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The Problem of Early Loan-words in Chinese as Illustrated by the Word *p'u-t'ao*

Owing to some peculiar features of the linguistic structure of Chinese the problem of loan-words in that language is not only a problem of Chinese historical lexicology; it is also a problem of special — though negative — importance from the point of view of the theory of the structural system of the Chinese language. Most scholars working in the field of Chinese linguistics admit that Chinese has always been in some sense monosyllabic, although their opinions to this effect are at considerable variance in details. There is no need to enter upon these details here, the more so as I have already had the privilege of making known to linguists, both Chinese and Western, my own viewpoint in this respect¹. For our present purpose it will be sufficient to recall the fact — which to my mind cannot be put to doubt — that in Chinese syllabic speech units have always played a peculiar structural rôle, not only phonetical or phonological but also morphological as well as syntactic: in Chinese, as a rule, the segmentation of the given linguistic complex into syllabic units does coincide with the syntactico-morphological analysis of that complex, while in Indo-European languages, for example, — in view of their different structure in which syllables are merely phonetical and phonological units but not usually morphological ones — the segmentation of the given complex into syllables is a purely phonetical (phonological) operation having as a rule nothing to do with morphological analysis, the latter being independent of the syllabism².

¹ See in particular: *Han-yü-ti kü-fa ho hing-t'ai wen-t'i* 漢語的句法和形態問題, *Chung-kuo yü-wen* 中國語文 no. 30, 1954, pp. 7–11; *Shang-ku Han-yü-li-ti shuang-in-ts'i wen-t'i* 上古漢語里的雙音詞問題, *ibidem*, no. 52, 1956, pp. 23–25 (the latter article also summarised in *Pei-king ta-hüé hüé-pao* — *Jen-wen k'o-hüé* 北京大學學報, 人文科學, 1956, no. 1, p. 140); *The Problem of Syntax and Morphology in Chinese*, "Rocznik Orientalistyczny" XXI, 1957, pp. 71–84; *Remarques sur le problème des mots dissyllabiques en chinois archaïque*, "Mélanges publiés par l'Institut des Hautes Études Chinoises de l'Université de Paris", vol. I, 1957, pp. 423–445.

² In this connection I think it useful to refer the reader to the paper by J. Kuryłowicz, *Contribution à la théorie de la syllabe*, "Bulletin de la

Now, it goes without saying that loan-words introduced into Chinese from foreign languages had to be adapted to the exigencies of the Chinese phonetical and phonological system alone; mostly dissyllabic or polysyllabic in their original form, those words had to be cut to pieces and deformed in such a way as to fit the rigid phonetical and phonological frames of Chinese syllabic units, other structural features of the Chinese language — and especially the morphological rôle of the syllabic units — being necessarily disregarded. The result is that dissyllabic and polysyllabic loan-words in Chinese — unlike the bulk of purely Chinese words — being phonetically (phonologically) analysable into «purely Chinese» syllables, are at the same time non-analysable into syllabic morphemes and by this very fact seem to invalidate the all-important structural feature of the Chinese language. The latter difficulty, however, is merely apparent since phonetical transcriptions (or better: phonetical imitations) of polysyllabic foreign words are, after all, foreign words, not Chinese, and by this very fact cannot constitute a problem from the point of view of the Chinese linguistic structure³. Thus it becomes clear that as soon as a dissyllabic or polysyllabic lexical formation, apparently Chinese but non-analysable into syllabic morphemes⁴, has been recognised as a loan-word borrowed from a foreign language and proved to be one — the formation in question

Société Polonaise de Linguistique" VIII, Kraków 1948, pp. 80—114, which — although limited to the Indo-European material alone — may indirectly contribute to the elucidation of the problem of the structural rôle of syllabic units in Chinese as compared to the rôle of syllabic segmentation in Indo-European languages.

³ The non-relevancy of loan-words in the discussion of the linguistic structure of Chinese has been rightly stressed by Kao Ming-k'ai 高名凱 in his work on Chinese grammar *Han-yü yü-fa lun* 漢語語法論, Shanghai 1948, p. 35.

⁴ What I have in view is, of course, critical linguistic analysis, not the forced «etymologising» which the Chinese scholars of the past were so much fond of. Even the true loan-words were often subject to forced and uncritical explanations reminiscent of our 'popular etymology' and «analysed» into Chinese meaningful syllables (syllabic morphemes), cf. for instance Li Shī-chen's «etymology» spoken of *infra*, p. 16—17. What is more, in some cases the very Chinese written form of the loan-word shows that the word in question is generally but quite wrongly conceived — at least by those who are literate — as being composed of meaningful syllabic units, cf. the well-known examples: *k'u-li* 苦力 'coolie', *kü-lo-pu* 俱樂部 'club'. This perseverance in analysing polysyllabic lexical units — including some true loan-words — into syllabic morphemes itself constitutes negative evidence against the thesis put forward by an American scholar who says that "Chinese is at most a syllabic language, as are all other languages in the world" (J. De Francis, *Nationalism and Language Reform in China*, Princeton 1950, p. 156; italicised by me).

may be safely excluded from the discussion of the linguistic (morphological) structure of Chinese. On the other hand, as long as a dissyllabic or polysyllabic formation, non-analysable into syllabic morphemes, has not been proved to be of foreign origin — it constitutes an exception to the general principle of the Chinese linguistic structure, and therefore an exception which the linguist must reckon with and try to explain in some way⁵. Thus, it goes without saying that the preliminary task to be carried out in dealing with the problem of dissyllabic (polysyllabic) words in Chinese is to clear the field of research by removing from it all non-monosyllabic lexical formations of foreign origin. That is precisely why I consider such formations as being of 'negative importance' from the point of view of the theory of the structural system of the Chinese language. All this, of course, does not concern monosyllabic loan-words in Chinese, which, however, are very few in number. As is easily seen, they are indifferent from the point of view of the morphological structure of Chinese since they fulfil the requirement for syllabic units as being at the same time morphological units. The same may be said of specific formations composed of a monosyllabic root-morpheme borrowed from a foreign language and a purely Chinese auxiliary (suffixal or quasi-suffixal) morpheme — but I am not going to dilate on this problem here, the present paper being deliberately limited to non-monosyllabic loan-words in Chinese.

Words of foreign origin appeared in Chinese on a rather considerable scale first in connection with the introduction of Buddhism and its dissemination on Chinese soil. As is known, the introduction of Buddhism into China falls within the Han period, but it may be taken for granted that the invasion of Sanskrit Buddhist terms (I mean, of course, Chinese phonetic transcriptions of Sanskrit terms, as opposed to the use of purely Chinese words — Taoist terms in the first place — for the rendering of Buddhist notions) took place only later, in post-Han time. Independently of the invasion of Buddhist terminology, in connection with the growing military, political, commercial and cultural contacts between China proper and the outside world — especially since the T'ang period — there gradually appear in Chinese texts (which is not necessarily the same as the living Chinese language) a considerable number of foreign words from various Asiatic languages. In more recent times and down to our own days there have appeared in Chinese a lot of loan-words borrowed from European languages. Slightly simplifying the matter it may be said that the bulk of all these borrowings, various as they are in their chronology, origin and importance, have represented a comparatively easy task for philologists and linguists. There is no need to insist on the fact that most recent loan-words borrowed from European languages (and occasionally those

⁵ In this connection also the reader is referred to my above-mentioned paper, *Remarques sur le problème des mots dissyllabiques en chinois archaïque*.

borrowed from Japanese) are as a rule easily recognisable at the first glance. What is more, even with regard to much older Buddhist transcriptions from Sanskrit and transcriptions of words taken over from Tibetan, Uighur, Mongol, etc. since the T'ang period and later it may be said that, with comparatively few exceptions, there has been no special difficulty in tracing back their original forms. This, of course, does not mean that I underestimate the merits of those who have been dealing with the above-mentioned categories of foreign words in Chinese: I simply mean that in most cases here in question scholars have been able to arrive at positive and definitive results. It must also be stressed in this connection that very many transcriptions of foreign words (special terms, official titles, etc., not to speak of proper names) recorded in Chinese texts (Buddhist works, historical documents, etc.) have only philological interest. They do not concern Chinese historical lexicology since they have remained mere transcriptions created *ad hoc* and have never been used in the living Chinese language. Chinese lexicology *sensu stricto* is, of course, concerned with true loan-words which penetrated into the living Chinese language and it must be stated that true loan-words — especially those which have survived in the living language up to this day — are only a small part of foreign words recorded in various Chinese texts.

On the other hand, Chinese has also preserved to our own days some loan-words dating back to an epoch earlier than the invasion of Buddhist terms: the last centuries before and the first centuries of our era, approximately corresponding to the Han period. The formations in question — I call them early loan-words — form a special category characterised by specific difficulties and have constituted up to the present a delicate problem of Chinese lexicology. The specific difficulties of this problem consist not only in the scantiness and incompleteness of philological data but also in the fact that the original foreign forms of the loan-words here in question are not directly attested. The reason for this is that in most cases they must have belonged to languages (or dialects) unwritten and long extinct, and therefore they may be arrived at only through reconstructive research based on indirect data. Thus, dealing with the problem of early loan-words in Chinese involves both an extensive philological knowledge of early Chinese sources and a not less extensive knowledge of various ancient Asiatic languages (being the possible source of borrowings, or even only akin to those which may have been such a source) combined with the strict application of linguistic methods. Now, it may be said in advance that Western linguists dealing with the problem of early loan-words in Chinese often lacked sufficient scholarship in Chinese philology, while Chinese scholars — I chiefly mean those who worked with traditional methods — in spite of their enormous erudition in purely Chinese matters as a rule lacked both the knowledge of the respective ancient foreign languages and the strictness of the linguistic method. It goes without saying that this must have led in some

cases to the results which are either valueless abstract speculations incompatible with the philological data attested in Chinese texts, the chronology of texts in which the words in question are recorded, etc., or the equally unfounded and valueless «etymologising» having nothing in common with linguistic science. It also happens, as is still less excusable, that meritorious and important work already achieved in this field by some scholar of unusual competence has remained entirely unknown to another one who instead of pushing the research further on actually pushed it back by propounding a purely speculative and empirically unfounded hypothesis of his own. Finally, let it be remarked that the problem of early loan-words in Chinese, which we have already qualified as one of historical Chinese lexicology and one having 'negative importance' from the point of view of the theory of the structural system of the Chinese language, is also a problem of importance from the point of view of Chinese historical phonetics. It is clear that adequate identifications of early loan-words and their original foreign forms may throw additional light on some much-discussed points of early Chinese phonetics and furnish us with important external evidence, whether positive or negative, with regard to phonetic reconstructions based on Chinese material alone.

All these aspects of the problem of early loan-words in Chinese may be best exemplified by the word *p'u-t'ao* 葡萄 'vine, grape' — and this is precisely the reason why I have selected this word as the subject-matter of the present paper. First of all, it must be recalled that the problem of the origin of this word — first appearing in Chinese in the former (Western) Han period in connection with the great Chinese expeditions into Central Asia and attested for the first time by Sī-ma T's'ien 司馬遷 in his 'Historical Records' *Shī-ki* 史記, chapter 123 (where the word has the graphical form 蒲陶) was already correctly posed some forty years ago by the German-American scholar B. Laufer, one of those few unusual minds who combined an extensive knowledge of both Chinese sources and various Asiatic languages with a strict and critical methodology in approaching linguistic problems. In his book on Sino-Iranian cultural and linguistic relations — which actually goes far beyond the Sino-Iranian domain and which has remained the most systematic study in its field up to this day — Laufer was successful in ascertaining the Iranian origin of the Chinese form *p'u-t'ao*, although at that time it was not yet possible to solve the problem in its entirety⁶. It is only the progress achieved during the last decades in the fields of both Chinese historical phonetics and Iranian linguistics which allows us to push the research further on and improve on Laufer's etymology, to which I shall have to revert later

⁶ B. Laufer, *Sino-Iranica*, Chicago (Field Museum of Natural History) 1919; see the chapter on "The Grape-vine", pp. 220—245. Laufer believed that the original Iranian form was **budāwa*, cf. *infra*, pp. 36 and ff.

on. It is to be regretted, however, that *L a u f e r*'s recognition of the Iranian origin of the word in question, together with the pertinent source-material gathered by him and indeed his entire book, remained unknown to certain scholars, both Western and Chinese, who have tried to deal with the same subject. This ignorance of *L a u f e r*'s work is precisely the topic which I should like to illustrate first. Let us begin with Western linguists.

In a paper published in 1936 dealing with the problem of monosyllabism and polysyllabism as evidenced in the loan-words in Chinese and Japanese the well-known Swiss linguist *H. F r e i* — who noticed the phonetical (and graphical) resemblance between the words *p'u-t'ao* 葡萄 and *P'u-t'ao-ya* 葡萄牙, i.e., the Chinese adaptation of the country name 'Portugal' — judged it possible to put forward the following hypothesis: "Bien que le rapport entre les deux termes ne soit pas senti aujourd'hui, «raisin» peut provenir de «Portugal» si le fruit est originaire de ce pays, pour la même raison que l'orange a été appelée «portugaise» dans les pays qui l'ont reçue directement ou indirectement du Portugal". *F r e i*'s hypothesis is illustrated by Modern Greek *πορτοκάλι*, Rumanian *portocala*, etc.⁷ It is surely not necessary to say that if the author had any knowledge of *L a u f e r*'s book — not to speak of Chinese sources themselves — he certainly would never have formulated his purely speculative supposition, since he would then have known that the word he was dealing with does appear in Chinese ten odd centuries before the Chinese could have the slightest knowledge of Portugal and before the very establishment of that European kingdom as well. I am far from ignoring *H. F r e i*'s merits in the field of general linguistics but I think that this hypothesis of his is a striking illustration of the fact that even an expert linguist can not possibly approach the problem of loan-words in Chinese if his linguistic knowledge is not combined with a sufficient philological knowledge of Chinese source-material.

As another example we may cite the British linguist *R. A. D. F o r r e s t*. In his otherwise useful book, which is a kind of general survey of Chinese linguistics published as late as about thirty years after *L a u f e r*'s *Sino-Iranica*, *F o r r e s t* has been particularly unsuccessful in dealing with the problem of loan-words in Chinese. Curiously enough, the problem of loan-words is dealt with in the chapter on "Proto-Chinese", and it may be said in advance that much of what has been written by the author with regard to that problem

⁷ *H. F r e i*, *Monosyllabisme et polysyllabisme dans les emprunts linguistiques*, "Bulletin de la Maison Franco-Japonaise" VIII, no. 1, Tōkyō 1936, pp. 149 — 150. The paper has not been unknown in China since it was reviewed by *W a n g L i* 王力 in *Ts'ing-hua hñe-pao* 清華學報 vol. XII, no. 2, 1937, pp. 403—417.

is untenable and incompatible with philological data⁸. I cannot here enter upon all the particulars involved and shall limit myself, of course, to what is said by the author with regard to the word *p'u-t'ao* alone. Now, Forrest does not venture to suggest a hypothesis of his own and actually considers the problem unsolved; he only recalls the obsolete hypothesis deriving the word *p'u-t'ao* from the Old Greek word βότρυς 'bunch of grapes' which he erroneously ascribes to H. A. Giles⁹. Forrest himself does not attach much faith to the latter hypothesis but the reason for his misbelief in the certainly unfounded formula 葡萄 = βότρυς is of a very strange kind. The only obstacle against accepting the above formula he sees in the pretended fact that the word *p'u-t'ao* is "difficult to dissociate" from the word *p'u-t'i-tsi* 菩提子 which latter — according to Forrest — interchanges in dialects with the word *p'u-t'ao* in the sense of 'grape' as well as in that of 'rosary beads' and in which the dissyllabic element *p'u-t'i* 菩提 is duly recognised as the Chinese adaptation of Sanskrit *bodhi*¹⁰. Thus, the only conclusion to be drawn from the author's argumentation — which, however, has not been explicitly drawn by himself — is that he suspects the Chinese form *p'u-t'ao* of being connected in some way with Sanskrit *bodhi*. Now, leaving aside the question whether the form *p'u-t'i-tsi* does really occur in Chinese dialects in the sense

⁸ R. A. D. Forrest, *The Chinese Language*, London 1948; see pp. 119 — 120. The book was reviewed from the sinological point of view by P. Demiéville, "Bulletin de la Société de Linguistique de Paris" XLV, 2, pp. 267—273; most etymological problems, however, were omitted from the review.

⁹ See H. A. Giles, *A Chinese-English Dictionary*, 2nd ed., 1912, no. 9497. But as had already been pointed out by Laufer, *Sino-Iranica*, p. 225, the attempts to derive the word *p'u-t'ao* from the Greek βότρυς were made first by W. Tomaschek as early as 1877 and then by T. Kingmill in 1879. It is also Laufer, *l.c.*, who refuted that hypothesis for both linguistic and historical reasons.

¹⁰ A slip, however, is to be noted also with regard to the latter question since Forrest explains (p. 120 of his book) *bodhi* simply as 'pipul, false banyan tree' — which seems to be due to the confusion of Sanskrit *bodhi* (fem.) 'awakening, enlightenment' and its compound *bodhidruma* or *bodhivṛkṣa* (both masc.) 'tree of enlightenment (*Ficus religiosa*)', the latter commonly rendered in Chinese as *p'u-t'i-shu* 菩提樹. It is true that the word *bodhi* may appear by itself as a masculinum in the sense of 'tree of enlightenment' but the phenomenon is secondary and due to ellipsis of the second member of the compound; in Chinese, at any rate, *p'u-t'i* = *bodhi* actually means only 'awakening, enlightenment' (= *küe-wu* 覺悟). It may be added that the Sanskrit word *pippala* 'pipul' has its direct phonetic rendering in Chinese too: *pi-po-lo* 畢鉢羅, attested since the early T'ang period by Hsüan-tsang 玄奘 in his *Si-yü-ki* 西域記 where *p'u-t'i-shu* is stated to be the *pi-po-lo*-tree (畢鉢羅之樹).

p'u-t'ao had already been borrowed from Iranian as early as the 2nd century B.C. while *p'u-t'i* = Sanskrit *bodhi* must have been introduced into Chinese only much later, viz. in connection with the invasion of Buddhist terms, most probably not earlier than about the year 400 of our era¹². Needless to say, Forrest also is ignorant of Laufer's book, which he omits in the bibliographical list attached to his chapter on "Proto-Chinese".

gestion is the more important as the Tibetan form here in question seems to be independent of Sanskrit *bodhi*, which term, as is known, is rendered in Tibetan non-phonetically: *byan-čhub*. Thus, if the Chinese form *p'u-t'i-tsi* (together with Sino-Japanese *bodaishi*) does really come from Tib. *bo-de*, it is evident that the element *p'u-t'i-* in *p'u-t'i-tsi* — despite all subsequent associations — is genetically not *p'u-t'i* = *bodhi* but a secondary phonetic substitution for the Tibetan word. Japanese lexicographical compendia to which I could have access generally agree — except in some minor details — with *Hôbôgirin* both in giving *bodaishi* a botanical sense (they consider it a kind of fruit) and in deriving the term itself from Tibetan. Let it be sufficient to quote as a single example the dictionary *Genrin* 言林 by Shimura Izuru 新村出 (Kyôto, 24th year of Shôwa) where *bodaishi* is explained, p. 2100, as the name of the fruit of a tree growing in the mountainous regions of India (?) and called *bode*. Irrespective of the problem of an adequate etymology of the Chinese word *p'u-t'i-tsi* — a problem which does not seem to have been definitively solved — it seems certain that the word in question is a popular botanical term of rather vague extension applied in the course of time to different plants (and their fruit) of which rosary beads were made, but not to rosary beads themselves. At any rate, I must stress that in no Chinese nor Sino-Japanese lexicographical work have I come across an explanation or quotation from earlier sources to the effect that *p'u-t'i-tsi* itself may mean 'rosary beads'.

¹² Being myself not a Buddhologist and having had direct access only to second-hand information, I have not been able to trace exactly the earliest occurrence of the word *p'u-t'i* (Ancient Chinese **b'uo-d'iei*, Sino-Japanese *bodai*) = Sanskrit *bodhi* in Chinese sources. *Hôbôgirin* s.v. *bodai*, p. 85 ff., despite its rich documentation gives no exact indications in this respect and Chinese lexicographical compendia commonly used, as is known, are not very reliable with regard to Buddhist terminology. Thus, among the Chinese Buddhist texts to which reference is given in *Ts'ï-hai* s.v. *p'u-t'i* and its compounds, the earliest one (quoted s.v. *p'u-t'i-sin* 菩提心) is *Kûan wu-liang-shou fo king* 觀無量壽佛經 = **Amitâyurbuddhānusmṛtisūtra* translated into Chinese by Kālayāśas (Chinese name: Kiang-liang-yeshê 量耶舍 between 424 and 442 (cf. P. Demiéville, *La Yogācārabhūmi de Saṅgharakṣa*, "Bulletin de l'École Française d'Extrême-Orient" XLIV, fasc. 2, 1954, p. 354) while Chu K'ï-feng, *Ts'ï-t'ung*, p. 387, s.v. *p'u-t'i* has a quotation from *Kin-kang king* 金剛經 = *Kin-kang pan-jo po-lo-mi king* 金剛般若波羅蜜經, i.e., the Chinese translation of *Vajracchedikā-prajñā-pāramitā*. The latter work was translated into Chinese not less than six times at different epochs, the first version being due to Kumārajīva (about 400 A.D.), the second one to Bodhiruci (Chin-

of 'grape'¹¹, it must be said that not only is it not "difficult to dissociate" *p'u-t'ao* and *p'u-t'i-tsi* but — contrarily to what the British linguist maintains — it is plainly impossible to associate the two words genetically in any way since

¹¹ I think that Forrest's statement to this effect must likewise be due to some misunderstanding. I cannot find, in either printed sources or oral information from Chinese scholars consulted by me, any confirmation of the word *p'u-t'i-tsi* as interchanging in dialects with the word *p'u-t'ao*. The only exception I know of is the recent big Japanese dictionary *Daijiten* 大辭典 edited by Shimomaka Yasaburō 下中彌三郎, vol. 22 (Tōkyō, 28th year of Shōwa), p. 314, s. v. *budō* (= Chin. *p'u-t'ao*) 蒲陶, where *budō* (= *p'u-t'ao*) 蒲陶, *yabudō* (= *ye-p'u-t'ao*) 野葡萄, and *bodaishi* (= *p'u-t'i-tsi*) 菩提子 are quoted as 'Chinese names' of the plant in question. But this very unmethodical enumeration (where 蒲陶 is but another graphical form — the earliest one — of 葡萄 while *ye-p'u-t'ao* does, of course, designate the wild variety of the plant having also other names in Chinese and generally treated by Chinese scholars as something quite different from the cultivated *p'u-t'ao*) speaks itself against its being reliable. In the same dictionary, by the way, s. v. *bodaishi* (*p'u-t'i-tsi*) 菩提子, vol. 23, p. 239, there is no trace of the word in question as being associated in any way with *budō* (*p'u-t'ao*). On the other hand, it cannot be denied that 'grapes' and 'beads' — both the things and their names — may be associated in some way, and there are good proofs that they were so in Chinese. Thus, for example, it is reported that in Hang-chou some varieties of grape were styled *chu-tsi* 珠子 'beads' (cf. Laufer, *Sino-Iranica*, p. 229) and Li Shī-chen in the 33rd chapter of his *Pen-ts'ao kang-mu* (cf. *infra*, p. 17, footnote 13) gives among the synonyms of *p'u-t'ao* a trisyllabic expression designating the round variety of grape and containing the word 'beads', viz. *ts'ao-lung-chu* 草龍珠 — but all this has nothing to do with *bodhi*. — Furthermore, even the meaning 'rosary beads', unconditionally attributed in some Chinese-European dictionaries and Forrest's book to the word *p'u-t'i-tsi*, seems very doubtful as will be seen from the following remarks. As is known, the common Chinese word for 'rosary (beads)' is *nien-chu* 念珠 or *su-chu* 素珠 (variant form *shu-chu* 數珠, as e.g. in *Pen-ts'ao kang-mu*, see below) while *p'u-t'i-tsi* is referred to by Li Shī-chen (*Pen-ts'ao kang-mu*, chapter 35) only as a botanical term, viz. one of the variant names of the tree *wu-huan-tsi* 無患子, i.e., *Sapindus mukorossi* of the genus *Sapindaceae*; Li Shī-chen also explains the name in the following terms: 'the Buddhists use it for making rosary beads (*shu-chu*) — hence its name *p'u-t'i-tsi*' 釋家取爲數珠, 故謂之菩提子 — but this is by far not the same as calling rosary beads themselves *p'u-t'i-tsi*. It is certain, however, that the term was also applied to other plants and their fruit or seeds which were used for making rosary beads. Thus, in the *Hôbôgirin* (1st fascicule, Tōkyō 1929, p. 91) the Sino-Japanese form *bodaishi* (= *p'u-t'i-tsi*) is explained as „espèce de graines dont on faisait des chapelets” and the term itself is supposed to have some connection with Tibetan *bo-de* „nom d'un arbre dont les fruits servent à faire des chapelets”. The latter sug-

of the word *p'u-t'ao*. In his well-known work *Pen-ts'ao kang-mu* 本草綱目,¹³ chapter 33, under the word in question (p. 54) Li Shī-chen writes: 可以造酒入醕, 飲之則醕然而醉, 故有是名 'it can be used for making wine of it which is served at drinking parties (*p'u* 醕); on drinking it one becomes greatly intoxicated (*t'ao* 醕) — and that is why it has its name' — thus having evidently in view that 葡 = 醕 'drinking-bout, drink much wine' and 葡 = 醕 = 醕 醕 'greatly intoxicated'.¹⁴ Of course, we are not going to reproach a scholar of the XVIth century for such a 'popular etymology', which is a mere playing with written characters. Li Shī-chen's «explanation» is quoted here only *à titre de curiosité*.

Certainly much less excusable is what I find with regard to our problem in the otherwise very useful repertory (already referred to *supra* on another occasion) of the variant forms of Chinese dissyllabic expressions, i.e., *Ts'i-t'ung* 辭通, compiled by the well-known modern Chinese scholar Chu K'i-feng 朱起鳳 and published as recently as 1934. Now, Chu K'i-feng does not venture to give an etymology of his own and he leaves the origin of the term *p'u-t'ao* unexplained, but he commits a grave error with regard to our problem insofar as he considers the loan-word *mo-t'o* 末陀, which he had picked in

¹³ Already quoted on various occasions, cf. *supra*, p. 14 footnote 11. I have used the recent edition in 6 vols. (and index) of the *Shang-wu in-shu kuan*, Shanghai 1954. The work was printed for the first time towards the end of the XVIth century.

¹⁴ Cf. also Laufer, *Sino-Iranica*, p. 225, footnote 4. The characters *p'u* 醕 and *t'ao* 醕 involved in Li Shī-chen's «etymology» are not very common and deserve some additional remarks. 醕 is attested already in Archaic Chinese as it appears in the *Chou-li* — but it is used there as a *kia-tsie* 假借 for the homophonic monosyllable designating a malevolent deity; in sense of 'drinking-bout' (or, as the *Shuo-wen* defines it, *ta-in-tsiu* 大飲酒) it is attested only since the Han period (Sī-ma Ts'ien's *Shi-ki*). 醕 is of much later date, as it is attested only since the T'ang epoch (in a poem by Po Kū-i 白居易) and later on in the lexicographical works *Kuang-yün* and *Tsi-yün* as well as in a poem by the Sung poet Ch'ao Pu-chī 晁補之. Most important, however, is the fact that *t'ao* was never used as an independent monosyllabic word nor even as a semi-independent semanteme, but only in the dissyllabic reduplicated formation *t'ao-t'ao* 醕醕 (Po Kū-i) or the equally dissyllabic formation of the *tie-yün* type *mao-t'ao* 醕醕 (*Kuang-yün*, Ch'ao Pu-chī), the latter formation being considered by Chinese lexicographers as an 'impressive' word for 'greatly intoxicated'. Thus, it is evident that the «adverbial» formation *t'ao-jan* 醕然 — where *t'ao* contrarily to its real quality of a specific syllabic element in 'impressive' words is put in the rôle of a semanteme — was coined *ad hoc* by Li Shī-chen, the better to serve his purpose. All this shows, of course, the over-strained artificiality of the «etymology» proposed by Li Shī-chen.

Proceeding to what has been done in China with regard to our problem I shall start with Li Shī-chen 李時珍, the famous scholar of the Ming period who was probably the first one to offer a tentative etymology

ese name: P'u-t'i-liu-chī 菩提流支 — which is not without interest to our problem) working in China shortly after 500 A.D. The first version is also considered the most widely current and the quotation given by Chu K'i-feng probably refers to it. On the other hand, it is known that the Indian-Central Asian monk Kumārajīva (Chinese name: Kiu-mo-lo-shī 鳩摩羅什) who flourished in China about the year 400 of our era wrote much on p'u-t'i-sin 菩提心 = bodhicitta (cf. L. Wieger, *Histoire des croyances religieuses et des opinions philosophiques en Chine*, 3rd ed., Hienhien 1927, p. 426) and translated the treatise of which the Chinese title is *Fa p'u-t'i-sin king* 發菩提心經. Thus the existence of the term p'u-t'i in Chinese in Kumārajīva's time seems assured, although even in that period it must have been not very common. It may be added in this connection that P'ei-wen yün-fu s.v. p'u-t'i-sin gives only a quotation from a poem by Emperor Yang 煬帝 of the Sui dynasty who reigned in the beginning of the 7th century. — I also learn from H. Hackmann, *Chinesische Philosophie*, München 1927, p. 246, that chapter XIX of the *Sī-shī-er chang king* 四十二章經 — which work is considered to have been compiled as early as the second half of the first century of our era and to be the first Chinese text on Buddhism — is concerned with *bodhi*. Having no access to the text in question, I cannot see how the term is rendered there, but I think that p'u-t'i is plainly impossible at so early an epoch. As is known, Chinese Buddhist terminology was for centuries under the strongest influence of Taoist vocabulary and *bodhi*, for example, was often translated up to Hüan-tsang's time as *tao* 道 (or *chī* 知, etc.). By the way, the very reliability of the *Sī-shī-er chang king* is not unquestionable (on controversies concerning that text see, e.g., T'ang Yung-t'ung 湯用彤, *Han Wei liang Tsin Nan-pei-ch'ao fo-kiao shī* 漢魏兩晉南北朝佛教史, vol. I, 1955, pp. 31—46, and also by the same author *The Editions of the Ssū-shih-êrh-chang-ching*, translated by J. R. Ware, "Harvard Journal of Asiatic Studies" I, 1936, pp. 147—155). Quite especially the reliability of early Chinese Buddhist texts for lexicological investigations seems very doubtful since it is known that early texts now extant must have often undergone subsequent terminological normalisation (cf. P. Demiéville, *op. cit.*, p. 342, footnote 4). In conclusion, I think that the existence of p'u-t'i = *bodhi* in Chinese in Kumārajīva's time, i.e., about 400 A.D. is to be taken for granted. This is, of course, a *terminus ante quem* which on closer inspection by Buddhologists may be slightly pushed back, but I do not believe that it can be placed long before Kumārajīva. One will note, however, that even the earliest time limit theoretically possible, i.e., the second half of the first century, is some two hundred years later than the appearance of the word p'u-t'ao in Chinese. It goes without saying that the secondary application of p'u-t'i to render Tibetan *bo-de* — if the etymology of the form p'u-t'i-tsī spoken of *supra*, p. 14 — 15 in footnote 11 may be admitted — must have been of much later date than the appearance of the word p'u-t'i = *bodhi* in Chinese.

the *I-ts'ie king in-i* 一切經音義¹⁵, as a variant form of *p'u-t'ao*. He expressly says, *Ts'ï-t'ung*, p. 718, s.v. *mo-t'o*: 葡末聲之轉, 葡之爲陀, that is to say, 末 is set forth as a supposed 'derivate' of 葡, and 陀 as a phonetic variant of 葡; Chu K'i-feng seeks to corroborate this purely speculative statement of his by quoting (p. 719) a two-line passage from a T'ang poem¹⁶ containing the word *p'u-t'ao* in which the character *p'u* 葡 for some

¹⁵ As is known, there are two different works commonly styled *I-ts'ie king in-i*, the first (in 25 *küan*) by H ü a n - i n g 玄應 and the other (in 100 *küan*) by H u i - l i n 慧琳 — both of the T'ang period. Chu K'i-feng, *Ts'ï-t'ung*, p. 718, s.v. *mo-t'o*, gives a reference to the 13th chapter of *I-ts'ie king in-i* without indicating which of the two works is concerned, but I think that the earlier one, i.e., H ü a n - i n g 's work is probably meant. If so, the reference seems to be erroneous, since I could not trace *mo-t'o* as a separate item in the chapter indicated. As far as I know (cf. also L a u f e r, *Sino-Iranica*, pp. 240—241) the word in question does appear — along with two other loan-words from Sanskrit: *su-lo* 窣羅 = *surā* and *mi-li-ye* 迷麗邪 = *maireya* — but only in the 24th chapter of H ü a n - i n g 's *I-ts'ie king in-i*, where it is correctly explained as follows: 末陀謂蒲萄酒也 and not 末陀酒謂蒲桃酒 as stated by Chu K'i-feng, *l.c.* The difference is of importance since *mo-t'o* by itself designates a kind of wine (and precisely the 'grape-wine', *p'u-t'ao-tsiu*) while *p'u-t'ao* in Chinese designates only the fruit from which wine is made. Thus, the explanation given in H ü a n - i n g 's work is strictly correct (*mo-t'o* = *p'u-t'ao-tsiu*) whereas the quotation contained in the *Ts'ï-t'ung* not only disagrees with H ü a n - i n g 's text, but is also incorrect *in re* (*mo-t'o-tsiu* = *p'u-t'ao-tsiu*, implying *mo-t'o* = *p'u-t'ao*). It seems that Chu K'i-feng has «improved» H ü a n - i n g 's text to make it suit his own purpose better, although it is not impossible that the «improvement» may be due to H u i - l i n, if Chu K'i-feng quoted from the latter. To H u i - l i n 's *I-ts'ie king in-i* I have not had access.

¹⁶ I do not know why Chu K'i-feng gives no exact reference and introduces the quotation rather vaguely with the words 唐人詩云. The two-line passage cited by Chu K'i-feng is also reproduced in the *K'ang-hi ts'ï-tien* 康熙字典 s.v. 蒲 and it is stated there that the quotation is from the *Kie-in pi-ki* 芥隱筆記 — a work compiled in the Sung period by K u n g I - c h e n g 龔頤正. On the other hand, the words *Lo-t'ien sh'ï* 樂天詩 introducing the quotation in the latter work (and in the *K'ang-hi ts'ï-tien*) clearly refer to a poem by P o K ü - i, *Lo-t'ien* being the *ts'ï* 字 of the famous T'ang poet. In fact, as Mr. P a i T u n - j e n 白敦仁, University of Warsaw, kindly points out to me, the passage in question occurs in a poem written by P o K ü - i (the title of the poem, amounting to 49 characters, is too long to be cited here), cf. *Po-sh'ï Ch'ang-k'ing tsi* 白氏長慶集 (*S'ï-pu ts'ung-k'an* ed.), *küan* 67, f. 2 r. It is to be noted that in both *Kie-in pi-ki* and *K'ang-hi ts'ï-tien* (which latter, as already stated, gives the quotation s.v. 蒲) the word is written 蒲萄 — just as it stands in the original poem —

obscure reason (but *cf. supra*, footnote 16) is stated by C h u K ' i - f e n g to be read as *mo* 末. Needless to say, all this has nothing to do with linguistic method. The case of *mo-t'o* 末陀 — Ancient Chinese reading of the early T'ang period: **muât-d'â* — is easily explained in a quite different way: it is a borrowing from Sanskrit corresponding to either *madhu* 'wine' (as already suggested by L a u f e r), or rather *mada* 'drunkenness; alcoholic drink' (as

whereas C h u K ' i - f e n g has 葡萄. The whole poem being composed in lines of five syllables, the word *p'u-t'ao* in the line in question — the whole of which runs: 燕姬酌蒲萄 — evidently is an ellipsis for *p'u-t'ao-tsiu* (*cho p'u-t'ao* 'pours grape(-wine)', instead of *cho p'u-t'ao -tsiu*), an ellipsis due to the requirements of the metre. C h u K ' i - f e n g's speculative reading 葡 (蒲) = 末 probably is due to this unusual application of the word *p'u-t'ao* as replacing here the compound word *p'u-t'ao-tsiu* — which latter term, as we know, semantically corresponds with *mo-t'o*. In other words: I guess that C h u K ' i - f e n g interpreted the rather unusual phrase of the poem *cho p'u-t'ao* not as an ellipsis for *cho p'u-t'ao-tsiu* (which certainly is the only correct way) but as *cho mo-t'o* 'pours (grape-)wine'. Moreover, in view of the fact that the poem belongs to the genre known as *lü-shi* 律詩 or 'regulated poetry', it is also probable that C h u K ' i - f e n g noticed in the case in question the violation of the well-known rule applying to this kind of poetry and requiring the second and fourth syllable of the line to be of different tone-categories. As is known, the four 'classical' tones of Chinese were defined by S h e n Y a o 沈約 (441—513) as *p'ing-sheng* 平聲, *shang(-sheng)* 上, *k'ü(-sheng)* 去 and *ju(-sheng)* 入; for prosodic purposes these four tones were further classified into two tone-categories, that of 'level tone' or *p'ing-sheng*, and that of 'oblique tone' or *tsê-sheng* 仄聲, the latter embracing the *shang*, *k'ü* and *iu* tones. Now, in the line under discussion both the second (姬) and fourth character (蒲) are *p'ing-sheng*, i.e., belong to one tone-category — which goes against the versificatory rule just mentioned. Thus, C h u K ' i - f e n g may have seen additional reason for reading 葡 (蒲) as 末 in the fact that 末 (Ancient Chinese **muât*) is a *ju-sheng* character (i.e., one belonging to the *tsê-sheng* tone-category) fitting in the prosodic scheme of the poem, and may have believed that his reading saved the versificatory rule violated by the poet. It may be added that the compilers of the *K'ang-hi tsi-tien* (s.v.) must have followed a similar line of reasoning as they ascribed the *k'ü-sheng* tone-value (also belonging, as already stated, to the *tsê-sheng* category) to the character *p'u* in the line under discussion. To my mind, all this shows the unreliability and fallaciousness of all reasoning uncritically operating with notions of traditional Chinese phonology and prosody. On the other hand, it is known that the prosodic rules were often violated by the T'ang poets, and it goes without saying that in the case in question we simply have to do with a 'defect' in versification which certainly cannot be saved by speculations like the one propounded by C h u K ' i - f e n g.

I think, because of the closer phonetic resemblance). Obviously, there is no reason whatever to connect *mo-t'o* with the much earlier loan-word *p'u-t'ao*.

The only Chinese contribution I know of which is of some importance in relation to our problem is the learned article '*P'u-t'ao*' *yü-yüan shih-t'an* 「葡萄」語源試探 by the contemporary Chinese historian (of the Nan-k'ai University in T'ien-tsin) Yang Ch'iu-kiu 楊志玖¹⁷. The paper — which has been published in an obscure and short-lived provincial review — is little known even in China and I think that it must have remained entirely unknown among Western Sinologists. Thus, a more extensive critical survey of the paper in question, supplemented by some references lacking in the paper itself, will not be out of place here. The paper is divided into four short chapters which are concerned with: 1) the author's critical review of the earlier attempts to give an etymology of the Chinese form *p'u-t'ao*; 2) the country-name *Pu-t'ao* 樸桃¹⁸ of the *Han-shu* 漢書; 3) the problem of viticulture in *Pu-t'ao-kuo* 樸桃國; and 4) a concluding chapter in which the author's own hypothesis deriving the word *p'u-t'ao* from the country-name *Pu-t'ao* is put forward and defended. It is to be emphasised that the author gives evidence of being perfectly well acquainted with the Western literature on the subject — not to speak, of course, of Chinese sources. He starts his paper by quoting T'o -

¹⁷ Printed in the review *Chung-hing chou-k'an* 中興周刊, no. 6, Ts'ing-tao 1947, pp. 11—14. It is regrettable that the article was published without its 31 footnotes. The omission, which has deprived it of nearly all necessary references, is apologised for by the author in a notice appended to the printed text. I am greatly indebted to Prof. Kao Ming-k'ai for first drawing my attention to the article in question, and to the National Library in Peking for having put at my disposal their copy of this rare publication, difficult to obtain even in Peking.

¹⁸ Yang Ch'iu-kiu's article being written in Chinese, the author has believed it possible to do without transcription (in either Ancient or Modern Chinese) and gives all Chinese terms involved in his reasoning in characters only. This, however, must have led to a considerable and unallowable simplification of the subject-matter, unnoticed by the author himself. As will be seen later on, one of the main tenets in Yang Ch'iu-kiu's hypothesis is the supposed phonetic equation 蒲 = 樸, implying that he must have read the character 樸 here in question as *p'u* (initial aspirated) and the country-name 樸桃 as *Pu-t'ao*. In anticipation of what will be said later with regard to the untenableness of the equation because of the difference of finals in the characters involved (see *infra*, p. 27—28) let it be remarked in this place that the reading *p'u* of the character 樸 here in question, i.e., as a part of the country-name 樸桃, although theoretically possible since it is one of the several variant readings of that character, is not consistent with Yang Ch'iu-kiu's own argumentation put forward in the 2nd chapter of his article. On the contrary, there are good reasons for interpreting the country-name 樸桃 as *Pu-t'ao* (*pu*, initial unaspirated). Cf. also *infra*, p. 22—23, footnote 20, and p. 23—24, footnote 21.

maschek's hypothesis (already referred to *supra*, p. 13, footnote 9) deriving *p'u-t'ao* from Greek βότρυς and also pays some attention to Laufer's derivation of the word from Iranian, an hypothesis which he even considers more plausible than the former, although he disapproves of both. However, Yang Chī-kiu must be reproached for having unjustifiably simplified Laufer's viewpoint. The latter's idea was, of course, that Chinese *p'u-t'ao* was of Iranian origin, which has been wrongly interpreted by Yang Chī-kiu as if Laufer had considered the Chinese form in question to be of Persian origin. Of course the hypothetical form **budāwa* — of which we shall have to speak later on — could not be and was not conceived by Laufer as a Persian word but only as an Iranian one or, strictly speaking, Ferghanian, whereas Yang Chī-kiu believes to have found the common starting point of both Tomaschek's etymology and that put forward by Laufer in the pretended fact that both scholars did presuppose a «borrowing from either Greek or Persian» as the origin of the Chinese word (二者蓋先假定中國葡萄一詞, 係借自希臘或波斯 — as is expressly stated in the article). This misinterpretation of Laufer's idea must have had considerable influence on Yang Chī-kiu's own conception since the preliminary point in his argumentation on behalf of his own hypothesis is the question whether it is really necessary to take for granted that Chinese *p'u-t'ao* was a borrowing from either Greek or Persian. The answer to such a question must, of course, be in the negative — which, as Yang Chī-kiu thinks, does away with the principal premise on which both Tomaschek and Laufer built their etymologies and, consequently, with the respective etymologies as well. All this, of course, is due to a misunderstanding, and the question itself has been put in an incorrect way. To put it correctly, Yang Chī-kiu should have asked: Is it necessary to take for granted that Chinese *p'u-t'ao* was of Iranian origin? — and here according to what is known from both Chinese sources and Iranian linguistics the answer, to my mind, may only be in the positive. Having got rid of the ideas of his predecessors the author recalls the well-known fact that imported plants and fruits are often called after the name of the country they have come from. Yang Chī-kiu — somewhat in the line of H. Frei's reasoning spoken of *supra*, p. 12 — does in fact suppose the same with regard to Chinese *p'u-t'ao*. He believes the word to have had its origin in the country-name *Pu-t'ao* 樸桃 recorded in the *Han-shu*.

In chapter 2 the country-name *Pu-t'ao* and the geographical localisation of *Pu-t'ao-kuo* are discussed. Now, the actual form of that name recorded in the editions of the *Han-shu*, *küan* 96 *shang*, is *P'u-t'iao* 撲挑¹⁹. Yang Chī-kiu points

¹⁹ The name does occur in the passage concerned with the country *Wu-i-shan-li* 烏弋山離 of which *P'u-t'iao* (*Pu-t'ao*) was the northern neighbour.

out, however, that there are good reasons to read it 樸桃. It was the Sung scholar Sung K' i 宋祁 (XIth century) who left a gloss saying that he had seen 桃 instead of 挑 in a copy of the *Han-shu* extant at his time, and the same character 桃 is also attested in a quotation from the corresponding passage of the *Han-shu* inserted by Ma Tuan-lin 馬端臨 (XIIIth century) in his *Wen-hien t'ung-k'ao* 文獻通考. On the other hand, the Ts'ing scholar Sū Sung 徐松 (1781–1848) in his *Han-shu Si-yü-chuan pu-chu* 漢書西域傳補注 suggested that 樸 in the country-name in question might be a mistake for 樸. Yang Ch' i-kiu adopts this suggestion and seeks to corroborate it by comparing the various *fan-ts'ie* 反切 of the characters in question²⁰. What seems most decisive, however, is the fact that the whole

See p. 0607₁ of the *K'ai-ming shu-tien* edition of the *Er-shih-wu shi* 二十五史. Let it be remarked at once that the character 挑, generally read *t'iao*, may also have the reading *t'ao* — which makes an impression as if 挑 and 桃 might be strictly homophonic. There was, however, a marked phonetic difference between *t'ao* 挑 and *t'ao* 桃 in both Archaic and Ancient Chinese in which 挑 had the voiceless initial *t'* while 桃 had the voiced one *d'*, cf. B. Karlgren, *Grammata Serica*, series no. 1145. The characters 樸 and 樸, too, cannot be considered as homophonic since 樸 had several variant readings which 樸 had not, see *Gram. Ser.*, series no. 1211. Thus, it becomes clear that the emendation proposed by Yang Ch' i-kiu is not a merely graphical one but also has some bearing on the phonetic aspect of the problem. On the other hand, as has already been remarked *supra*, p. 20, footnote 18, and will be seen later on, not all phonetic implications of the emendation put forward by Yang Ch' i-kiu actually agree with his own argumentation.

²⁰ Without entering upon the rather complicated details let it be said that both Sū Sung and Yang Ch' i-kiu build on critical analysis of a phonetic gloss left by the well-known commentator of the *Han-shu*, Yen Shih-ku 顏師古 (early T'ang period), who says: 樸音布木反. Yen Shih-ku's *fan-ts'ie* seems to be inadequate since it implies unaspirated initial *p* in 樸 while other T'ang sources and the *Kuang-yün* 廣韻 indicate aspirated initial *p'* for the same character. That is why Sū Sung concluded that either Yen Shih-ku's *fan-ts'ie* must be a mistake for 普木 (which latter is the *fan-ts'ie* of 樸 given in the *Kuang-yün*) or the character 樸 itself must be a mistake for 樸 (which in one of its readings does agree with Yen Shih-ku's phonetic gloss). Yang Ch' i-kiu approves of the latter suggestion precisely because of the fact that according to the *Kuang-yün* (cf. Chou Tsu-mo 周祖謨, *Kuang-yün kiao-pen* 廣韻校本, 1951, vol. V, *küan* v, f. 3v) 樸 has only 普木 as its *fan-ts'ie* — which is incompatible with Yen Shih-ku's gloss, — while in the same *Kuang-yün* the character 樸 is put under three different *fan-ts'ie* one of which, viz. 博木 (see *ibidem*) is phonetically the same as Yen Shih-ku's 布木. From this Yang Ch' i-kiu concludes that what Yen Shih-ku must

dissyllabic form 樸桃 is actually attested as early as the T'ang period in the *T'ung-tien* 通典, a work compiled about 800 A.D. by Tu Yu 杜佑, where there is a quotation concerning *Wu-i-shan-li* drawn from the *Han-shu*. Thus, there is no difficulty in accepting Yang Ch'iu's conclusion to the effect that originally and up to the T'ang period the country-name in question was written 樸桃 which only subsequently — and mistakenly — was changed to 撲挑. Let it also be stressed that this graphical emendation involves a phonetic one which in its second part, i.e., as far as the second member of the country-name is concerned, may easily be put in accord with Yang Ch'iu's etymological conception; on the other hand, with regard to the first character — as has already been remarked — the phonetic implications of the emendation run against the author's own hypothesis. It is the previously mentioned Ts'ing scholar Sū Sung who seems to be the first to have identified the name *Pu-t'ao* just spoken of as being the same as the country-name *Pu-ta* 濮達 recorded in the *Hou-Han shu* 後漢書, chapter 118²¹.

have had in view was not 撲 but 樸 — which latter is actually attested in the *T'ung-tien*, see above, p. 23. However, Yang Ch'iu seems to have overlooked an important conclusion which must be drawn from his argumentation, viz. that the character 樸 here in question is to be read and transcribed in accordance with Yen Sh'iu's gloss. As is easily seen, Yen Sh'iu's *fan-ts'ie* gives **puk* as the Ancient Chinese reading of the character in question, and the Modern Chinese 'projection' of the *fan-ts'ie* is *pu* (not *p'u*) — just as I have transcribed the character in the present paper (*Pu-t'ao*). It will be seen that the reading *pu* is also corroborated by the identification *Pu-t'ao* (of the *Han-shu*) = *Pu-ta* (of the *Hou-Han shu*) mentioned in the following footnote. But, as has already been remarked *supra*, p. 20, footnote 18, this very reading *pu* deduced from Yang Ch'iu's own argumentation runs against his phonetic equation put forward in the final part of his article as one of the main tenets on which his etymological hypothesis is based.

²¹ Cf. the *K'ai-ming shu-tien* ed., p. 0905₁ (in the passage concerning *Ta Yüeh-chi* 大月氏). The character 濮 is generally transcribed *p'u* (as e.g. in *Combined Indices to Hou Han-shu and the Notes of Liu Chao and Li Hsien*, Harvard-Yenching Sinological Index Series no. 41, Peking 1949, p. XXVII) probably because of the Modern Chinese reading of that character (occurring — with the reading *p'u* — up to now in some geographical names in China proper). This modern reading *p'u*, however, seems irregular as it is not in accord with the *fan-ts'ie* recorded in the *Kuang-yün* and other sources. In the *Kuang-yün* the character is placed under 博木切 which gives *pu* as the Modern Chinese 'projection', just as has been the case with 樸 (of 樸桃). As is easily seen, the identification 濮達 = 樸桃 does corroborate (with regard to 樸) the phonetic deductions drawn from Yang Ch'iu's emendation: it is only 樸 read as 博木 which is homophonic with 濮. This homophony of 樸 and 濮 is the more important as there is — and

The identification 濮達 = 樸桃 (撲挑) was also adopted by some Western scholars including E. Chavannes who went a step further and identified Chinese *Pu-t'ao* or *Pu-ta* with *Bactra* (Balkh), capital of Bactria²². The latter identification was called in question by the Japanese scholar Shiratori Kurakichi 白鳥庫吉 who also put forward a hypothesis of his own to the effect that the Chinese name designated a country around Kabul (which latter, however, is called *Kao-fu* 高附 in the *Hou-Han shu*, chapter 118). Yang Ch'iu-k'iu criticises the opinions of the Japanese scholar, adopts the identification put forward by Chavannes — with a suggestion of his own that the identification holds good not only with regard to *Bactra* but also with regard to *Bactria* (designating the country of which Bactra was capital) — and adduces additional reasons drawn from Chinese sources on behalf of the viewpoint adopted. Thus, the conclusions to be drawn from what is discussed in the whole chapter may be summarised in the following formula: 樸桃(撲挑) = 濮達 = Bactra (Bactria).

In chapter 3 it is argued that Bactria was situated in the zone of intensive viticulture, for which fact the author seeks evidence in the ancient written sources, both Western and Chinese. It may be remarked, however, that in view of the scantiness of data concerning *Pu-t'ao* or *Pu-ta* in Chinese sources there is no direct evidence from those sources to the effect that viticulture ever flourished in *Pu-t'ao-kuo*. Yang Ch'iu-k'iu's opinion is chiefly based on extra-Chinese material concerning Bactria while some indirect data drawn from early Chinese works are only of secondary importance, if any — but the problem in itself does not deserve detailed discussion in this place.

In chapter 4 the author puts forth and discusses his hypothesis to the effect that Chinese 蒲陶 — which, as we know, is the earliest graphical form recorded by Si-ma Ts'ien of the word subsequently written 葡萄 — has its origin in the country-name *Pu-t'ao* 樸桃. The author believes that he has founded his etymology on a strong phonetic basis, having proved — as

there was in early Chinese — a marked phonetic difference between 桃 and 達. According to Karlgren's reconstructions 樸桃 was in Ancient Chinese **Puk-d'âu* and in Archaic Chinese ***Puk-d'og*, while 濮達 was in both **Puk-d'ât* (or **Puk-t'ât*). The difference concerning the second member of the two names in question cannot be considered as an obstacle against their identification; it may be explained by the assumption that we have to do with two different attempts to render approximately a foreign syllable non-existent in Chinese.

²² See his critical review of J. Marquart's *Untersuchungen zur Geschichte von Eran*, "T'oung Pao", série II, vol. VI, 1905, p. 514. Chavannes transcribes the Chinese forms in question as *P'ou-t'iao* and *P'ou-ta*; he was, of course, aware that the first member anciently had a final -k.

he thinks — a strict phonetic equivalence of the two forms in question (蒲陶 = 樸桃) in the Chinese language of the former (Western) Han period, when the Chinese first got acquainted with the grape in Central Asia. For the sake of convenience, I am going first to summarise as accurately as possible Yang Chī-kiu's line of reasoning on behalf of his etymology without adding any criticism of my own; the latter will follow only after the summary of the contents of the chapter has been given. With regard to the phonetic aspect of the equation 蒲陶 (葡萄) = 樸桃 it is argued, first of all, that the characters of the 甫 series (i.e., the *hie-sheng* 諧聲 characters having 甫 as the so-called 'phonetic', 蒲 being one of them) belong to the 魚 category (魚部, i.e., the rime-category 魚) while the characters of the 業 series (樸 being one of them) are considered by all scholars as belonging to the *ju-sheng* 入聲 of the 侯 category (the author says expressly: 「業」聲, 諸家以爲, 侯部之入). Then, Yang Chī-kiu refers to a paper by Chou Tsu-mo where it is stated (on the ground of an analysis of the riming words in some poems written by the early Han writers) that in the Western Han period the 魚 category and the 侯 category must have become one rime-category — this unification of 魚部 and 侯部 being considered by Chou Tsu-mo as one of the four differences between the phonetic system of Archaic Chinese of the Chou era and that of the language of the Western Han epoch²³. From this Yang Chī-kiu concluded that in the Western Han period the characters 蒲 and 樸 must have become homophonic — for which he also gives a curious example of his own drawn from the geographical section of the *Han-shu*: the *hien*-name 樸剗 there recorded is glossed by Meng K'ang 孟康 — a commentator of the *Han-shu* living in the early San-kuo period — in a way implying phonetic identity of 樸 and 蒲, viz.

²³ The paper in question is *Yen-kiu Han-tai shī-wen yün-tu chī fang-fa* 研究漢代詩文韻讀之方法 (published in *Fu-chen ta-hüe yü-wen hüe-huei kiang-yen tsi* 輔仁大學語文學會講演集, 1st issue, Peking 1940, pp. 31 — 38). — The 侯部 as a separate rime-category (distinct from 魚部 and 尤部) does appear for the first time in the classificatory system of seventeen *pu* 部 put forward by Tuan Yü-ts'ai 段玉裁 (1735 — 1815), cf. Wang Li, *Han-yü in-yün-hüe* 漢語音韻學, 1956, p. 309; it is to be emphasised that in Tuan Yü-ts'ai's system the category in question has no *ju-sheng*. The fact that in the Han period there was confusion with regard to certain Archaic rime-categories — and with regard to 魚部 and 侯部 in particular — had already been remarked by Tuan Yü-ts'ai; however, as pointed out by Chou Tsu-mo in his article referred to, Tuan's statements to this effect are both vague and far-fetched.

音蒲環²⁴. Consequently, Yang Ch'ü-kiu's conclusion is that 樸 and 蒲 were strictly homophonic: 樸蒲二字發音完全相同. There is obviously no difficulty in showing that 陶 and 桃 were homophonic too, — and thus the author believes he has demonstrated the phonetic equivalence of the country-name 樸桃 and the word for 'grape' 蒲陶. This equivalence, as has already been stated, is the phonetic basis of the author's hypothesis to the effect that the word *p'u-t'ao* has its origin in the country-name *Pu-t'ao-kuo*. On the other hand, the author is, of course, perfectly well aware of the fact that the Chinese of the 2nd century B. C. became acquainted with both the word 蒲陶 and the thing it designates not in Bactria, but in *Ta-yüan* 大宛, i.e., Ferghana, and he tries to put his hypothesis in accord with that fact by arguing that the Ferghanians had received the fruit from *Pu-t'ao-kuo* 樸桃國 (Bactria) and, consequently, had given it the name 樸桃; when the Chinese got the fruit for the first time in Ferghana, they were not aware of the etymology of its name and rendered the name phonetically as 蒲陶. Even later on, when the Chinese became acquainted with 樸桃國, they could not see the intimate connection between the two words, 蒲陶 and 樸桃, being of the same origin but having come to China by different ways and at different times — which, in the author's opinion, adequately accounts for the difference of characters in the written forms 蒲陶 and 樸桃 (or 濮達). As is easily seen, the final part of Yang Ch'ü-kiu's argumentation encroaches upon the Indo-European linguistic domain since it involves the problem of the etymology of the Iranian (Ferghanian) word itself which was subsequently borrowed by the Chinese.

Most of this highly sophisticated argumentation is hardly tenable on closer inspection. First of all, let it be remarked that even from the purely philological point of view some of the author's statements are not very accurate. For example: despite what is suggested by Yang Ch'ü-kiu, it is not true that all Chinese scholars dealing with the problem of the classification of archaic rimes were agreed in considering the 僕 series as belonging to the *ju-sheng* of the 'rime-category' 侯 since e. g. in Tuan Yü-t's'ai's system the 侯部 has

²⁴ See *Han-shu*, *küan* 28 *hia* (in the passage concerning *Wu-wei-kün* 武威郡), *K'ai-ming shu-tien* ed., p. 0425₄; the name also appears in *Hou-Han shu*, *küan* 33, same ed., p. 0709₃. The fact that in the *Kuang-yün* 樸 (as a part of the *hien*-name here in question) is placed under 薄胡切 (see Chou Tsu-mo, *Kuang-yün kiao-pen*, I, f. 36 v) is evidently due to Meng K'ang's gloss; the same phonetic gloss is also reproduced s.v. 剗 (a character of unknown meaning and attested only as a part of the *hien*-name in question) which is explained as follows: 樸剗縣名在武威樸音蒲, see *ibidem*, I, f. 60 r.

no *ju-sheng* at all²⁵. The philological aspect of Yang Chī-kiu's argumentation is closely connected with the purely linguistic one to which my criticism must necessarily be limited in this place. Now, with regard to all the speculations on the traditional 'rime-categories' (部) it must be recalled that: first, the 部 take, of course, no account of the initials; second, being groups of characters having related — but not necessarily identical — 'rimes', they are not phonetically homogenous even with regard to the respective 'rime'; third, a 'rime' is not necessarily the same as 'real final', the latter including medial vowel, if any, and the 'rime'. All this implies that admitting the coalescence of two 'rime-categories' into one at a given epoch — as e. g. the coalescence of 魚部 and 侯部 for the Western Han period — we are by no means allowed by this very fact to conclude that a character belonging to one of these categories became homophonic with one belonging to the other. Yet that is precisely what Yang Chī-kiu seems to do in demonstrating the alleged phonetic equivalence of 樸 and 蒲. There is no strong objection against his phonetic equation 陶 = 桃²⁶ — the more so as among the early graphical variants of 蒲陶 there is also one written 蒲桃 (*cf. infra*, p. 35, footnote 36), but there may be no doubt that the alleged phonetic equivalence 樸 = 蒲 is unsound. As has already been remarked (*supra*, p. 22, footnote 20, and p. 23, footnote 21) Yang Chī-kiu's own emendation of the country-name in question 撲挑 > 樸桃 and the identification 樸桃 = 濮達 clearly imply unaspirated initial *p*- in 樸 (Arch. and Anc. **puk*) while 蒲 certainly had aspirated voiced initial *b*'- in both Archaic and Ancient Chinese²⁷. Still more important is the fact that the whole series of characters with the 'phonetic' 業 are *ju-sheng* words having a final guttural *-k* which, as is known, has been conserved to this day in both Southern Chinese dialects and Sino-Japanese. This must certainly have been the case also for the Western Han period, and it may be recalled that this may well account for the rendering of the syllable *bak*- in

²⁵ *Cf. supra*, p. 25, footnote 23; according to Tuan Yü-ts'ai the 業 series belongs to the *ju-sheng* of the 幽部. Yang Chī-kiu probably had in view Wang Nien-sun 王念孫 (1744–1832) and others.

²⁶ Although according to Karlgren there was a slight difference of vocalism in Archaic Chinese: 陶 ***d'óg*, 桃 ***d'og*; both coalesced, however, into Ancient Chinese **d'áu*.

²⁷ The fact that in the *Kuang-yün* 樸 also appears with the reading 蒲木切 indicating the same initial (*b*'-) in 樸 and 蒲 does not matter here, since Yen Shī-ku's gloss involved in the emendation — as has already been stated *supra*, p. 22–23, footnote 20, — clearly points to another reading of 樸 here in question: Yen Shī-ku's 布木反 is the same as 博木切 of the *Kuang-yün* — both indicating initial *p*-.

Bactra as 樸 or 濮. On the other hand, the characters of the 甫 series have always had a vocalic *auslaut* with no consonant at all²⁸. Thus irrespective of all the statements concerning the coalescence of archaic rime-categories in the former Han period it must be taken for granted that 樸 (of 樸桃) and 蒲 (of 蒲陶) could not be homophonic: contrary to what Yang Ch'ī-kiu maintains 樸桃 ≠ 蒲陶 the first monosyllables in these dissyllabic terms differing in both initial (*p-* : *b'-*) and final (*-k* : vocalic *auslaut*).

Some words must be added concerning Meng K'ang's enigmatic gloss adduced by Yang Ch'ī-kiu in connection with his alleged phonetic equivalence 樸 = 蒲 and considered by him as a kind of touch-stone for his phonetic speculation. The gloss deserves our special attention since it is the only early document²⁹ which, in fact, seems to support the otherwise unfounded

²⁸ I follow, of course, Karlgren's phonetic reconstructions summarised in his *Grammata Serica* and his recent publication *Compendium of Phonetics in Ancient and Archaic Chinese*, "Bulletin of the Museum of Far Eastern Antiquities" no. 26, Stockholm 1954, pp. 211–367. It is to be emphasised that when speaking above of the characters of the 甫 series I mean exclusively those having 甫 as 'phonetic', not those having 尊 (= 博); the latter form a separate phonetic series characterised by a final *-k* or *-g*, cf. *Grammata Serica*, no. 771. — As is known, Lu Ch'ī-wei 陸志韋 — in contradistinction to Karlgren — does admit final gutturals in the 魚 category to which the 甫 series is reckoned to belong, see his *Ku-in shuo-lüe* 古音說略, Peking 1947, p. 87. He also accepts the coalescence of 魚部 and 侯部 since the later Chou epoch, *ibidem*, p. 316. It should be remarked, however, that even according to Lu Ch'ī-wei there must have been a difference in finals between 樸 and 蒲 the former having *-k* and the latter *-g*. To Lu Ch'ī-wei's far-fetched theory of final plosives in Archaic Chinese I shall revert briefly at the end of this paper, see *infra*, p. 42–43.

²⁹ It may be added that the *K'ang-hi ts'ien* s.v. 樸 not only reproduces the *Kuang-yün* reading 薄胡切 (based on Meng K'ang's gloss, cf. *supra*, p. 26, footnote 24) but also ascribes to the character in question another non-*ju-sheng* reading, viz. 普故切, i.e., Ancient Chinese **p'uo*, which is stated to be drawn from the *T'ang-yün* 唐韻 and is claimed to refer to a passage in the *Huai-nan-tsi* 淮南子, chapter VII: 明白太素, 無爲復樸 (the passage being, of course, a variant of *Chuang-tsi* 莊子, chapter XII: 夫明白入素, 无爲復朴). I can find no early evidence to the effect that 樸 (朴) in the passage quoted was really considered as a non-*ju-sheng* word, and I think that the reading reproduced in the *K'ang-hi ts'ien* is due to some late speculation considering the passage as rimed (with 素 and 樸 as riming words). Thus, Meng K'ang's gloss is the only early philological support (or better: quasi-support) — dating back to the San-kuo period — of Yang Ch'ī-kiu's phonetic speculation.

formula put forward by Yang Chī-kiu, a formula the non-validity of which we have just demonstrated. It must be said at once that the gloss in question — in spite of its having some tradition in Chinese philology — cannot be conclusive proof for various reasons. Here again let it be recalled that Meng K'ang's gloss indicates initial *b*- in 樸, whereas we have demonstrated that the character 樸 in 樸桃 must have had initial *p*-. This fact by itself is sufficient to show that in the two proper names in question, 樸真 and 樸桃 two different readings of the character 樸 are involved. From this it follows that Meng K'ang's gloss concerning the name 樸真 — even if taken as it stands — cannot be directly applied to the character 樸 in 樸桃. However, most important in this connection is again the problem of finals. As already stated, according to what is known from various sources and Chinese historical phonetics, the character 樸 in all its variant readings represents a *ju-sheng* word having final *-k* while 蒲 (in its primary use, not as a secondary substitute for 薄, see *infra*) is a non-*ju-sheng* word with no final consonant. Thus, Meng K'ang's phonetic equation 樸 = 蒲 implies the following alternative: either in this particular case (1) the character 樸 contrarily to its normal use stands for a non-*ju-sheng* monosyllable or (2) the character 蒲 contrarily to its normal use stands for a *ju-sheng* monosyllable. Yang Chī-kiu leaves the problem unconsidered and I cannot know for sure which alternative he would choose — but it is evident that both are incompatible with his alleged equation 樸桃 = 蒲陶. As shown by the *fan-ts'ie* of the *Kuang-yün* (cf. *supra*, p. 26, footnote 24, and p. 28, footnote 29) the traditional way of interpreting Meng K'ang's gloss was to make 樸 a non-*ju-sheng* word — which corresponds to our alternative (1). If so, and if this non-*ju-sheng* reading be applied to the character 樸 in the geographical name 樸桃, it is obvious that this would be in flagrant incompatibility not only with Yen Shī-ku's *fan-ts'ie* involved in Yang Chī-kiu's reasoning, but also with the very identification 樸桃 = *Bactra* (where the *ju-sheng* character 樸 having final *-k* is considered as rendering the syllable *Bac*-). But, as we have seen (cf. *supra*, p. 24), Yang Chī-kiu seems to be well aware of the fact that the character 樸 involved in his reasoning must have been a *ju-sheng* word and thus he perhaps would vote for our alternative (2) assuming that the 蒲 of the gloss stands for a *ju-sheng* monosyllable. The latter alternative, although contrary to the traditional interpretation of Meng K'ang's gloss, seems possible, but only as far as the gloss in question is concerned, while it is not difficult to see that it is incompatible with Yang Chī-kiu's conclusions — just as has been the case with our alternative (1). As has already been remarked, the character 蒲 may stand for 薄 (with 專 = 博 as phonetic component) representing a true *ju-sheng* monosyllable sounding **b'âk* (variant reading **p'âk*, see *Gram. Ser.*, no. 771 p). At least one instance of this secondary use of 蒲 = 薄

b'āk* is sufficiently attested in early texts³⁰ and thus the possibility cannot be excluded *a priori* that the case of *Meng K'ang's* gloss is of the same kind. The difference in vocalism (*b'uk* : *b'āk*) may provisionally be explained e. g. by some dialectal peculiarity. This may seem probable as far *Meng K'ang's* gloss is concerned. However, *Yang Chī-kiu's* line of reasoning implies that we have to extend the supposed reading *b'āk* here in question not only to the character 樸 in the country-name 樸桃 (with regard to which the reading *b'āk* disagrees in initial and vocalism with *Yen Shī-ku's fan-ts'ie* adopted by *Yang Chī-kiu*), but also to the character 蒲 in the word 蒲陶—and this is plainly impossible. Let it be stressed once more that there is no evidence whatever from either graphical variants or *fan-ts'ie* or anything else to the effect that 蒲 in 蒲陶 may have ever been equivalent to 薄 or another *ju-sheng* character, while all extant evidence is for 蒲 (in 蒲陶) as being a non-*ju-sheng* monosyllable. Thus, our alternative (2) is likewise incompatible with *Yang Chī-kiu's* basic phonetic equation with which his etymology stands or falls. Perhaps somebody will think that *Yang Chī-kiu's* deductions may be saved by a slight modification of the whole problem according to *Lu Chī-wei's* theory of voiced final plosives in Archaic Chinese (cf. *supra*, p. 28, footnote 28). According to this theory one may argue that 蒲 had voiced final *g*, 樸 and 蒲 being thus much alike in sound although not strictly homophonic; this would allow us to interpret both *Meng K'ang's* gloss 樸 = 蒲 and *Yang Chī-kiu's* conclusion 樸桃 = 蒲陶 as imperfect but satisfactory phonetic equations, based on sufficient sound-similarity, although not on perfect homophony. This also would satisfy, it might seem, the requirement for the 蒲 in 蒲陶 as being a non-*ju-sheng* monosyllable, although one having a final ***g* and developing into an open syllable only in Ancient Chinese. Now, setting aside the preliminary problem of the applicability of *Lu Chī-wei's* far-fetched theory, it must again be said that the hypothesis now in question is also hardly tenable, as the final ***g*—even if there had ever been any in 蒲—must have disappeared before the San-kuo period. Consequently, *Meng K'ang's* gloss cannot be due to a supposed Archaic final ***g* in 蒲 allegedly making the latter character quasi-homophonic with the true *ju-sheng* character 樸. Indeed, *Meng*

³⁰ In the *Tso-chuan* (Chao kung, 9th year and 20th year) there appears a place name written 蒲姑; in *Sī-ma Ts'ien's Shī-ki (kūan 4)* the same place name is recorded as 薄姑. The Han writer *Ing Shao 應劭* (end of the Eastern Han period), quoting *Tso-chuan* in a gloss inserted in the *Han-shu (kūan 28 shang)*, has 薄姑, and the *Tso-chuan* commentator *Tu Yü 杜預* (222–284) says with regard to the character 蒲 in the place name 蒲姑: 又音薄.

K'ang's gloss — in whatever way interpreted — can hardly justify (or even have any bearing on) Yang Ch'iu's formula on which his etymology is based and with which his chief idea stands or falls.

As a matter of fact, Meng K'ang's gloss — which, as far as I know, has not yet been dealt with and adequately explained by anybody — must certainly be interpreted in a way leaving no room for its being connected in any respect with the problem of the etymology of the loan-word *p'u-t'ao*. One may wonder why the Ts'ing scholars working on the geographical section of the *Han-shu* paid no special attention to this curious gloss suggesting a rather unusual sound-equation, and instead contented themselves with mere references to the gloss without any attempt to explain it³¹. This is probably due to the fact that the gloss had been incorporated into such authoritative phonetic compendia as the *Kuang-yün* and, consequently, was considered as representing one of the variant readings (in the ordinary sense of this term) of 樸. However, the very fact that the gloss and the *Kuang-yün* reading based on it actually concern one single instance — that of the place name 樸剗 — shows that in the case in question we have to do with a rather exceptional phenomenon which should be explained in some way. As has already been remarked, one may try to explain the gloss by considering 蒲 as a substitute for the *ju-sheng* character 薄, an explanation which corresponds to our alternative (2) previously discussed. It will be recalled that in dealing with this alternative above we have only demonstrated its incompatibility with Yang Ch'iu's reasoning, but not with the gloss itself. This, however, would be a very poor explanation, not only because of the difference in vocalism (*cf. supra*, p. 30), but chiefly because of lack of evidence to the effect that the 蒲 of the gloss had ever been conceived as 薄. Indeed, it seems certain that 蒲 was occasionally used instead of 薄 only in the pre-Han period, and it is hardly imaginable that a scholar of the San-kuo epoch would have chosen an unusual and secondary reading of 蒲 to use it as *definiens* in a phonetic gloss. Alternative (1) previously discussed, the traditional one, crediting the 蒲 of the gloss with its ordinary phonetic value, is certainly much more plausible, and I think that it is only this alternative which gives us a clue to the gloss. I think that what Meng

³¹ Cf. among the works collected in the *Er-shih-wu shi pu-pien* 二十五史補編 (Chung-hua shu-kü reprint, 1956, of the K'ai-ming shu-tien edition): Wang Yüan-sun 汪遠孫, *Han-shu ti-li-chi kiao-pen* 漢書地理志校本 (p. 444₂); Wu Cho-sin 吳卓信, *Han-shu ti-li-chi pu-chu* 漢書地理志補注 (p. 894₁); Ts'ien Tien 錢站 and Sü Sung, *Sin kiao-chu ti-li-chi tsi-shi* 新輯注地理志集釋 (p. 1140₃). None of these authors quoting Meng K'ang noticed the phonetic difficulties involved in the gloss.

K'ang wanted to point to was precisely the unusual non-*ju-sheng* value of 樸 in the place-name 樸 賣 and that he actually rendered this value by means of the non-*ju-sheng* character 蒲 of the gloss. The curious phenomenon here in question — which one might call 'dejushengisation' of a primary *ju-sheng* monosyllable — must be due, I think, to intersyllabic assimilation (*sandhi*), — a factor not often taken into consideration in relation to early Chinese. The character 賣, as has already been remarked, is not attested for the Archaic period; it appears only in the *Han-shu* and only in the place-name under discussion. Thus, it has not been listed in Karlgren's *Grammata Serica*. However, Meng K'ang's equation 賣 = 環 and the very structure of the character itself sufficiently point to an Archaic (and Han) reading ***g'wan*, Ancient Chinese **ɣwan* (cf. *Gram. Ser.*, series no. 256). Consequently, we have to reconstruct the original reading of the place-name recorded in the *Han-shu* as ***b'uk-g'wan* which, I think, must have soon become ***b'ug-g'wan* because of intersyllabic *sandhi*. Clearly this secondary final ***g* of the first syllable must in its turn have had only short life: first, it may have been wholly absorbed by the initial of the following syllable (*b'ug-g'wan*); second, even if not absorbed, it had to follow the general line of Chinese phonetic evolution according to which all voiced final plosives were dropped shortly after the beginning of our era. Thus, we arrive at the form ***b'u* of the first monosyllable — which, however, was an unusual sound-form in early Chinese, both Archaic and Ancient. As is known to anybody even slightly acquainted with Chinese phonetics and the problem of the phonological structure of Chinese monosyllables, by no means all combinations of initials and finals actually occur in practice, and so it was in early Chinese too. Following Karlgren's reconstructions we find that in Archaic Chinese there was no monosyllable sounding ***b'u* nor even any combination *labial plosive + u*, although there were labial plosives as initials and *-u* as real final occurring in other combinations. Again, in Ancient Chinese Karlgren finds no *-u* as real final. Taking this for granted we can see the instability of the secondary syllable ***b'u-(g'wan)* which, consequently, had to be adapted to the exigencies of the phonological system of early Chinese and take some form actually occurring in monosyllables and resembling ***b'u* as much as possible: this may well have been Archaic ***b'wo*, Ancient Chinese **b'uo*. The whole of the above argumentation may be summarised as follows: 樸 賣 ***b'uk-g'wan* > *b'ug-g'wan* > *b'u-g'wan* > Arch. ***b'wo-g'wan*, Anc. Chin. **b'uo-ɣwan* — the latter two forms corresponding to Meng K'ang's 蒲 環³². Let it also be emphasised that the

³² Perhaps one will suppose that the secondary form ***b'u* < ***b'uk* must not necessarily have developed into ***b'wo* (**b'uo*) and that the actual reading to be rendered by the commentator was ***b'u-g'wan*. If so, his ***b'wo-g'wan* may be explained as an attempt, necessarily imperfect, to render the unusual

two monosyllables composing the place-name in question and forming thus a dissyllabic lexical unit were certainly spoken in close juncture with each other — which fact makes the intervention of the intersyllabic *sandhi* (together with the subsequent development of the first monosyllable in accordance with the exigencies of the phonological system of the language) more than probable. It follows from all this that the sound-value ***b'wo*, **b'uo* indicated in M e n g K ' a n g ' s gloss is not a mere phonetic variant (in the ordinary sense of this term) of 樸 as the Chinese lexicographers and philologists seem to have understood it, but a specific secondary phenomenon limited to this particular case. One also sees that the case of M e n g K ' a n g ' s gloss instead of being a kind of touch-stone of Y a n g C h i - k i u ' s etymology has, in fact, nothing to do with it.

Having thus demonstrated the non-validity of the alleged formula 樸桃 = 蒲陶 we must state that there is no reason to accept the hypothesis that the latter form may be derived from the former or, to say it more correctly in accordance with Y a n g C h i - k i u ' s line of reasoning, that the two Chinese forms involved may represent the same foreign word (foreign country-name, supposedly Bactra or Bactria). This is sufficient, I think, to make clear the unsoundness of the attempted etymology based almost exclusively on the formula discussed. It must also be stressed that this untenable formula is far from being the only weak point in Y a n g C h i - k i u ' s theory. It should be recalled, for instance, that there is no evidence from Iranian linguistic material that the Iranians ever employed the name of Bactra (or Bactria) for the designation of the 'grape-vine' — a plant which had been known and cultivated by the Iranians for ages — and that a supposition to this effect is to be considered as highly improbable. One must also put the question whether it is methodologically justified to construct a roundabout and highly complicated hypothesis involving data picked from Chinese works of different chronology — as are the *Shi-ki*, *Han-shu* and *Hou-Han shu* — as well as insecure phonetic (and geographical) identifications and various risky and unsupported suppositions in order to explain a fact which, as we shall see, may be explained in a much more simple and much less disputable way by what we

combination of phonemes not occurring in isolated monosyllables by means of a character phonetically akin to the combination to be rendered: *b'u ~ b'wo*. This is possible, but I do not believe it. The combination ***b'u*, although it could not be adequately rendered by a single character, was none the less perfectly transcribable by means of the *fan-ts'ie* method which was known to M e n g K ' a n g and which he occasionally used elsewhere in his glosses. If the commentator had had to render an actual ***b'u*, he most probably would have used this method as the only adequate one for such a case. Thus, M e n g K ' a n g ' s 音蒲 must be based, I think, on perfect homophony.

several times in connection with the country of *Ta-yüan* (Ferghana) and that of *An-si* 安息 (Parthia)³⁶. The passage in question, which is a narrative of Western (Central Asiatic) countries visited by the famous general *Chang K'ien* 張騫 on his expedition, seems to be the reproduction of a report addressed by *Chang K'ien* to the emperor. As was rightly pointed out by *Laufér*, the speech of the people of *Ta-yüan*, i.e., Ferghana (and the more so that of *An-si*, i.e., Parthia) must have been Iranian, and the simplest and most probable hypothesis is that the Chinese form 蒲陶 recorded by *Sī-ma Ts'ien* phonetically reproduces an Iranian word which *Chang K'ien* learned for the first time in Ferghana. Now, in the chief representative of modern Iranian languages, namely New Persian, there is a word for 'wine' sounding ۛۛۛ *bādā* which goes back to Middle Persian *bātak*. The latter form is not a reconstruction since it is attested in the Middle Persian glossary *Frahang i Pahlavik*³⁷. The word, of which the Indo-European etymology is not clear, is not directly attested in Old Persian³⁸, but there can be no doubt that the stem had existed in early Persian for ages since *Athenaios*, a Greek author living about 200 of our era, recorded in his *Deipnosophistai*, bk. xi, the form βατιάκη as the Persian word for 'cup' (Περσικὴ φιάλη); the latter form, in its turn, is related to New Persian ۛۛۛ *bādiyā* 'wine cup'. All these facts were known to *Laufér*³⁹ and he was certainly right in suppos-

work: Н. Я. Бичурин, *Собрание сведений о народах, обитавших в Средней Азии в древние времена* (3 vols., Moscow—Leningrad 1950—1953), vol. II, pp. 147—168.

³⁶ *Laufér*, *Sino-Iranica*, p. 225, is wrong when giving the earliest graphical form of the word as 蒲桃. The latter variant is recorded only later. According to *Chu K'ī-feng*, *Ts'ī-t'ung*, p. 718, it appears in the commentary (*chu* 注) on *San-kuo chī* 三國志, compiled, as is known, by *P'ei Sung-chī* 裴松之 in the first half of the 5th century of our era; thus, the form 蒲桃 is attested some five centuries after *Sī-ma Ts'ien*'s 蒲陶. It goes without saying that it is the earliest form, i.e., that of the *Shī-ki*, which should be the starting point of the discussion.

³⁷ Cf. H. F. J. Junker, *Das Frahang i Pahlavik in zeichengemässer Anordnung*, Leipzig 1955, p. 20. The heterogram *BSR* used for *bātak* corresponds to Aramaic ܒܫܪܗ *bēšīrah* 'vintage'.

³⁸ There are silver vessels with inscriptions claiming to date back to the epoch of Artaxerxes I, i.e., 5th century B.C., in which the stem *batu-* 'wine' appears in the compound word *bātugara-* 'drinking cup' (*bātu-* 'wine' + *gara-* 'devouring, drinking'), but these inscriptions are suspected of being a later forgery; cf. R. G. Kent, *Old Persian*, New Haven 1950, p. 199.

³⁹ Chiefly through P. Horn, *Grundriss der neupersischen Etymologie*, Strassburg 1893; cf. p. 36, no. 155.

know from one single Chinese source, i.e., Sī-ma Ts'ien's *Shi-ki* alone, together with Chinese historical phonetics and Iranian linguistics.

To make the review of Chinese contributions concerning our problem more complete (and, as far as I know, exhaustive) I will quote the highly interesting book on *Language and Culture* by the leading Chinese authority on linguistics, Prof. Lo Ch'ang-p'ei 羅常培³³. In his important chapter on loan-words in Chinese as reflecting cultural contacts between China and the outside world, the writer has also included a paragraph dealing with the word *p'u-t'ao* in which a short account is given of all the etymologies of this word ever proposed. Special attention is paid, of course, to Laufer and Yang Ch'ik iu, but no critical remarks are expressed with regard to any of the divergent etymologies referred to and one cannot see which of them is considered by the writer as most probable. Lo Ch'ang-p'ei's treatment of our problem shows, I think, that he simply considers it unsolved.

I have less information with regard to Japanese studies in the field and do not know for sure whether there is not some special paper by a Japanese linguist dealing with loan-words and the etymology of *budō* (=Chinese *p'u-t'ao*) in particular. But judging from the Japanese lexicographical compendia recently published or republished which I could have access to,³⁴ no serious attempt has been made in Japan with regard to our problem. Japanese lexicographical works occasionally dealing with etymological problems either leave the problem of the etymology of *budō* unconsidered or reproduce Laufer's etymology — or even only the obsolete hypothesis deriving the Sino-Japanese word from Greek βότρυς. The only (and insignificant) exception I know of is the *Daigenkai* 大言海 compiled by Ōtsuki Fumihiko 大槻文彦 (Tōkyō, 10th year of Shōwa) where we find s.v. *budō* (vol. IV, p. 191) a slight suggestion that the word — or rather its compound form *budōka* 蒲桃果 'budō-fruit' — may be suspected of having some connection with Sanskrit *mṛdvika* 'grape'.

It is now time to revert to Laufer's etymology of *p'u-t'ao* supplemented by some remarks of my own. As has already been stated, the earliest Chinese source in which the word *p'u-t'ao* occurs is Sī-ma Ts'ien's *Shi-ki*, chapter 123³⁵. The word has there the graphical form 蒲陶 and appears

³³ Lo Ch'ang-p'ei, *Yü-yen yü wen-hua* 語言與文化, ed. by Kuo-li Pei-king ta-hüe, preface dated 1950. Chapter IV (on loan-words), pp. 18—54; for the *p'u-t'ao* problem see pp. 23—24.

³⁴ I should like to acknowledge my indebtedness to Dr. W. K o t a ń s k i who has kindly put at my disposal his rich collection of Japanese lexicographical works otherwise inaccessible in Poland.

³⁵ See *Er-shi-wu shi*, *K'ai-ming* ed., p. 0267. The earliest translation of the whole chapter into an European language is that published as early as 1851 by the Russian Sinologist N. B i č u r i n, see the recent re-edition of his

ing some indirect connection between the Iranian forms just quoted and Chinese *p'u-t'ao*. However, in his tentative reconstruction of the Iranian form as borrowed by the Chinese Laufer was led astray by his erroneous reconstruction of the readings of the Chinese characters here in question. Publishing his book at a time when Chinese historical phonetics was in its very beginnings, he only could imagine — basing himself on the Sino-Japanese reading *budō* — that the Chinese form in Chang K'ien's time sounded like **bu-daw*; hence his reconstruction of the Iranian (Ferghanian) form directly borrowed by the Chinese as **budāwa*. This Laufer considered as being composed of the stem *buda-* (related to Persian *bādā*, etc.) and the suffix *-wa* or *-awa*⁴⁰. Now, the alleged **budāwa* is pure speculation for which there may hardly be any evidence in Iranian linguistics. There is nothing astonishing in this, since the reconstructed Chinese form **bu-daw*, being the starting point for the Iranian **budāwa*, is a failure. As is easily seen, Laufer's **bu-daw* corresponds very roughly only to the reading of the characters in question in what is called Ancient Chinese, i.e., the Chinese language of the Sui and early T'ang period. According to Karlgren the reading of 蒲陶 in Ancient Chinese is **b'uo-d'âu*, but it must be remarked at once that we are by no means allowed to operate with readings of so late an epoch of the Chinese language with regard to loan-words borrowed by the Chinese in the early Han period, that is to say, some seven centuries earlier than the Sui period. This restriction is the more important as it is precisely towards the end of the Han period and immediately afterwards that the Chinese phonetic system seems to have undergone most important modifications. Archaic Chinese of the Chou epoch is not only closer in time to the early Han period, but there are also good reasons to believe that in the early Han time and up to the middle Han period the Chinese phonetic system, with but a few exceptions, was still much like that of Archaic Chinese. Thus, it goes without saying that in our search for the original foreign form of the Chinese loan-word 蒲陶 recorded by Sī-ma Ts'ien we should start by giving this word its Archaic Chinese reading. Now, according to Karlgren's reconstructions the Archaic Chinese reading of 蒲陶 is ***b'wo-d'ôg* (*Gram. Ser.*, nos. 102 n' and 1047 d) — and this, to my mind, inevitably reminds us of the Middle Persian word *batak* 'wine' mentioned above and already adduced by Laufer in connection with our problem.

We are certainly not allowed to suppose that the Chinese form is derived directly from early Persian since we know from the *Shi-ki* that Chang K'ien must have learned the word (and become acquainted with the grapevine) in Ferghana where it was not Persian that was spoken, but some other

⁴⁰ *Sino-Iranica*, p. 225. Laufer adds that the Chinese form "might also be conceived as a dialectic form of Avestan *maḍav*".

Iranian dialect which is not directly attested. Thus, we have first to demonstrate that the Iranian form in question (corresponding to Middle Persian *bātak*, etc.), sufficiently attested in Persian, was not specifically Persian, that is to say, that it belonged to the common lexical stock of the ancient Iranian dialects spoken in Central Asia. As is perfectly well known, those Iranian dialects, differentiated as they were, were none the less much alike and mutually intelligible, and the Chinese explorers of Central Asia in the Western Han period must have been perfectly aware of this peculiar unity of the Iranian speech, since Sī-ma Ts'ien says in the same chapter 123 of his *Shi-ki*: "From Ta-yüan westwards as far as the country of An-si, although the people differ in speech, yet their customs are much alike and they understand each other."⁴¹ It may be recalled that slightly later than Sī-ma Ts'ien and quite independently of the Chinese experience with the Iranians just the same judgment on the unity of the speech of the ancient Iranian peoples was recorded by the Greek geographer Strabon (living just before and shortly after the beginning of our era) who qualified various Iranian peoples as *ὁμόγλωττοι παρὰ μικρόν* 'of nearly the same language'. Thus, there should be nothing improbable in assuming that a word attested in early Persian may also have existed in other ancient Iranian dialects. What is most important, however, is that in the present case the word in question may also be proved to have existed outside the Persian linguistic domain and to have penetrated deeply into Central Asia since it is attested as *bātaa*- 'wine' in the medieval manuscripts written in the Middle Iranian dialect of Khotanese Saka⁴². As is evident to anyone

⁴¹ *Er-shi-wu shi*, K'ai-ming ed., p. 0268₃. The same statement (in a slightly altered form) is also repeated in the *Han-shu*, chapter 96 *shang*, see *Er-shi-wu shi*, p. 0607₄. Bičurin, *op. cit.*, vol. II, p. 161, footnote 4, believed that the passage concerned the Turks, but we know, at least since Gauthiot, that only the Iranians could be meant. Cf. R. Gauthiot, *Essai de Grammaire Sogdienne*, Paris 1914–1923, p. V (where, however, only the *Han-shu* is referred to and nothing is said of the *Shi-ki* which is the earliest source), and Laufer, *Sino-Iranica*, p. 212 (where the passage is wrongly considered as belonging into Chang K'ien's report).

⁴² See S. Konow, *Primer of Khotanese Saka*, "Norsk Tidsskrift for Sprogvidenskap" XV, Oslo 1949, p. 115. Konow seems to be the first scholar working on Saka who correctly interpreted *bātaa*- as 'wine' and noticed the connection between this Saka word and New Persian *bādā*. The word appears in an important Saka text worked up by E. Leumann and edited posthumously by M. Leumann, *Das nordarische (sakische) Lehergedicht des Buddhismus*, Abhandlungen für die Kunde des Morgenlandes hrsg. von der Deutschen Morgenländischen Gesellschaft, XX. Band, Leipzig 1933–1936. Here the word has been badly misinterpreted as 'Windbeutel' (here evidently: 'a kind of cake'), see pp. 12–13, line 50, for context and translation, and p. 472 of the glossary. This is a curious error since Leumann not only miscon-

d' for the rendering of the Iranian unaspirated plosives *b*, *d* in **bādaga*⁴⁴. The apparent difference in vocalism may easily be explained either by the limitations imposed on combinations of existing initials and finals in Chinese or by some purely Ferghanian peculiarities of vocalism which are not otherwise known — or by both. Thus, for instance, there was no such monosyllable as ***b'a* in Archaic Chinese to render the Iranian syllable *bā-*; on the other hand, the rendering of the long *ā* by some kind of diphthong *wo* is quite possible⁴⁵. Finally, it should be stressed that linguistic semantics can raise no objection against the shifting from the Iranian meaning 'wine' to the Chinese meaning 'grape-vine', i.e., 'plant of which wine is made'. Furthermore, the Chinese form as recorded in the *Shī-ki* occurs first in the compound expression *p'u-t'ao-tsiu*, which may well mean not only 'wine made of grapes' — as generally translated — but also 'wine called *p'u-t'ao*'. Thus the application of the word *p'u-t'ao* in Chinese for the designation of the plant itself may be secondary⁴⁶. To my mind, the Chinese form 蒲陶, Karlgren's Arch. Chinese ***b'wo-d'ôg* > Anc.

⁴⁴ It may be recalled that in Ancient Chinese of the Sui-T'ang period there certainly was no unaspirated initial *b-* or *d-* (the latter, limited in Archaic Chinese to the combination ***dž-*, disappeared before the epoch of Ancient Chinese). Thus there is no reason to suppose that in Chang K'ien's time, that is to say after the epoch of Archaic Chinese (in the narrow sense of the term) for which Karlgren's reconstructions are chiefly meant, unaspirated initial *b-*, *d-* may have appeared as an innovation in the phonetic system.

⁴⁵ It may also be remarked in this connection that in Khotanese Saka the long *ā* had a sound approaching *u* or *o* (König, *Primer of Khotanese Saka*, p. 11). This, of course, cannot have a direct bearing on our problem since the latter concerns not Saka of the 7th century of our era but Ferghanian of the 2nd century B.C. However, it does illustrate the tendencies in the development of the Iranian vocalism which in some dialects — possibly in Ferghanian — may have appeared quite early.

⁴⁶ I wonder whether even the phrase 以蒲陶爲酒 occurring in the *Shī-ki*, *Er-shī-wu shī*, p. 0268₂, which is generally interpreted 'they use grape to make wine of it', cannot mean 'as wine they use what they call 蒲陶'. On the other hand, it is to be noted that the Iranian form itself may have had wider extension than 'wine'; cf. βατιάκη 'Περσική φιάλη', New Persian *bādā* 'wine', but in some cases also 'poculum vini' (Vullers, I, p. 165; Steingass, p. 140), and the heterogram of Middle Persian *bātak* which corresponds to 'vintage' (cf. *supra*, p. 35, footnote 37). It should also be recalled that the Iranian suffix *-ka* here in question was not a specialised formant, but one having a rather vague application. In view of all this it seems quite probable that **bādaga* may have designated 'grape-vine' in Ferghanian. At any rate it must be stated that the semantic aspect of the loan offers no difficulty from any point of view.

even slightly acquainted with Iranian linguistics, the form *bātaa-* is originally one with the suffix *-ka* and certainly goes back to **bātaka* which, in its turn, strictly corresponds to Middle Persian *bātak*. It is to be stressed that many similar examples of both purely Iranian words and loan-words from Sanskrit in Khotanese Saka may be quoted (as e.g. *banaa-* < **bandaka* 'slave, servant', corresponding to Old Persian *bandaka*, Middle Persian *bandak*, New Persian *بند*, *bāndä*, Sogdian *βntk*; *pūstaa-* < **pūstaka-* 'book', Sanskrit *pustaka-*, Sogdian *pwstk*, etc.). Thus there may be no doubt that the Saka word *bātaa-* is a suffixal formation with *-ka* which latter suffix, let it be added, had early become extremely common in all Iranian dialects. Texts in Khotanese Saka now extant are of a comparatively late epoch (as K o n o w states, the earliest ones are of the beginning of the 7th century of our era), but for an early stage of the language, such the early Western Han period, in the 2nd century B.C., the word must certainly have been **bātaka*. Moreover, it is known that intervocalic voiceless plosives must have become voiced in some Iranian dialects at a quite early date, and thus it may be safely assumed that in the 2nd century before our era **bātaka* alternated in Iranian dialects with **bādaga*. It may be objected that Khotanese Saka, after all, is not the same as Ferghanian from which the Chinese took the word, but the very fact that the same formation, composed of the same stem and the same suffix, is attested in Iranian dialects geographically so distant as are Persian and Khotanese Saka gives the necessary probability to the assumption that the same formation must also have existed in other Iranian dialects extending over the vast Central Asiatic regions between Persia and Khotan. On the other hand, the remarkable similarity of the Iranian form **bātaka*, **bādaga* and the Archaic Chinese form ***b'wo-d'ôg* shows that the word must have existed in Ferghanian, most probably with the pronunciation **bādaga*⁴³. The rendering of the Iranian plosives by the corresponding Chinese aspirates *b'*, *d'* offers no problem at all, since according to K a r l g r e n 's system of reconstruction Archaic Chinese had no initial unaspirated *b-*, while unaspirated initial *d-* was possible only before *-i-*. If so, the Chinese of C h a n g K ' i e n 's time had no other means except *b'*,

ceived the word as a derivate of *bāta-* 'wind' — which is understandable — but evidently invented his 'Windbeutel' by projecting into Iranian the word-formative and semantic associations (very specific in this case) of his own language. It may be added that *bātaa-* 'wine' suits the context perfectly well: *hurō bātā phāṣṣe* 'hurā-liquor, wine (and) sorbet (?)' as against L e u m a n n 's 'Surā (-Getränk und) Windbeutel (und) Pfirsiche' (*op. cit.*, p. 12).

⁴³ Or even **bādag*, i.e., with the *-ka* suffix reduced to *-g* — along the line of evolution similar to that neatly attested in Ossetic, cf. W. M i l l e r, *Die Sprache der Osseten* (supplement to W. G e i g e r and E. K u h n, *Grundriss der iranischen Philologie*, vol. I), Strassburg 1903, p. 88.

Chin. **b'uo-d'âu* > Modern Pekinese *p'u-t'ao* — subsequently written 葡萄 up to this day — may only represent the ancient Ferghanian word sounding something like **bādag(a)*⁴⁷.

As has been remarked at the beginning of this paper, the problem of early loan-words in Chinese is also of importance from the point of view of Chinese historical phonetics. It may, at least in some cases, furnish us with additional evidence — the more important as it is external, i.e., involving extra-Chinese phonetic data — for or against results arrived at by methods of internal reconstruction, that is to say, those chiefly (if not exclusively) based on purely Chinese linguistic material and Chinese philological data. Such is the case with the loan-word just analysed, and I am now going to close my paper with a few remarks concerning this point. I shall limit myself to the problem of the final voiced plosives in Archaic Chinese, which, it may be added, is one of the most controversial ones. Throughout this entire paper, and particularly in search for the origin of the Chinese loan-word 蒲陶, I have followed Karlgren's Archaic Chinese readings, and it has been shown that this may lead to plausible results, especially with regard to the consonantal skeleton of the word forms under discussion. As is known, Karlgren's method of reconstructing final voiced plosives in Archaic Chinese is chiefly based on two criteria concerning either the script (the 'phonetic loans', *kia-tsie*, and the structure of the *hie-sheng* characters) or the rime contacts in the Archaic poetry — or both. There is no need to enter here upon the particulars of the method which have been discussed in a long series of Karlgren's works and recently further expounded — along with typical examples of the practical application of the method to concrete linguistic material — in his *Compendium* (already referred to *supra*, p. 28, footnote 28). The following remarks may therefore be limited to what appears to be strictly pertinent. According to Karlgren neither the structural features of the characters of the 甫 series (to which 蒲 belongs) nor the rime contacts of the series⁴⁸ allow us to reconstruct a final consonant in 蒲 (or any character of the series) which, consequently, is supposed to have been an open syllable in Archaic Chinese: **b'wo*. On the contrary, the character 陶 which in the *Shi-king*, ode no. 79, occurs as riming with a *ju-sheng* mono-

⁴⁷ Being not myself an Iranist, I am particularly indebted to Prof. J. Kurylowicz for discussing with me some important points of the Iranian aspect of the problem.

⁴⁸ See, however, the remarks in *Compendium*, pp. 359–360, on rime contacts of the phonological group to which the 甫 series belongs with the -*g* group. Since, however, these contacts are not supported by contacts with -*k* words, Karlgren considers them as unsufficient for the reconstruction of -*g* in the group under discussion and explains the attested contacts by some phonetic phenomena within the -*g* group alone.

syllable having final *-k*, 軸 ***d'ǝk*, is supposed because of that fact to have had a final *-g* in Archaic Chinese: ***d'ǝg*. It is to be emphasised that the case of 陶 ***d'ǝg* according to Karlgren's methodological principles cannot be considered as entirely certain, since the reconstruction of the final *-g* is almost exclusively based on this single instance of rime contact with a *-k* word in the *Shi-king*. Besides, there is no other strong internal evidence in Archaic Chinese to the effect that the monosyllable here in question had a final guttural; Karlgren himself considered the case of 陶 as uncertain since he was not sure whether in the *Shi-king* ode in question the words 軸 : 陶 had really been intended to rime with each other⁴⁹. None the less, the etymology of 蒲陶 as representing Iranian **bātaka*, **bādaga* (provided it be correct) gives important external evidence not only to Karlgren's supposition that 蒲 had been an open syllable, but also to his reconstruction of the final voiced guttural in 陶 as rendering in the case of the loan-word in question the Iranian suffix *-k-* (possibly pronounced *g*).

On the other hand, it is known that both Karlgren's theory of final voiced plosives in Archaic Chinese, and indeed his very methodological principles as well, have been put to doubt by many scholars, both Western and Chinese, [who — curiously enough — sometimes conducted their attacks in quite opposite directions. No detailed survey of all the criticisms expressed by many a scholar with regard to Karlgren's method and his reconstruction of Archaic Chinese can be attempted in this place, but it seems necessary to recall here some selected viewpoints directly concerning our present problem, i.e., that of final voiced plosives. Among the Western scholars the chief opponent

⁴⁹ Cf. his *Problems in Archaic Chinese*, "Journal of the Royal Asiatic Society", 1928, pp. 769—813; see p. 807 where the problem of the Archaic reading of the character 陶 is dealt with for the first time. However, there seems to be general agreement among Chinese scholars that the whole of the ode here in question follows the rime pattern *a: a: a: a*, cf., e.g., Lu K'an-ju 陸侃如, *Feng-Ya yün-li* 風雅韻例, "Yenching Journal of Chinese Studies", no. 20, 1936, p. 136. The above pattern — adopted also by Karlgren in *Grammata Serica*, p. 94 — implies that 陶 also rimes in the same stanza with 抽 and 好 which, however, are not *-k* words and in which the final *-g* is again a reconstruction. Some additional evidence may be sought in the fact that 陶 seems to belong into a large family of cognate monosyllables showing alternations of various final gutturals, see Karlgren, *Word Families in Chinese*, BMFEA no. 5, 1934, pp. 68—69, series nos. 263—294. All this, except for the rime contact with 軸, is no strong evidence, and the case remains unsupported by the script since neither the 'phonetic' 陶 nor any of two other characters of the phonetic series here in question (*Gram. Ser.*, no. 1047) represents a *ju-sheng* monosyllable having *-k*.

of Karlgren's methods of reconstruction and the results arrived at by those methods was undoubtedly the eminent French Sinologist H. Maspero⁵⁰. Karlgren, as we know, reconstructs final voiced plosives as accounting for the fact that certain *ju-sheng* characters (i.e., characters representing monosyllables having *-p*, *-t*, *-k* as finals) occur as 'phonetic loans' (*kia-tsie*) for and as phonetic components in non-*ju-sheng* ones, and *vice versa*. He finds a corroboration of this in Archaic poetry, especially in the *Shi-king*, where *ju-sheng* and non-*ju-sheng* characters frequently rime with each other. Instead of this, Maspero preferred to admit that in Archaic Chinese script many readings, no matter whether *ju-sheng* or non-*ju-sheng*, could as a rule be ascribed to a character (the number of readings being limited, however, by considerations of the intelligibility of the script), and that the scribes used various characters as *kia-tsie* (and also invented the *hie-sheng* characters) quite arbitrarily, without care for either the *ju-sheng* or non-*ju-sheng* original value of the character used or that of the monosyllable to be written therewith. According to Maspero, the only limitation was here the first arbitrary choice ("le premier choix limite nécessairement les choix suivants", see the review referred to, p. 179), not the alleged correspondence *-k*: *-g* (or *-g*: *-k*) or the like. Maspero's conclusion may be best formulated in his own words: "Il n'y a pas, à mon avis, aucune nécessité d'admettre des occlusives sonores finales" (*l.c.*, pp. 179—180). Thus, according to Maspero's point of view both 蒲 and 陶, which were open syllables in Ancient Chinese of the Sui period, are to be considered as having been open syllables in Archaic Chinese too.

Prof. Lu Ch'ü-wei's theory of final voiced plosives in Archaic Chinese offers a striking contrast to Maspero's point of view. In his highly interesting book *Ku-in shuo-lüe* (already referred to *supra*, p. 28, footnote 28), which is a kind of strong polemic against Karlgren, the Chinese scholar developed and advocated a paradoxical theory to the effect that there were, on principle, no open syllables at all in Archaic Chinese. He judged it necessary to put a final voiced consonant in whatever group of characters is considered as representing open syllables in Karlgren's system (see pp. 87—109 of his book). Lu Ch'ü-wei is, of course, well aware of the fact that his theory sounds queer, and himself adduces some phenomena militating against his hypothesis; these he tries to explain (pp. 106—108). He even says at the end of the chapter that Archaic Chinese may be suspected of having had some open syllables, but he declares his lack of knowledge with regard to the question what characters may have corresponded with them⁵¹. For my part, I think

⁵⁰ See his critical review of Karlgren's *Word Families in Chinese* in "Bulletin de la Société de Linguistique de Paris" XXXVI, 3, 1935, pp. 175—183.

⁵¹ *Ku-in shuo-lüe*, p. 109: 心裏不妨存一疑問，上古語是有開音綴的，可是不知道哪些字是的。

that Prof. Lu Chī-wei's theory is largely due to a too liberal interpretation of what he considers as a 'correspondence' between various phonological groups of characters, including facts like the one referred to *supra*, p. 40, footnote 48, in the interpretation of which Karlgren has shown more prudence. As it stands, we must state that according to Lu Chī-wei's point of view there must have been in Archaic Chinese a final voiced guttural in both 蒲 and 陶.

In addition to the two extremist theories due to Maspero and Lu Chī-wei, I will mention two other scholars from among those who have criticised Karlgren or have built reconstructional systems of their own differing from his with regard to the problem of 'lost finals' with which we are here concerned. The critical opinion of R. Shafer deserves special attention since it is based on comparative studies from which we would expect supporting evidence for Archaic Chinese reconstructions arrived at by internal methods. On the contrary, Shafer states that in his Sino-Tibetan comparisons (for which he used with some success Karlgren's Ancient Chinese readings) he has found only one single instance which seems to support Karlgren's reconstructions of an Archaic final voiced stop in a pair of monosyllables, while in all other cases, he says, the Tibeto-Burmic and T'ai languages either show no final consonant or have a consonant radically different from that reconstructed for Archaic Chinese⁵². Karlgren himself was aware of this difficulty (also concerning some true *ju-sheng* words in which the final stop cannot be put to doubt, as, e.g. 𣎵 'sun', Arch. Chin. ****ñiět**, Anc. Chin. ***ñziět**, corresponding to Tibetan *ñi-ma*, Burmese *ne > *niy*), which he tried to explain by both parallel dropping of finals in various Sinitic languages and specific innovations in Chinese (see *Word Families*, pp. 37–39), and we may hope that further progress in Sino-Tibetan linguistics, phonetics and morphology, will contribute to the elucidation of the problem. None the less, for the time being, Shafer's objection is serious, and we must reckon, as he states, with three different sets of restored final consonants: for Chinese, Tibeto-Burmic, and T'ai. This discrepancy — even if considered as not invalidating Karlgren's reconstructional system built on internal evidence and showing a remarkable coherence — fails, it must be said, to support Karlgren's Archaic Chinese reconstructions, and this is certainly a point deserving emphasis.

⁵² See Shafer's review of Karlgren's book *The Chinese Language — An Essay on Its Nature and History*, in "Journal of the American Oriental Society" LXX, 2, 1950, pp. 139–141. — Approximately the same objection — although based on less extensive comparative material — had already been formulated before Shafer by Maspero, see his review of *Word Families in Chinese* (already referred to), p. 181.

I shall close this short review of Karlgren's critics by mentioning Prof. Wang Li's book on the history of the Chinese language, the first volume of which — dealing with phonetics — has just appeared⁵³. The author presents his own reconstruction of the phonetic system of Archaic Chinese which radically differs from that of Karlgren (severely criticised⁵⁴) and still more so from that of Lu Chī-wei. On the other hand, Wang Li's system recalls Maspero's theory insofar as no final voiced consonants are admitted. This, however, does not mean that there are no 'lost finals' in Wang Li's system, since he believes that there were two *ju-sheng* tones in Archaic Chinese, a long one and a short one⁵⁵, both having final voiceless consonants (-p, -t, -k) and differing in the quantity, long or short, of the main vowel; only the 'short *ju-sheng*' words conserved their final stops, while the 'long *ju-sheng*' ones under the influence of their long vowels lost their consonantal finals before the epoch of Ancient Chinese (see p. 65 of the book referred to). Thus, Archaic final stops (voiceless) are reconstructed for a number of monosyllables having no final consonant in Ancient Chinese, viz. those which are considered as having evolved from Archaic 'long *ju-sheng*' monosyllables (see the chapter on the evolution of Archaic Chinese 'rimes' with final stops, pp. 83—90). For our present purpose it deserves to be mentioned that both 蒲 and 陶 appear as open syllables in Wang Li's system (p. 77 and 79 respectively).

As is easily seen, if the etymology of the early Chinese loan-word 蒲陶 = 葡萄 put forward by me in the present paper is correct—and I hope to have demonstrated that there are good reasons to consider it as correct — it

⁵³ Wang Li, *Han-yü shī-kao* 漢語史稿, vol. I, Peking, K'o-hüeh ch'üpan-shē, 1957.

⁵⁴ Among Prof. Wang Li's objections against Karlgren's treatment of the problem of 'lost finals' there is also his disapproval of the fact that according to Karlgren's system there were, as Wang Li believes, too few open syllables in Archaic Chinese. The author suggests that there has actually been no language in the world as poor in open syllables as Karlgren's Archaic Chinese (see p. 64 of the book). Compare Lu Chī-wei's theory referred to *supra*, pp. 42—43.

⁵⁵ Wang Li refers to the well-known gloss inserted by Ho Hiu 何休 (2nd century of our era) in the *Kung-yang chuan* 公羊傳, Chuang kung, 28th year, according to which the same character 伐 occurring twice in a rather enigmatic passage of the text glossed means 伐人 'to attack (people)' if pronounced long, and 見伐 'to be attacked' if pronounced short—this according to Ho Hiu being a peculiarity of the dialect of Ts'i 齊. The gloss had already attracted the attention of Ku Yen-wu 顧炎武 (1613—1682) who saw in it an early reference to tones, cf. Liu Fu, *Étude expérimentale sur les tons du chinois*, Paris 1925, p. 2—3.

not only offers positive external support for K a r l g r e n ' s reconstructions of the Archaic Chinese readings of the characters in question, but it also tends to refute all the contrary views just referred to. Corroborating K a r l g r e n ' s reconstructions to the effect that 蒲 was an open syllable and 陶 had a final** -g, the etymology proposed at the same time gives negative evidence both with regard to 陶, supposed to have been an open syllable in Archaic Chinese (M a s p e r o, W a n g L i), and with regard to 蒲, supposed to have had a final guttural (L u C h i - w e i). This single fact, of course, cannot be conclusive proof; I think, however, that it is one of the facts the student of Chinese historical phonetics must reckon with.