

PROBLEMS OF THE SIAMESE ALPHABET

BY PROF. DR. F. OTTO SCHRADER.

The review, signed J. B. in vol. XX of the Journal of the Siam Society (pp. 175 - 178) is such a gross misrepresentation of the aim and character of my paper "Transcription and Explanation of the Siamese Alphabet" (Asia Major, 1924, pp. 45 - 66) that I feel it my duty to reply to it.

Let me first of all thank my critic for having enabled me to detect two errors: in my table of the 'aksor klān (p. 49) the addition "(j)" to the letter *c* must be cancelled; and, on p. 48, "preserved in Tibetan" should read (as is evident from the preceding) "preserved in Siamese and Tibetan." J. B. speaks of "quelques erreurs" which a knowledge of Mr. Bradley's paper "Indications of a Consonant-Shift in Siamese" etc. would have spared me, but I can find no more than the former. Nor do I see how Mr. Maspero's paper "Contribution à l'étude du système phonétique des langues Thai" could have helped and not rather misled me. For the rest I need hardly assure J. B. that I should have mentioned both these papers, had they been available to me.

In the meantime my reviewer will have corrected his opinion as to my use of the term Indo-Chinese: I did not nor shall use it in the sense of the French *indo-chinois* (referring to the peoples and languages of Further India), but only in the one in which it is used (e. g. in vol. XXVI p. 929 of the Encyclopaedia Britannica) as an analogue of the term Indo-European, viz., with reference to that

great family of languages the two principal branches of which are the Tibeto-Burman and the Siamo-Chinese groups.

It is the importance of Indo-Chinese linguistics for the problems of the Siamese alphabet which my paper was intended to call attention to, and I meant to draw attention especially to the unique importance, in this respect, of the Tibetan language. The latter, as has been noticed long ago, is the key to Indo-Chinese linguistics. Mr. Karlgren's researches on Chinese phonology are no doubt very useful and much to be admired, but they only prove that it is impossible to reconstruct the supposed polysyllabic stage of that language without the help of Tibetan. I am, of course, well aware of the dangers involved in comparing just two languages only of a large linguistic family many members of which are as yet hardly known at all to us, but I can prove that even through this imperfect method valuable results may be attained, and I would ask J. B., who denies this—which, indeed, seems to be his main objection to my paper—the one question: How did Indo-European linguistics originate? Did it start with the axiom that all investigations about the supposed mutual connection of the languages believed to form the Indo-European family must be postponed until each member of that family was perfectly known? that it was inadmissible, e.g., to compare Gothic with Old Bulgarian before both Teutonic as well as Slav philology would have spoken their last word?? Need it be said that in that case even now the existence of an Indo-European family of languages would be nothing but a vague hypothesis? and that the said philologies would not be nearly as advanced as they are? No, Bopp was perfectly right to operate with the few languages known to him (only five when he began) and leave it to his followers to correct and elaborate his system. And the same holds good with Conrady. He had at his disposal practically all the cultivated members of the Indo-Chinese family of speech, i.e., just those languages of which more than their present condition is known—among them Tibetan, the Sanskrit of Indo-Chinese linguistics—and moreover quite a number, though comparatively few, of the rustic members of that family. What a monstrous injustice, in view of this

fact, to state that Conrady was in the position of the man who was going to construe the comparative Indo-European grammar out of "Latin, an Armenian dialect, and a bit of Tokharian" (J. B. on p. 176)! As for myself, not being a Sinologue nor an expert in Indo-Chinese linguistics, the only service I could render the latter was to approach the problems of Siamese phonology by means of the Tibetan, and I venture to predict (in spite of Mr. J. B.) that my conclusions, some of them at least, will be appreciated and corroborated by future inquirers in the field. One such corroboration has, indeed, been furnished by J. B. himself, viz., for the antiquity of the letter \tilde{n} which I supposed to be an ancient (not introduced or newly evolved) sound, because it exists in Tibetan too, while J. B. points out its existence in most languages of the Tai family—which latter fact by itself is not sufficient to prove its antiquity.

The most difficult problem of the Siamese alphabet is the existence of the voiceless mediae ṅ and ṅ by the side of the surds ṅ and ṅ now pronounced as sonants (see my article, p. 49 fl.). That none of the Tai languages gives a clue to their explanation, is sufficiently clear from the lists in Mr. Maspero's paper mentioned above. Here, then, is a case where we must either say Ignorabimus or step beyond the borders of Tai philology. I have pointed out the existence of these sounds in the neighbouring Mon, and I have also suggested, through Tibetan parallels, that they may owe their origin partly to a following r (e. g., in ṅṅ = Tib. *dran* "straight", ṅṅ = Tib. *phra* "thin, fine"), partly to a lost prefix (as in ṅṅ = Tib. *s-tan* "small bench", ṅṅ = *dam* (*h-dam*) "mud", ṅṅ = Tib. *byed-pa* (*h-byed-pa*) "to open").

Concerning the unvoicing of Indian (Sanskrit and Pali) initial g, j, d, b — now pronounced *kh, ch, th, ph* — and the voicing of t and p , Prof. Bradley is as right in observing that it is "possible that the present tonal distinction is the vestige and token of an earlier distinction in 'voice' which time has effaced" (loc cit. p. 27) as he is wrong in stating that "unfortunately no means for determining this historical question as yet appears, nor does one discern in what quarter to look for it" (p. 26). The quarter where to look for it is pre-

cisely the one overlooked in his paper, viz., comparative Indo-Chinese phonology which proves, at least so far as Tibetan is concerned, beyond a shade of doubt that those Siamese initials now pronounced *kh*, *ch*, *th*, *ph* but written *g*, *j*, *d*, *b* must have been originally the latter sound(1). It may be objected that "originally" in the sense here used does not really refer to the Siamese language as such but rather to the hypothetical common source of all Indo-Chinese (Tibeto-Chinese) languages. But is it not sound to maintain that a Siamese letter now pronounced but not written *kh* which on the one hand, viz., in Indian words, does duty for Indian *g*, and on the other hand, viz., in purely Siamese words, corresponds with *g* in Tibetan (where it is clearly not the result of a shift)—that such a letter cannot have been anything else, in early Siamese, but *g*? Exactly analogous is the case of the three other letters of the set. And just the same holds good with those letters now pronounced *d*, *b*, but written *t*, *p*: the written pronunciation is in Indian words the Indian one, and in genuinely Siamese words the original Indo-Chinese sound(2).

There is another point in Mr. Bradley's thoughtful paper which I may be allowed to settle here, in compliance with Mr. B.'s appeal to students of Indian dialects. Mr. B., though inclined to speak of two consonant-shifts—the mediae becoming tenues aspiratae, and *t*, *p* becoming *d*, *b*—is not sure whether not one of them at least "is a consonant-shift indeed, but one imported ready-made from India, in a provincial pronunciation of Pali on the part of the

(1) Examples: คน = Tib. *gan-zag* "person, man", คู่คน = Tib. *goms-pa* "accustomed", ครัว = Tib. *grva* "corner, cell, school"; ขา "torpor, lethargy": Tib. *'ja-ba* "lame", ชั่ง = Tib. *'jal-ba* "to weigh", วัตถุ = Tib. *brjid* "brightness, splendour", ทั่ว = Tib. *dan* "with, and"; หน = Tib. *mlun* "spear", ทุน = Tib. *duñ-ne* "constant, continual"; ทุบ = Tib. *buns* "mass, heap, bulk", กระเป๋า = Tib. *bag* "a narrow space", หนา = Tib. *bra-ba* "to have in great plenty".

(2) For examples (ตล = Tib. *tol-ba* "to reach, arrive", etc.) see Schrader, loc. cit., p. 50. Those words where the Indian pronunciation is preserved or little changed, as *tri-*, *trī* and *pra-*, *pra* (Bradley, p. 24), are easily explained phonetically (ibid. p. 28).

Buddhist missionaries who came to Siam" (p. 28/29), and he refers to Burnouf's remark that *gramana gāotama* has become *samana-kālom* in the South of India. This a pertinent query, indeed, and the answer seems to be in the affirmative in so far as the Tamil language, to which initial sonants are unknown, converts all initial sonants of Sanskrit and Pali into surds (e. g., *gō*, *duḥkham*, *jayu*, *buddha* are changed to *kō*, *tukkam*, *cayam*, *puttan*, resp.). But in Siamese we have, in the place of those Indian sonants, not simple surds, as in Tamil, but surd-aspirates which (as all aspirates) are thoroughly un-Dravidian; and still less can the shift, in genuinely Siamese words, from the simple sonant to the surd-aspirate be attributed to the influence of Indian missionaries; while a pronunciation like Siam. *dēchā* (for Skt. *tejas*) or *bāp* (for Skt. *pāpa*) is impossible in any Dravidian (as well as Aryan) tongue.

We may, then, take it for certain that there have been in Siamese two real consonantal shifts, and for possible, at least, that of the two the unvoicing came first. I do not see how the voicing could have come first, unless we assume that the tonal system proved a sufficiently strong agent to save the new sonants from being shifted, along with the old ones, to surd-aspirates. As to the third alternative of "Professor Hempl's formidable trilemma" (Bradley, loc. cit., p. 26 fl.), viz., that both changes have gone on together, I agree with Mr. Bradley that "two exactly opposite tendencies prevailing at once" is "a state of things difficult to conceive".

A system of transliteration is the more practical the less diacritical marks it employs. In my system, all purely Siamese consonants have no such mark, including *ṭ*, *ṽ*, *ṿ*, and *ṛ*. The first two I transcribe by small capitals (*ṭ*, *ṽ*)*, the third, for its tonal value, by *w*, and the fourth by *z* (i. e., the *z* in English *zeal*) which I have proved to be the original value of the letter. In all these cases, besides others, Mr. Cœdès resorts to underlining, not avoiding even a letter with a dot and a line under it (*ṽ*). As regards vowels, his system is simple indeed, but neither as exact nor as consistent as mine. E. g., he does not write at all the initial *ə*, though this is a

real 'aksor klān (transcribed by me, as at the beginning of this term, by means of the spiritus lenis which is its exact phonetic value), whereas he does write the Visarga (ः), which, as I have shown, is nothing but a device for indicating shortness in the case of those short vowels which could not be taken over ready-made from the Indian script. And finally, as to the accents, a single glance at one of Mr. C.'s transliterated texts reveals the awkwardness of their having been taken over untransliterated instead of in the form of numerals (added at the lower end of the word), as in my system⁽³⁾.

"M. S. méconnaît cette vue simple qu'une transcription ne peut pas être phonétique et en même temps respecter une graphie traditionaliste". This is wrong, since I have expressly declared (on p. 57 of my paper) that "to the transcription of vowels the historical principle is, if ever, anyhow not applicable now". But it may interest my critic to learn that his objection could be made to apply to the "translitération commode et irréprochable" of Mr. Cœdès who, in introducing his system (Recueil des Inscriptions du Siam, première partie, p. 10 fll.), declines making "quelques concessions à la prononciation Siamoise" and still transcribes ฝ (in spite of its being a "low letter" and in spite of its being derived from ฝ) by *f* (underlined) instead of by *w*, as I do, ฝ by *kia*, and ฝ by *kua* (in spite of ฝ = *y* and ฝ = *v* in other places). As for me, I have denied that the system wanted is possible without a combination of the two methods. The following little consideration will make my meaning sufficiently clear.

Looking at ฝ and ฝ (Tāi and nai) we should expect ฝ and ฝ to be read as *yāo* and *hao* (or rather *yāu* and *hav*). As a matter of fact, however, the last word is sounded *hua*. To consistently express this latter pronunciation we should have to write ฝ plus a symbol for short *a*, which symbol, however, does not exist. That is to say: in writing *hua* and not *hav* we (including Mr. Cœdès) correct an orthographic inaccuracy by renouncing the

(3) Mr. Cœdès, whose excellent work in the field of Siamese epigraphy I otherwise sincerely admire, will, I hope, forgive me this criticism which was caused by an unfair review.

traditional in favour of the phonetic method(4). This is the more conspicuous in view of Mr. C.'s transcribing, e.g., สุน by *svan*, keeping here to the traditional method. But in this case too, in my opinion, we ought to stick to the actual pronunciation and write *suon*, as long as it is unproved that there ever was a pronunciation *svan* or *suan* (which latter, with an *a* later on becoming *o* through the labializing effect of the preceding *u*, is indeed, likely enough). Again, comparing, e.g., เล่น "mud" with เล่น "to play," is it not intolerable for the linguist (as distinct from the epigraphist) to leave the difference of quantity of the two vowels unexpressed and write both times *e* or *ē*, though we have every reason to believe that เล่น was never pronounced with a long vowel but only written so for the sake of convenience, there being no symbol in the Siamese alphabet for medial short *e*, *è*, *è*,⁽⁵⁾ and the *lekh pèt*⁽⁵⁾ seeming dispensable because of the accent together with the nasal ending ?

We have, consequently a right, with the vowels at least, to make a concession to the popular demand of a phonetic transcription and this manner of transcribing the Siamese vowels, though perhaps not acceptable to the epigraphist (whose duty it is to faithfully transcribe the inscriptions with all their inaccuracies), is, to some extent at least, a necessity for the linguist.

For the Siamese a system of transliteration is wanted (in addition to whose of the phonetician and, may be, the epigraphist) which — like those established for the Sanskrit and the Arabic at the International Congress of Orientalists in 1894 and now in common use — is both exact enough for the linguist and practical

(4) That the traditionalist should, indeed, write *xāy*, *nay*, *yāv*, *hav*, results also from comparing, e.g., ไ้ with ไร่. These two words are now both pronounced with the same short diphthong *ai*, but the difference of their spelling can be only explained through the assumption that the final of ไร่ was originally not a vowel. And the very same relation exists, orthographically at least, between ๑๓ and ไร่.

(5) I am using here Mr. Cœdès' system, because I do not know to what extent the press is in a position to print the vowels in the way suggested by me.

enough for the educated author in general(6). A system of this kind is the one suggested by me (with a view to further improvement) in 1924 in vol. I of the journal Asia Major and defended in this paper.

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(6) For the need of such a system see, e. g., the plight expressed (in spite of Mr. Cœdès whose works he uses) by Sir Charles Elliot on p. 78 (with foot-note) of the third volume of his monumental work "Hinduism and Buddhism."