR*, 111). The *Hsin T'ang shu* too (221 A, 10 b) copies the *Chiu T'ang shu*, but gives the usual form *pei-ch'ih*.

Although Chao Ju-kua's « P'êng-ch'ieh-lo of India » (西天 Hsi-t'ien; the bare translation « the West » in *HR*, 93, 97, 98, is inadequate) has been believed not to be Bengal (see BANGALA), two products mentioned there are less unindicative than was thought by HIRTH and ROCKHILL. One is *tou-lo-mien*, « *tūla* floss »; in the Middle Ages this term was really applied to Bengal cotton velvets (see COTTON). The other is the shell currency. The text says : 以白呀螺殼磨治爲錢. In *HR*, 97, the translation runs : « They use (pieces of) white conch shells ground into shape as money. » This is almost unimpeachable, and yet I wonder whether it be the true rendering. In principle, the *ya-lo*, « polishing gastropod », ought to be the *tzū-pei* or « purple shell » (cf. *supra*, p. 542); but the text expressly says it was white, as Polo does for the cowries used in Yün-nan. In view of the very small value of the shell currency, I doubt whether the people in India would have taken the trouble to « grind them into shape ». We might perhaps think of grinding a hole to string the shells, a practice which did not obtain in ancient China alone, but which I have never found reported for India proper (cf. SCHILDER, 320). On the whole I am inclined to believe that Chao Ju-kua did not exactly understand what his informants told him at Ch'üan-chou, and that the sentence actually describes the usual cowry currency which was extensively employed in Bengal.

On the other hand it is certainly Bengal which Wang Ta-yüan, writing in 1349-1350, described in his *Tao-i chih-liao* under the name of P'êng-chia-la (cf. ROCKHILL, in *TP*, 1915, 435-436). ROCKHILL retained the often corrupt text of the current edition, and so did FERRAND (in *JA*, 1920, 83); but, as was already suspected by FUJITA, 98 a, the translation should read : « The government casts a silver coin called *t'ang-chia* (*ṭaṅka* > *ṭaṅga*, 'tanga'; on this word, which I shall not discuss here, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 896-898 [but suppressing « *tangah*, 'fer-blanc' », which in fact should be read *iänäkä*]; *Y*<sup>1</sup>, 54, 59, 63, 138; *JA*, 1920, II, 293; 1935, I, 239; MOOKERJI, *Harsha*, 68 [*ṭaṅkaka*]; *Bl*, II, 526 is absurd), every one of which weighs 2 mace (*ch'ien*) 8 candareens (*fên*); they are regularly used in trade. They are exchanged for 11, 520 odd cowries (*pa-tzū*), which are a substitute for small cash; this is convenient for the people and truly advantageous. » This text has passed into the *Ming i-t'ung chih*, 90, 9 b, and the *Shu-yü chow-tzū lu*, 11, 3 a, but curtailed and so corruptly divided that it is the « tanga » which is said to be used as « a substitute for small cash »; they still give to the *ṭaṅga*, however, the correct weight of « 2 mace 8 candareens », which has become simply « 8 candareens » in the current text of the *Tao-i chih-liao*.

In the first half of the fifteenth century, the *Ying-yai shêng-lan* says of Bengal (TP, 1915, 437; 1933, 423-424) : « In trade they use a silver coin called *tang-ch'ieh* (*taŋga*), weighing three mace, an inch and two tenths in diameter and with writing on either side (?) . . . For small dealings in the market they use cowries (*hai-pa*), the foreign name of which is *k'ao-li* (« cowry ») . . . » In 1564, the *Huang-Ming ssü-i k'ao* (2,73) merely says that the people of Bengal « in trade use silver coins and cowries (*hai pa*) », and likewise the *Huang-Ming hsiang-hsü lu*, 7, 31 b.

Such are the Chinese texts on cowry currency in Bengal; but the *Tao-i chih-liao* mentions its use also in Wu-tieh (Orissa), with additional detail (cf. TP, 1915, 445 [not always correct]; FUJITA, 114 a) : « Each of their silver coins weighs two mace eight candareens, and is equivalent to ten taels of *chung-t'ung* paper money (cf. *supra*, p. 553); it exchanges for 11, 520 odd cowries (*pa-tzū*), which are used instead of cash; 250 cowries buy one « pointed (?) basket » (尖羅 *chien-lo*) of cooked rice, which, in our official peck (*tou*), is of one peck six pints (*shêng*). Every coin can [thus] purchase 46 baskets (*lo*) of rice, which make a total of 73 pecks and two pints (read « six pints »), enough to feed two men for a year, with something to spare. » Orissa is one of the regions of India where it is possible to follow the exchange value of the cowry through a fairly long period. « In 1740, a rupee exchanged for 2,400 cowries; in 1756, for 2,560 cowries; in 1833, 6,400 cowries; and in 1845, 6,500 cowries » (JACKSON, 166-167). So the depreciation of cowries in Orissa was parallel and contemporary with that which we have noticed in Siam.

The main centre of production of the cowries was the Maldives. In its description of these islands, the *Ying-yai shêng-lan* says (cf. TP, 1915, 389; 1933, 417) : « As to the cowries (*hai-pa*), these people collect them and heap them in mounds; they let them rot, and then sell [the shells] to Siam and Bengal, where they are used as cash. » Then follows the description of the other standard product of the Maldives, the dried bonito or « cobily mash ». The two have sometimes been unduly confused. When Ibn-Baṭṭūṭah speaks of the « fish » which is found in the Maldives, FERRAND (*Fe*, 438), following DULAURIER, interprets this « fish » as *Cypræa moneta*; but it must be the bonito. More than two centuries after the *Ying-yai shêng-lan*, but almost in the same terms, RUMPHIUS states that the cowries were carried in full shiploads « from the Maldives to Bengal and Siam » (*D'Amboinsche Rariteit-kamer*, Amsterdam, [1705], folio, 1741 ed., 117).

Fra Mauro has a notice on the Maldives, which escaped both YULE (in *Hobson-Jobson*<sup>2</sup>, 546-548) and A. GRAY (in his survey of ancient notices of the Maldives at the end of his edition of *Pyrard of Laval*). It begins : « Divamoal (not « Diviamoal » as in ZURLA, 51), which is the head of 12,000 islands, as appears from the evidence of those who navigate that sea; in them cowries (*porcelele*) are produced in plenty, and they are used as currency . . . » Fra Mauro's « Divamoal » is the exact counterparts of « *Āibat-al-maḥal* » in Ibn-Baṭṭūṭah and « *Dīwah-maḥal* » in 'Abdu-'r-Razzāq. *Divā* goes back to Skr. *dvīpa*, Pali *dīpa*; the meaning of *maḥal* or *mal* is still a matter of controversy. I do not know the source of Fra Mauro's notice. Numbers approaching 12,000 occur in various mediaeval writers, but are referred to islands in the Indian Ocean in general, and not to the Maldives only : « 12,700 », inhabited and uninhabited, according to Polo (cf. Vol. I, 434; Y, II, 424, 425); « more than 12,000 » (Monte-Corvino; cf. *Wy*, I, 344); « more than

10,000 inhabited », or « more than 12,000 inhabited and more than 8,000 uninhabited » (Jourdain Cathala, in CORDIER, *Les Merveilles de l'Asie*, 74, 95; this would almost suggest that we ought to read « 2,000 inhabited » instead of « 12,000 », and « 2,000 » would agree with the figures given by Arabic sources). Our main authority for the conditions anciently prevailing in the Maldives, François PYRARD of Laval, who lived there from 1602 to 1607, states that the King of the Maldives called himself « *Sultan Ibrahim dolos assa ral tera atholon* » : that is to say, « Ibrahim Sultan, King of 13 provinces and 12,000 toles » (GRAY, *Pyrard of Laval*, II, 95). It must be remarked, however, that the literal translation of the title would seem to be, « [King] of 12,000 countries (*ral*) and 13 islands (*atholon*, our « atoll ») ».

Without attempting to give a general sketch of the use of the cowry currency in India, I may state that this currency, restricted in modern times to states bordering on the sea or lying in the southern half of the peninsula, has been shown by STEIN (*Rājatarangiṇī*, II, 323-324) to have been extensively used in Kashmir at an early date, and the passages of the *Mahāvīyutpatti* which will presently be adduced would almost suggest that it was familiar to the Tibetans in the ninth century. We must not forget, either, that in the eighth century *pei-ch'ien*, « shell money », is expressly entered in the *Fan-tsa-ming* of the Kuchean Li-yen, whose interests were mainly Central-Asian. Yet there is no trace of a shell currency in Chinese Turkestan. In that country I have myself picked up a few cowries, which the natives called *yīlan-bašī*, « snake-heads » (cf. SCHILDER, 325; RADLOV's Dictionary, IV, 126, Kirghiz *gīlanbas*), but there is nothing to suggest that they were ever used save as ornaments. I-ching had added to his translation of the *Suvarṇaprabhāsa*, ch. 6, a note on the value of the *kārṣāpaṇa* in *pei-ch'ih*, i. e. in cowries (黃, IX, 23 b), and part of this note has been included in the Uighur translation (edited by RADLOV and MALOV, in *Bibl. Buddhica*, XVII, 442<sup>13-14</sup>). But the Uighur translators have merely rendered *pei-ch'ih* as *yartmaq* (> now *yarmaq*), the usual Turkish name of the Chinese copper cash, clearly because the Uighurs had no notion of a cowry currency. Nor do I know of any Mongol or Manchu tradition referring to the cowry; its names in Mongolian, *ibau*, *iba'u*, *iba'un*, and in Manchu, *ubioo* (cf. *Ssū-t'i ho-pi wēn-chien*; KOWALEWSKI, 281; ZAKHAROV, 149), may be artificial creations due to Ch'ien-lung's scholars, which, however, I cannot explain; and so far no text has yielded the name by which the Mongols referred to the cowry currency of Yün-nan during the Mongol dynasty. Outside the range of Indian culture, the only part of the world where the cowry currency met with any real success was Africa, where its use has been carefully investigated in the general works mentioned at the beginning of the present note (to which add DELAFOSSE's remarks in *JA*, 1926, I, 177-184).

I have already alluded to the fact that the « string » of 80 cowries formerly used in Yün-nan betrayed a southern, probably Indian, origin. A glance at *Hobson-Jobson's* article on « cowry » is enough to show the importance in modern times of the *paṇ* (< Skr. *paṇa*) of 80 cowries, which could be divided into 20 *ganda* (< Skr. *gaṇḍaka*) of 4 cowries each. PRZYLUŚKI has devoted to this scale part of a very interesting paper on *La numération vigésimale dans l'Inde (Roczn. Oriental., IV [1926], 230-237)*, and has shown that it was closely related to the modes of reckoning of some pre-Aryan tribes of India, like the Santali. But, for my present purpose, it will be enough to draw attention to the Tibetan translations made from Sanskrit terms in the ninth

century, and preserved in the *Mahāvvyutpatti* (SAKAKI ed.). The Tibetan name of the cowry is *mgron-bu* or *'gron-bu*.

Here are the terms of interest for the present inquiry :

No. 9375 : «Skr. *kākaṇi*; Tib. *ka-ka-ni*; is worth 20 cowries (*mgron-bu*).» Skr. dictionaries give *kākiṇī*, also with the value of 20 cowries. A Japanese Buddhist work also speaks of the *kākiṇī* and of its value of 20 cowries (*Bongo jiten*, 454). Pali texts have *kākaṇa* (and *kākaṇikā*), but the commentaries do not seem to have retained any exact recollection of its value.

No. 9376 : «Skr. *māṣakaḥ*; Tib. *Ma-ṣa-ka*; is worth 80 cowries.» *Māṣaka* (Pali *māsaka*) is identical with *māṣa*, «bean», and became the designation of a small coin; in the present case it plays the same part as *paṇ* in the modern reckoning of cowries. Our «mace» comes from *māṣa*, through Javanese and Malay *mās* (the derivation from Mal. *mās* > *amās*, «gold», proffered in the second edition of *Hobson-Jobson* and accepted by FERRAND in *JA*, 1920, II, 296, is a failure). The *kārṣāpaṇa* (cf. *infra*) was divided either into 16 *māṣa* or into 20, and finally was identified with the ounce or tael of silver (cf. the important Chinese and Japanese texts quoted in *Bongo jiten*, 114-115 and 454-455). In the sixteenth century, the tael of Malacca was divided into 16 «mace», and it is generally believed that the reference of the word «mace» to the *ch'ien*, *i. e.* the tenth part of a Chinese tael, is to be ascribed solely to early European traders in China. But it seems just as possible that, in the *lingua franca* of Indonesian trade with China, this designation had already been adopted, and that the Europeans took it from the Malay traders (cf. *infra* for the analogous case of «cash»). This would not be without some consequence in the present case. If we suppose that the *Māṣa* > *mās* > «mace» may have been identified with the Chinese *ch'ien* at an early date, and since the *māṣaka* = *māṣa* was 80 cowries, Polo would in a way be justified in stating that 80 cowries have the value of one *saggio* of silver, *i. e.* one *ch'ien*; and on the other hand we must remember that *sa* = *so*, *i. e.* originally a «string» of 80 cowries, is given as the term for one *ch'ien* in a modern Lolo vocabulary. The *māṣa*, which was in principle a certain weight, could also be the designation of a small gold «ball» (*wan*) which, according to Hui-lin, ch. 60 (爲, IX, 152 a), was «about the size of a seed of *wu-t'ung* (*Sterculia platanifolia*)». Hui-lin valued it at about 80 Chinese copper cash (*t'ung-ch'ien*); this is surprisingly little.

No. 9377 : «Skr. *kārṣāpaṇaḥ*; Tib. *kar-sa-pa-na*; is worth 1600 cowries.» Skr. *kārṣāpaṇa* is well known, and is generally explained as being a coin (*paṇa*) of the weight of one *karṣa*, the *karṣa* itself being a weight of 16 *māṣa*; but Pali *kahāpaṇa*, may rather be a sanskritization of an old dialectical form (cf. PISCHEL, *Grammatik der Prākṛit-Sprachen*, 263; RHYS DAVIDS, *Pali-English Dictionary*, s. v. *kahāpaṇa*). In the scale used for cowries in India in the eighteenth and nineteenth centuries, four *paṇ* (of 80 cowries each) made one *āna*, and four *āna* one *kāhan*; the *kāhan* was thus worth 1280 cowries, and the word is merely the modern representative of the earlier *kārṣāpaṇa* (*kāhāvaṇa*) of 1600 cowries.

The *kārṣāpaṇa* repeatedly occurs in Chinese Buddhist texts, and there are various glosses of T'ang times about it. According to SOOTHILL and HODOUS, *A Dictionary of Chinese Buddhism*, 315, it was worth «400 candareens», which, in modern reckoning, would mean four Chinese silver taels; but «candareens» must be a mistranslation. In Hui-lin, ch. 13 (爲, VIII, 97 a), we are told that the *kārṣāpaṇa* was a piece of gold of the value of 400 *ch'ien*; it was round

in shape and of the size of a *chiang* bean (*dolichos sinensis*). *Ch'ien* means «cash», «copper coin», and is used by both I-ching and Li-yen to render Skr. *paṇa* (BAGCHI, *Deux lexiques sanskrit-chinois*, 50, 229), but the *ch'ien* here intended must have been much below the *paṇa* in value. This is proved by another gloss of Hui-lin, ch. 26 (爲, x, 2b) : «*Paṇa* means a copper coin (*t'ung-ch'ien*); 16 *paṇa* make one *kārṣāpaṇa*.» So the modern scale of 16 *paṇ* (<*paṇa*) to one *kāhan* (<*kārṣāpaṇa*) goes back at least to the beginning of the ninth century. On the other hand the traditional equivalence of the *kārṣāpaṇa* to one *karṣa* of 16 *māṣa* is not in agreement with the scale adopted in the *Mahāvvyūtpatti* where the *kārṣāpaṇa* is said to be worth 1600 cowries, and consequently to represent not 16 but 20 *māṣaka* (= *māṣa*) of 80 cowries each. This value of 1600 cowries to the *kārṣāpaṇa* is expressly stated by I-ching, in a note added to ch. 6 of his translation of the *Suvarṇaprabhāsa*, to have been the one universally used in Magadha in his time, *i. e.* in the second half of the seventh century (cf. 黃, IX, 23b). The two values of 1280 and 1600 cowries for the *kārṣāpaṇa* have been known to Chinese commentators, and they are also given by Tibetan sources (cf. Sarat CHANDRA DAS, *Tibetan-English Dict.*, 300). Such variations in value may be partly due to fluctuations in the modes of reckoning with 4 or with 5 or, still more, to attempts to pass from the vigesimal reckoning expressed by the unit of 80 cowries to a purely decimal system based on a unit of 100. Other translators, probably misinformed, and because of the ambiguity of *ch'ien* (which is either 0.1 of an ounce, or a coin in general, or a Chinese copper cash) have understood the *paṇa* as being one copper cash (*ch'ien*) and the *kārṣāpaṇa* as being 16 *ch'ien*, or have even merely rendered *kārṣāpaṇa* as *ch'ien*. It must be owing to some misunderstanding of this kind that *grar-ṣa-pa-ni*, *i. e.* *kārṣāpaṇa*, is given as a synonym of *iba'u*, «cowry», in KOWALENSKI, 281. I-ching further remarks that, in the text which he translates and which speaks of «100 *kārṣāpaṇa*», one ms. gives «100 陳那羅 *ch'ên-na-lo*», *i. e.* «100 *dīnāra*» or «gold coins»; a substitution rendered possible by the fact the *kārṣāpaṇa*, being in principle a certain weight, could, theoretically at least, be used of gold as well as of silver. More details are available on the *kārṣāpaṇa*; but I cannot discuss them here, and must refer the reader to I-ching (*loc. cit.*), Hsüan-ying, ch. 22 (爲, VII, 86a), Hui-lin, ch. 13 (*ib.*, VIII, 97a), ch. 29 (*ib.*, VIII, 180a), ch. 71 (*ib.*, x, 2b), K'o-hung, ch. 2 (*ib.*, I, 21b), *Bongo jiten*, 114-115 and 454-455, and ODA Tokuno, 210.

The scale of the cowries in Yün-nan was 4, 16 (4 × 4), and 80 (16 × 5), but in modern India it was 4 and 80 (4 × 20). The «four» basis of the reckoning also occurs in other ways in the values attributed to the *kārṣāpaṇa* : for since it was worth 400 *ch'ien* or 1600 cowries, the «*ch'ien*» of the first text of Hui-lin must have been worth four cowries, and must render not of course *paṇa* as usual, but *ganḍaka* > *ganda*, *i. e.* four cowries. At the same time, the number «400» itself, *i. e.* 20 × 20, supports PRZYLUCKI's view of a vigesimal reckoning, which also appears in the higher values : 400 *karṣa* make one *tulā* (> Hind. *tolā*; cf. *Hobson-Jobson*<sup>2</sup>, 928; FERRAND, in *JA*, 1920, II, 293 [where I doubt that we should start from Pers. *tōlah*], 294). I do not know the authority for GILES's statement (*Glossary of Reference*, 63) that 200 cowries make «one *āna* or about three halfpence»; the *āna* was usually worth 4 *paṇ*, *i. e.* 320 cowries.

Both *paṇa* and *karṣa* have become usual terms in foreign trade.

*Paṇa* is the *fa-nan* (= *fanam*) and *pa-nan* (= *panam*) of Chinese texts of the early fifteenth century (cf. *TP*, 1933, 409), the *pun* (of 80 cowries) and the *fanam* of Anglo-Indian, the *fanon* of the French settlements in India (cf. *Hobson-Jobson*<sup>2</sup>, 348, 737). As to the *karṣa*, it was originally a weight, which was mainly used in ancient times in connection with gold and silver, but became the designation also of a copper coin; and it is in this capacity that it has survived. Sanskrit *karṣa* > Tamil *kāsu*, Singh. *kāsi*, is the etymology of the Portuguese *caixa*, our «cash». But it is a mistake to state (*Hobson-Jobson*<sup>2</sup>, 167) that the Portuguese are responsible for the use of this term as a designation of the small money of Indonesia and of the Chinese copper coins. Long before the arrival of Europeans, *chia-shih*, \**kaš*, is given by a Chinese traveller of the first quarter of the fifteenth century as the native designation of the tin coins of Pasè in Sumatra (cf. *TP*, 1936, 220), and *kas* is the name of the Chinese copper «cash» in Cambodian. This would tend to support the view expressed above (p. 561) that the use of «mace» (< Skr. *māṣa*), as a designation of the Chinese *ch'ien* (in the sense of 0.1 of an ounce), may not have originated with European traders as is generally believed, but have begun much earlier among the natives of Indonesia.

The main purpose of these notes on Indian weights has been to show that the whole scale of value of the cowries in Yün-nan during the Middle Ages, based on 4, 16 ( $4 \times 4$ ), and 80 ( $16 \times 5$ ), connected this cowry currency with the Indian Ocean, more precisely with India, and not with the decimal scale of the usual Chinese practice. To this Chinese decimal practice there is now only one exception. The Chinese «pound» or *chin* (lit. «axe», a significant indication that bronze axes once served as a unit of weight) is divided into sixteen «ounces» or *liang*. But it would take us too far from our subject if I should attempt to trace the history of this apparent exception. It has to be studied in connection with the ancient measures *hsün* of 8 feet and *ch'ang* of 16 feet, with the 筥 *sou* of 16 pecks (*tou*), with the *shu*, which was 1/24 of a *liang*, and also with the *shih*, «stone», of 120 pounds (gradually replaced by the *tan*, «picul», of 100 pounds) and its fourth part, the 鈞 *chün* of 30 pounds, as well as its multiples, the 乘 *ping* of two *shih* (or of 16 斛 *hu*; cf. *K'ang-hsi tzü-tien*, and *Ch'ien-Han shu*, 89 B, 13 b), etc. All of these were out of use or were becoming obsolete when a cowry currency made its appearance in Yün-nan, and moreover they would not account for the details of a system which, like the product to which it was applied and the tribes who adopted it, is decidedly non-Chinese.

Although I shall not discuss the cowry trade in Africa, going back as it does to the Middle Ages, I cannot refrain from quoting a passage from Aluise da Ca' da Mosto, the Venetian who travelled along the coasts of western Africa from 1455 to 1463 and discovered the Cape Verde in 1460. Speaking of tribes of the interior of the continent (somewhat north of Senegal), he says (RAMUSIO, I, 100 EF; cf. SCHNEIDER, 119-120) : «In that land of the Berrettini no coins are made... I have heard, however, that inland these Azanaghi (= Berrettini) and also the Arabs are accustomed, in some places, to use white cowries (*porcellette blanche*), of those small ones which are brought to Venice from the Levant; and they give a certain amount of them according to the nature of the goods which they have to buy.» I shall make use of this text in my note on «Porcelain».



## 185. CREMOSI

*carmosi* L  
*charmexini* V  
*chremexi, cremexi* VA  
*cremesi* VL

*cremesina* R  
*cremosi* F, L  
*ceriosi* P

*crimisi* Z  
*quermesis* FA, FB  
*vermaulz* FB

Polo uses this word only once, and as the name of a costly textile woven in Bagdad. It is of course the same word as the French *cramoisi* (> Engl. *cramoisy*) and the Old French *cramosin* (> Engl. *crimosin* > *crimson*), which, like the French *carmin* (> Engl. *carmine*), go back to an Arabic قَرْمِزِي *qirmizī* (< *qirmiziyun*), an adjectival form of قَرْمِز *qirmiz*, «kermes». The *-in* of *carmin* is not easy to explain, but I do not believe that it is due to a contamination of *minium*, as is still stated by LOKOTSCH, No. 1219.

Although it has become customary in French to speak of the «kermes» as «cochenille», cochineal, this is an American insect, *Coccus cacti*, while the kermes is a kindred species, *Coccus ilicis*, living on an oak, *Quercus coccifera*, and during the Middle Ages was found in many parts of the Mediterranean area. Another insect, the *Porphyrophora Hamelii*, was abundant in Armenia and went also by the name of *qirmiz*. Both were used for dyeing purposes, and the Armenian one was exported even to India (cf. HEYD, II, 607-609; also LECLERC, *Traité des simples*, in *Not. et Extr.*, XXVI, I, 74-75).

The history of the word *qirmiz* has been attempted by various scholars, particularly by KARABAČEK (in *Mitteil. d. K. K. Oesterr. Mus. f. Kunst u. Industrie*, 1880-1881, VIII, 98-103), BERNEKER (*Slav. Etym. Wörterbuch*, 490-491), LOKOTSCH (No. 1219) and MARKWART (in *Ungar. Jahrbücher*, IX [1929], 92-94). Yet it still offers many obscure points for elucidation.

All agree that *qirmiz* is not a true Semitic word, and that it must be traced ultimately to the Indo-European word for «worm», Skr. *kṛmi-*, Pers. *kirm*, old Slav *č'rm'*; most authors derive *qirmiz* more precisely, either from Skr. *kṛmija*, lit. «born of a worm», or from its feminine form *kṛmijā*, taken as a noun. But *kṛmija* is an adjectival formation, and *kṛmijā*, as the name of the kermes (? or of the lac insect, *Coccus lacca*), is an unattested form known only from lexicographers. Moreover, one would expect the Indian term to have reached the Arabs through the Persians, and there is no reason why a Skr. *k-* should have given *q-* in Middle Persian. Finally, there is an Iranian form connected with, but not dependent on, *qirmiz*, and its final is quite different: Middle Persian *kalmīr*, (< \**karmīr*), > Hebr. *karmīl*, Arm. *karmir*; Sogd. *karm'ūr*; all meaning «red». This form does not seem to be an Iranian development, as was believed by MARKWART, but goes back rather to the Skr. *kṛmila*, as suggested by KARABAČEK. The *Mahāvīyutpatti* (No. 9173) registers *kṛmilikā* as the name of a red textile. KARABAČEK thought that the French *carmin* was derived from *karmīr*, which for reasons of geography and chronology is difficult to believe. It was also KARABAČEK's idea that the Sanskrit term must have referred originally not to kermes, *Coccus ilicis*, but to the lac insect, *Coccus lacca*; and this is quite plausible.

POPPE's attempt (*Ungar. Jahrbücher*, VI [1926], 115) to connect *qirmiz* > Turk. *qirmiz* with Turk. *qizil*, «red», can hardly be retained; it would require a reversal of the problem, and would pre-suppose that *qirmiz* was originally Turkish. Such a solution would dispose of the difficulty of the -z of the Arabic *qirmiz*, but at the same time we should have to abandon all connection with *kirmi-* and *kirm*, and to suppose that the word passed from the Turks to the Arabs at too early a date to be acceptable.

Through unknown channels, the word has reached Mongolian, where *harminjin* is the designation of a red cloth; it may also represent the Russian *karmazin*, of identical meaning (cf. VLADIMIROV, in *Zap. Koll. Vostok*. v, 81).

BLOCH (*Dict. étymol.*, I, 187) dates in 1315 the first occurrence of *cramoisi* in French; once more, French lexicographers have left Polo out of account.

Like «purple» (*purpura*), «cremosi» was first a colour and later became the designation of a textile, and one might suppose that, as in the case of «purple», it was later used irrespective of the colour (the case of «scarlet», *écarlate*, is the reverse; it was first a textile, and afterwards became a colour). However, in the early mentions I find of the textile, no colour is indicated (cf. for instance FRANCISQUE MICHEL, *loc. cit.* II, 261, 474); Pegolotti (EVANS ed., p. 430 : *carmusi*, *chermisi*, *chermusi*) and Uzzano (cf. Y, I, 65) give no indication of its nature. From Polo's text we can deduce that the «cremosi» manufactured at Bagdad was a costly fabric; perhaps it was a sort of red velvet, and was dyed with the Armenian *qirmiz*, *Porphyrophora Hamelii*, as surmised by HEYD (II, 608). That it was most probably red is confirmed by the general value of the term in the East in Polo's day. In the *Codex Cumanicus* (KUUN ed., 108), «cremixi» ( $x = z$ ) is transcribed in the same manner for Latin, Persian, and Turkish, as the name of a colour which is listed between *virnilium*, «red», and «bachami» (identical in the three languages), *i. e.* *baqqami*, lit. «dyed with brazil-wood», «reddish» (see «Brazil»). For later mentions of «cramoisi», cf. GAY, *Glossaire archéologique*, I, 487-488.

## 186. CUBLAI

<i>acublay</i> , <i>cubtai</i> L	<i>chelabas</i> , <i>cholai</i> , <i>choli</i> , <i>tubeli</i> V	<i>cublai</i> F, FA, L, VA,
<i>cablau</i> , <i>chablay</i> , <i>choblay</i> ,	<i>chublai</i> VA	VL, Z; R
<i>chobray</i> TA <sup>1</sup>	<i>chublay</i> VL	<i>cublau</i> FA <sup>t</sup>
<i>cabli</i> (?) VL	<i>clobai</i> , <i>clobi</i> , <i>poblai</i> VB	<i>cublay</i> FA, FB, L, LT,
<i>chabbav</i> , <i>chabbay</i> , <i>chabblau</i> ,	<i>coblai</i> VB, Z	O, P, P <sup>5</sup> , VL, Z; G
<i>chobblai</i> , <i>choblai</i> , <i>chonblai</i>	<i>comblay</i> , <i>cublim</i> LT	<i>cumblay</i> O
TA <sup>3</sup>	<i>croblai</i> F	<i>lubai</i> V, VB

The true Mongolian form of the name of this famous Emperor is «Qubilai», clearly a derivative form of *qubi*, «lot», «portion», but the mode of its derivation is obscure. VLADI-

MIRCOV (*Doklady Ak. Nauk*, 1924, 55-56; 1929, 170) saw in *qubilai* an ancient form of the present participle of a denominative verb *qubila-*, so that *qubilai* would mean «one who apportions»; he may be right, but I have never come across the word except as a proper name. It occurs first, before the time of Chinghiz-khan, in the *Chin shih* (cf. WANG Kuo-wei, *Kuan-t'ang chi-lin*, 14, 25 a), which speaks of 胡必刺 Hu-pi-la, \*Qubila, of the Salji'ut. Another Qubilai is often mentioned in the *Secret History*; for various homonyms, cf. also *Ber*, I, 213; WANG Hui-tsu<sup>2</sup>, 38, 1 a.

In Chinese, the name of the Emperor Qubilai is written 忽必烈 Hu-pi-lieh. In his edition of Rašidu-'d-Dīn, BLOCHET has adopted قوبلاي Qūbilāi, but the usual reading of the mss. is قوبلاي Qūblāi (or Qūbilāi; cf. *Bl*, II, 7), and the latter form is supported by قوبلا Qūblā (or Qūbilā) in Juwainī (I, 255) and in Waśśāf (*Ha*<sup>2</sup>, 32). The Syriac form is Qūblāi (cf. BUDGE, *The monks of Kūblāi Khān*, 138, 159, 160). In all these transcriptions, as in Polo's «Cublai», the *i* of the second syllable is slurred, because this syllable was not accentuated. But an Armenian text gives «Qubila» (*Rec. Hist. Crois.*, Arm., I, 606), and Hethum the historian writes «Cobila» (*ibid.*, II, 160, 186, 294). As usual, the Georgian spelling is uncertain and abnormal; it hesitates between «Qubul» and «Qubli» (cf. BROSSET, *Hist. de la Géorgie*, I, I, 538). In a letter sent in 1278 by Nicholas III to Abaya, mention is made of «Quobley» (cf. GOLUBOVICH, *Bibl. bio-bibl.* II, 427). We find «Cobla» in the Latin translation of Aryun's letter of 1285 to Honorius IV (CHABOT, *Histoire de Mar Jabalaha III*, 190). A letter was addressed by Nicholas IV in 1289 to «Cobla Chan» (*ibid.*, 216). The name was altered to «Holubeim» in the Catalan Map of 1375 (cf. Y<sup>1</sup>, I, 300-301; CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 15).

Qubilai, fourth son of Tolui, was born on September 23, 1215, ascended the throne on May 5, 1260, and died on February 18, 1294.

According to Polo, Qubilai was born «on the 28 day of the moon of the month of September» (cf. Vol. I, 220). MOULE is the first, I think, to have shown, in his foot-note to that passage, that Polo was absolutely correct. Polo, using a rough calculation, makes the Chinese year begin in February (cf. Vol. I, 222), which he counts as the first month; as a consequence, the 28th day of the moon of September is the 28th of the Chinese eighth moon, the very day of Qubilai's birth.

Polo's calculations of the other dates concerning Qubilai are based on the assumption that he had begun to reign in 1256, so that he had been 42 years on the throne in 1298, the year in which Polo dictated his text (cf. Vol. I, 192-193); Polo, on the other hand, was not very far off the mark in saying that, in 1298, Qubilai was aged some «fourscore and five years»; as a matter of fact, had he been still alive, he would have been 84 years old in the Chinese way of reckoning, 83 years old for us. The date «1288» in some mss. is clearly corrupt. I hold the same opinion in regard to «MCCC et VIII» in FD, which, I think, gives no clue whatever to the year in which the FG version was made.

Polo's error in making Qubilai's reign begin in 1256, while the Emperor ascended the throne only in 1260, may be accounted for to a certain extent by the fact that, on his return from Yün-nan, Qubilai settled on the borders of North China outside of the Great Wall, and founded there in 1256, as his summer residence, the city of K'ai-p'ing which, a few years later, became

Shang-tu (see «Chemeinfu» and «Ciandu»); from that moment, he acted almost as if he was independent of Qara-qorum.

Qubilai having died on February 18, 1294, the news of his death must have been known to the Mongols in Persia before the end of that year. At that time, the three Polos had probably left Persia, but the report may have reached them on their way home, at or near Trebizond or Constantinople. It seems hard to believe that the news of the death of the Great Khan, mentioned in RAMUSIO's text, was an arbitrary addition which corresponded to the truth by a mere coincidence (cf. Vol. I, 92). Of course, this is difficult to reconcile with the passages in which, in 1298, Polo speaks of Qubilai as if he were still alive. It may be that, after having heard the news, Polo disbelieved it for lack of more precise confirmation.

Polo says that Qubilai had twenty-two sons from his four wives, and twenty-five from his concubines (cf. Vol. I, 206-207; Y, I, 356-360). Like all the early Mongol Emperors, Qubilai had four *ordo*, or, in the plural, *ordos*, a term which Chinese texts either transcribe phonetically, or render 行宮 *hsing-kung*, lit. «moveable palace» (see «Cinghis», p. 309); so PALLADIUS's statement, reproduced in Y, I, 358, that Chinese authors translate *ordo* by «harem» is not quite accurate. The four *ordos* of Qubilai's principal wives were those of (cf. YS, 106, 2-3; also 95, 7-8. BLOCHET's translation [Bl, II, 353] is useless, because he has mixed up the order of the *ordos*, and mistaken 右 *yu*, here meaning «what has been mentioned above», for a designation of *ordos* «of the right» which never existed) :

(1) 帖古倫 T'ieh-ku-lun, *i. e.* Tägülün (not «Toughloulk» as in Bl, II, 353), a feminine name in -lün derived from the same *tägü-* as in Tägüdär (see «Caraunas»). From YS, 106, 2 *b*, we know that Tägülün, head of the «Great *Ordo*», had received the title, certainly posthumous, of «Great Empress» (*Ta Huang-hou*); nothing more is said of her in the general table of empresses and concubines, nor is she even so much as named in the biographical notices of Qubilai's empresses (YS, 114, 1 *b*). Rašidu-'d-Din ignores her. In YS, 118, 2 *b*, we are told that Tägülün was the daughter of To'oril, himself a grandson of the Qonyrat Alčün-noyan (Alčün-noyan was the elder brother of Börtä, Chinghiz's principal wife; on the name, cf. my paper *Sur un passage du 'Cheng-wou ts'in-tcheng lou'* [disfigured by misprints], in *The Ts'ai Yüan P'ei Anniversary Volume*, Peiping, 1934, I, 907-938). The *pên-chi* of Qubilai (YS, 4, 5*b*) give a list of members of the Imperial family who, according to an edict of January 14, 1261, were henceforth to receive an annual grant in silver and silk; amongst the beneficiaries was «the former Empress (先朝皇后 *hsien-ch'ao huang-hou*) Tägülün». The wording is strange, and *hsien-ch'ao* would suggest that we have here to deal with a consort of a former emperor; I agree, however, with T'u Chi (19, 7 *b*) that the Tägülün of the table of empresses is certainly meant. If she really was Qubilai's consort, she must have died before Qubilai's accession to the throne, or at any rate before January 14, 1261, and *hsien-ch'ao* would be a clumsy rendering from the Mongolian. The grant was made to her *ordo*, which must have continued its separate existence after her death, but the texts give no indication as to who was in charge of it after her. But this solution is open to doubt, and there may be an error in the YS as to Qubilai's first *ordo* (cf. YANAI, 692-694).

(2) 察必 Ch'a-pi, چابوی Čabui in Rašidu-'d-Din, altered to Čambui in «Sanang Setsen».

«Čabui» remains unexplained; contrary to BLOCHET (*Bl*, II, 352), it can have nothing to do with Skr. *jambu*. Čabui was a Qonyrat, and both the *YS* (106, 2 *b*; 118, 2 *b*) and Rašidu-'d-Din agree in making her a daughter of Alč'in-noyan; this is hard to believe, as it would make her three generations older than Tägülün (cf. also T'u Chi, 19, 7 *b*). She was already one of Qubilai's wives before his accession to the throne, was promoted «Empress» (*Huang-hou*) in 1260, and died in 1281 (cf. *YS*, 11, 5 *a*; 106, 2 *b*; 115, 3 *a*; T'u Chi, 19, 9 *a*; the date «1277» in *YS*, 114, 2 *a*, is an error, and so is «1283/1284» in Rašid, *Bl*, II, 354). Rašid insists on her rare beauty (*Ber*, I, 152; *Bl*, II, 353). He also mentions her Chinese title of قونقو *qonqu* (*Ber*, I, 152), which, despite CORDIER's note in *Y*, I, 75, regularly renders *huang-hou*. According to the Chinese, she was the mother of Činkim (or Ĵingim?; see «Cinchim»), Mangala (see «Mangalai»), and Nomoyan (see «Nomogan»). Rašid says (*Ber*, I, 152) that she bore Qubilai four sons and five daughters. The four sons must be Dorĵi, Činkim, Mangala, and Nomoyan; in the case of Činkim, however, Rašid (*Bl*, II, 354-355) speaks of his mother as if she had been some one other than Čabui, and gives her the «name» of تايهو *taihu*, i. e. 太后 *t'ai-hou*, «Empress Dowager», a title which Chinese texts never mention in the case of Čabui and which would seem to fit only Tägülün (cf. *YANAI*, 694); the *Mo'azz* is in agreement with Rašid. After Čabui's death, she was succeeded in her *ordo* by another Qonyrat, 喃必 *Nan-bi* (\**Nam-bi*), نمبوی *Nambui* in Rašid (*Ber*, I, 152, where the name is misread «Täjiui»; *Bl*, II, 372), نانبوی *Nanbui* in the *Mo'azz*. The name «Nambui» (perhaps *Nombui*) is not explained. According to Rašid, *Nambui* was the daughter of Nač'in *kürügän* (i. e. Imperial son-in-law), but the Chinese authorities must be more accurate when they state that she was the daughter of Nač'in's grandson Hsien-t'ung (*YS*, 114, 2 *a*). She was proclaimed Empress (*Huang-hou*) in 1283. Chinese sources say that, as Qubilai was already well advanced in age, the ministers took to the habit of making their reports to her (*YS*, 106, 2 *b*). In spite of the Emperor's age, *Nambui* bore him a son, whose name is not given in the genealogical tables of *YS*, 107, 8-9, is mistaken for that of Oyruč'i by Rašid in his notices of the tribes (*Ber*, I, 152, where «Čarquič'i» is a wrong reading), and is omitted by him in Qubilai's history (*Bl*, II, 372), but has been preserved in *YS*, 114, 2 *b*, where it is given as 鐵蔑赤 *T'ieh-mieh-ch'ih*, *Tämäči*.

(3) The Empress (*Huang-hou*) *Taraqai* (lit. «Bald»). Nothing is known of her, nor of the concubine (*fei-tzü*) \**Nuqan*, who belonged to the same *ordo*.

(4) The Empress *Baya'uč'in* and the Empress *Kökölün*. Chinese sources merely give their names, but from Rašid (*Ber*, I, 178; *Bl*, II, 369-370) we know that *Baya'uč'in* was a *Baya'ut*, the daughter of *Buraqč'in* (?) and that she was the mother of *Toyön*.

These are the traditional four *ordos*, and Polo must have had them in mind when he spoke of Qubilai's four wives. We do not know to which *ordos* was attached *Hušij'in*, of the *Hušin* tribe, the mother of *Ayač'i* (*Ber*, I, 168; *Bl*, II, 367), or *Dörbäjin*, of the *Dörbän* tribe, the mother of *Hügäč'i* (see «*Cogacin*») or *Oyruč'i* (cf. *Ber*, I, 195 [where the names are misread]; *Bl*, II, 364-366).

*YANAI* has supposed that Qubilai's first *ordo* (the «great *Ordo*») was at *Ta-tu* (= Peking; see «*Taidu*»), the second at *Shang-tu* (see «*Ciandu*»), the third at *Čayän-nör* (see «*Ciagannor*») and the fourth at *Liu-lin* (see «*Cacciar-modun*»), but this is quite hypothetical.

Polo says that Qubilai had twenty-two sons from his four principal wives, and twenty-five from his concubines. These figures do not tally either with Persian or with Chinese sources. The Chinese genealogical tables list only ten sons (YS, 107, 8 b; cf. 95, 5-6); but Tāmāčīn must be added, and Rašīd's list of twelve sons seems to be accurate. So I agree with YULE's hypothesis that «twenty-two» («XXII») in Polo is probably a mistake for «twelve» («XII»). In YULE's list (Y, I, 361), some names are corrupt; moreover, the twelfth name is due to HAMMER's corrupt duplication of that of Nomoyan. «Gantanpouhoa» is An-t'an-pu-hua, Altan-buqa, who was not a son, but a grandson of Qubilai. Except that the Chinese list omits Quridai, and reverses the order of Qutluq-tāmūr and Toyōn, it agrees with Rašīd's. The list of twelve names is to be established as follows: 1, Dorji; 2, Činkim; 3, Mangala; 4, Nomoyan; 5, Kuridai; 6, Hügäči; 7, Oyrucçi; 8, Ayači; 9, Kōkōči; 10, Qutluq-tāmūr; 11, Toyōn; 12, Tāmāči.

Various opinions have been expressed in China on the personality and character of Qubilai; I cannot discuss them here. One point, however, may be noticed, and that is Qubilai's tendency to resort to the services of men who were not Mongols. He was more than once reproached by people of his own kin for showing favour to and placing confidence in Chinese or Mussulmans, but replied that they were more efficient. Although Polo did not hold at his Court the high position which the text, as written down by Rustichello, would make us believe, there can be no doubt that Qubilai treated him well. It seems as if the rough Mongol, once established in China, had come to feel the value of her ancient culture, without being hampered by the traditional prejudices of the *literati*. In that respect, and of course in rather different conditions, his long reign may be compared with that of the later Manchu Emperor K'ang-hsi.

### 187. CUGIU (< \*SINGIU, cc. 154, 155)

*chagu* TA<sup>1</sup>  
*chaguj* TA<sup>3</sup>  
*čuču, cuču* Z  
*cingui* LT  
*cinguy, siguy* FA

*cogio* G  
*cugiu* F, L  
*cuguj, zinguj, zuguj* VB  
*cuinguy* P  
*cuyguy* FB

*engui* VL  
*gieza* R  
*guguin* V  
*sigui* S

CHARIGNON was the first to propose what I believe to be the true solution (*Ch*, III, 101): «Singiu» is the same as the modern Kuang-hsin in north-eastern Chiang-hsi. Polo's form represents 信州 Hsin-chou, a name which goes back to 758, and which was replaced by Kuang-hsin only in the Ming dynasty. In the Yüan dynasty, Hsin-chou belonged not to Chiang-hsi, but to the province the seat of which was at Hang-chou (YS, 62, 7 a); Polo is correct on that point also. South of Hsin-chou, the road crossed the Wu-i Mountains to reach Ch'ung-an, Chien-

yang and Chien-ning (see «Quenlinfu»). BENEDETTO contradicts himself by accepting CHARIGNON's identifications as far as Ch'ang-shan (south-west of Ch'ü-chou), and then rejecting Hsin-chou in favour of Ch'u-chou, which is very far to the south-east (*B*<sup>1</sup>, 441, 442). It is on account of the identification with Hsin-chou = Kuang-hsin that I have restored an original form «Singiu», although it is not the one which has the best support in the mss.

Fra Mauro writes «Chuçu», one more striking parallel with Z, and with Z only (the «Chuzu», of *Zu*, 38, and HALLBERG, 146, is not the form which actually appears on the map).

## 188. CUI

*caitus* P<sup>5</sup>  
*calcur* P  
*chim* TA<sup>3</sup>

*chui* VA, VB; G  
*cin* TA<sup>1</sup>  
*cui* F, L, LT, VB

*uy* FA, FB  
*yn* R

This is the Great Khan Güyük or Kүйük; for the conditions in which the final *-k* is dropped, see «Ulau». Plan Carpine gives the proper form, which he writes «Cuyuc», «Cuyuccan», «Cuyuckan» (cf. *Wy*, 64, 85, 93, 94, etc.), and Salimbene owes to him his correct «Cuiuch-Chan» (cf. *Pel*, 12). But Rubrouck has «Keuchan», and Hethum «Guio Can» (see «Ulau»). Georgian texts have «Kuk» (BROSSET, *Hist. de la Géorgie*, I, 521); «Ghoik», given as an Armenian form by BROSSET, *Hist. de la Géorgie*, I, Add., 326, and «Gayug» adopted by PATKANOV, *Ist. Mongolov*, I, 38, must both be wrong readings. The *YS*, 2, 4 *a*, transcribes 貴由 *Kui-yu* (= Güyük); Juwainī writes كيوك *Güyük* (or *Kүйük*), and so does Rašidu-'d-Dīn (*Bl*, II, 4), although BLOCHET, after p. 227 of his edition, adopts consistently the exceptional spelling كيوك *Güyük* (or *Kүйük*); cf. *Bl*, II, 227 (the would-be homonymous كيوك of p. 570 is probably a misreading for كيوك *Köbäk*). The name is perhaps derived from *güyü-*, «to run»; *güyü-* has a subsidiary form *güi-*; a secondary form \*Güik would account for «Cui» < \*Güik, but is in itself unlikely, and the Chinese and Persian transcriptions favour a regular Güyük. Nevertheless, the etymology remains doubtful, and the name may even be of Turkish origin.

Güyük was Ögödäi's eldest son, and was born in 1206; Plan Carpine was present when he ascended the throne near Qara-qorum on August 24, 1246. Güyük died in March-April 1248 at Qum-sängir, seven days' journey from Beš-ballıq; cf. *Pel*, 196-197 (I have noticed since that the name of Qum-sängir, which was only a reconstruction on my part, really appeared, under the slightly different form Qum-singgir, in the *Secret History*, § 158, as a region through which the Urungu flowed; it was already known through the abridged Chinese version translated by PALLADIUS; cf. *Ber*, II, 280, where the name is incorrectly given as Qumšingir).

## 189. CUIGIU (c. 130)

## City

*cintigui* R  
*ffuniglu* P  
*fungul* FA  
*funyglu* G  
*ougni* V

*singul* VB  
*sinuigil* TA<sup>3</sup>  
*siuuglil* TA<sup>1</sup>  
*sugçu* Z

*suiugul* L  
*sunilgul* LT  
*suruglu* VA  
*suuigul* (? *siungul*, *suingul*) F

## Province

*caguy*, *suingul* FB  
*caigui* Fr  
*caingiu* Ft  
*chogui*, *chugi* TA<sup>3</sup>  
*chugiu* TA<sup>1</sup>, TA<sup>3r</sup>  
*cingiu* VL (?)  
*cinguy* P

*cugiu* TA<sup>1r</sup>  
*cuguy* FA<sup>t</sup>, FB  
*cuigiu*, *cugui* F  
*cuiugiu* L  
*cyngui* LT; G  
*cyugui* LT

*dingim*, *dingin* VA  
*enguy* FA  
*ginguy* Pr  
*ougni* V  
*sugçu* Z  
*vintiuui* VB

F's «Cuigiu» led PAUTHIER and YULE astray when they thought here of Kuei-chou; the place can only be 叙州 Hsü-chou, Hsü-chou-fu or «Sui-fu» of our maps, on the Yang-tzü, and the various readings of other mss. might have been called upon to justify a correction to «\*Ciugiu», if *-iu-* and *-ui-* did not constantly alternate throughout the whole book (cf. «Ciugiu», for which F has both «Ciugiu» and «Cuigiu»). In «Ciugiu», we would seem to have *hsü* (*siü*) noted by *ciu*, although Polo's *ci-* is generally *ç-* or *çi-*; either *c-* has been taken here before *i* with its French value, or *ci-* has replaced very early an original *si-*; I am in favour of the second solution, with some hesitation. In the present case, I dare not attach much value to Z's reading «Sugçu», as the *-g-* is wrong, and suggests a contamination with the name of Su-chou (see «Succiu»).

Although I adopt \*Siugiu as the original form, \*Suigiu would not be impossible if we could establish that the Sui pronunciation of the modern dialectical «Suifu» goes back to the Mongol period.

CHARIGNON (*Ch*, II, 269) has admitted that Polo's return journey started from Lin-an, far to the south of Yün-nan-fu, which he takes as the geographical equivalent of Polo's «Amu». BENEDETTO (*B*<sup>1</sup>, 448), taking too literally Polo's later vague and second-hand information about the Gulf of Tonking (see «Cheynam») bordering upon the provinces of «Amu» and «Toloman», has looked for «Toloman» on the eastern (and even south-eastern) border of Yün-nan. This is impossible in view of Polo's itinerary, and for anybody who accepts at the same time, as CHARIGNON and BENEDETTO do, that «Cuigiu» is Hsü-chou-fu. But the chapters on Burma, Bengal, «Caugigu» and «Amu» are *hors d'œuvre* in Polo's account of Yün-nan, and I think YULE was right (*Y*, II, 131) in making the return journey start, in spite of the mss. (in particular of F), not from «Amu» (*q. v.*) but from Yün-nan-fu; we have here another example of these digressions which Rustichello or an early copyist has turned into portions of the main itinerary. Chinese texts of the time are full of incidents relating to the water and land communications between Yün-nan-fu and Ssü-ch'uan, and always mention the difficulties of the track of land south-west of Hsü-chou, when passing the territory of the T'u-lao-man. That is one of the



reasons why we must admit that the «Toloman» are the T'u-lao-man, and that «Cuigiü» is the Hsü-chou of the Mongol period, Hsü-chou-fu (*vulgo* Sui-fu) of modern times. The equivalence of «Cuigiü» and Hsü-chou-fu had been suggested to YULE, with very good reasons, by RICHTHOFEN, and it is a pity that YULE did not adopt it outright (Y, II, 129-130). CORDIER's note (Y, II, 131) and the map opposite still wrongly assume that Polo's route passed east of Hsü-chou-fu. The result is that the traveller is supposed to go up to Ch'êng-tu by the To-chiang, while there can be no doubt that his twelve days of travel by river from «Cuigiü» to Ch'êng-tu are from Hsü-chou-fu (Sui-fu) up the Min river via Chia-ting. RICHTHOFEN was also right on this point.

### 190. CUIUCCI

<i>canici</i> P	<i>cuiuci</i> (? <i>cuuici</i> ) F	<i>tanity</i> FB
<i>cimici</i> (?) LT	<i>cunici</i> FA	<i>tinucci</i> TA <sup>3</sup>
<i>cinitil</i> S	<i>cuuici</i> L	<i>tinuci</i> TA <sup>1</sup>
<i>ciuici</i> R	<i>cuuti</i> VA	<i>zentil</i> VL

The word is explained in F as «celz que tienent le chien mastin», and in RAMUSIO as meaning «signori della caccia». PAUTHIER and YULE could make nothing of this term, and BENEDETTO (*B*<sup>1</sup>, 143, 452) still adopts a reading *cinuci*. It seems that he has not been convinced by the note I had given in 1920 to CORDIER (Y, III, 70), unfortunately disfigured by a misprint «censeurs», instead of «coureurs» (to which I have already called attention in *JA*, 1927, II, 267). But if we take into account that, according to the Chinese texts, the 貴赤 *kuei-ch'ih* (or 貴由赤 *kuei-yu-ch'ih*; cf. WANG Hui-tsu<sup>2</sup>, 50, 1 a), numbering more than 10,000 men, were established under the command of Mingyan (see «Mingan», that Rašidu-'d-Din names a Bayan *güyükči* (see «Baian»), and that Polo speaks of two groups of *cuiucci*, each numbering 10,000 men, placed under the command of each of the two brothers «Mingan» and «Baian», no doubt can be entertained as to the identity of the *cuiucci* and the *kuei-ch'ih* or *kuei-yu-ch'ih*. The Mongol original of the Chinese form is clear. In a text I translate below, we read that *kuei-yu-ch'ih* means «to run quickly»; and the Mongol verb meaning «to run» is *güyü-* and *güi-*. Moreover, gutturals ending a syllable are generally not reproduced in Chinese transcriptions of the Mongol period; we have thus *güikči* and *güyükči*, both meaning «runners», exactly represented by *kuei-ch'ih* and *kuei-yu-ch'ih*.

The main text on the *güyükči* is that of the *Cho-kêng lu* of 1366, which says (original edition, I, 24 a) : «*Kuei-yu-ch'ih*, that is [those who] run quickly. Every year, there is a competition, which is called 'letting run' (放走 *fang-tsou*); the one whose legs are the swiftest wins an Imperial reward. For that purpose, the supervising officials put all the candidates in line and hold them by a rope so that there should be no quarrel about a difference in the moment of their departure; afterwards they take off the rope and let them go. If [the competition] is in Ta-tu (Polo's «Taidu», *i. e.* Peking), the start is from 河西務 Ho-hsi-wu (a well-known ancient customs station between Tientsin and Peking). If in Shang-tu (see «Ciandu»), the start is from 泥河兒 Ni-ho-erh ('the Muddy River'). In three [double] hours, they run

180 li and finish directly in front of the Emperor, prostrate themselves and shout 'Wan-sui' ('Ten Thousand Years'). He who arrives first is given a silver ingot (*ting*); to the others are given silk in varying quantities.» This text of the *Cho-kêng lu* is in fact copied from the *Shan-chü hsin-hua* of 1360 (*Chih-pu-tsu-chai ts'ung-chu* ed., 25 a-b), but our edition of this last work seems slightly corrupt.

In Mingyan's biography (YS, 135 a), we read : «In the 13th year *chih-yüan* (1276), Shih-tsu (Qubilai) ordered by edict that the people who were homeless, the Buddhist and Taoist monks who had escaped registration and men of the various races (*i. e.* not Mongol, or Northern or Southern Chinese) who did no service in public works, to the number of more than 10,000, should form the *kuei-ch'ih* (*güikküi, güyükküi*), and he ordered Mingyan to lead them.» The *güyükküi*, thus formed with more or less dubious elements, were nevertheless a fighting force which bore the name of «army» (*chün*) at least from 1284. In 1287, they became a 衛 *wei*, that is to say one of the divisions of the Imperial Guard, according to YS, 99, 2b. They were also called «the personal (親) army of the *kuei-ch'ih* (*kuei-ch'ih ch'in-chün*). Data for a more detailed study of the *kuei-ch'ih* are not lacking, but they have not been collected.

From the Chinese texts, the *güyükküi* do not appear in the same light as in Polo's text, where they are said to be keepers of the hounds and to have had as their principal task to help in the Imperial hunts. This may have been one of their duties, but it does not seem to have been the only one, nor even their main duty.

I have shown above that the *güyükküi* or *güikküi* had become one of the components of the Imperial Guard. Odoric (*Wy*, 475) speaks of four barons who always accompanied the Mongol Emperor when he rode out in a car, and these have been, naturally enough, identified with the *käšiktän* (see «Quesitan»). But Odoric calls them «Çuche» (var. «Zuche», «Cuthe», «Chuche»), which cannot be restored to *käšik* or *kašiktän*; we ought perhaps to read «Çuche» = *Güikküi*. As a matter of fact, the *käšiktän* had already been described by Odoric, although not named, in the preceding paragraph. On the other hand, it is clearly said in Mingyan's biography that he always accompanied Qubilai; his successors may have had the same duties.

## 191. CUNCUN

<i>cuncun</i> FB	<i>cinghu</i> TA <sup>3</sup>	<i>cuncon</i> F'
<i>chimchim</i> P	<i>concongne</i> FB, FBr, t	<i>cuncu</i> L
<i>chinchis</i> VL	<i>cunccur</i> LTr	<i>cuncun</i> F, FA <sup>t</sup> , L
<i>chuchin</i> V	<i>cunchin</i> R	<i>cuncur</i> LT
<i>chunchum</i> TA <sup>1</sup>	<i>cuncin</i> VA	<i>cuntun</i> FA

This is one of the very few Chinese names in Polo the original of which cannot yet be restored with almost absolute certainty.

From Hsi-an-fu («Quengianfu»), Polo marched three days to the West in the plain, then twenty days through the mountainous province of «Cuncun», and reached level country again at «Acbalect Mangi»; from «Acbalect Mangi», he proceeded to Ch'êng-tu in Ssü-ch'uan. There can

be no doubt that the mountains of «Cuncun» are the 秦嶺 Ch'in-ling, separating the basin of the Wei from that of the Han, but the «twenty days» are difficult to account for and the name «Cuncun» is puzzling.

As to the distance, YULE (*Y*, II, 34) has made use of a reading «VIII» instead of «XX» in a ms. of the FB class used by PAUTHIER (*Pa*, 364), but this same ms. gives «XX» the second time like all the others, and no stress can be laid on its single discrepancy.

In 1828, KLAPROTH stated «Cuncun» to be 漢中 Han-chung, on the Han River, and, while observing that the phonetic equivalence was not satisfactory, YULE (*Y*, II, 32) concurred with him; this identification is adopted without qualification in *RR*, 420. PAUTHIER (*Pa*, 363) maintained against KLAPROTH that, in Mongol times, Han-chung formed the department (*lu*) of 興元 Hsing-yüan, which is true, and that it was Hsing-yüan which Polo rendered as «Cuncun». But the phonetic equivalence is still less satisfactory than with Han-chung, and moreover, the names of *lu* do not seem to have then been much used in current speech; although Han-chung-fu, as such, does not go further back than the Ming dynasty, the region had been known, for centuries, as the «commandery» (*chün*) of Han-chung, and was probably designated as Han-chung in Polo's day; it is so called in a passage of Mongka's *pên-chi* which will be mentioned further on.

Against Han-chung, there is, however, a more serious objection. As suspected one moment by YULE and stated by CHARIGNON, it must be Han-chung which is meant by Polo under the Turkish-Persian designation «Acbalect Mangi» (*q. v.*). This was the reason which prompted CHARIGNON (*Ch*, II, 184) to propose another explanation of «Cuncun», to wit 關中 Kuan-chung, lit. «[the region] Within the Passes». Phonetically, Kuan-chung ought to be rendered \*Cancion, not «Cuncun», but Han-chung fares no better, since we should expect it to be transcribed \*Cancion. The real difficulty is that Kuan-chung, which never was an official designation of the province, is a comprehensive name which refers to the whole of Shàn-hsi province, including Hsi-an-fu itself and the region east and north of it; there was no reason to use it as a designation of the mountainous tract south of the Wei.

Rašidu'd-Din's account of Mongka's campaign from Shàn-hsi to Ssü-ch'uan (*Bl*, II, 324-328) might be of some use if it were properly elucidated; but unfortunately, BLOCHET's notes are not reliable. We know, however, from *YS*, 3, 4*a*, that Mongka, coming from the 六盤山 Liu-p'an-shan (see «Cinghis», p. 311), entered the mountainous tract south of 寶鷄 Pao-chi (at the very place where Polo probably did the same), then attacked the 重貴山 Chung-kuei-shan («Chung-kuei Mountain»), and afterwards stopped at Han-chung, which must be Han-chung on the Han River. But I do not think that the obscure name of the «Chung-kuei Mountain» can have anything to do with Polo's «Cuncun».

I feel inclined to admit that Polo's «Cuncun» phonetically renders Han-chung, but not to locate it at Han-chung on the Han River, which is his «Acbalect Mangi». During the second half of the 13th cent., the region of 鳳翔 Fêng-hsiang, which included Fêng-hsiang, Fu-fêng, Ch'i-shan, Pao-chi, and Lin-yu had been made the seat of the 陝西漢中道肅政廉訪司 *lien-fang-ssü* of the «region» (*tao*) of Han-chung of Shàn-hsi (cf. *YS*, 60, 2*b*). Fêng-hsiang and its various districts are all located north of the Ch'in-ling (not south of it, like the historical and modern Han-chung), and exactly correspond to the tract of land meant by Polo when he left the

plain and entered the mountains. That this administrative designation of Fêng-hsiang was in actual use in the 13th cent. is established, for instance, by a report from the said authority, dated 1293, which has been preserved in *Yung-lo ta-tien*, 19423, 21 a.

If my hypothesis be right, «Cuncun» ought to be \*Cancion (see «Cacionfu»), or perhaps \*Canciun. Only one reading shows an *-a-* in the first syllable, perhaps by accident. Of more importance is the *-i-* of the second syllable in V, VA, and R; it may point to an original form *-iū* (= *-iun*), later altered into *-in*. There is also a difficulty due to the *ch-* before most of the *-i-* readings; but VA gives «Cuncun» and the Stockholm ms. (FC<sup>1</sup>) «Cucim». So a restoration of the name as \*Cancion or \*Canciun is not impossible, but of course too uncertain to be introduced into the text.

## 192. CURD

*car* VB  
*card* F, FB, L  
*cardi* P, VL

*chardi* VA  
*chutel* V  
*curd* Z

*curdi* R  
*gard* FA

The Kurds (see also «Curdistan»). Hethum always uses «Cordins» (cf. *Hist. des Crois.*, Arm., II, 901). Ricold calls them «Curti», and says that their name comes from the Persian word for «wolf». YULE (Y, I, 62) objects on the ground that the Persian word for «wolf» is *gurg* and that «*kurt*» is Turkish; he adds that the name appears in classical texts before the Turkish language could have spread to that part of Asia. Of course Ricold was wrong, but even more so than YULE thought, as the Kurds are كُرد Kurd, whereas the Turkish word is *qurt*; I may add that *qurt* is «wolf» only in Osmanli, but means «worm», «insect», in all other Turkish dialects; *qurt* had originally a general meaning of «wild animal» (BROCKELMANN, *Kāšyari*, 165), and has probably become the name of the «wolf» in Osmanli on account of the taboo which seems to have suppressed its true name in many Turkish dialects.

On the Kurds, cf. *EI*, notice «Kurds» by MINORSKY; *LS*, Index, 515; *Mi*, 497 (the *Hudūd* used the name «Kurd» in a very loose way).

## 193. CURDISTAN

*cardisstan* VB  
*chudischan* V  
*ciudistam* VA  
*cordistan* F

*curdiscam* FA  
*curdistam* VL  
*curdistan* F, FB, L, P, Z; R

*distam* TA<sup>1</sup>  
*distan* TA<sup>3</sup>  
*turdistam* LT

The land of the Kurds (see «Curd»), or Kurdistan. Polo makes it the second of the eight «kingdoms» of Persia, and this has passed, with the name of «Curdistan», to Fra Mauro's map. But, while Polo speaks of the inhabitants of Kurdistan as «Curd», Kurds, Fra Mauro calls them

«Curdistani». Schiltberger speaks of the land of «Churt», and Clavijo names twice «Curchistan» (not «Koordistan», as in HALLBERG, 171-172).

On Kurdistan, cf. *LS*, Index, p. 515, and particularly p. 190; *EI*, s. v. «Kurdistan», by MINORSKY.

## 194. CURMOS

<i>achamas, charmusa, chiuri-</i>	<i>churmos</i> L, V, VA	<i>cremosa</i> F, LT
<i>mandi</i> TA <sup>3</sup>	<i>cormes, cormussa, cremos,</i>	<i>cremosor</i> F, L, P
<i>achurmesa, carmos, cormorso,</i>	<i>cremosa, ormessa</i> VB	<i>crymosce, kurnos</i> G
<i>cormoso</i> LT	<i>cormos</i> F, Fr, FA, FB, L,	<i>curmos</i> F, Ft, L, V, Z
<i>arcomes, cremes</i> VL	P, VB, VL, Z	<i>curmosa</i> F, FA, L, P
<i>chormos</i> TA <sup>1</sup> , TA <sup>3</sup> , VA, VL, Z	<i>cormosa</i> L	<i>formosa</i> LTr
<i>chormoxe, chremosor, cremoxa</i>	<i>cormot, hormes, quermos,</i>	<i>homos</i> FAr
VA	<i>turmos</i> FA	<i>hormos</i> FAt, FB
<i>chremoso, chuimese, churmai,</i>	<i>cremessor</i> R	<i>ormus</i> VB; R
<i>cromos, uiruosa</i> V	<i>cremo, cremosu, curmaso</i>	<i>tremesor</i> FA, FB
<i>chumasa, churimasa, churmaso,</i>	TA <sup>1</sup> , TA <sup>3</sup>	<i>urtumosa</i> FB
<i>churmoso</i> TA <sup>1</sup>	<i>cremon</i> LT	
Plain		
<i>bellissima</i> V	<i>formoso</i> VL	<i>formoxa</i> VA
<i>formosa</i> LT, P, TA <sup>1</sup> , TA <sup>3</sup>	<i>formosse</i> VB	<i>ormus</i> R
<i>formose</i> F, FA, FB, L		

For the Polian form of the name of حرمز Hurmüz or حرمز Hurmuz (Hormuz or Ormus in the Persian Gulf), «Cormos», adopted in *RR*, 418, would have as much ms. support as «Curmos». YULE's «Hormos» (*Y*, I, 107) is the result of some editing by copyists. BENEDETTO, who adopted «Curmos» in *B*, 265, preferred «Cormosa» in *B*<sup>1</sup>, 442, but the balance of the best mss. is against a final *-a*. Chinese mediaeval transcriptions are based on *-u-* pronunciations in both syllables, but most western ones suppose *-o-* in the first. But on second thought, I am not certain that I have been right in deciding for «Curmos» and not for «Cormos». For another word in which Polo rendered *h-* as *c-*, see «Cogacin». A similar form «Cormus» is used by Giovanni d'Empoli (cf. *Y*, I, 87), but I doubt whether such transcriptions of *h-* as *c-* ought to be ascribed, as they are by YULE, to the Tuscan origin of both Rustichello and Giovanni d'Empoli.

Through an early corruption, the plain of Hormuz has become the plain of «Formose» in all our mss.; with a possible attraction of *formosa*, «beautiful» (hence «Bellissima» in *V*), this graphic error has counterparts in other names, for instance «Fungul» instead of «Cuigiu» (*q. v.*), another clerical corruption. In this passage, RAMUSIO's «Ormus» either is a case of editing, or may point to the fact that his prototype of *Z* possibly gave the correct form. All commentators are agreed that «Formose» is Hormuz.

The same unanimity has not been reached in respect to «Cremosor», which BALDELLI-BONI

(II, 40), PAUTHIER (*Pa*, 60), YULE (*Y*, I, 75) and CHARIGNON (*Ch*, I, 49) have explained as Gärm-sir, « Hot Region ». But MARSDEN (*Marco Polo*, 72) was certainly right when he saw in « Cremosor » merely a clerical error for « Curmos » or « Cormos ». For other cases in which possibly the terminal flourish of a copyist may have become -r, see « Caccia modun ».

Polo's « Curmos » is not the Hormuz Island, but Old Hormuz on the mainland. RAMUSIO's statement to the contrary certainly is not based on ms. tradition, but is due to scholarly editing (cf. Vol. I, 44, 123). Old Hormuz was a very ancient place, since there seems to be no reason to doubt its identification with Nearchus' Ἀρμολεία (c. 325 B. C.), as well as Ptolemy's Ἀρμουζα πόλις and Ἀρμολον ἄκρον (VI, VIII, 5). This would dispose of the theory according to which Hormuz was founded by and received its name from the first Sassanian Emperor (cf. BARBIER DE MEYNARD, *Dict. histor. de la Perse*, 595; *Pa*, 86; *EI*, s. v. « Hormuz »). As to the name, like some others in Persia, it must represent Ahura-mazda. But, after the early Western classical mentions, several centuries elapse before Hormuz again occurs in any texts. The Nestorian bishop of Hormuz, mentioned under the date c. 540 by YULE (*Hobson-Jobson*<sup>2</sup>, 646) from ASSEMANI (*Bibl. Or.*, III, I, 147-148), seems to be of doubtful authority; no such see appears in CHABOT's *Synodicon Orientale*. As to Theodorus, bishop of « Hormuzdadschir », mentioned under c. 655 by YULE from the same source, he certainly was bishop of « Hormizd-Ardašir », later Hormuzšir, i. e. Ahwāz in Hūzistān, which has nothing to do with Hormuz (cf. BARBIER DE MEYNARD, *loc. cit.*, 57-61; CHABOT, *Synodicon Orientale*, 673; *LS*, 233; *Mi*, 381).

Hormuz became a place of some importance only well after the Mussulman conquest. HIRTH and ROCKHILL (*HR*, 14) proposed to see Hormuz in the 末羅 Mo-lo (\*Muât-lâ) of Chia Tan's itineraries (end of the 8th cent.), and this solution, accepted by BARTHOLD (*ZVOIRAO*, XXI [1913], 0164), has been mentioned by STÜBE in *EI* (s. v. « Hormuz »), not, however, without some qualification. But HIRTH and ROCKHILL's hypothesis was connected with their erroneous identification of Fu-ta with Old Cairo, while it is really Bagdad (see « Baudac »). Mo-lo must be inland, on the Euphrates and not far from its mouth. HERRMANN equated it to Bašra, a solution which is satisfactory from the triple point of view of geography, history, and phonetics. As a matter of fact, and since Hūwārezmi's « Armuza », c. 833, is merely copied from Ptolemy (*Fe*, 593), the earliest mediaeval mention of Hormuz which I can trace is that of Ibn Hordādbāh who, in 844-848, cites « Urmūz » as one of the ports of call on the sea route from Bašra to China. Its remains lie near the hamlet of Kumbil, some ten miles to the south of Mināb (cf. *Y*, I, 110-111; SINCLAIR, *Pedro Teixeira*, 155; Sir A. STEIN, in *Geogr. Journal*, LXXXIII [1934], 129, and his map).

The great prosperity of Hormuz dates from c. 1100, when it was ruled by an Arabic dynasty. Under « Rucnedin Acmat », I shall discuss who must have been the « melic », or king, of Hormuz mentioned by Polo. The city was transferred from the mainland to the island of جَرُون Jārūn (or Jārāun, or Zārūn) shortly after the return of the Polos to Europe, and the name of Hormuz passed to this new settlement. One fact is perfectly plain : Hormuz was already on the island at the time of Odoric's visit in 1321. But the date and conditions of the transference have not yet been established satisfactorily. The date 1302, generally adopted, is based on a passage of the lost *Šāh Nāmāh*, or Chronicle of the Kings of Hormuz, written by the king « Turonxá » (Ṭūrān-

šāh), and translated by Pedro TEIXEIRA in 1593-1597 probably into Portuguese, and thence in 1605-1609 into Spanish, more or less completely and accurately, and with many digressions (cf. W. F. SINCLAIR, *The Travels of Pedro Teixeira*, Hakluyt society, 1902, XVI, xci). There we read (*ibid.*, 160-162) : « But in the year 700 of the Moors, which fell in 1302 A. D., there came out of Turkestan great hordes of Turks, and conquered many lands in Persia. They attacked the kingdom of Kermon (= Kirmān), and next that of Harmuz (= Hormuz), and wasted it all... The Harmuzis, unable to withstand such troubles, made up their minds to abandon their lands, and so they did... » According to TEIXEIRA's translation, the king of Hormuz was then « Mir Bahadin Ayaz Seifin »; he moved his people first to the island of « Queixome » (= Kišm), and soon after to that of « Gerun » (= Jārūn). BARROS (Dec. II, liv. II, ch. II) in 1553 had already made use of what seems to have been an earlier translation from the same Chronicle, and ascribes the migration from the mainland to the island to « Gordunxá » in 1273 (cf. SINCLAIR, *loc. cit.*, xcvi, 162, 169. BARROS has A. H. 680 = 1273; in fact 1273 = 671, 680 = 1281). Still before TEIXEIRA, an abridged translation of Tūrān-šāh's Chronicle had been published in 1570 as an Appendix to Gaspar DA CRUZ' *Tractado... da China* (but I see no reason to ascribe this translation, as SINCLAIR surmises, p. xcvi, to Gaspar DA CRUZ himself), and there it is said, without any date (SINCLAIR's translation, 260) : « ... The king of Cremam (= Kirmān)... came with many men... against Hormuz to destroy it. King Cabadiṁ (? read « Bahadim »), who at the time reigned in Hormuz, ...embarked with all the people that could go... and betook himself to the island called Queixome... After he had been there a few months..., he... crossed over with his people to the island that is now called Hormuz. » Although both translations are from the same chronicle, the one added to DA CRUZ' *Tractado* says that « Jarum » means « jungle », while TEIXEIRA makes « Gerun » to be the name of an old man. On the whole, neither translation seems to be very strict; even for the reign of Tūrān-šāh himself, editors have to suppose either an error of date of nearly a century, or the omission of several reigns (cf. SINCLAIR, 188-189), which shakes to a serious extent our faith in TEIXEIRA's accuracy. A late Mussulman author, Muhammed Maḏdī, says that Old Hormuz was abandoned by the king Šamsu'-d-Dīn in A. H. 715, *i. e.* 1315 A. D. (cf. BARBIER DE MEYNARD, *loc. cit.*, 595). Other sources give Faḥru'-d-Dīn Aḥmad as the prince who moved from Old Hormuz to the island of Jārūn (cf. Y, I, 121). Abū-'l-Fidā († 1331) says of Hormuz (REINAUD and GUYARD transl., II, II, 104) : « Some one who visited it recently told me that Old Hormuz had been mined by the incursions of the Tatar, and that the inhabitants had migrated to an island called Zarūn... ».

STÜBE (*EI*, s. v. « Hormuz ») has taken exception to Abū-'l-Fidā's text, because « the Mongols hardly reached the shores of Kirmān ». According to SINCLAIR (*loc. cit.*, 161), the invaders must have been subjects of the Mongol Ikhans of Persia, but probably of Turkish stock, in agreement with TEIXEIRA. SINCLAIR adds that their encroachments must have been progressive along the coast, and have spread over several years « previous to A. H. 700, A. D. 1302 », the latter date being that of the settlement on the island.

Neither opinion seems to me to take full account of the real facts, I shall not lay too much stress on the statement of Stephan Orbelian, who, writing towards the end of the 13th cent., says that the conquests of the Mongols, already before the middle of the century, had extended

as far as Hormuz (cf. BROSSET, *Hist. de la Siounie*, I, 227; PATKANOV, *Istoriya Mongolov*, I, 33). But YULE (*Y*, I, 121) already connected the transfer of Hormuz to the island with the great raid of Qutluy-šāh's bands in 1299, in the course of which, according to Waśśāf, Hormuz (= Old Hormuz) was besieged; the invaders were repelled by the king Bahā-ud-Dīn Ayaz (*Ha*<sup>1</sup>, II, 106). This king is evidently the same as TEIXEIRA's «Mir Bahadin Ayaz Seifin», and the chronological agreement is even greater than has been said hitherto. It is a matter of surprise that even SINCLAIR should have accepted and repeated in his own name TEIXEIRA's equivalence «A. H. 700 = 1302 A. D.», while it is 16 September 1300 — 5 September 1301 (*L'art de vérifier les dates*, I, 1783, 26-27). With such an agreement between Waśśāf and the Chronicle, it seems safe to date the transfer towards the end of 1300, under the reign of Bahā-ud-Dīn. Faḥru'-d-Dīn, whom YULE adduced with caution, must have been then far from Persia (see «Caçan», «Cocacin», and «Rucnedin Acmat»).

It may even be that the Mongol inroads left a lasting trace in the designation which the new settlement on the Jārūn Island adopted for the mainland; but I readily admit that I am here advancing on slippery ground. Ibn-Baṭṭūṭah, who visited the island of Hormuz c. 1330, gives to the Persian coast opposite the island of Hormuz a name which the translators have read «Mūyostān», adding: «The name Mūyostān, or Moyistān, refers to the part of the province of Kirmān which borders on the Persian Gulf» (DEFRÉMERY, *Voyages d'Ibn Batoutah*, II, 230, 455). As results from DEFRÉMERY's reference to TEIXEIRA, his transcription «Mūyostan» was determined by the similar mention of «Mogostam» in TEIXEIRA's translation of the Chronicle (cf. SINCLAIR's transl., 156, 166, 173, 186); «Mogostam» also occurs in TEIXEIRA's extracts from Mirḥōnd (*ibid.*, 197; I cannot trace the name in Mirḥōnd, and this is clearly a digression due to TEIXEIRA); the earlier Dominican abridged version of the Chronicle gives «Magostam» (*ibid.*, 258). The alternative form «Mūyistān» has been adopted by LE STRANGE (*LL*, 319), «Mogistan» by SINCLAIR (p. 156), and «Mūghustān» by DAMES (*Duarte Barbosa*, I, 90); «Mughostān» has been adduced by HIRTH and ROCKHILL (*HR*, 14) as a possible original of Mo-lo in their unfortunate attempt at identifying this Mo-lo with Hormuz.

By interpreting the name as Mūyistān or Mōyistān, the various authors clearly understood it as one compound name, which ought then to be written مویستان \*Mōyistan in one word, or مویستان \*Moyistān if the first vowel be short. In both spellings it might mean «place of the Magi»; with the second, MENINSKI gives it with the sense, mentioned also by BARROS, of «palm garden», with a reference to KAEMPFER; cf. MENINSKI, *Lex. Arab.-pers.-turc.*, 1780, IV, 640: «مویستان *mughystān*. p. n. s. Palmetum. *Kæmpf. amæn. exot.*». But KAEMPFER, *Amoenitates exoticæ*, 1712, 665, only says that the palm is called *Mogh* (مُغ *moy*) in the Hormuz dialect, *Moch* (مُغ *moh*) in the neighbouring dialects. But Ibn Baṭṭūṭah's form is different; مویغان *Moy-āstān*, something like «resting place of the Magi». In the Portuguese transcriptions, -m is always used instead of -n; so the final makes no difficulty. But «Magostam» of the Dominican, «Mogostam» of TEIXEIRA prove that the second vowel was not -i-, but either -ā- as in Ibn Baṭṭūṭah, or -ō- (-ū-); while on the other hand, a survival of the old form *mōyu* of the name of the Magi is out of the question for the 14th cent. and later. Now, in several notices on his map, Fra Mauro gives to the Persian coast opposite the island of Hormuz the name of «Mogolistan» (cf. HALL-



BERC, 244; *Zu*, 43 : « La provincia dita Mogolistan posta qui de sopra a man dextra e in dromo de la isola dita Hormuz la qual ha el suo viver de la sopra dita provincia Mogolistan . . . », etc.). I do not think that we can separate this «Mogolistan» from the above «Moy-āstān» or «Magostam», «Mogostam». In Persian, «Mogolistan», «Land of the Mongols», is usually written مغولستان *Moyolistān*, but there is also a form مغول *Moyūl* of the name of the Mongols, and an alternative spelling which would mark the first -o- vowel is quite conceivable. There is no explanation for the use of «Moy-āstān» or \**Moyistān*; but it would be quite natural, for a population which had deserted the mainland on account of Mongol inroads, to designate henceforth that mainland as *Moyolistān*. An alteration of *Moy-āstān* into «Mogolistan», though not impossible, is hard to admit. I wonder whether it is not rather مرغ استان which is corrupt instead of مرغولستان \**Mōyōlistān* or مرغولستان *Moyōlistān*, of which «Magostam» or «Mogostam» would represent a later shortened form.

Hormuz was well known to Western travellers, and its name occurs in various transcriptions, most of which have been collected by HALLBERG (pp. 242-246). I shall not repeat them here, and shall add only G. ADAM's «Hormutz» (*Rec. Hist. des Crois.*, Arm., II, 552, 553). For a later date, Portuguese forms beginning with *a-* are common : «Armusa» or «Armuzza» in CAMOËNS, «Armuzia» in St. FRANCIS XAVIER, «Armuz» in several other works of the 16th cent. (cf. YULE, *Hobson-Jobson*<sup>2</sup>, 646; DALCADO, *Glossário Luso-Asiático*, I, 57); moreover, TEIXEIRA always writes «Harmuz», and maintains that such is the true form (cf. SINCLAIR, p. 17); FERRAND is inaccurate when stating (*JA*, 1920, II, 34) that the ancient Portuguese texts always give «Ormuz». All of these show a curious recurrence of the classical Ἀρμύζεια and Ἀρμουζα.

Odoric wrote «Ormes», as can be seen not only from his own account but also from early reports based on it (cf. GOLUBOVICH, *Bibl. bio-bibl.*, II, 81, 89, 98, 111); this is not without interest in view of the form occurring in Hethum, «Hormes» (cf. *Rec. Hist. des Crois.*, Arm., II, 126, 266). Hethum says that the tradition was that the city of «Hermes» had been founded by «Hermes the Philosopher»; Maundeville copied him when he spoke of «a city that is clept Hermes, for Hermes the Philosopher founded it»; Fra Mauro knew the same tradition (*Zu*, 43); KAEMPFER (*Amoenitatum Exoticarum*, 756) still quotes it from KIRCHER, and does not reject it. Wrong as it is, the confusion is easily explainable, since Hormuz, هرمز *Hurmuz*, represents the very name of Ahura-mazda, but the name of Ahura-mazda is also occasionally written هرمس *Hurmus*, and at the same time *Hurmus* is the form taken in Persian by the name of Hermes Trismegistus.

There is just a remote possibility that the name of Hormuz may occur in a Chinese text of the 12th cent., in connection with the account of an embassy which had come in 1071 from the kingdom of 層檀 *Tsêng-t'an*. HIRTH and ROCKHILL (*HR*, 127), who knew this embassy only from the *Sung shih*, 490, 9 a, have tentatively identified *Tsêng-t'an* with Zanzibar, but I have left out the name in my notes on the latter country (see «Çanghibar»), because I could not explain it. The text of the *Sung shih* must be studied together with the parallel paragraphs in *Hsü Tzū-chih t'ung-chien ch'ang-pien* (332, 3 a-b), *Ch'ing-po tsa-chih* (*Chih-pu tsu-chai ts'ung-shu* ed., *pieh-chih* section, ch. 2, 14 a-b), and, above all, *Wên-ch'ang tsa-lu* (*Hsüeh-chin t'ao-yüan* ed., I, 4 a; 4, 5 b; on the *Wên-ch'ang tsa-lu*, see «Cotton», *supra*, p. 437). The prince of

Tsêng-t'an was called Amīr-amīrān, «Emir of emirs», which is a Persian title, and his family had ruled for ten generations during 500 years (which is certainly excessive, since it gives an average of 50 years of reign for each prince). Tsêng-t'an (\*Zāng'dān, \*Zāngtān?) suggests no known name; -ān may or may not be a Persian plural. The products of the country are partly Asiatic, partly African, but some may have come there by trade. The *Wên-ch'ang tsa-lu* alone, the author of which had access to the Court archives, gives the names of the countries conterminous to Tsêng-t'an: to the east, Tsêng-t'an reached the sea; to the west, it bordered upon the kingdom of 胡盧沒 Hu-lu-mu; to the south, upon the kingdom of 霞勿檀 Hsia-wu-t'an; to the north, upon the kingdom of 利吉蠻 Li-chi-man. None of these names seems to be traceable anywhere else (I have not tried the *Sung hui yao*, the contents of which are hard to check). Hu-lu-mu (where *mu* is \**muat*) would be a good transcription of Hormuz, but the geographical data would hardly fit.

In the Chinese map of the *Ching-shih ta-tien*, dated c. 1330 and based on a Mussulman map, the name of Hormuz does not occur now, but originally it must have been included, since it is found in the corresponding list of YS, 63, 16 *b*, where it is written 忽里模子 Hu-li-mu-tzū, Hurmuz (cf. *Br*, II, 130; T'u Chi, 160, 23 *b* [but T'u Chi has mixed up with Hormuz the Qurumši of the *Secret History*, which is the old Mongol form of an unidentified name which may or may not be Hormuz]). In YS, 123, 8 *a*, we read that Qubilai detailed an Alan (Asut), 忽兒都苔 Hu-êrh-tu-ta, to accompany the *noyan* 不羅 Pu-lo on an embassy to «the place (地 *ti*) of 哈兒馬某 Ha-êrh-ma-mou». BRETSCHNEIDER (*Br*, II, 89) supposed that *mou* could be a mistake for *ssū*, and that the hypothetical Ha-êrh-ma-ssū might represent Hormuz, and later on (II, 132) spoke as if Ha-êrh-ma-ssū actually was the reading given in the YS; in Pu-lo, he proposed to see the famous Bolod, the informant of Rašīdu-'d-Dīn on Mongol history and customs; the whole of this second passage has been reproduced by CORDIER in *Y*, I, 115. In his unfortunate biography of Marco Polo, T'u Chi (117, 5 *b*; cf. also 102, 14 *a*) maintained that Pu-lo was no other than Polo, and that Ha-êrh-ma-mou was «Bombay the Black», Qara-Bombay (!). T'u Chi's solutions may be rejected without further ado. But the passage is puzzling. The only *noyan* Po-lo or Pu-lo in Qubilai's time is Bolod, in Persian Pulād, who actually was sent by Qubilai to Persia where he arrived in 1285 and remained until his death. On the other hand, Hu-êrh-tu-ta seems to represent \*Hurtuqta, and an Urtuqta actually arrived at the Persian Court on February 24, 1286, bringing the Imperial edict of investiture delivered to Aryun by Qubilai (cf. *Oh*, IV, 13 [where the name is read «Ordoucaya»], and *Bl*, I, 230 [where it is read «Urtukhata»; cf. also p. 133]). In spite of several difficulties which I cannot examine here, things really look as if Hu-êrh-tu-ta had been sent on a mission to Persia. But that does not imply that Ha-êrh-ma-mou may be Hormuz; apart from -r- in the second syllable, not one character is phonetically acceptable for such an equation. As a matter of fact, I do not know what name of man or of place is intended; *ma-mou* sometimes renders Maḥmūd, which gives no sense here; and we must not forget that the passage comes in a biography, probably based on a document of private origin. No solution can be reached for the present, but Hormuz is most improbable. As a matter of fact, if we except the Hu-li-mu-tzū based on a Mussulman map, Hormuz occurs in Chinese texts of the Mongol dynasty only once, in a document which remained unknown to

BRETSCHNEIDER. It is the brief notice on 楊樞 Yang Ch'u's embassy, which, in 1307, brought back to 忽魯模思 Hu-lu-mu-ssü, Hormuz, the mission sent by Ghazan to China nine years before (see « Caçan »).

As long as the Ilkhans remained on the throne in Persia, it seems that Hormuz was considered in China as belonging to them, and no diplomatic intercourse took place. Even Chinese traders must have taken small notice of the place, since there is no paragraph on it in the *Tao-i chih-liao*, written in 1349-1350. Hormuz comes to the front in Chinese texts only in the first half of the 15th cent.; its first embassy arrived at the Court of the Ming in 1414 (cf. *Br*, II, 132-135; *TP*, 1933, 243, 431-440; 1934, 296, 308; add. WANG Ch'i's *Hsü Wên-hsien t'ung-k'ao*, 236, 13 b; and his *Pai-shih hui-pien*, quoted by T'U Chi, 160, 23 b). The wrong distinction between a kingdom of 忽魯謨斯 Hu-lu-mo-ssü and a kingdom of 忽魯母恩 Hu-lu-mu-ên, due to a corruption of 思 ssü into ên, seems to occur first in the *Ming i-t'ung chih* of 1461, 90, 23, and was taken over by HUANG Hsing-tsêng in his *Hsi-yang ch'ao-kung tien-lu*, and by CHÊNG Hsiao in his *Wu-hsüeh pien* (68, 40 a) as well as in the independent edition of his *Huang-Ming ssü-i k'ao* (*Kuo-hsüeh wên-k'u* ed., 128); the author of the *Huang-Ming hsiang-hsü lu* (5, 22 a) felt doubtful about it, and it was dropped from the *Ming shih* (326, 5-6). An additional proof that no embassy from Hormuz came in 1405 is provided by the fact that, according to the *Huang-Ming hsiang-hsü lu*, this embassy brought an ostrich, and Yung-lo ordered Chin Yu-tzū to extoll the bird in an ode; now, this ode is given in the *Shu-yü chow-tzū lu*, 9, 7-8, and it is dated 1419.

The main interest of the Chinese texts is to establish that the king whose embassy, arriving in 1433, must have been sent in 1432, was Saifu'd-Dîn. From 'Abdu'r-Razzâq, we know that the king reigning in 1442 was Faḥru'd-Dîn Tûrân-šâh. Both fall within a period for which TEIXEIRA's translation of the Chronicle gives no information (cf. SINCLAIR, *loc. cit.*, 188-189).

The king of Hormuz acknowledged Portuguese suzerainty in 1507, and Albuquerque finally conquered the island in 1515 (not « 1509 » as in *Br*, II, 132, nor « 1510 » as in *Y*, I, 121). With the help of a British naval force, Šâh 'Abbâs took it from the Portuguese in 1622 (not « 1621 » as in *EI*), and removed the trade to the near place on the mainland which had been known as Gombroon and which was henceforth called Bândâr 'Abbâsi, « Port of [Šâh] 'Abbâs » (cf. *Hobson-Jobson*<sup>2</sup>, s. v. « Gombroon »).

The name of Hormuz has long survived in the West as the designation of a textile : It. *ormesino*, *ermesino*, *armesino*, Port. *armezim*, French *armoisin*, *armosin*, Engl. *armesie*, *armosyn*, *armozeen*, *ormesine*. It was a light plain taffeta, generally black, of both western and eastern manufacture (cf. KARABAČEK, in *Mitteil. d. K. K. Oesterr. Mus. f. Kunst u. Ind.*, VII, [1879], 304-305; LOKOTSCH, No. 1596; GAY, *Vocabulaire archéologique*, I, 71; *Hobson-Jobson*<sup>2</sup>, s. v. « Ormesine »; DALGADO, *Glossário Luso-Asiático*, I, 57; LUDWIG, in *Italien Forschungen*, I [Berlin, 1906], 297, 372; BOXER, *Jan companie*, 184). The various forms of the word betray the influence first of the mediaeval transcriptions of « Hormuz », and later of the Portuguese transcriptions beginning with *a*.

In the Introduction to the present notes, I have discussed the reasons which may have induced the Polos, once at Hormuz, to abandon their original scheme of reaching Qubilai's dominions by sea.

## 195. ÇAITON

<i>airon, çaitem, çaiton</i> F	<i>caytum, çaintum, çaitum,</i>	<i>sarcam, sarcan</i> FB
<i>cacar, çarcairon, çarton, çayton,</i>	<i>çaytum, çaytun, zaytun</i> Z	<i>sartam</i> FBr
<i>zayton</i> LT	<i>çaiton, zaitoni (?) zaython</i> VL	<i>sartan</i> FA <sup>t</sup> , FB
<i>caiten, cancaron, caron, zerito (?)</i>	<i>charia, zanto, zaton</i> TA <sup>3</sup>	<i>zaito</i> TA <sup>1</sup> , VA
VB	<i>çatan, zartom</i> TA <sup>1</sup>	<i>zaitom, zaizen</i> VA
<i>caiton</i> F, L, VB	<i>çaairon, zairon</i> L	<i>zaiton</i> TA <sup>1</sup> , V
<i>carcon, kayten, sarcon, tarcon</i>	<i>çairon</i> F, L	<i>zaitum</i> R
FA	<i>çartan</i> Fr, t	<i>zarton</i> TA <sup>1</sup> , TA <sup>3</sup>
<i>carton</i> FA, FB	<i>marchon, zaitore, zandon,</i>	<i>zaycem</i> G
<i>carcan</i> P, P <sup>5</sup>	<i>ziargati</i> V	<i>zaytem</i> P
<i>cayton</i> FB, Z		

Without the slightest doubt, the various readings represent a pronunciation «Zaiton». In the jumble of the mss. readings of the name, I fail to discover any clue for a difference between the name of the city and that of the port. Former efforts in that direction have proved futile (cf. *Pa*, 527; PHILLIPS, *JNCB*, XXI [1887], 41-42; *Y*, II, 234, 240). Yet the text speaks of the «port» and of the «city of this port»; cf. Vol. I, 351. Andrea da Perugia, who arrived in the city in 1318 and became the local bishop in 1322, speaks in 1326 of that «great city», *que vocatur lingua persica Zaiton* (*Wy*, 374). Peregrino da Castello's much-discussed letter is said to have been written at «Zayton» on December 10, 1318 (*Wy*, 368). The same spelling is used by Odoric (*Wy*, 460) and Marignolli (*Wy*, 536). John of Cora's «Racon» is of course a clerical error (*JA*, VI [July 1830], 68; *Y*<sup>1</sup>, III, 100). «Zayton» appears on the Catalan Map (cf. CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 32-33). Fra Mauro's map gives «Çaiton», «Zaiton» and «Zaidon» (cf. *Zu*, 38; HALLBERG, 95; and the map itself). Persian and Arabic sources uniformly write زیتون Zāitūn; for instance, Ibn Sa'īd († 1274 or 1286; *Fe*, 349), Rašīdu-'d-Dīn (*Bl*, II, 490), Ibn Baṭṭūṭah (transl. DEFREMERY and SANGUINETTI, IV, 269, 272, etc.; *Fe*, 427-429, 454-455), Abū-'l-Fidā (in REINAUD, *Géogr. d'Aboulféda*, II, II, 122-124), down to the *Āin-i Akbarī* of 1595 (*Fe*, 554). Waśśāf's ms., as edited by HAMMER (Persian text, 43), writes لکنین فرزیتون وچین کلان, which HAMMER (*Ha*<sup>2</sup>, transl., 43) has rendered «Lonkin Ferwesetium», but which evidently represents «Lonkin-fu (Chin. Lung-hsing-fu [フ]; *vide infra*), and Zāitūn (زیتون), and Čin-kālān (= Canton; omitted by HAMMER)». I quote this example to show that the vocalic signs in HAMMER's ms. are arbitrary, and in particular that they cannot be cited to support any reading different from Zāitūn.

Already in 1655, the Jesuit MARTINI, in his *Novus Atlas Sinensis* (§ on «Civencheu»), proposed to identify «Zaiton» with 泉州 Ch'üan-chou on the coast of Fu-chien. DE GUIGNES concurred with him (*Hist. gén. des Huns*, III, 169). Finally, KLAPROTH published in 1824 (*JA*, v [July 1824], 41-44; also XI [1833], 342) a note in which he explained «Zaiton» as a transcription of 刺桐 Tz'ü-t'ung, saying that he had found Tz'ü-t'ung given as an ancient name of Ch'üan-chou in the *Ta-Ch'ing i-t'ung chih*. This equivalence was accepted for half a century,

until it was challenged by G. PHILLIPS and Carstairs DOUGLAS, and more recently by CHARIGNON. I think that KLAPROTH was probably right, in spite of some weak points in his argument.

In the first place, KLAPROTH was mistaken when he quoted as an independent source on « Zaiton » a late Turkish geography which in the paragraph in question is quite evidently an almost *verbatim* retranslation from Polo himself (PAUTHIER in *Pa*, 528, and PHILLIPS in *TP*, 1895, 455, still follow KLAPROTH here, but YULE knew better; cf. *Y*, II, 230). KLAPROTH moreover misunderstood *tz'ü-t'ung* as meaning two different plants, « thorns and *Bignonia tomentosa* ». This last error has unfortunately been repeated by PAUTHIER (*Pa*, 528), CORDIER (*L'Extrême-Orient dans l'Atlas Catalan*, 32) and BLOCHET (*Bl*, II, 490). But *tz'ü-t'ung* is the name of a thorn-bearing tree, probably here the *Acanthopanax ricinifolium* (but it is also used as a name of the *Erythrina Indica*, which was adopted in the present case by KUWABARA in *Mem. of the Research Department of the Toyobunko*, II [1928], 30). According to KLAPROTH, *Tz'ü-t'ung* was an ancient name given to Ch'üan-chou because its city walls were all planted round with *tz'ü-t'ung*.

YULE, who was always in favour of the identification of « Zaiton » with Ch'üan-chou, thought that *Tz'ü-t'ung* had been, according to KLAPROTH, the real official name of Ch'üan-chou; and as the name Ch'üan-chou already existed « in the 7th or 8th century », the use of *Tz'ü-t'ung* and its passage into foreign languages as *Zäitün* was bound to be of a very early date (*Y*, II, 237). Of course, KLAPROTH made the best of his case by being vague on the nature and use of *Tz'ü-t'ung* as a name. DOUGLAS rightly objected that the name was not *Tz'ü-t'ung*, but *Tz'ü-t'ung-ch'êng* [城], the « City of the *tz'ü-t'ung* », and that it was an epithet rather than the true name (*Y*, II, 236). CHARIGNON (*Ch*, III, 115), going further, said that even *Tz'ü-t'ung-ch'êng* never existed, but only *T'ung-ch'êng*, which cannot give the etymology of « *Zäitün* ». According to CHARIGNON, the true etymology of the name is 瑞桐 *Jui-t'ung*, which was found by HIRTH as a name of Ch'üan-chou in a work of 1274, and which CHARIGNON himself met with in a modern Chinese work. But CHARIGNON blundered here. In the work of 1274, there was no mention of Ch'üan-chou under the name of *Jui-t'ung*; HIRTH, in a note of his own (*TP*, 1894, 388; cf. also *JRAS*, 1896, 73), mentions Ch'üan-chou with the alternative name *Jui-t'ung* between brackets, without, however, giving any reference. Two years later (*TP*, 1896, 224), PHILLIPS replied to HIRTH's note by saying that, familiar as he was with the main works on Fu-chien, he had never seen the name of *Jui-t'ung* in any of them. I think there is here a misunderstanding first of HIRTH, and later of CHARIGNON. In the *Ta-Ch'ing i-t'ung chih*, the very book where KLAPROTH found his *Tz'ü-t'ung*, the quotation in which *Tz'ü-t'ung* appears goes on by saying that when the *tz'ü-t'ung* give forth first leaves and afterwards flowers, the five cereals grow in plenty; « for that reason these (*tz'ü-t'ung*) are called *jui-t'ung* ('auspicious *t'ung*') ». There seems to be no name here, and the case is probably the same in the modern compilation used by CHARIGNON. Even in the late parallel passage of the local monograph *Chin-chiang-chih* cited by ARNAIZ in *TP*, 1911, 679, I think that *jui-t'ung* is merely another name of the *tz'ü-t'ung*, and not of the city as in ARNAIZ's translation.

As to CHARIGNON's argument that *Tz'ü-t'ung-ch'êng* did not exist, but only *T'ung-ch'êng*,

it is true that the quotation reproduced in the *Ta-Ch'ing i-t'ung chih* only gives T'ung-ch'êng, and it is also only T'ung-ch'êng which is mentioned by the 泉南雜誌 *Ch'üan-nan tsa-chih* of the Ming period (cf. *Ssü-k'u...*, 77, 20 b-21 a), as quoted s. v. «T'ung-ch'êng» in the modern dictionaries *Tz'ü yüan* and *Chung-kuo ku-chin ti-ming ta tz'ü-tien*. But DOUGLAS already spoke of Tz'ü-t'ung-ch'êng, and the *Chung-kuo ku-chin ti-ming ta tz'ü-tien*, pp. 431 and 713, gives Tz'ü-t'ung-ch'êng as an alternative equivalent for T'ung-ch'êng. I can add that the term Tz'ü-t'ung-ch'êng actually goes back to Sung times. In the *Yü-ti chi-shêng*, completed in 1221, Tz'ü-t'ung-ch'êng is the catchword to introduce a poem which speaks only of Tz'ü-t'ung (130, 9a); and another poem, of the second quarter of the 12th cent., speaks of the author's occasional appointment at Tz'ü-t'ung-ch'êng (*ibid.* 130, 16 b). Moreover, similar names are known elsewhere. A cursory glance at the geographical dictionary suffices to reveal a Tz'u-t'ung Barrier (*kuan*) in Yün-nan, a district (*hsien*) of T'ung-ch'êng in An-hui, and a garrison (*chên*) of T'ung-ch'êng in Shan-tung.

But even then, two questions still remain : (a) Is this name of Tz'ü-t'ung[-ch'êng] likely to have been adopted by foreigners as a name of the city? (b) Is «Zäitün» a regular transcription of Tz'ü-t'ung?

The first point is not easy to decide. Ch'üan-chou, as an official name, existed first from ca. 585 to 606, and regularly since 623, but until 711 it was the name of our Fu-chou, and only in that year was the name transferred from Fu-chou to the modern Ch'üan-chou (*Yüan-ho chün-hsien t'u-chih*, 29, 12 a, 15 a). No mention of *tz'ü-t'ung* seems to occur in connection with a town in any T'ang work; but the tradition was firmly established in Sung times. The quotation given in the *Ta-Ch'ing i-t'ung chih*, 328, 5, and partly translated by KLAPROTH is taken from the 方輿勝覽 *Fang-yü shêng-lan*, a geography in 70 ch., completed in 1239 (*Ssü k'u...*, 68, 10 a-11 a). It reads as follows : «As to the walls (*ch'êng*) of the *chou*, Liu Ts'ung-hsiao built them up with double planks; on either side he planted *tz'ü-t'ung* which went all round; it was called the T'ung-ch'êng. If they (*i. e.* the trees) first gave forth leaves, and afterwards flowers, in that year the five cereals would grow in abundance. If not, the contrary would be the case. That is why these [*tz'ü-t'ung*] were called *ju-t'ung* ('auspicious *t'ung*')». Liu Ts'ung-hsiao died in 962 (cf. *Yü-ti chi-shêng*, 130, 3 a); his city wall was built in 943-958 (cf. ECKE and DEMÉVILLE, *The Twin Pagodas of Zayton*, 24). The *tz'ü-t'ung* of Ch'üan-chou must have been famous, since the lexicographical work 爾雅翼 *Êrh-ya i*, completed in 1270 (cf. *Ssü-k'u...*, 40, 18 a-19 b), has the following sentence (quoted in *P'ei-wên yün-fu*, s. v. *tz'ü-t'ung*) : «The *tz'ü-t'ung* grows at Ch'üan-chou; when flowers come first and leaves afterwards, the five cereals ripen» (this saying must be corrupt; it is the opposite of the one which has been quoted above from the *Fang-yü shêng-lan*, and which occurs, in almost the same words, in a poem of c. A. D. 900 quoted in *Yü-ti chi-shêng*, 130, 16 b; cf. also *TP*, 1911, 679). The *Yü-ti chi-shêng* (130, 16 b) mentions the circuit of *tz'ü-t'ung* and the name of T'ung-ch'êng, but not the part played by Liu Ts'ung-hsiao nor the *ju-t'ung* epithet. Moreover, these are not the only names which have been borne by the walls of Ch'üan-chou. The *Ta-Ch'ing i-t'ung chih* quotes the «ancient gazetteer» as saying that, under the Five Dynasties, the walls of the city were called «Gourd walls» (葫蘆城 *Hu-lu-ch'êng*), because the walls were not square; and that after they

had been partly rebuilt, they were called « Carp Walls » 鯉魚城 *Li-yü-ch'êng*). I may add that in the 10th cent. the city had been surrounded with three concentric walls, all of which were already in a state of complete dilapidation in 976. The poem quoted under the catch-word *Tz'ü-t'ung-ch'êng* in the *Yü-ti chi-shêng* of 1221 is a lament on the ruin of the walls. As for the modern walls, *i.e.* those in « carp shape » (cf. ECKE and DEMIÉVILLE, *loc. cit.* 24), they are the ones which were enlarged in 1352 by a high official « Uighur » of the great Hsi family (*Ta-Ch'ing i-t'ung chih*, 328, 5 a; he is the « marquess Hsi » of *TP*, 1912, 451). The result of this inquiry is that the city walls of Ch'üan-chou have had several different names. In Chinese, the same word *ch'êng* designates the walls as well as the city enclosed within them. Such names do not have of course anything to do with the official appellations of administrative centres, but they may very well have been in popular use. One may perhaps feel somewhat disturbed when noticing that the works of the Sung period speak more often of *T'ung-ch'êng* than of *Tz'ü-t'ung-ch'êng*. Moreover, even starting from *Tz'ü-t'ung-ch'êng*, it is somewhat surprising that the word *ch'êng* should have been dropped by foreigners; we should feel safer if we could imagine that, popularly, the Chinese used the term *Tz'ü-t'ung* alone. But these difficulties are not in themselves of enough weight to make us reject the equivalence « *Zäitün* » = *Tz'ü-t'ung* proposed by KLAPROTH, if it be phonetically sound (PHILLIPS's attempt to explain *Zäitün* as a transcription of « *Guet-kong* », the Chang-chou pronunciation of 月港 *Yüeh-chiang* [*JNCB*, 21, 42; *TP*, 1890, 229-231] need not be discussed; moreover, cf. ARNAIZ. in *TP*, 1911, 690-691).

We now come to the second question : Is « *Zäitün* » a normal transcription of *Tz'ü-t'ung*? I leave out of account CORDIER's opinion (*Odoric de Pordenone*, 271) according to which the Arabs had given to Ch'üan-chou the name of *Zäitün*, which in Arabic means « olive » and « olive-tree », because there were at Ch'üan-chou *t'ung*-trees which the Arabs considered to be a sort of olive-tree. The *tz'ü-t'ung* tree has nothing in common with any sort of olive, and *Zäitün* is either a transcription of *tz'ü-t'ung* or has no relation, semantic or other, to it. But is it a good transcription, and, if so, can it be dated?

FERRAND has already tackled the problem, and starting from *tz'ü-t'ung*, he declared « *Zäitün* » to be an erroneous form for « *Zitün* » which he adopted in his *Relations de voyages* (II, 427, 455), and which he regarded as absolutely correct from the point of view of comparative phonetics. I am not so certain that the change is a happy one. If the tradition about Liu Ts'ung-hsiao be true, the name of *T'ung-ch'êng* or *Tz'ü-t'ung-ch'êng* could not have existed for Ch'üan-chou before the 10th cent.; moreover we have no mention of « *Zäitün* » before the 13th. But then all sources, Mussulman as well as European, know only *Zäitün*, « *Zaiton* », never *Zitün*. A text of Abū-'l-Fidā, as quoted by KLAPROTH (*JA*, v [July 1824], 43) and PAUTHIER (*Pa*, 528), says that the name of the place, according to travellers, is to be pronounced exactly like the Arabic word for « olive », *zaitün* (this is not a quotation from the Geography translated by REINAUD and GUYARD). Andrea da Perugia says that the place is called « *Zaiton* » « in Persian » (*Wy*, 374). It may or may not be that the vocalism of the name had been influenced by the Arabic word for « olive », but the fact remains that the name was pronounced *Zäitün* or « *Zaiton* » at the time it was known, and that is therefore the form we must retain.

But even apart from the *-a-*, the transcription is not so satisfactory. Beginning with KLA-

PROTH, and down to PAUTHIER, BLOCHET (*Bl.* II, App. 48) and FERRAND, the term *tz'ü-t'ung* has always been transcribed, although wrongly, as if it were *tzü-t'ung*. Now *tz'ü-t'ung* is *ts'ie-d'ung*; if we had to do with a transcription prior to the 10th century we should expect \**sidun*. As the form must have passed abroad after the 10th century, the normal transcription would have been \**situn*, not \**zītūn* or \**zāitūn* (in the same way as *ts'an* [*< ts'am*] then gave *sam-* in Persian transcription [see « Vonsamcin »], while *tso-*, unaspirated, gave *zo-*, and Man-tzū gave Manzi [see « Mangi »]; *ts-* and *tz-* render the same phonem in our romanization of Chinese). So here again we are confronted with a difficulty, though a minor one, which we can waive if we satisfy ourselves, on historical grounds, that « Zaiton » is Ch'üan-chou.

The two main opponents of the identification of « Zaiton » with Ch'üan-chou have been C. DOUGLAS and PHILLIPS, both of whom have decided in favour of 漳州 Chang-chou, south-west of Amoy. Their chief arguments were that Chang-chou's harbour had a better title than that of Ch'üan-chou to the high praise lavished upon it by Polo and Ibn Baṭṭūṭah; that Chang-chou had been at various times during the Mongol period the capital of the province, alternating in this regard with Ch'üan-chou; that later the Spaniards and Portuguese often spoke of the great port of « Chincheo » which YULE took at first to be Ch'üan-chou, but which is in fact Chang-chou; that « Zaiton » had been an important Christian centre in the Mongol period and Christian remains of mediaeval times have been found at Chang-chou, not at Ch'üan-chou; and finally that Ibn Baṭṭūṭah spoke of the textiles of Zāitūn in terms which could only refer to those of Chang-chou. Although YULE always maintained his opinion in favour of Ch'üan-chou, he was much impressed in his latter years by these arguments. As for CORDIER, he was unhesitatingly for Ch'üan-chou in 1891 in his *Odoric de Pordenone* (p. 281), pronounced in favour of Chang-chou in 1895 (*L'Extrême-Orient dans l'Atlas Catalan*, 33), and wavered between the two in 1903 when re-editing YULE (*Y*, II, 241). ARNAIZ's paper in *TP*, 1911, 678-704, is partly a refutation of PHILLIPS. But that paper, written in Spanish, has not received due attention; CORDIER does not even allude to it in *Y*, III, 100. So I shall examine the question again, sometimes with fresh material, and in greater detail than in KUWABARA's discussion of *Mem. of the Research Department of the Toyo Bunko* in 1928 (II, 30-33).

I agree that the Chang-chou estuary, including the bay of Amoy as an advanced port, constitutes a magnificent harbour, with the proportions of which Ch'üan-chou cannot compete. But we cannot to-day judge of the conditions which obtained several centuries ago, when the process of silting was less advanced at Ch'üan-chou than it is now. The fact remains that Chinese mediaeval sources give a much greater importance to Ch'üan-chou than to Chang-chou. Inspectorates of maritime trade (市舶司 *shih-po-ssü*) existed at the main ports of call of foreign vessels. One was established at Ch'üan-chou in 1087, soon suppressed, but re-established in 1103. Transferred to the more northern port of Fu-chou, it came back to Ch'üan-chou in 1132 (*Yü-ti chih-shêng*, 130, 5a; cf. also *HR*, 20-21), and retained there the official name of « Inspectorate of maritime trade of the *lu* of Fu-chien ». This is the title which Chao Ju-kua had in 1225 when he compiled his *Chu-fan chih* from the foreign merchants trading at Ch'üan-chou (*TP*, 1912, 449). The conditions remained unchanged when the Mongols conquered southern China. Four Inspectorates of foreign trade were created in 1277, one being at Ch'üan-chou, and in 1293 these



had grown to seven (*Yüan tien-chang*, ch. 22; *YS*, 17, 7*b*; 94, 10*b*-11*b*). But there was none at Chang-chou (the text of *YS*, 94, 10*b*, referred to by PHILLIPS in *JNCB*, 21, 42, and in *TP*, 1895, 453, is irrelevant). Moreover, the one at Ch'üan-chou enjoyed until 1293 a privileged position : while the duties levied at the six other ports on imported heavy goods were one-fifteenth of their value, they were only one-thirtieth at Ch'üan-chou (for the general rules, cf. *HR*, 21-22). It looks indeed as though it had been the policy of the Southern Sung to concentrate foreign trade as far as possible at Ch'üan-chou. In 1178, for instance, when an embassy came from San-fo-ch'i (Palembang-Jambi) which the Emperor did not wish to permit to proceed to the Court, orders were issued to receive and treat the envoys at Ch'üan-chou (*Sung shih*, 489, 6*b*). On September 8, 1278, a high-sounding title was bestowed by Qubilai upon the goddess of sailors at Ch'üan-chou (*YS*, 10, 4*a*); more epithets were added in 1288, 1299, 1329, 1354 (cf. WANG Hui-tsu<sup>2</sup>, 2, 5*b*).

These facts would suffice, in my opinion, to establish that the great centre for foreign trade in the Middle Ages was not at Chang-chou, but at Ch'üan-chou, and consequently that Ch'üan-chou is Zäitün. But PHILLIPS's other arguments must also be dealt with.

Rašidu-'d-Din (*Bl*, II, 490) has the following paragraph on Fu-chou and Zäitün in his list of the Chinese provinces (YULE's translation in *Y*, II, 239, and *Y*<sup>1</sup>, III, 126, based on KLAPROTH's, is inaccurate; that of *Oh*, II, 638, is unsatisfactory) : « The seventh *šing* (see 'Scieng') [is that of] the city of Fu-ju, [one] of the cities of Manzī (see 'Mangi'). Formerly the [seat of the] *šing* was there. [Then] it was transferred to Zäitün; but now it has returned there (*i. e.* to Fu-ju). » Even from an incomplete translation, YULE had concluded that there was a striking parallelism between what Rašid said of Fu-ju and Zäitün and what PAUTHIER (*Pa*, 525) had translated from Chinese sources about Fu-chou and Ch'üan-chou. PHILLIPS objected (*JNCB*, XXIII [1889], 24-27, and *TP*, 1890, 234) on the ground that Chang-chou had also been, from time to time, the provincial capital of Fu-chien during the Mongol period, so that it may have been Rašid's Zäitün. ARNAIZ's reply to PHILLIPS (*TP*, 1911, 686) that the compilation from which PHILLIPS quoted must have mixed up the place-name is not pertinent. Of course — and this ARNAIZ did not notice — PHILLIPS is wrong when he makes a work which is supposed to have been written in 1328 quote from the *Yüan shih* of 1369, while its real source is a Ming compilation. But the source of that Ming compilation has been correctly reproduced; it is *YS*, 12, 7*a*, which says : « In the 20th *chih-yüan* year, . . . the third moon,] . . . on [the day] *jên-wu* (April 26, 1283), the *hsüan-wei-ssü* of the circuit (*tao*) of Fu-chien was suppressed; the 'moving Grand Secretariat' (*hsing chung-shu-shêng*) was re-established (復立 *fu li*) at Chang-chou. » It must not be forgotten that the «moving Grand Secretariats», *hsing chung-shu-shêng*, or in an abbreviated form *hsing-shêng*, of the Yüan period have given their name to the *shêng*, or «provinces», of later times, and can almost be equated to the «provinces» themselves (see «Scieng»). Surprising as the text of the *YS* may appear, it seems at first sight difficult to think here of a corrupt reading. Not only does the same text occur in the *Yüan shih lei-pien*, which may merely copy the *YS*, but PHILLIPS cites another text, the real origin of which I have unfortunately not been able to determine, according to which the «moving *shêng* of Chang-chou» must have been in existence already in 1280; that would explain how, after a temporary suppression, it came to be «re-established» in 1283. Still on this point I have certain doubts which will be expressed hereafter. At any rate, such a

« moving *shêng* » cannot have lasted much after 1283, since it has left no trace in the later administrative geography of the Mongol period, as described in YS, 62, 9 a. Perhaps the *hsing-shêng* of Chang-chou, if it existed, was never more than a *fên* [分] *hsing-shêng*, a « detached moving Secretariat », as it seems to have been again at the time of the rebellions which finally brought the dynasty to an end (cf. PHILLIPS, *ibid.* 26).

But it was Ch'üan-chou, and Ch'üan-chou alone, which, in the first years following the conquest of southern China by the Mongols, alternated with Fu-chou as the seat of the real « moving Grand Secretariat » of Fu-chien. The connection between the two cities had always been very close. As I have shown above, the very name of Ch'üan-chou was at first an official designation of Fu-chou, that is before it was transferred in 711 to the modern Ch'üan-chou. When the Mongols crossed to south of the Yang-tzŭ, they resorted, for the administration of the newly acquired provinces, to many temporary measures, some of which have left but insufficient traces in our sources; the geographical section of the YS is explicit only in regard to the organization that obtained half a century later. It summarizes the successive changes (though not always correctly) as follows (YS, 62, 8 a) :

« [Moving Grand Secretariat of Chiang-chê and other places... (= of Hang-chou, of which Fu-chien formed part, after Qubilai)] :

« *Lu* of Fu-chou... Under the Sung, it was the *lu* of Fu-chien. In the 15th *chih-yüan* year (1278) of the Yüan, it became the *lu* of Fu-chou. In the 18th year (1281), [the seat of] the moving Secretariat of Ch'üan-chou was transferred to this *chou* (*i. e.* to Fu-chou). In the 19th year (1282), it again returned to Ch'üan-chou. In the 20th year (1283), it was transferred once more to this *chou*. In the 22nd year (1285), it was [suppressed and ] merged with Hang-chou. »

« *Lu* of Ch'üan-chou... In the 14th *chih-yüan* year (1277) of the Yüan, [the Yüan] established [there] a moving *hsüan-wei-ssü* which attended at the same time to the affairs of the moving 'generalissimo's office for repression in the south' (*chêng-nan yüan-shuai fu*). In the 15th *chih-yüan* year (1278), the *hsüan-wei-ssü* became a moving Grand Secretariat (*hsing chung-shu-shêng*), and [Ch'üan-chou] was promoted to a General administrative office of the *lu* of Ch'üan-chou (*Ch'üan-chou tsung-kuan-fu*). In the 18th year (1281), the moving *shêng* was transferred to the [seat of the] *lu* of Fu-chou (*i. e.* to Fu-chou). In the 19th year (1282), it again returned to Ch'üan-chou. In the 20th year (1283), it was transferred once more to the [seat of the] *lu* of Fu-chou.

There can be no doubt that, in agreement with YULE's conclusions, the alternations between Ch'üan-chou and Fu-chou in the texts of the geographical section of the YS correspond to Rašidu-'d-Dīn's indications as to the alternations between Zäitün and Fu-ju, and this again is decisive for the identification of Zäitün with Ch'üan-chou. But the history of the « moving *shêng* », as summed up in that section of the YS, is only a rough approximation; the real facts, which we are not always in a position to retrace in full detail, are much more intricate, as may be seen from the following texts in the *pên-chi* :

1. (YS, 10, 1 b) : « In the 15th *chih-yüan* year, ... the third month, ... on [the day] *i-yu* (March 26, 1278), an Imperial edict [prescribed] that Mêng-ku-tai (\*Monyutai?, \*Monyoltai?;

cf. *YS*, 131, 3-4; see 'Mongatai'), So-tu (see 'Sogatu') and P'u Shou-kêng (cf. KUWABARA, *P'u Shou-kêng*, in *Mem. of the Research Department of the Toyo Bunko*, II [1928], 1-79, and VII [1935], 1-104) should be in charge of the affairs of the 'moving Grand Secretariat' at Fu-chou and govern the regions (*chün*) along the sea[-coast].» A promotion in the «moving Grand Secretariat» was given to So-tu and to P'u Shou-kêng on September 18, 1278 (*YS*, 10, 4*b*), but the text does not say where (cf. WANG Hui-tsu<sup>2</sup>, 2, 6*a*).

2. (*YS*, 10, 3*a*) : [In the 15th *chih-yüan* year,] . . . the sixth month . . . , on [the day] *chia-hsü* (July 13, 1278), an Imperial edict [prescribed] the reduction in the excessive [number of] officials south of the [Yang-tzü]-chiang. South of the [Yang-tzü]-chiang, the Yüan had established the four [moving] *shêng* of Huai-tung, Hu-nan, 隆興 Lung-hsing and Fu-chien. Lung-hsing was [suppressed and] merged in Fu-chien . . . » Lung-hsing is the modern Nan-ch'ang in Chiang-hsi, the 洪州 Hung-chou of the T'ang, which had been promoted to Lung-hsing-fu by the Sung and became the seat of a «moving Grand Secretariat» in 1275. In 1278, the seat of the «moving Grand Secretariat» was transferred to Kan-chou (= Kan-chou-fu, also in Chiang-hsi), but came back to Lung-hsing in 1279. In 1280, it was suppressed and merged into Fu-chien, but was re-established in 1282. In 1284, the name was changed to 龍興 Lung-hsing. This is the «moving Grand Secretariat of Chiang-hsi and other places» of the geographical section of *YS*, 62, 9*b*. Waššāf mentions the *šing* of لُنْكِيْن Lunkin-fü (so written in the ms. quoted in *Bl*, II, 490; altered in HAMMER's text, *Ha*<sup>2</sup>, 43; cf. above), and the name is written لُونْكِيْن Lū-kin-fü in Rašidu-'d-Din (*Bl*, II, 491), probably a misreading for لُونْكِيْن Lūnkīn-fü. YULE (*Y*<sup>1</sup>, III, 126, 130) had thought of Ssü-ch'uan or of Kuang-hsi, and BLOCHET felt sure that Ssü-ch'uan was meant. But it is evident that Lūnkīn-fü is Lung-hsing-fu, the modern Nan-ch'ang; BLOCHET is mistaken (*Bl*, II, 493) when he tries to identify with Lung-hsing-fu another of the provinces mentioned by Rašid. Lūnkīn-fü must of course not be confused with the لُونْقِيْن Lūqīn which is mentioned in the 10th cent. as the country whose people had recently invaded Champa and which was said to be at four days' distance from Hānfü (= Canton) by boat and twenty days by land (*Fe*, 30, 123); Lūqīn should most probably be corrected to لُونْفِيْن Lūfīn and identified with 龍編 Lung-pien, the modern Hanoi.

3. (*YS*, 10, 3*b*) : « [In the 15th *chih-yüan* year,] . . . the seventh month, . . . on [the day] *ping-shên* (August 4, 1278), the *yu-ch'êng* T'a-ch'u (cf. *YS*, 133, 1*a-b*), [the *tso-ch'êng*] Lū Shih-k'uei and the *ts'an-chih-chêng-shih* Chia Chü-chêng were put in charge of the affairs of the 'moving Grand Secretariat' at Kan (= Kan-chou-fu in Chiang-hsi). The [administrations of] Fu-chien, Chiang-hsi, and Kuang-tung were all under their dependence. » Cf. WANG Hui-tsu, II, 5.

4. (*YS*, 10, 6*b*) : « In the 16th *chih-yüan* year, . . . the second month, . . . on [the day] *chia-shên* (March 20, 1279), . . . order was given to the four [moving] Secretariats of Yang-chou, Hu-nan, Kan-chou and Ch'üan-chou to build 600 war vessels. » As WANG Hui-tsu remarks (WANG Hui-tsu, II, 7*a*), « Kan-chou » must here be an error for Lung-hsing, because the seat of the «moving Grand Secretariat» had been ordered back from Kan-chou to Lung-hsing on February 18, 1279 (*YS*, 10, 6*a*).

5. (*YS*, 11, 1*a*) : « In the 17th *chih-yüan* year, the first month, . . . on [the day] *chia-tzü* (February 23, 1280), orders were given to the 'moving Secretariat' of Ch'üan-chou to lead troops

to reduce those prefectures and districts and mountain strongholds within its jurisdiction which had not yet submitted, and to massacre those who had submitted and rebelled again.»

6. (YS, 11, 1 a) : « [In the 17th *chih-yüan* year, . . . the first month,] . . . on [the day] *wu-ch'ên* (February 27, 1280), a 'moving Grand Secretariat' was established at Fu-chou.»

7. (YS, 11, 2 a) : « In the 17th *chih-yüan* year, . . . the fourth month, . . . on [the day] *ping-shên* (May 25, 1280), . . . as it was not expedient to have the three [moving] Secretariats of Lung-hsing, Ch'üan-chou and Fu-chien, order was given to the Court officials to deliberate in common [on the subject] and to make a report.»

8. (YS, 11, 2 a) : « [In the 17th *chih-yüan* year,] . . . the fifth month, . . . on [the day] *kuei-ch'ou* (June 11, 1280), the [seat of the] 'moving Secretariat' of Fu-chien was transferred to Ch'üan-chou.»

9. (YS, 11, 2 b) : « [In the 17th *chih-yüan* year,] . . . the seventh month, . . . on [the day] *chi-yu* (August 6, 1280), the 'moving Secretariat' of Ch'üan-chou was transferred to Lung-hsing.»

10. (YS, 11, 5 a) : Mention is made of a report sent in by « the *tso-ch'êng* of the ' [moving] Secretariat' of Fu-chien, P'u Shou-kêng ».

11. (YS, 12, 4 a) : « In the 19th *chih-yüan* year, . . . the ninth month, . . . on [the day] *jên-shên* (October 18, 1282), an order was issued that P'ing-luan (in Ho-pei), Kao-li (Corea), Tan-lo (Quelpaert), as well as Yang-chou (see 'Yangiu'), Lung-hsing and Ch'üan-chou should build together 3000 vessels, big and small.» The last three names are clearly those of « moving Secretariats ».

12. (YS, 12, 6 b) : « In the 20th *chih-yüan* year, . . . the third month, . . . on [the day] *ting-ssü* (April 1, 1283), the General administrative office of the Inspectorate of foreign trade (*shih-po tsung-kuan-fu*) of Fu-chien was abolished, but the *t'i-chü-ssü* was retained. The 'moving Secretariat' of Ch'üan-chou was [suppressed and] merged in the 'moving Secretariat' of Fu-chien.» This shows that, contrary to the notices of the geographical section, a « moving Secretariat » of Ch'üan-chou and a « moving Secretariat » of Fu-chien had co-existed.

13. (YS, 12, 7 a) : « [In the 20th *chih-yüan* year, . . . the third month,] . . . on [the day] *jên-wu* (April 26, 1283), the *hsüan-wei-ssü* of the circuit (*tao*) of Fu-chien was suppressed; the 'moving Grand Secretariat' was re-established at Chang-chou.» This is the text which was quoted, but at second-hand, by PHILLIPS.

14. (YS, 13, 1 b) : « In the 21st *chih-yüan* year, . . . the second month, . . . on [the day] *hsin-ssü* (February 19, 1284), the *hsüan-wei-shih* of Fu-chien, Kuan Ju-tê, was made *ts'an-chih-chêng-shih* of the 'moving Secretariat' of Ch'üan-chou.»

15. (YS, 13, 3 b-4 a) : « [In the 21st *chih-yüan* year,] . . . the ninth month, . . . on [the day] *chia-shên* (October 19, 1284), . . . the Grand Secretariat said : 'The food supplies for the army are extremely scarce in the « moving Secretariat » of Fu-chien, and have to be transported from [that of] Yang-chou, which entails many delays and errors; it would be more convenient to merge the two [moving] Secretariats into one, and to order detailed Secretariat officials to reside at Ch'üan-chou.' An Imperial edict [prescribed] that the *chung-shu yu-ch'êng* [in charge of] the affairs of the 'moving Secretariat' Mang-wu-t'ai (the same as the Mêng-ku-tai of text No. 1) should be *p'ing-chang chêng-shih* (see 'Bailo') of the 'moving Grand Secretariat' of Chiang-huai and

other places, and that the *tso-ch'êng* of his ' [moving] Secretariat ' Hu-la-ch'u (\*Qulaču ?; cf. YS, 133, 3 b-4 a) and P'u Shou-kêng and the *ts'an-chêng* Kuan Ju-tê should have a ' detailed [moving] Secretariat ' (*fên-shêng*) at Ch'üan-chou. » Chiang-huai was the name of the « moving Grand Secretariat » established at Yang-chou, which was transferred to Hang-chou in 1284, and then named the *hsing-shêng* of Chiang-chê. It went back to Yang-chou in 1286, and was again called the *hsing-shêng* of Chiang-huai in 1287. In 1289, it was moved for the second time to Hang-chou (YS, 15, 7 a), and in 1291 received again the name of *hsing-shêng* of Chiang-chê which it retained till the end of the dynasty (see also « Yangiu » and « Quinsai »); cf. WANG Hui-tsu<sup>2</sup>, 26, 5 b.

16. (YS, 13, 5 b) : « The 22nd *chih-yüan* year, the first month, . . . on [the day] *i-wei* (February 27, 1285), Lu Shih-yung asked to abolish the ' moving Grand Secretariat ' of Fu-chien and to establish a *hsüan-wei-ssü* which would be in the dependence of the ' moving Grand Secretariat ' of Chiang-hsi. » According to the geographical section translated above, the « moving Grand Secretariat » of Fu-chou was suppressed in 1285, and merged with Hang-chou; WANG Hui-tsu<sup>2</sup>, 26, 7 a, is of opinion that Hang-chou is an error for Chiang-hsi.

17. (YS, 15, 6 b and 7 a) : The « moving [Grand] Secretariat » of Fu-chien is mentioned twice, for the dates of February 11, and March 6, 1289. Consequently, it must have been re-established between 1285 and 1289; but the various sections of the YS say nothing of it (cf. WANG Hui-tsu<sup>2</sup>, 26, 7 a). According to a quotation from the 三山續志 *San-shan hsü-chih* of 1328 in ch. I of the *Pa-Min t'ung-chih*, the « moving Grand Secretariat » of Fu-chien was re-established in 1286 (cf. KUWABARA, in *Mem. of the . . . Toyo Bunko*, VII [1935], 92).

18. (YS, 16, 6 a) : « In the 28th *chih-yüan* year, the second month, . . . on [the day] *kuei-yu* (March 6, 1291), the ' moving (Grand) Secretariat ' of Fu-chien was changed to a *hsüan-wei-ssü*, in the dependence of Chiang-hsi. » This is confirmed by the *San-shan hsü-chih* (cf. KUWABARA, *ibid.*).

19. (YS, 17, 2 a) : « In the 29th *chih-yüan* year, . . . the second month, . . . on [the day] *i-hai* (March 2, 1292), . . . the *ch'üan-fu t'ai-ching* I-hei-mi-shih (Yiyimš), the myriarch of the ancient army of Têng-chou, Shih Pi (cf. YS, 162, 4 b-6 a), and the *yu-ch'êng* of the ' moving [Grand] Secretariat ' of Fu-chien, Kao Hsing (cf. YS, 162, 6 a-7 b), were all named *p'ing-chang chêng-shih* of the ' moving Grand Secretariat ' of Fu-chien, [and ordered] to lead troops to punish Chao-wa (Java) . . . » Kao Hsing's biography confirms that the « moving Grand Secretariat » of Fu-chien was re-established in 1292. It was an emergency measure, explained by the fact that the Chinese armada sent against Java started from Fu-chien.

20. (YS, 19, 4 b) : « In the 1st *ta-tê* year, . . . the second month, . . . on [the day] *chi-wei* (March 20, 1297), the ' [moving Grand] Secretariat ' of Fu-chien was changed to the ' moving Grand Secretariat ' of Fu-chien, P'ing-hai and other places, and its seat was transferred to Ch'üan-chou. The *p'ing-chang chêng-shih* Kao Hsing said that Ch'üan-chou was not far from Liu-ch'ü, and that either by summoning [the Liu-ch'ü people], or by going [to them], it was easy [from there] to win their affection. That is why the seat was changed. »

21. (YS, 20, 1 b) : « In the 3rd *ta-tê* year, . . . the second month, . . . on [the day] *ting-ssü* (March 8, 1299), . . . the ' moving Grand Secretariats ' of Ssü-ch'uan and of Fu-chien were abolished . . . » From the biography of K'o-li-chi-ssü (Giwargis, George) in YS, 134, 9 a, it appears that Fu-chien was at that time made a dependency of Chiang-chê, that is of Hang-chou

(cf. also WANG Hui-tsu<sup>2</sup>, 26, 7 a). The same state of affairs must have prevailed c. 1330, i. e. at the time when the materials later used by the geographical section of the YS were compiled.

22. (YS, 44, 4 b) : « In the 16th *chih-chêng* year, in the spring, the first month, on [the day] *jên-wu* (February 2, 1356), the Generalissimo's office (*tu yüan-shuai fu*) of the *hsüan-wei shih-ssü* of Fu-chien was changed to a 'moving Grand Secretariat' of Fu-chien. It is probable that the «moving Grand Secretariat» of Fu-chien re-established in 1356 had its seat at Ch'üan-chou as in 1297-1299. In such a case, the «detailed moving Secretariat» mentioned by PHILLIPS for 1358, but without any reference, would be the same «moving Grand Secretariat» of Fu-chien. But I cannot prove it.

Nor can I find any trace of the «detailed moving Secretariat» which PHILLIPS mentions as having been at Chang-chou in 1363 (read «1362»). In the *pên-chi*, s. a. 1362, I only find (YS, 46, 2 b) that on May 24, 1362, «Sai-fu-ting (Saifu-'d-Dîn) of Ch'üan-chou took possession of the *lu* of Fu-chou. The *p'ing-chang chêng-shih* of the 'moving Secretariat' of Fu-chien, Yen-chih-pu-hua (\*Älji-buqa = Älbi-buya) attacked and defeated him. The remnant of the bands [of Saifu-'d-Dîn] took to sea, and went back to their possession of Ch'üan-chou. The *ts'an-chih-chêng-shih* of the 'moving Secretariat' of Fu-chien, Ch'ên Yu-ting, recovered the *lu* of Fu-chou.» Saifu-'d-Dîn had rebelled and taken possession of Ch'üan-chou in 1357 (YS, 45, 1 a). Perhaps it was on account of the rebellion of Saifu-'d-Dîn that the «moving Grand Secretariat of Fu-chien», expelled from Ch'üan-chou, had become a «detailed moving Grand Secretariat» at Chang-chou.

Although sometimes deficient in minor particulars, our information is comprehensive enough to make the main facts quite certain. For a score of texts referring to the change of the «moving Grand Secretariat» at Fu-chou and at Ch'üan-chou, there is in the whole of the YS only one mention of a «moving Grand Secretariat» at Chang-chou. That text (No. 13) speaks of a «moving Secretariat» re-established at Chang-chou on April 26, 1283, while there is in the *pên-chi* no earlier mention of its existence or of its suppression. On the other hand, our text No. 12 says that the «moving Secretariat» of Ch'üan-chou was abolished on April 1, 1283, and we see that same «moving Secretariat» of Ch'üan-chou existing again in February 1284 (text No. 14), without any former mention of its re-establishment. If we had only the YS, it would have been very tempting to think that the text referring to April 26, 1283, is corrupt and that what was then re-established was the «moving Secretariat» of Ch'üan-chou, not of Chang-chou. But there is, besides, the text of enigmatic origin according to which the «moving Secretariat» of Chang-chou must have existed in 1280; and also the «detailed moving Grand Secretariat» of Chang-chou mentioned by PHILLIPS for 1362. WANG Hui-tsu<sup>2</sup>, 26, 7 b, accepts the text of April 26, 1283, on Chang-chou, without having any other source to corroborate it. I do the same, though with considerable hesitation. There are many other cases where the items of information of the *pên-chi* are incomplete. Moreover, the «moving Grand Secretariats», although they became in time almost equivalent to provinces, were originally emergency institutions, established on certain occasions, and suppressed when no longer necessary. At the time of the conquest of South China, there may have been a «moving Grand Secretariat» at Chang-chou, the existence of which was short-lived. More puzzling is the «detailed moving Grand Secretariat» of Chang-chou in 1362; but, with the rebellions which preceded the fall of the dynasty, emergency measures

similar to those of the time of the conquest may have been taken. At any rate, this «moving Grand Secretariat» of Chang-chou could only have been something created *ad hoc* and of temporary nature, not to be placed on a par with the one which was at times at Fu-chou, at others at Ch'üan-chou, and which is certainly the *king* of the Fūjū and Zāitūn spoken of by Rašidu-'d-Dīn.

PHILLIPS's next argument in favour of Chang-chou was the frequent mention of «Chincheo» in early Spanish and Portuguese accounts; at first YULE had taken «Chincheo» to be Ch'üan-chou, while it is in fact Chang-chou. But this has no great bearing on the Zāitūn question, since the conditions prevailing at the time of the Spanish and Portuguese travellers of the 16th and 17th centuries were probably quite different from those which obtained in the 13th and 14th. Moreover, while I readily admit what YULE has himself acknowledged, that he has confused the two places in regard to «Chincheo», I am far from being sure that all Spanish and Portuguese authors gave the same meaning to «Chincheo». Some at least of the references to «Chincheo» point in fact to Ch'üan-chou (cf. also YULE, *Hobson-Jobson*<sup>2</sup>, 200), but a discussion of the passages would require much space, without throwing any light on Zāitūn.

Another of PHILLIPS's arguments, that Christian remains had been found at Chang-chou, not at Ch'üan-chou, becomes a boomerang against his own hypothesis. MARTINI's «Chang-chou» Bible has a good chance of being the same Bible as was later obtained at Ch'ang-chou in Chiang-su by COUPLET (cf. *Mo*, 85-86). The «Chang-chou» stone crosses of faulty Dominican reports of the 17th cent. have turned out to be Ch'üan-chou stone crosses for which we possess details of place and date in Jesuit engravings of the time (cf. ARNAIZ, in *TP*, 1911, 687-688; *Mo*, 78-83). These crosses have disappeared, but a new one was discovered in 1906, and it was again at Ch'üan-chou; there is also at Ch'üan-chou the tomb of the Persian Christian Ḥudādār or Ḥudādād (cf. *TP*, 1911, 727; 1914, 644; *Mo*, 80-81; ECKE and DEMIÉVILLE, *The Twin Pagodas*, 22, and Pl. 70 b). Moreover, the Christians were only a minority among the foreigners of Zāitūn; the Mussulman community was of far greater importance. Now, Ch'üan-chou possesses the most ancient and the best-built mosque of China, with important inscriptions and with a cemetery (cf. ARNAIZ and VAN BERCHEM in *TP*, 1911, 677-727). When in 1217 a Buddhist priest brought back to his monastery in Japan a specimen of Arabic writing, he had had it written by a Mussulman merchant at Ch'üan-chou (*JA*, 1913, II, 181-185). Sculptural remains of approximately the same date testify to the presence also of a South-Indian colony (cf. *Ostasiat-Zeitschr.* 1933, 5-11; NILAKANTA SASTRI, *The Cōlas*, II, I, 440; ECKE and DEMIÉVILLE, *The Twin Pagodas*, p. 22 et Pl. 69).

The mass of arguments in favour of Ch'üan-chou and against Chang-chou is so overwhelming that a discussion of Polo's statement concerning the porcelain produced at «Tingiu», a city of the same «province» as «Zaiton», or of Ibn Baṭṭūṭah's notices on the textiles of Zāitūn cannot change our conclusions. The question of «Tingiu» will be discussed in another note (see «Tingiu»). I shall only say here a few words on the textiles, not to establish the identification of Zāitūn, but because the subject is of some interest in itself.

Ibn Baṭṭūṭah speaks of the velvet damasks (*kimḥā*; see under «Camut») and of the satin (*atlas*) made at Zāitūn and known as *zāitūniyyah*, «Zaitunese» (transl. DEFRÉMERY and SANGUINETTI, IV, 269; the translation of *kimḥā* as «velvet» is doubtful; cf. *Y*<sup>1</sup>, IV, 118). Elsewhere he

mentions 500 pieces of *kimḥā*, of the sort manufactured at Zāitūn, which had been sent to the sultan of Delhi by the Chinese Emperor (iv, 1). In 1441, 'Abdu-'r-Razzāq saw the king of Bisnagar dressed in «satin of Zāitūn» (*aṭlas-i zāitūni*), his feet resting on a cushion of the same material (cf. *Y*, II, 241-242; HEYD, *Hist. du commerce*, II, 701-702; YULE, *Hobson-Jobson*<sup>2</sup>, 797). Both YULE and HEYD were convinced that our word «satin» was derived from *zāitūni*. This is accepted by BLOCH, *Dict. étymol.* II, 255, for the French word (but «*zatoui*» must be a misreading for «*zaton*», and «Tsia-Toung» is a monstrous perversion of the modern pronunciation of Ch'üan-chou). Although SKEAT and HATZFELD upheld the etymology of Engl. and French «satin» with Lat. *seta*, I do not doubt that YULE and HEYD were right. Forms like *aceytuni* in a Spanish document, *zatomy* in a French inventory of 1352, *setuni* in Clavijo, etc., leave almost no room for doubt. But it may be that the final form of Ital. *setino*, Engl. and French *satin* is partly due to a contamination of Lat. *seta*. The German word for satin, Atlas, is of course the Arabic word *aṭlas*, the literal meaning of which is «close-shaven», Ital. *raso* (on *aṭlas*, cf. the long and important note of QUATREMÈRE, *Hist. des Sultans Mamlouks*, II, I, 69-70); *zetani raso* occurs in an Italian text of the 15th cent. Already in Clavijo, *setuni* is not solely used with its original meaning of «satin of Zāitūn», but is also applied to the satin made in Persia.

PHILLIPS tried to establish that the manufacture of silk was much more developed in Chang-chou than in Ch'üan-chou (*JNCB*, XXIII [1889], 28-30; *TP*, 1895, 451-452). It seems to be a fact that at the end of the 19th cent., silk-weaving was thriving in Chang-chou, but perhaps owing to the accidental enthusiasm of an official (cf. ARNAIZ, in *TP*, 1911, 689). In point of fact, the mulberry-tree grows very well in both districts, though no better than it does farther to the north. In the first half of the 8th cent., silk taffeta was sent as «tribute» to the Court by Fu-chou and Chien-chou, but not by Ch'üan-chou or Chang-chou (cf. *Yüan-ho chün-hsien t'u-chih*, 29, 12 a, 14 a, 15 a, 16 a). Moreover, foreigners were not likely to burden themselves with excessive nomenclature. Satin manufactured at Ch'üan-chou, at Chang-chou, and also in other parts of Fu-chien would go by the name of «Zaitunese», because they were sent abroad from Zāitūn. As a matter of fact, I have a suspicion that much of the «Zaitunese» silk material was brocade manufactured at Chien-ning (see «Camut» and «Quenlinfu»).

The name Zāitūn disappears at the dawn of modern times. How it ever found its way at the end of the 16th cent. into BOTERO's *Relazione universale*, to be located between Canton and «Liampo» (= Ningpo), is as mysterious to-day as when YULE wrote his note in *Y*, II, 239. In the *Āin-i Akbarī* of 1595, the mention of Zāitūn is merely literary and artificial. There was current in the 16th cent., however, a term which recalled the greater days of Zāitūn's past: the first period of the south-western monsoon was then known as *mavsin-i Zāitūni*, «the Zāitūn monsoon» (cf. *Fe*, 486).

Before leaving the question of the name «Zaiton», I wish to add something on another name given to the place by Abū-'l-Fidā († 1331). Abū-'l-Fidā said that, according to a traveller who had recently come from China, خانقو Ḥānqū (to be read خانقو Ḥānfū) was then called خانسآ Hansā, and شنجو Šānjū Zāitūn (transl. REINAUD and GUYARD, II, II, 122, 123). In a note, GUYARD remarks that Abū-'l-Fidā mentions two «Ḥānqū», but that in both cases it must be Hansā, Polo's «Quinsai». As to Šānjū (which GUYARD read Šīnjū), YULE (*Y*, II, 237), HIRTH



and ROCKHILL (*HR*, 18), and FERRAND (*Fe*, 427) are all agreed that it is a transcription of Ch'üan-chou, and FERRAND insists that its identification was made by Abū-'l-Fidā long before European scholars (PHILLIPS's «Scheikham» [*JNCB*, XXI, 42], or «Scheikhoun» [*TP*, 1890, 231], taken from d'HERBELOT, can be nothing but a misreading of Šānjū). I see things quite differently. What Abū-'l-Fidā says is that he cannot ascertain the true forms of names given to Chinese towns, rivers, etc., in ancient Western works, and therefore omits them. But for the identification of two of these names he thinks he has good authority to fall back on. So the two names must have been known in early Arabic geographical lore. One is obvious, Hānfū. But the Hānfū of the 9th cent. is not Hānsā, Hang-chou, but Canton (see «Quinsai»). I am afraid that the informant who blundered over Hānfū did the same with Šānjū. Only one Šānjū occurs in the ancient Mussulman works which treat of China, and this was Shan-chou in the region of Hsi-ning, in Western China (see «Silingiu» and «Saciou»). It seems probable that Abū-'l-Fidā inquired from the traveller about Šānjū, an ancient name of the true value of which the man knew no more than he did of Hānfū. But, for a Mussulman traveller of the 14th cent. who had gone to China by sea, the two important places were certainly Hang-chou (Hānsā) and Ch'üan-chou (Zāitūn). Hānfū (perhaps already altered to Hānqū in Abū-'l-Fidā's source) became Hānsā, and Šānjū was given as a former name of Zāitūn. Perhaps, the consonance between Šānjū and Ch'üan-chou (Ts'yüan-čeu) helped the mistaken identification, if the Arabic traveller ever heard and noticed the Chinese name; and with this possible explanation I am afraid I concede too much already.

Ibn Baṭṭūṭah (IV, 269) says that at Zāitūn the Mussulmans had a city by themselves. Whatever the facts may be about the Moor's very «Pintoan» account of China, he almost spoke the truth in the present case, inasmuch as most foreigners, including the Mussulmans, lived in the southern suburb of Ch'üan-chou, in a part called 泉南 Ch'üan-nan, «South of Ch'üan[-chou]» (cf. HIRTH, in *JRAS*, 1896, 75; ECKE and DEMIÉVILLE, *The Twin Pagodas*, 4). In his *Chu-fan chih* of 1225, Chao Ju-kua speaks of the Arab who established a cemetery for foreigners outside the south-eastern corner of the city; this Arab himself lived at Ch'üan-nan (*HR*, 119). In another section of his book, Chao Ju-kua mentions two men from Nan-p'i (= Namburi, Brahmans of Malabar), father and son, who had settled at «South-of-the-wall of Ch'üan[-chou]» (泉之城南); the term certainly refers to Ch'üan-nan (*HR*, 88). The name of Ch'üan-nan continued to be used. I have quoted above, unfortunately at second hand, a sentence from a Ming work entitled *Ch'üan-nan tsa-chih*, «Miscellaneous memoirs of Ch'üan-nan».

In speaking of non-Chinese tribes like the «Çardandan» in south-western Yün-nan and the people of «Caugigu» (Upper Tonking), Polo describes the practice of tattooing, but it is only in the chapter on «Çaiton» that he mentions it as in use in China proper; people came from «Upper India» to have their bodies adorned by the local artists! I have no other information on tattooing at Ch'üan-chou. But in the *Yüan tien chang*, 41, 24 a, there is mentioned the curious case, in 1309, of a man, at Hang-chou, who had forcibly tattooed the back and thighs of his wife with dragons and devils in blue, and displayed her naked in the streets; as there was the aggravating circumstance that he had beaten his mother-in-law, he was sentenced to 87 blows with the big bamboo and his wife was sent back to her own kin.

Polo speaks of the «five very beautiful bridges of Çaiton», the largest being «quite three miles in length». As the passage occurs only in Z, it has naturally not been commented on by YULE; but there is no remark made about it in RICCI and ROSS, PENZER or BENEDETTO. I am not in a position to identify all the bridges. But one of them must be the 順濟橋 Shun-chi-ch'iao, which spans the Chin-chiang south of the walls of Ch'üan-chou and on the highway to Chang-chou; it was built in 1211 (cf. *Ta-Ch'ing i-t'ung chih*, 328, 6 a; the form Shun-chih[治]-ch'iao used by PHILLIPS in *TP*, 1894, 7, is erroneous, although it has been copied by ECKE in *Sinica*, vi, 296; the correct form is given in ECKE and DEMIÉVILLE, *The Twin Pagodas*, 4 and Pl. 6 a). As for the bridge of «quite three miles in length», it is certainly the famous 洛陽橋 Lo-yang-ch'iao or 萬安橋 Wan-an-ch'iao, built over the Lo-yang River at the northern end of the bay of Ch'üan-chou, on the highway to Fu-chou, and certainly one of the most striking achievements of early Chinese engineering. The construction of the bridge took six years, from 1053 to 1069, and was due to the initiative of the well-known author and statesman Ts'ai Hsiang (1012-1067; GILES, *Biogr. Dict.* No. 1974), himself a native of the district of Hsien-yu, to the north-east of Ch'üan-chou. It is difficult to estimate the real length of Polo's «three miles», but Chinese sources give to the bridge a length of 3600 feet, which amounts to five-sevenths of a mile. PHILLIPS (*TP*, 1894, 7) said that the bridge had been described by the Augustine Fathers who visited Fu-chien in 1575 (not «1577»). But this would require the tacit assumption that «Megoa» in MENDOÇA's book means Ch'üan-chou, while it seems more likely to be the Hsing-hua suggested in a note by the editor of MENDOÇA in the Hakluyt Society publications (II, 75). The first certain description I know of, and a very enthusiastic one it is, occurs in MARTINI's *Novus Atlas Sinensis* of 1655. MARTINI had crossed the bridge on two different occasions. He does not fail to note that the pillars of the bridge are bevelled both up and down stream, describing them in almost the same terms as in Polo's then unknown passage. Further details and views of the Lo-yang Bridge are given by ECKE, in *Sinica*, vi, 271-272, 296, and Pl. 22, 4; 23, 9 and 11; and in ECKE and DEMIÉVILLE, *The Twin Pagodas*, p. 4 and Pl. 71 a.

## 196. ÇANGHIBAR

<i>canghibar</i> F, Fr	<i>çangibar</i> L	<i>zanchibar</i> TA <sup>3</sup> , VA
<i>canzibar, zamzibar</i> P	<i>ganghibar</i> Ft	<i>zançiber</i> LT
<i>cvngibar, zungibar</i> V	<i>gangibar</i> VB	<i>zangibar</i> L, VA
<i>çacchibar, zaçecchibar, zachibar</i>	<i>tanguibar</i> VL	<i>zanguibar</i> FAt
TA <sup>1</sup>	<i>zacchibar, zeccibar</i> TA <sup>3</sup>	<i>zanquibar</i> FA, FB
<i>çachibar</i> TA <sup>1r</sup> , TA <sup>3</sup>	<i>zaghybar, zanghibar, zanghy-</i>	<i>zanziber</i> LTr
<i>çanchibar</i> F	<i>bar</i> Z	<i>zarachnar</i> G
<i>çanghibar</i> F, Z	<i>zamquibar</i> FA	<i>zenzibar</i> R

The readings of Z and of the French mss. leave no doubt that the intended pronunciation is «Zangibar», and not the «Zanjibar» which V and L would seem to suggest. RAMUSIO's «Zenzibar» is a comparatively late form, also used by ORTELIUS, and very near Barbosa's «Zinzibar»

(DAMES, I, 26). The Catalan Map omits the whole eastern coast of Africa south of Cape Gardafui. Fra Mauro gives «Châcibar» as an island, and «Xêgibar» on the continent. Cf. Y, II, 424; III, 123-124; YULE, *Hobson-Jobson*<sup>2</sup>, 978; DEVIC, *Le pays des Zendjs*, Paris, 1883; Fe, 740; Mi, 471-472.

The first element of the name is of early occurrence in reference to the African negro countries bordering on the Indian Ocean. Ptolemy speaks of the *Zygyis âpra* (I, 17, 9) or *Zîγγισα ἀπρα* (IV, 7, 11), and Cosmas in the 6th cent. mentions several times τὸ Ζίγγιον. In Arabic sources, the name is written زنج and vocalized as Zānj and Zinj; originally it must have been pronounced Zāng or Zing, with the pronunciation *g* of ج which still obtains in the Egyptian dialect of Arabic. This Zānj < Zāng can hardly be, as FERRAND says (*JA*, 1924, I, 240; cf. also STORBECK, in *MSOS*, XVII [1914], II, 101-102), an Arabized form of Persian زنگ Zāng, which has the same meaning; both forms may more probably go back to a common foreign original, or the Persian form may even have been borrowed from the Arabic.

From the name Zāng of the coast of East Africa, the Persians formed زنگی Zāngī, «negro [of Eastern Africa]», and زنگستان Zāngistān, «country of the Zāng», also arabized as Zānjistān; Zāngistān is the form used in 982 by the Persian author of the *Hudūd al-Ālam*. The Arabic form زنجبار Zānjbār (Zānjibār?) is met with for the first time in Ya'qūt (1224; cf. DEVIC, *loc. cit.*, 10; STORBECK, *loc. cit.*, 101; the apparent «Zanguebar» in Idrīsi's text of Fe, 173, is misleading; the original gives Zānj); the corresponding Persian one زنگبار Zāngibār (not «Zangi-bār» as in *Hobson-Jobson*<sup>2</sup>, 978) occurs in Qazwīnī (+ 1283; cf. STORBECK, *loc. cit.*, 101-102), in the Persian version of Ištāhri (*ibid.*), c. 1470 in 'Abdu'r-Razzaq (Fe, 474), etc. The name had long designated a considerable portion of the littoral, from a little south of Cape Gardafui to Sofāla. But Polo's informants left out of «Çanghibar» and north of it the whole region of Mogadiscio (see «Mogedaxo»). Later on, a still more restricted application prevailed; at the time of the Portuguese discoveries c. 1500, the name was specifically referred to the island at it is now (cf. DAMES, *Barbosa*, I, 26-28). On the final -bār of Zāngibār, see «Melibar».

Although Zāng must have been at first the name of a country, the colour of its inhabitants and the number of slaves which came from it soon gave the name the two accessory meanings of «negro» and of «slave». In the early days of the Abbassid califate, the Zānj had increased in Mesopotamia to such numbers that for fifteen years, from 868 to 883, they were able to conduct a bloody servile war in Mesopotamia, recalling those of Eunus and of Spartacus (cf. MASSIGNON, in *EI*, s. v. «Zandj»).

It is with the value of «negro», and often of «negro slave», that the word Zāngī passed to Indonesia, Central Asia and the Far East. A Javanese inscription of 860 mentions the Jēngī, while other spellings Jaŋgi and Jēni occur in inscriptions dated 1135, 1140 and 1294 (cf. FERRAND, in *JA*, 1921, I, 164; 1924, I, 241). Jaŋgi or Jēngī is still the name of the negroes in Malay; it is Jongi in Battak (cf. BEFEO, IV, 291).

With all the modern intercourse between Egypt, Persia and Turkish-speaking countries, it is no wonder that zānji and zāngi should mean «negro» in Osmanii Turkish and in the Turkish of Crimea respectively. But the occurrence of the word in the *Qutadju bilig* of 1069-1070 deserves more attention: in two passages, the appearance of the world when the sun is gone is

compared to the face of a *zāngi* (113, 3, and 177, 20). The meaning is confirmed in the second case by the variant *Habaš*, «Abyssinian», of the Cairo ms.; the Uighur spelling *sānggi* or *sāngi* given in RADLOV's edition in Uighur letters (p. 131, 216) and in his dictionary (iv, 448) is a misreading, or a wrong form due to the late scribe who had to use the unfamiliar Uighur script. The transcription in Latin letters accompanying Radlov's translation gives *zāngi* (p. 337) and *sānggi* (p. 514); I have no doubt that the Cairo ms., written in Arabic script, correctly gives in the first passage *zāngi*, which RADLOV adopted; but RADLOV retained in the second passage the wrong form, *sānggi*, of the Uighur ms. (it ought, however, to be *sāngi* according to his edition of that ms. in Uighur script) because this time the ms. in Arabic script had *habaš* instead of *zāngi*. We may safely conclude that in the 11th cent., the Turks of Central Asia had at least a scholarly knowledge of *zāngi* as a word meaning «negro».

The same word occurs in Chinese sources. In 1904 (*BEFEO*, iv, 289-291) I called attention to a few texts mentioning slave boys and girls called 僧祇 *sēng-ch'i* (\**səng-g'jie*) or 僧耆 *sēng-ch'i* (\**səng-g'ji*) who had been sent as tribute by some Indonesian kingdoms: two *sēng-ch'i* girls in 724 by Śrīvijaya (= Palembang, or Palembang-Jambi; cf. *Hsin-T'ang shu*, 222 C, 4 a; *Ts'ê-fu yüan-kuei*, 971, 6 a; 975, 4 b); four *sēng-ch'i* boys in 813 (*Hsin-T'ang shu*, 222 C, 3 a; *T'ang hui yao*, 100, 2 b; in 815 according to *Ts'ê-fu yüan-kuei*, 972, 7 a) and two *sēng-ch'i* girls in 818 by Ho-ling (= Kalinga, Java; *T'ang hui yao*, 100, 2 b; *Ts'ê-fu yüan-kuei*, 972, 7 b). Already in 1904, I had identified these *sēng-ch'i* as *zāngi*; the solution has been accepted, and I do not think it can be doubted. There are, however, two difficulties. The first one is of a phonetic nature, which I failed to notice in 1904: *sēng-ch'i* normally renders an original \**sāngi*, not *zāngi*.

To account for such an anomaly, I can only suggest either that the Persian term reached China through intermediaries who had no *z* and pronounced it as *s*, or that the transcription was contaminated by the earlier existence of 僧祇 *sēng-ch'i* as a ready-made Buddhist term transcribing *sāngi*[ka] and entering into transcriptions based on Prakrit forms of *saṅkhaṣika* and *asaṅkhyeya* (cf. ODA Tokuno's dictionary, 1072). But both explanations are mere hypotheses.

I alluded to the second difficulty in 1904: 僧耆 *sēng-ch'i* occurs in the 蠻書 *Man shu* of c. 860 (6, 6 b) as the name of a «tribe» which there is no apparent reason to locate as far away as Africa (cf. *BEFEO*, iv, 291). But in the *Études asiatiques* published in 1925 by the École Française d'Extrême-Orient (II, 261-263), I have since translated another text which must be mentioned in connection with that of the *Man-shu*. In one of the notes of his *I-ch'ieh ching yin-i*, completed in 817, Hui-lin speaks of the 崑崙 *K'un-lun* (\**Kuən-luən*) or 骨論 *Ku-lun* (\**Kuət-luən*) barbarians of the southern islands, very black, naked, capable of taming rhinoceroses and elephants and adds: «They are of several sorts: there are the 僧祇 *Sēng-ch'i*, the 突彌 *T'u-mi* (\**D'uət-mjie*), the 骨堂 *Ku-t'ang* (\**Kuət-d'äng*), the 閩蔑 *Ko-mieh* (\**Káp-miet*) and others; all are vile people, without rites or laws, living by pillage, and fond of eating men... Their language is irregular and different from that of the [other] barbarians. They are accomplished in going into water (= for diving into it) and [can remain in it] a whole day without dying».

Of the different names occurring in Hui-lin's text, *T'u-mi* and *Ku-t'ang* are unknown, but *Ko-mieh* transcribes the name of the Khmêr or Cambodians, and *K'un-lun* is a general designation

of southern Indo-Chinese and Indonesian races. K'un-lun was not always used with the pejorative meaning attributed to it by Hui-lin; we hear for instance of Buddhist literature in the K'un-lun language. Nevertheless, the name soon became a designation of blackish people, with curly hair, who often came to China as slaves. The K'un-lun thus began to be mixed up with the Zāngi. In other words, Indonesian Negritos may have been called Zāngi without being African negroes, while African Zāngi came also to be known in China as K'un-lun. In exactly the same terms which Hui-lin employed for the K'un-lun barbarians, Idrīsi speaks of the natives of Jālūs (? read Bālūs = Baros), probably in Sumatra, who he says are Zānj (transl. JAUBERT, I, 77, 79; DEVIC, *Le pays des Zendjs*, 110; Fe, 182, 184).

Since I discussed the problem of the K'un-lun in 1904 (*BEFEO*, IV, 215-231), FERRAND has examined it in great detail in *JA* (1919), and I have added further texts in *TP*, 1923, 271-272, 290, and in *Études asiatiques*, II, 257-263. A T'ang Sino-Sanskrit vocabulary renders K'un-lun as Skr. Dvipāntara (cf. *Mémorial Sylvain Lévi*, 392-397), meaning something like «the other continent». Despite FERRAND's researches, we cannot yet tell what the original was of the word transcribed as K'un-lun. FERRAND was not far from believing that it was Qomr, Qomoy, and he would identify with that name the name of the Khm̄r and the African Qomr (cf. *JA*, 1919, II, 212). The question is made more obscure by the earlier existence of a K'un-lun in Chinese legend (the original Qomr according to FERRAND!), and also by the deficiency of Middle Chinese which did not have a phonem like -om or -um. Moreover, the traditions relative to Kauṇḍinya, which is sometimes transcribed 俱倫 Chū-lun (\*Kju-ljuēn) may have reacted on the Indo-Chinese Ku-lun or K'un-lun; finally, at a later date, K'un-lun became the Chinese name of Pulo Condor (see «Condur»). To try to unravel such an entangled skein would require a whole monograph, only to reach perhaps an indifferent result. Suffice it to say here that the name K'un-lun was applied by the Chinese to black curly-haired (or frizzy-haired) races at least as early as the 4th cent.

I have dilated somewhat on the name K'un-lun because we find it in Sung times much more closely connected with Zāngi than in Hui-lin's note of 817. Chou Ch'ü-fei, whose *Ling-wai tai-ta* was written in 1178, devoted a paragraph to 崑崙層期 K'un-lun Ts'êng-ch'i (\*Kuən-luən Dz'əng-g'ji), an island where there were giant birds which could mask the sun in their flight (see «Ruc») and the inhabitants of which, black as lacquer and with frizzled hair, were carried off as slaves to Arab masters. This text was copied almost word for word by Chao Ju-kua in his *Chu fan chih* of 1225 (*HR*, 149). HIRTH and ROCKHILL, interpreting K'un-lun Ts'êng-ch'i as «The Zanj (or Blacks) from K'un-lun», saw in K'un-lun a transcription of Kanbalu, the native name of our Pemba Island, north-north-east of Zanzibar. For FERRAND (*JA*, 1919, II, 208-210), K'un-lun represented Qāmrūn, in a series Qomr (in Central Asia) > Qāmrūn (in Indo-China and Indonesia) > Qomr (at Madagascar and on the oriental coast of Africa); as the form Qāmrūn was not known in Africa, the true original of K'un-lun Ts'êng-ch'i must have been «Zang of Qomr». With all due regard for FERRAND's industry, the whole theory seems to me ruinous. Particularly, in so far as K'un-lun Ts'êng-ch'i is concerned, I do not accept for one moment that the Chinese should, when they had heard «Zang of Qomr», feel that Qomr was the same as a Qāmrūn which they elsewhere transcribed K'un-lun. In the case of Chou Ch'ü-fei, we have to deal with a man who had never left China, and who, when collecting his information from merchants and sailors, could

but try to connect what he was told with what he already knew. Ts'êng-ch'i was unknown to him; but of course he knew K'un-lun, and its use as a designation of negroes or of negritos. I do not believe that K'un-lun Ts'êng-ch'i is a genuine compound name transmitted from Africa. But Chou Ch'ü-fei, hearing of the Zāngi of Africa, coupled their name with the one which was more commonly used in China, and the combination resulted in K'un-lun Ts'êng-ch'i.

This combined form was artificial, and Chao Ju-kua found it only in Chou Ch'ü-fei. The result was that when Chao Ju-kua's informants spoke to him of the land of the Zāngi and mentioned it under the name then in use, Zangibār, Chao Ju-kua had no suspicion that Zangibār was the same as K'un-lun Ts'êng-ch'i and devoted a chapter of his own to 層拔 Ts'êng-pa (\*Dz'êng-b'wat), a perfect transcription of Zangibār (HR, 126-127). This first occurrence of the form Zangibār in 1225 strictly falls in with the first use of Zanjibār by Ya'qūt in 1224; it is quite possible that the forms in -bār of that particular name were not yet in use, or were scarcely in use, when Chou Ch'ü-fei wrote half a century earlier.

Zangibār is found a second time in Chinese in the *Tao-i chih-liao* of 1349, where 層搖羅 Ts'êng-yao-lo has justly been corrected to Ts'êng-pa[拔]-lo, Zāngibār, by the various commentators (cf. ROCKHILL, in *TP*, 1915, 622-623). On the other hand, I think that HIRTH and ROCKHILL were mistaken when they thought of phonetically connecting 中理 Chung-li, the name of the Somali coast in Chao Ju-kua, with Zāni or Zāng (HR, 131, 272).

The land of the Zāngi raises a last Polian problem, for which something can be added to what has been stated by YULE (*Y*, I, 120) and by BENEDETTO (*B*, CCXII-CCXIII; cf. also LANGLOIS, in *Hist. litt. de la France*, xxxv, 255). Pietro d'Abano (c. 1250-c. 1316), who knew Polo in Venice, has a paragraph on a star «as big as a sack» (in YULE's translation), which is seen in the country of the «Zinghi» (YULE). Instead of YULE's «Zinghi», BENEDETTO's Latin quotation gives «in regionibus Çingorum», with a note to the effect that probably the country meant is Zanzibar, «called Zinz in the map of the world of a contemporary of [Pietro] d'Abano, Marino Sanudo the elder». That Zanzibar, or rather the Zāngi country, is meant is beyond discussion, but the rest of BENEDETTO's note is not quite correct. Like Pietro d'Abano's Venetian edition of 1476, which gives «Zingorum», the mss. Vat. Lat. 2447, 100c, and Pal. Lat. 1171, 123c, which MOULE was kind enough to collate for me, give «Zingorum» and «Çingorum», respectively. As to Marino Sanudo's map, as reproduced in BONGARS's *Gesta Dei per Francos*, it gives «Zine et ideo Zinzibar», and I think that «Zine» is more likely to be a misreading of «Zinc» (= \*Zing) than of \*Zinz. It is after the mention of the «star» of the country of the Zāngi that Pietro d'Abano adduces by way of confirmation what he had been told by Polo. The whole discussion is concerned about the possibility of living on the equator (a possibility which is denied in Marino Sanudo's map: *regio inhabitata propter calorem*). Pietro d'Abano quotes various authorities, according to which the climate is less torrid at the equator than north of it. He goes on with the passage on the «star», which I would translate as follows:

«The stars (*signa*) support this same [view], since Albumasar says in the book of the *Dialogues with Sadan*: 'In the country of the Zinghi there is seen a big star in the shape of a sack (*stella magna ut saccus*). I have known a man who saw it, and he told me that it had a faint light like a piece of a cloud and that it was always in the south'. I have also been told of

that [star] — amongst other [things] — by Marco the Venetian, the most extensive traveller and diligent explorer that has ever been known, that he saw the same star under the Antarctic Pole : and it is big, with a tail (*et est magna habens caudam*, according to both mss.; in printed editions *et est magnam habens caudam*), and he drew a figure of it *thus*. He also reported that he saw [there] the Antarctic Pole at an altitude above the earth apparently equal to a soldier's long lance, while the Arctic Pole was hidden [below the horizon]. It is from there also, he says (*nuntiat*, mss.), that they export to us camphor, lign-aloës, and brazil (I agree here with YULE, not with B, CXIV, n. 1). He testifies that the heat there is intense, and the habitations few. He saw all that on a certain island where he arrived by sea. He also says that men and rams are very big there (cf. B, CCXIII, n. 2) and have (*i. e.* the rams) very coarse and stiff wool like the bristles of our pigs, and that there is no means of access to such places except by sea.»

In another section, Pietro d'Abano says (B, CCXIII) : « . . . I have heard from Marco the Venetian, who had crossed the equator, that he found there bigger men than here, and this occurs because in such places there is no cold that condenses bodies and therefore reduces them (in size). »

According to YULE, the place to be reached only by sea where Polo saw the «star» «is of course Sumatra». While admitting of such a possibility, BENEDETTO objects that the only passage in which Polo speaks of men of gigantic stature is in the chapter on «Çanghibar» (Vol. I, 432). This is true, but since Polo never visited the African Coast, the only alternative to Sumatra would be Java, in case Polo visited it in the course of his earlier mission to the southern seas.

YULE's figure of Polo's drawing seems to be an inversion to the right of the figure with the curve at the left which is printed in the 1548 edition of Pietro d'Abano and is reproduced by BENEDETTO (B, CCXIII). Our figure, reproducing the 14th century ms. Pal. Lat. 1171, fol. 123 c, is much less elaborate, but shows the curve at the top, which I believe is the original arrangement.

Albumazar, quoted by Pietro d'Abano, is Abū Ma'shar Ja'far Ibn Muḥammad, of Balkh, astronomer and astrologer, who died a centenarian on A. D. March 8, 846 (cf. BROCKELMANN, *Gesch. der arab. Litteratur*, I, 221-222). The *Dialogues with Sadan* are his answers to the questions of Sa'id Šādān (BROCKELMANN, *ibid.*, Supplement, I, 395). The latter work has never been published, but there is a manuscript of it at Cambridge (Gg, 3, 19, dated 7 or 17 Jumāda II, A. H. 767 = 19 February or 1 March 1366; the passage occurs fol. 9 r<sup>o</sup>, ll. 12-14), and a Latin translation in the Bodleian, Oxford (Laud. Misc. 594 [fol. 138 a]). MOULE furnished me with the text of the Latin version, and, at his request, Prof. R. A. NICHOLSON was good enough to look up the original Arabic passage, the following translation of which I owe to my colleague L. MASSIGNON : «Abū Ma'shar said : In the country of the Zān] (or Zīn]?), people see rising a great star, looking like a sack, which is only visible in the region of the sky where it rises. I have seen somebody who had observed it, and he reported that it emitted a faint light, like a piece of a cloud in which there is a faint light, and also that it was always (? عظيم; read دائم = دائما) southern (since) the arc (qaws) of its trajectory was small.» The last part of the sentence, after «southern» (*australis*), is omitted in the old Latin version.

YULE suggested that the «star» was the great Magellanic cloud, and he was certainly right.

The important fact is that it was already known to the Arabs in the 9th cent. as being visible in the land of the Zāngi. Later Arabic lore gives the same information. In Ya'qūt's geographical dictionary, completed in 1224, there is a paragraph to the following effect (cf. DEVIC, *Le pays des Zendjs*, 21; STORBECK, in *MSOS*, XVII [1914], II, 111) : « [In the land of the Zinj, people] see in the sky something of the size of the moon, similar to an arch (*ṭāqa*) or to a small cloud, which neither disappears nor recedes. I asked many people about it, and all gave the same answer. The Zinj give it a name which I do not remember, and they do not know what it really is.» Of course, DEVIC here suggests the same explanation that it was a Magellanic cloud which YULE thought of for Pietro d'Abano. It may be worth pointing out that Ya'qūt's «arch» (which I prefer as an equivalent of *ṭāqa* to DEVIC's «fenêtre ronde», or to STORBECK's «Schleier»; the word is ambiguous) is in agreement with Polo's supposed drawing, especially when the latter is looked at vertically, as it is given in the ms., instead of being drawn horizontally as in *Y*, I, 120.

A manuscript note left by YULE and added in *Y*, I, 120, n. †, draws attention to the resemblance of the drawing in Pietro d'Abano's work with «cloud» designs in Oriental carpets, the «nebula» in heraldry, an undetermined Chinese term signifying «cloud», and a mysterious Persian term «*silen-i-khitai*». I am not in a position to throw any light on these points. A friend suggested that the drawing may be meant for the «Caput draconis», the usual sign for which is  $\Omega$ . The similarity is certainly striking, but can hardly be more than a coincidence. The «star» meant by the Arab sources and by Polo is something real, whereas the «Caput draconis» is merely an astronomical point, the ascending node of the moon. The designation of the ascending and descending nodes of the moon as «head» and «tail» of the dragon is a very old notion, common to ancient Greek as well as Chinese astronomy and astrology. Although they have often been regarded as «planets» (the Rāhu and Ketu of Indian lore), they were fictitious astral bodies, which nobody of course could pretend to have seen in the sky.

On the relative location of «Çanghibar» and «Mogedaxo» according to Polo, see «Mogedaxo».

## 197. ÇARDANDAN

*ardadam*, *zedandi* (?) G  
*ardanda* TA<sup>1</sup>, TA<sup>3</sup>  
*ardandam* LT, P  
*ardandan* VA

*arddandan* F  
*ardidan* VB  
*cardandan* R  
*chardadan* V

*çardandan* Fr, t, L, Z  
*iardandam* FA†  
*zardandan* FA, FB

Polo's form strictly transcribes the Persian زر دندان *Zar-dandān* «Gold-teeth», of exactly the same meaning as the corresponding Chinese name 金齒 *Chin-ch'ih*. The Catalan Map gives «p. zardadain», = Provincia Zardadain (misread «Perzae-dadain» by BUCHON, *Not. et Extr.* XIV, II, 142, and by CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 16, 22; other misreadings in



HALLBERG, 568-569); Fra Mauro, « Çardandam » (not « Zardandam » as in HALLBERG). The identification was first proposed by KLAPROTH (*JA*, Febr. 1828, 119); QUATREMÈRE must have overlooked it when he put forward the same solution as his own discovery eight years later (*Histoire des Mongols*, xcvi).

The earliest notice of the « Gold-teeth » occurs in the 蠻書 *Man shu* (Chien-hsi-ts'un-shê ed. 20 b, 21 a), written in 864 (cf. BEFEO, iv, 132), from which it is quoted (under the alternative title « Description of the Southern Barbarians », *Nan-i chih*) in the *T'ai-p'ing yü-lan*, 789, 19 b, and was partly copied in *Hsin T'ang shu*, 222 A, 4 b; the gist of the passage in the *Hsin T'ang shu* is in its turn summed up, though not very accurately, in *YS*, 61, 12 b, and this is translated (with many errors) in *Pa*, 397. The text of the *Man shu* has been poorly established, and I cannot undertake to publish here a critical edition of the passage. But in the following provisional translation I take account of the various relevant texts :

« The Barbarians (*Man*) 'Black Teeth' (黑齒 Hei-ch'ih), the Barbarians 'Gold Teeth' (金齒 Chin-ch'ih), the Barbarians 'Silver Teeth' (銀齒 Yin-ch'ih), the Barbarians 'Embroidered Legs' (繡脚 Hsiu-chiao), and the Barbarians 'Embroidered Faces' (繡面 Hsiu-mien) are all south of the barrier (關 *kuan*; now altered to 開 *k'ai* in the *Man shu*, giving an apparent K'ai-nan which did not exist before the Mongols) of 永昌 Yung-ch'ang (see « Uncian »); they belong to various races. The Black Teeth Barbarians varnish their teeth with lacquer (漆 *ch'i*). The Gold Teeth Barbarians carve gold slips with which they sheathe their teeth; the Silver Teeth Barbarians do the same with silver. When they go out to see people on business, they use these as ornaments; they take them off to sleep and to eat. They all tie their hair in a knot at the top of the head. With a blue (*ch'ing*) cloth, they make a one-piece lower garment (通身袴 *t'ung-shên-k'u*) and also throw a strip of blue cloth obliquely over the shoulder. The Embroidered Leg Barbarians cut the skin all around [their legs] from the ankle to the calf thus making designs; they dress in purple cloth. The Embroidered Face Barbarians, one month after birth, have their face pricked with a needle, and then indigo (靑黛 *ch'ing-tai*) is applied over it, giving it the appearance of embroidery... ». I leave out the long text of the *Man shu* which comes almost immediately afterwards and which is devoted to the various tribes of the 茫 Mang Barbarians. It has been wrongly combined with the above in the *Hsin T'ang shu* and consequently in *YS*, 61, 12 b; but, in the original redaction, the only common points are that the Mang also lived « south of the Barrier of Yung-ch'ang », and that some of them had lacquered teeth; there is no mention in the *Man shu* of their having « gold teeth ».

It will not be without interest here to quote, after the text of the *Man shu*, what Rašid has to say of the Zar-dandān. Once he mentions them when giving the boundaries of the Qara-Ĵang (KLAPROTH, in *JA*, I [1828], 113; *Bl*, II, 378). The Zar-dandān again appear along with Tibet and are located, inaccurately enough, to the north-west of Northern China (QUATREMÈRE, *Histoire des Mongols*, xcvi; *Oh*, II, 640; *Bl*, II, 500). In the land itinerary from India to Yün-nan, Rašid speaks of the « Zar-dandān, [who are] so called because these people cover their teeth with gold. They puncture their hands and colour them with indigo... » (ELLIOT, *Hist. of India*, I<sup>2</sup>, 72-73; Y<sup>1</sup>, III, 131; YULE, in *JRAS*, NS, IV [1870], 354, 355). A last passage is quoted by QUATREMÈRE (*loc. cit.* xcvi), and it was moreover copied from Rašid by

Bänäkätī (cf. KLAPROTH, *loc. cit.* 120; *Pa*, 397) : «In one of the kingdoms [to the west of China], all the people cover their teeth with a gold case (*ylāfi*) which they take off when they take their meals and put on again afterwards».

Neither the *Man shu* nor Rašid makes any distinction between men and women as to the use of gold teeth; this may be an indication that those mss. of Polo which state that men and women use them are right. On the other hand, RAMUSIO alone gives in the chapter devoted to the Zar-dandān an account of tattooing shorter than, but very similar to, the one which occurs afterwards in the chapter on «Caugigu». This passage of RAMUSIO, inserted into BENEDETTO's text (*RR*, 192; *B*<sup>1</sup>, 198), is merely alluded to in a note of Vol. I, 281 of this edition. But both the *Man shu* and Rašid speak of tattooing in connection either with the Zar-dandān themselves, or with tribes of the same group, and consequently I think that the passage of RAMUSIO in question must have a genuine origin.

The «Directing Commissariat (*hsüan-fu-ssü*) of the Chin-ch'ih and other places», which was the last separate organization of that part of the country under the Yüan dynasty, was «to the south-west of Ta-li, had the Lan-ts'ang-chiang (=Mekong) as its eastern limit and bordered on the Burmese (Mien) territory on the west» (*YS*, 61, 12 *b*; *Pa*, 397, has mispunctuated and misunderstood the text). In 1260, the chief of the Chin-ch'ih sent his son to render homage to the Court. In 1261, a «Pacifying Commissariat» (*an-fu-ssü*) was created to take charge of the region, which was divided in 1273 into an eastern district (*lu*) for the 白夷 Pai-i and a western district for the Chin-ch'ih (the *YS*, 61, 12 *b*, dates this division in 1271, but the *pên-chi*, more reliable, relate that on April 8, 1273, «the Chin-ch'ih kingdom was divided into two *lu*»; *YS*, 8, 1 *b*). The «Pacifying Commissariat» became in 1278 a «Directing Commissariat», having six districts (*lu*) under its jurisdiction. It was suppressed in 1286, according to *YS*, 61, 12 *b*, and the whole territory included in the jurisdiction of a «Directing Commissariat of Ta-li, Chin-ch'ih, and other places». It is certainly the same event which is mentioned in the *pên-chi* (*YS*, 13, 8 *a*) as follows : «In the 22nd *chih-yüan* year (1285), . . . the ninth month, . . . on the day *wu-ch'ên* . . . , the two Directing Commissariats of Ha-la-chang (Qara-Jang) and of Chin-ch'ih were reduced to one, which had its seat at Yung-ch'ang (see 'Uncian')». As I show under «Caragian», and in agreement with Polo's text, Qara-Jang was the Mongol name of Ta-li. But the date given in the *pên-chi* is impossible (as it is with some other entries which precede), whereas it answers correctly for the year 1286, giving September 23, 1286; it seems as though some entries referring to 1286 were unduly inserted by the hasty compilers of *YS* into the *pên-chi* of 1285.

The territory of the Chin-ch'ih proper lay to the west of the Salween, either on the Nam-ti and Ta-ping, or on the Shweli River, all of them tributaries of the Irawadi (cf. *BEFEO*, IX, 665; *Ch*, II, 245-246; I think HUBER was mistaken when he extended the territory of the true Chin-ch'ih of about A. D. 1300 to the east of the Salween and to Yung-ch'ang [see «Uncian»]; but no certain solution can be reached until the last paragraphs of *YS*, 61, have been elucidated). The ethnic appurtenance of the Chin-ch'ih has not been determined, and there is until now, at least as far as I know, no critical study of the important information about them scattered in Chinese historical works. In the 18th cent., the 標人 Piao-jên (cf. *BEFEO*, VIII,

363), « far to the south-west beyond the limits of Yung-ch'ang », were said to be descendants of the Chin-ch'ih, these being represented as of Pyū origin (*Hsü Yün-nan t'ung-chih kao*, 162, 29 b); but nothing is any longer said about their teeth. The Piao-jên seem to be identical with the 驃 Piao mentioned in YS, 61, 12 b, as one of the eight tribes of « Local Barbarians » living in the jurisdiction of the « Directing Commissariat of the Chin-ch'ih and other places » (they are the « chevaux rapides » of *Pa*, 397; but Piao is merely the transcription of a foreign name). The form 縹 Piao also occurs in YS, 61, 13 b, in the name of a « military district of the area of Piao » (縹甸軍民府 Piao-tien *chün-min fu*), of which nothing is known beyond the name.

### 198. ÇERME

çerme VB

zerme VB<sup>2</sup>, R

Ar.  $\text{جرم}$  *Jarm*, pl.  $\text{جرم}$  *Jurūm*; large barges on the Nile. My own conception of the « Polo-Rustichello » spelling would be « germe », on the basis of « çerme » of VB and « zerme » of VB<sup>2</sup> and R; *B*<sup>1</sup>, 369, has kept « zerme »; *RR*, 424, write « jerm ». A passage very similar to Polo's occurs in Pegolotti (EVANS, 19) : « Giermo vuol dire in Saracinesco grossi navili che portano le mercatantie da Damiatia su per lo fiume insino al Cairo di Babilonia e dal Cairo su per lo detto fiume insino al mare dell'India ». Our text of Pegolotti is very corrupt, and we ought perhaps to read « gierma » (although « germi » are again mentioned, p. 72), as the word seems to have always been used as a feminine in Italian and in French. In 1538, we have a Venetian plural *zerme* in RAMUSIO, I, 274 b. For French forms *djerme* and *germe*, Italian *germa* and diminutive *germetta*, cf. JAL, *Glossaire nautique*, 592, 780. A number of other quotations will be found in HEYD, *Hist. du commerce*, II, 60.

In DALCADO's *Glossário Luso-Asiático*, I, 428, Polo's « zerme » (in R) and *zerbe* in a Portuguese text of 1538 are wrongly treated as identical with Port. *gelba* or *gelva*, which comes from quite a different Arabic word, *jalba* (on which cf. DOZY, *Glossaire*<sup>2</sup>, 276; YULE, *Hobson-Jobson*<sup>2</sup>, 361-362). LOKOTSCH's *Etym. Wörterbuch* mentions neither word.

### 199. ÇIC

cac L

çic F, Z

*ziziri* TA<sup>1</sup>

From the days of Strabo onwards, the western Circassians have been known to the Greeks as *Zυγοί*, *Zίχιοι*, and their land became the country *Zίχία* in Byzantine Greek (cf. D'AVEZAC, in *Rec. de Voy. et de Mém.*, IV, 497; REINAUD, *Géogr. d'Aboulféda*, II, I, 286, 322; Y, II, 492).

Plan Carpine mentions both «Circassos» and «*terram Siccorum*» (*Wy*, 90, 111-112). Simon de Saint-Quentin (in Vincent DE BEAUVAIS, *Specul. historiale*, xxxi, 149) and Rubrouck (*Wy*, 167) speak of «Ziquia», but Rubrouck had also heard of the «Cherkis», i. e. Circassians (*Wy*, 199, and add the «Kerkisorum» or «Verkisorum» of p. 209, probably for «Cherkisorum», with *ch* = *ç*). The «Giquis» are referred to in Martin da Canal for events of 1268 (cf. BRÄTIANU, *Rech. sur le commerce génois*, 249). The *Libellus de notitia orbis* of John III, archbishop of Sultaniyah, has a paragraph (cf. A. KERN, in *Arch. Frat. Praed.*, VIII, 111-113) on «Ziquia sive Tharquesia» (var. «Tharquasia»; read «Charquesia»?) and its two classes of inhabitants «Tarcasi (also 'Tarcazi', p. 108; read 'Carcasi'?) nigri et albi; nigri nomine non pelle». Circassian slaves of both sexes were then in great demand. For instances of sales of two «Zicha» women in 1281, and one man «de proienie Zichi» in 1289, cf. BRÄTIANU, *Actes des notaires génois*, 164-165, 243. In Pasqual de Vittoria's letter of 1338, the servant said to be «Ziquo natione» was clearly a Circassian (the passage was misunderstood in *Y<sup>1</sup>*, III, 85; cf. *TP*, 1922, 79-80; *Wy*, 504). There was a Latin archbishop of «Matrega» in «Zychia», called John (1349-1376); from the *Libellus de notitia orbis*, we learn that he belonged to a good Circassian family, but had in his youth been sold as a slave at Genoa (cf. KERN, in *Arch. Frat. Praed.*, VIII, 111). Pegolotti also gives some information on the trade with «Zecchia» (EVANS, 54-55). The *Directorium ad passagium faciendum* mentions the «Ziqui», in the French text «Ziques», strangely considered by the editors as the Sarmat «Jaziges» (*Hist. des Crois.*, Arm., II, 386). The Catalan Map has on the north-eastern coast of the Black Sea an «Alba Zechia» («White Zechia») and a «Maura Zechia» («Black Zechia»; BUCHON, in *Not. et Extr.* XIV, II, 81). On Fra Mauro's map, I find only «Cercassia». The region which appears in HALLBERG, 571, as «Zichia», without indication of origin, and is erroneously explained as «a name of Scythia», must also be Western Circassia. On these names, cf. DIRR, in *Petermann's Mitteil.* 54 [1908], 206 and 212, and in *EI*, s. v. «Čerkesses».

The name *Zıχία* is connected with that of «Adiye», «Adziye», by which the Circassians call themselves. It occurs in Abkhāz as *Zuḥunī* and *Azḥuā*, «Circassian». «Jik» is also the Georgian name of the Circassians (cf. BROSSET, *Hist. de la Géorgie*, I, 510; II, 18).

The other name, corresponding to our «Circassian», appears only in the Middle Ages, and renders a Turkish form «Čärkäs». According to DIRR, «Čärkäs» would represent a palatalized form of the name known in Greek as *Κερκέται*. MARQUART (*Ueber das Volkstum der Komanen*, 181) suggested a doubtful Persian etymology, \*čähār-Kas, the «four [tribes of the] Kas». This would connect the name with another designation of the Circassians, which is found in the *Hudūd al-Ālam* as *Kāsak*, in Byzantine sources as *Κασαχία*, in old Russian as *Kasogy*, and which survives in *Khäsäg*, the Osset name of the Circassians (cf. *Mi*, 446). Stephen Orbelian mentions the «Čergēzaç» (in the plural; cf. SAINT-MARTIN, *Mém. sur l'Arménie*, II, 121, 268).

Far Eastern texts mention the Circassians in connection with the Mongol conquest. They are the «Särkäsüt» and «Särgäsüt» of the *Secret History* (§§ 262, 270), plural of «Särkäs» and «Särgäs», respectively. The transcribers of the Mongol text into Chinese at the end of the 14th cent. had no means to distinguish between the *-k-* and the *-g-*, represented by the same letter in Mongolian script. The change of *ç* to *s-* is of Turco-Mongolian origin. But, even in Mongolian

the true form with č was known and has actually been retained in the name of individuals : a 撒兒怯思 Ch'ê-êrh-ch'ieh-ssü, Čärkäs, is mentioned in *YS*, 22, 12 *a*, and a 撒里哥思 Ch'ê-li-ko-ssü, \*Čärgäs, in *Yüan tien chang*, I, 12 *a*. On the Chinese map of c. A. D. 1330, the name appears as 撒耳柯思 Sa-êrh-k'o-ssü, which has been retained by BRETSCHNEIDER (*Br*, II, 90) and by T'ü Chi (160, 16 *a*). But this map is of Mussulman origin, and the Persian form of the name is Čärkäs, sometimes Čärküz (cf. for instance LE STRANGE, *Nuzhat al-Qulüb*, transl., 10, 248, 261; VULLERS, I, 570; also REINAUD, *Géogr. d'Aboulféda*, II, 2; Järkäs = Čärkäs in *Ibn Baṭṭūṭah*, II, 448). Since the name is written 撒耳柯思 Ch'ê-êrh-k'o-ssü in the corresponding list of *YS*, 63, 16 *a*, I have no doubt that in our late copies of the map of c. 1330, «Sa-êrh-k'o-ssü» is merely the result of a very common clerical error. The form «Särkäs» of the *Secret History* has survived in Kalmuk; it was transcribed 色爾克斯 Sê-êrh-k'o-ssü in the 18th cent. (cf. T'ü Chi, 160, 16 *a*); the modern Kalmuk pronunciation is «Serkš» (RAMSTEDT, *Kalm. Wörterbuch*, 326).

## 200. ÇIPINGU

<i>cianpagu</i> S	<i>cipingu</i> Fr	<i>sipangu</i> FA <sup>1</sup> , FB
<i>cimpagu</i> , <i>cypagu</i> , <i>ziampagu</i>	<i>cipingu</i> Ft, L	<i>sypangu</i> FB
VL	<i>çipingu</i> Z	<i>zipaghu</i> TA <sup>1</sup>
<i>çinpachu</i> , <i>zinpachu</i> TA <sup>3</sup>	<i>cipngu</i> , <i>cypngu</i> F	<i>zipangu</i> P, P <sup>5</sup> , R
<i>çinpuguj</i> , <i>zinpuguj</i> VB	<i>sapangu</i> FA	<i>zipugu</i> V
<i>çipagu</i> , <i>zanpagu</i> , <i>zianpagu</i> VA	<i>simpagu</i> LT	<i>zympagu</i> G

This has long been recognized as a transcription of 日本國 Jih-pên-kuo, «Kingdom of Japan». For a reading with *a* in the second syllable, cf. YULE, *Hobson-Jobson*<sup>2</sup>, 451. Nevertheless, one may just as well have theoretically «Cipingu», «\*Çibingu» or even «\*Çipungu», as «Çipingu»; «Çipingu» is supported in fact by Z. In spite of CORDIER (*L'Extrême-Orient dans l'Atlas Catalan*, p. 9), «Çipangu» cannot be a Fukienese pronunciation, as *kuo* has a guttural final in Fukienese. Fra Mauro seems to give «Cimpagu» as read by ZURLA, but not the «Zimpungu» also mentioned by HALLBERG, 497-498.

The Catalan Map of 1375 has, according to CORDIER in *Y*<sup>1</sup>, I, 301 : «Japan insula, à M. Paulo Veneto zipangri dicta...». But Polo could not know the modern form «Japan», and this is a slip of CORDIER's; the notice comes, not from the Catalan Map, but from ORTELIUS (cf. CORDIER, *L'Extrême-Orient dans l'Atlas Catalan*, 8-9).

The name of Japan appears in Rašidu-'d-Dīn (*Bl*, II, 498) as چمنگر Jīmingu (vowels uncertain), which represents evidently the same original as Polo's. But one would like to be more certain about Rašid's real spelling; D'OHSSON (*Oh*, II, 319) has transcribed «Tchépangou» (cf. also QUATREMÈRE, *Hist. des Mongols*, xci), which supposes چمنگ Jīpangu or Jībangu (vowels uncertain).

In «Die älteste türkische Weltkarte» (*Imago Mundi*, 1935, 23, 27), HERRMANN says that Japan appears in Kāšyari, as early as 1076, under the name «Djabanka» = Jih-pên-kuo. I cannot agree with the identification. The text, as well as the map, writes جابرقا Jābarqā, and I have no doubt that Kāšyari, as is stated in BROCKELMANN, 243, s. v. «Zābarqa», means the mythical country of that name which is generally located in the Far West, but which Kāšyari has transferred to the Far East. Although the name must not be corrected to «Djabanka», it might be supposed by some that Kāšyari's Far Eastern location of Jābarqā is due to some echo of Jih-pên-kuo; but the phonetic correspondence is not very satisfactory, and there is nothing to show that the name of Jih-pên-kuo was known in Central Asia in the 11th cent. Cf. *TP*, 1936, 361-362.

## 201. ÇIRAÇ

çehaa LT  
ceraci VA  
ceraçi F, L  
ceranti VB  
cerazi P

cietazi TA<sup>a</sup>  
çiraç Z  
iciagi S  
serasy FA, FB

siras R  
zelazi V  
zerazi VL  
zirazi TA<sup>1</sup>

This is the great city of شیراز Širāz in Fars, given by Polo as one of the eight «kingdoms» of Persia. *RR*, 434, and *Be*<sup>1</sup>, 447, have written «Serazi», but the consonantic ending has the support of R and Z; the -i- vowel of the first syllable, apart from R and Z, is also in TA<sup>1</sup>. Although «Çiraç» would rather represent, theoretically, an impossible \*Ziraz, I have adopted it, but the expected «Polo-Rustichello» spelling should have been \*Sciraç. In 1307, Hethum mentions «Seras» (*Hist. des Crois.*, Arm., II, 127, 267). The Catalan Map has «Ssiras» or «Sfiras» (? < «\*Sciras»); the Medici Map, «Syras»; Fra Mauro, «Ceraci» among the eight «kingdoms» of Persia, and «Sirax» (with Venetian *x* in value of *z*) as a town; Schiltberger, «Schiras»; Clavijo, «Xiras» (with Spanish *x* in value of *ʃ*); cf. HALLBERG, 470-471.

Širāz was a city of great fame; Rašidu-'d-Dīn and Waššāf name it along with Bagdad as an example of a very large town (cf. *Y*<sup>1</sup>, II, 178; III, 125). Cf. on it *Y*, I, 85-86; BARBIER DE MEYNARD, *Dict. hist.* 361-365; *EI*, notice «Širāz», by Cl. HUART; *LS*, 248-252; 295-298; *Mi*, 505.

Either Širāz or Sirāf has been supposed to have been mentioned in 1225 by Chao Ju-kua under the form 思蓮 Ssü-lien, after 白達 Po-ta which is Bagdad (see «Baudac»), and before 白蓮 Po-lien considered to be a transcription of Bahraïn or Baħaraïn (*HR*, 117, 122); but the transcription Ssü-lien would be a very bad one, and I suspect both Ssü-lien and Po-lien to be wrong duplicates of Po-ta itself, that is to say of Bagdad. In the account of Ch'ang Tê's mission of 1259, Širāz appears as 失羅子 Shih-lo-tzū (= Širāz, Široz), written 石羅子 Shih-lo-tzū in the corresponding text of *YS*, 149, 6 b (cf. *Br*, I, 144-145). I think Širāz is mentioned on the

Chinese map of c. 1330 and in the corresponding chapter of *YS*, 63, 16 *b*, not under the name of 泄刺失 Hsieh-la-shih as BRETSCHNEIDER supposed (see under «Cielstan»), but under that of 設刺子 Shê-la-tzü in which he saw the Šūlistān (cf. *Br*, II, 127-128). Shih-la-ssü is mentioned c. 1400 (cf. *Br*, II, 145). For the intercourse between 失刺思 Shih-la-ssü (Šīrāz) and China in the 15th and the first quarter of the 16th cents., cf. *Ming. shih*, 332, 8 *b*, *Br*, II, 292-294, and add *Y*<sup>1</sup>, I, 286 (T'ü Chi's paragraph on Šīrāz, 160, 24 *a*, contains several wrong forms).

## 202. ÇULCARNEIN

<i>chulitame</i> VA	<i>zul carmain</i> FA	<i>zulcarnen</i> TA <sup>1</sup> , R
<i>churzanen</i> V	<i>zul carman</i> FB	<i>zurleai</i> TA <sup>3</sup>
<i>çulcar ne in</i> F, L	<i>zulcarnei</i> LT	<i>zulearlle</i> VB
<i>culturi</i> VL, S		

Ar. ذو القرنين Dū-'l-Qarnāin, «Master of the Two Horns», an epithet of Alexander the Great (see «Alexandre<sup>1</sup>») popularized through its use in the Koran. It is, however, sometimes applied to other people as well. On the term, cf. *Y*, I, 160-161; NÖLDEKE, *Beiträge zur Geschichte des Alexanderromans* (in *Denkschr. d. k. Ak. d. W.*, Ph.-Hist. Kl., vol. 38 [1890], No. 5, 32-33). Mediaeval authors knew of Alexander's two horns, and gave symbolical explanations of them. For Roger Bacon, they figured the two kingdoms acquired by Alexander, Greece and Asia; another writer said they meant «power and knowledge» (cf. Ch.-V. LANGLOIS, *La connaissance de la nature*, Paris, 1927, 82).

## 203. ÇULFICAR

<i>çurficar</i> F	<i>surficar</i> FA	<i>zueficar</i> V
<i>oificar</i> VB	<i>vficar</i> LT	<i>zuficar</i> P, P <sup>6</sup> , TA <sup>1</sup>
<i>sufficar</i> FB	<i>yuficar</i> TA <sup>3</sup>	<i>zursichar</i> VA

In spite of «Çurficar» in F, Polo must have used «Çulficar», as he uses «Çulcarnein», and LT's «vficar» supports the correction. The name is of course Arabic ذو الفقار Dū-'l-faqār, vulg. Zulfiqār, «Master of the Vertebrae», the name of a sword taken by Mahomet from an Arab unbeliever; the sword passed from him to 'Alī. The name is fairly common, but Polo's «compains», since he was called «Zulfiqār», cannot have been a Turk from the eastern part of Chinese Turkistan; it is probable that he was a western Turk (from Russian Turkistan or even farther west), who had entered Qubilai's service and who came back to the Court after his three years' stay in the region of the T'ien-shan. It seems to be a safe conclusion that Polo never

visited the place, and his association with « Çulficar » must have occurred later on in China. From the context, and if we take into consideration the administrative customs of the time, it is probable that « Çulficar » was one of the foreigners whom Qubilai was so willing to appoint to fairly high posts, but that does not imply that he was more than a director of the local mines, just as Polo's three years' tenure of office at Yang-chou may have been simply in connection with the gabelle administration (see « Singiu »).

The exact location of the asbestos mines which were under « Çulficar »'s supervision during three years has not yet been determined. During the Mongol dynasty, the only known text which relates to the extraction of asbestos is the one which is met with twice in *YS* (6, 5 b, and 205, 1 b); from it we learn that, at Ahmad's request (see « Acmat<sup>1</sup> »), it was decided, on October 26, 1267, to extract asbestos in the 別法赤 Pieh-ch'ieh-ch'ih mountain (on this name, see LAUFER, in *TP*, 1915, 365-366; *Ch*, I, 149; the other form which embarrassed LAUFER, p. 365, is due to Ch'ien-lung's orthographical « reforms »). Pieh-ch'ieh-ch'ih is certainly a transcription, and supposes \*Bäkäči (the « red » [ch'ih] mountains of Pieh-ch'ieh spoken of by SCHLEGEL are just as wrong as his precise localization in Ssü-ch'uan is arbitrary; CHARIGNON's Bogda[-ōla] is even worse); and his foreign name suggests a probable localization out of China proper; it may or may not be the mountain mentioned by Polo.

The problem may however be approached from another angle. Polo says that the mountain where asbestos was found yielded also ordinary steel and « andanique » (*q. v.*) It is extremely tempting to see here the *pin-t'ieh* of the Chinese (cf. LAUFER, *Sino-Iranica*, 515-516; see « Andanique »); in 984, Wang Yen-tê, giving an account of his recent embassy to Kao-ch'ang (see « Carachoço »), says that there is *pin-t'ieh* at I-chou (= Qomul; see « Camul »). The mountain north of Beš-balıq which is mentioned by Wang Yen-tê and which yielded sal-ammoniac may also come under consideration; for a discussion of this point, see « Andanique » and « Ghinghin talas ».

In 1655, MARTINI inserted in his *Novus Atlas Sinensis* a paragraph on Mongolia, to which he gives the name of « Kingdom of Taniu » (see also the quotation which has passed into KIRCHER, *China Illustrata*, 1667, 206-207). YULE (*Y*, I, 215), who miscopied « Tangu », thought of the Tangnu-ōla, which cannot be a general name for Mongolia. Without being too positive, I think MARTINI, who was rather fond of archaisms, called Mongolia in this passage by the title of the ancient Hsiung-nu Emperors, 單于, read generally *shan-yü*, but for which *tan-yü* has often been used in the past (it has been revived in the extraordinary spelling *tan-hu* adopted by DE GROOT, *Die Hunnen des vorchristlichen Zeit*). It was in this great country of « Taniu », extending from Manchuria to Samarkand, that MARTINI mentions and describes a herb which passes through fire without suffering any change and which must be asbestos. MARTINI seems to have seen some of it, brought of course to China; and we may suppose, unless MARTINI's notice is second-hand and of an antiquarian character, that a certain quantity of asbestos still found its way from Mongolia to China in the second quarter of the 17th cent., although we are left in the dark as to the exact site where it was mined.