

PHONEMIC THEORY AND ORTHOGRAPHIC PRACTICE IN OLD TIBETAN

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While it is unquestionably true that the Tibetan grammarians' tradition¹ was more generally interested in phonemics than in phonetics, this observation ought not to be taken to imply that the purely articulatory level of linguistic description was neglected in their work, or even that this important, and essentially practical, level of linguistic analysis and theory was unknown in Tibet. To be sure, the Tibetan grammarians' principal concern everywhere on the level of phonology was with the phoneme, a fundamental concept of linguistic analysis with which they were notably well acquainted, if only because their own entire linguistic tradition was so completely grounded in the Indic sources of all linguistic science. The phoneme as a theoretical concept is found documented as early as the second century B.C. in the work of the Indic school represented by Patañjali; the same concept underlies all subsequent systems of Indic grammatical analysis; and in these Indic systems the concept of the phoneme is quite independent of any script or writing-system—even though in fact practical scripts or writing systems have all, to one extent or another, been based upon the phonemic principle²—if only for the reason that the earliest Indic systems of linguistic analysis themselves were all evolved prior to and independently of writing systems and script. The concept of the phoneme, like almost every other major idea in linguistic analysis and grammatical description, is an immensely old and important invention of the analytic genius of the ancient Indic civilization, a lasting tribute to the early flowering of the scientific method in that particular part of the world.

Plain as these facts are, they unfortunately are not even today apparently always understood in contexts where they are of basic importance. R.K. Sprigg, for example, has recently suggested that "... translating *yi-ge* . . . by 'phoneme' . . . [is a] significant infidelity [and an] anachronistic habit"; then, warming to his subject, he further writes that, "This has the effect of giving what is meant as a phonetic interpretation of the script the appearance of a phonemic analysis. It can hardly be that the painstaking and considered labours of Bloomfield, Sapir, Swadesh, Twaddell, and Bloch, to name only a few of the pioneers who strove to make the phoneme concept safe for linguistics, should have been anticipated by an eighteenth-century Tibetan orthoepist."³ These groundless slurs, showing as they do their author to be equally uninformed about the Tibetan grammarians' tradition, its Indic origins, and the history of linguistic science in the West, need not detain us longer here, except to note that they give new evidence for the impossibility

of doing justice to any portion of the Tibetan cultural heritage so long as one insists on treating it as if it had necessarily existed in a historical and intellectual vacuum. Of course, the theory and concept behind the term phoneme are no more a contemporary American concoction than is the term itself (English 'phoneme' simply reproduces French *phonème*, itself a neologism proposed by Dufriche-Desgenettes in 1873 as a handy equivalent for German *Sprachlaut*, which until then French scholars had calqued as *son du langage*⁴); to attempt, with Sprigg, to lay it to the charge of a handful of American scholars of the present century would be risible were it not so flagrantly egregious.⁵

But at the same time, and even as we acknowledge the longstanding Tibetan familiarity with the phoneme, we must always also keep in mind that for the Tibetans to have totally neglected phonetics, and to have concentrated instead almost entirely upon phonemics, would have been to show themselves but poor disciples of their Indic masters;⁶ and we know that they were not. One can hardly forget, or overlook, the categorization of the Indic tradition as one of *homo foneticus indicus*⁷; he was not solely *homo fonemicus indicus*, after all. But then the question becomes one of where, in the Tibetan materials, do we find evidence for this *homo foneticus* as distinct from a Tibetan *homo fonemicus*? And once we begin to consider the fuller implications of this particular question, we find ourselves confronted by an apparent paradox, almost a virtual enigma that probably must remain unresolved pending the intensive study of rather more of the corpus of the Tibetan grammarians and their tradition than has yet been explored, even in a preliminary fashion. It may well be that this paradox is only apparent, not real; but for the moment at least, it seems to be quite real enough to interpose serious obstacles in the path of our work.

Most briefly stated, the paradox is that in the Tibetan grammarians' tradition, we find very little mention of phonetics *per se*, and relatively little attention paid to the phonetic level of linguistic observation and theory as distinct from and as contrasting with the phonemic, at least in so far as we may at present judge the case from what we know of the expository writings of the grammarians themselves.

A significant exception—but probably only a case of the well-known principle of the "exception that proves the rule"—is represented by the fragment from the work of Bsod-rnams Rtse-mo (1142-1182) (*SGTT*, pp. 56-69), where, *inter alia*, his phonological observations are conflated with more than one fascinating, if always difficult, reference to language on the essentially phonetic level, e.g., his statement that consonant clusters in initial *g-* are "articulated first of all from the palate," which must indicate a phonetic realization of phonemic /gser/ as something along the lines of phonetic [χser], or the like.

But against the exception represented by this remarkable text, we must balance the general expository silence of the grammarians on the phonetic level. The heart of the paradox is that despite this rather overwhelming silence of the texts, we do indeed find plenty of overt evidence for Tibetan

observation and analysis on the phonetic level—but it is to be found, not in the expositions of the grammarians, but in the epigraphical data of the script itself. Furthermore, and to further heighten the paradox, the evidence for Tibetan observation and analysis on the phonetic level is chiefly to be identified in elements of the script that are known, at least to us, only from Old Tibetan Mss. and other epigraphical sources—sources with which, again to the best of our present conventional wisdom, the Tibetan grammarians were unacquainted, and to which, at any rate, they have not yet been detected at the work of describing. A puzzling paradox indeed—one that we may hope future studies will somehow resolve, but one that is, for the moment at least, truly a paradox.

What we have in mind in this connection is of course the evidence now available to us from Old Tibetan epigraphical sources for the employment of different, or at least altered, vowel signs of the Tibetan script in order to write different allophones of the vowels of the language, particularly in the case of the two vowel phonemes /u/ and /i/. The overt graphic symbolization of allophones presupposes their identification, at least on some level or another of linguistic analysis and sophistication; one cannot write what one does not hear, or what one does not recognize when one hears it. Such allophonic writings diverge, it is true, from the overall phonemic principle that underlies all practical and workable orthographies established on the alphabetic principle; but at the same time that they diverge from that principle, and by the very fact of their divergence, they also provide us with concrete evidence for the phonetic observation and awareness that must necessarily underlie any overt graphic symbolization, i.e., any “writing,” of allophones. This is where we find the most impressive evidence for the study, observation, and sophisticated recognition of the phonetic aspects of the language on the level of *la parole* within the Tibetan tradition—not in the overt statements of the grammarians, but in the covert manipulation of the orthography as we now know it from chance finds of precious fragments of non-canonical materials.

For writing an allophone of the vowel phoneme /u/, a phone that was probably both fronted and unrounded, and for which the cover-symbol [ü] may serve as a convenient transcription,⁸ we have overt epigraphical evidence from the Tun-huang *Ch'ien-tzu-wen* in Chinese script with Tibetan phonetic glosses, a manuscript in which

the scribe makes consistent use of not one but two subscript *u*-graphs. . . . One of his two *u*-graphs is of course the normal symbol for *u* in the Tibetan script—a subscript hook initially descending to the right, then curving smoothly around and ascending farther up to the left. The second of his two *u*-graphs, and the one which he uses for those cases where the pronunciation *ü* was involved (with its secondary, assimilatory fronting effect upon the previous vowel, or on occasion, with this fronting already indicated in the vocalization of the prior syllable) is identical with the first except that at the end of the left-ascending completion of the graph the line is abruptly terminated, and ended off either with a distinct downward

thrust, or with a heavier (and hence blacker) "pressure point" somewhat reminiscent of similar techniques in Chinese calligraphy.⁹

And for the writing of an acoustically distinctive allophone of the vowel phoneme /i/, a large number of early Mss. and other epigraphic sources employ the so-called "reverse *i*-graph" (known to our various colleagues as "le *gi-gu* inversé,"; or "das umgekehrte *gi-gu*"; it is surely significant that we know no old Tibetan name for this striking anomaly of the older script, nor to the best of my knowledge have the grammarians ever commented upon it). This is a graphic mirror-image of the usual *i*-graph, which arches up and to the right instead of up and to the left, as does the normal *i*-graph with which we are familiar from the later xylographs and from modern Tibetan orthography. The deviant *u*-graph for the *ü* allophone of /u/ is rare, and even when it is found, its employment appears to have been restricted to the graphic representation of non-Tibetan, particularly Chinese words; but the "inversed *i*" is extremely common in a very wide range of early Tibetan epigraphic materials, where it is used in the orthography of hundreds, even thousands, of purely Tibetan forms. If for no other reason than this, the "inversed *i*," which hereafter we shall transcribe as *ï*, must surely be reckoned with in any serious account of phonetics, phonemics, and graphs in early Tibetan.

Earlier attempts at correlating the epigraphic evidence for two differentiated *u*-graphs in certain early Tibetan transcriptions of Chinese have apparently been accepted in the field; at any rate, those attempts have been greeted with silence in the literature ever since they were first proposed in 1967, and under such circumstances one can do little but yield to the natural temptation to interpret silence as consent.

But this has scarcely been the case with attempts made a year earlier, in 1966,¹⁰ to marshal evidence that would allow a phonetic, and specifically an allophonic, interpretation of a significant number of the cases in which we find the *i*-graph in our early Tibetan sources, interpreting this *ï*-graph as a writing for an allophone of /i/, speculated in 1966 typically to have been found "in the context of an immediately preceding or following *a*-vocalization," where the graph in question apparently was "used to write an allophone of /i/ partially assimilated to the position of articulation of the /a/. . . In these terms we might well speculate that the resulting 'harmonized' vowel written with the *ï*-graph was something like a high open unrounded [I] in contrast to the 'unharmonized' /i/, probably a high close unrounded [i]. . ." (p. 263). But in this formulation of 1966, the key expressions were, and are, such carefully delimited goals as the aim to "do much to bring order into their otherwise apparently random usage" (p. 263), i.e., of the two *i*-graphs; and the boldest claim that was made at the time had to do simply with it now being "possible to observe a large measure of order" (*loc. cit.*). In other words, the search then (and now) is only for a phonetic interpretation of a significantly large number of the cases. It was stressed at the outset that "in [one and] the same document it is easy to see the copyist

wavering between using and not using the [i] notation," and also that the most one could reasonably hope for in this connection was to identify "the dominant patterns involving the use of *i* (both *loc. cit.*). The goal was clearly not to state some iron-clad rule or to discover a "final solution" that would then somehow admit of application to each and every notation of *i* in each and every early Tibetan text, whether now known and edited or still unknown and awaiting publication; and it certainly was not to formulate rules that would somehow explain why the *i*-graph was *not* used on any given occasion in a given document, since the entire concept of the formulation of negative rules that would cover the non-occurrence of evidence is one that is at best far removed from the usual limits of linguistic theory.

Above all, one would hardly expect to find here—nor did one so expect in 1966—a rigorously codified, absolutely exceptionless rule; and indeed, none was found. Again, it must be stressed that the chief concern of the 1966 study was the identification of "dominant patterns," nothing more—and by the same token, nothing less. This follows directly from the nature of the allophone, or phone, *vis-à-vis* the theory and definition of the phoneme. Phonemes are significant contrastive units of a language, on the level of *la langue*. They are fixed, finite in number, definite, and a matter of linguistic fact; if you know the language, you are able to recognize the contrasts and oppositions that operate within its phonemic system. Alphabetic writing systems are phonemic in principle, otherwise they would not be able to "work." (One must of course except from this general statement such writing systems as that of English, where the alphabetic, i.e., the phonemic, principle, has gone berserk, and where as a result the entire orthography, in a sense, hardly "works" at all.)

Allophones, or phones, are something quite different. They exist on the level of *la parole*; they are not a matter of fact, they are a matter of opinion, in the sense that their identification depends upon our keenness of ear. We cannot ask how many allophones or phones there are in a given language, only how keen or well-trained our ear is, and hence how many we will probably be able to detect. And even an ear that is keen one day may be dull the next. Allophones or phones are not fixed, finite in number, or definite matters of linguistic fact. Any writing system that begins to note them in anything approaching an even moderately exhaustive manner, down to and including the IPA, almost immediately finds itself with more symbols than anyone concerned with the whole operation can conveniently remember, much less employ. All allophonic notation, by its very nature and also within the terms of the definition of the phoneme itself, can never be expected to be totally rigorous, absolutely rigid, thoroughly exhaustive, or uniformly regular. Any and all such criteria would be within the realm of the phoneme, not the phone.

What was done in 1966 was rather to attempt to apply the theory of a phonetic, allophonic interpretation of the *i*-graph as a writing at some times and in some texts for an allophone of /i/. In the process of that attempt it

was found that such application made it possible to establish a significant hierarchy of categories within the Old Tibetan epigraphical materials then available to us: we found that, as we might expect from the nature of the matter, "the *i* was not always used when it might have been; . . . sometimes . . . a scribe begins to use it and then apparently grows tired or bored with the whole process as the manuscript continues" (p. 264), while in other texts, "the scribe . . . clearly remembers he prefers the vowel-harmony role of the *i*-graph" (p. 273), and in still others, the scribe had become a victim of what we called "graphic mannerism," and eventually "lost all interest in recording the distinction" (*ibid.*). From this there followed in turn certain tentative conclusions concerning what these correlations of writing and language might possibly have to tell us about which texts were earlier, and which were later, within the confines of that large body of Old Tibetan epigraphic materials for which we have no other means for establishing datings.

Particularly in consideration of the extremely tentative nature of these early approaches to the question of the *i*, it is gratifying in the extreme to learn that they have now been substantially supported by—and what is even more important, that they have proven themselves to be useful in explaining the epigraphical evidence of—a large number of early Tibetan documents that were still unpublished and largely unavailable in 1966. In particular one is pleased to learn that Dr. Manfred Taube has, in his painstaking studies of *ca.* 263 fragments of early Tibetan texts now to be found in the Turfan-Sammlung of the Academy of Sciences in Berlin, DDR,¹¹ found the 1966 formulation on the allophonic role of the *i* to be of utility in bringing "a large measure of order" to the data: "In der Tat ist auffällig, daß auch bei diesen Texten unterschiedlichster Provenienz das nach rechts eingerollte *i*-Zeichen in über Hälfte aller vorkommenden Fälle (28 von 50) in engem Zusammenhang steht mit einer Silbe, die den Stammvokal *a* enthält: *dge ba'i* (Tib 129), *bdagi* (Tu 91), *lhan čig* (Tib 110), *č'ab sri'd* (Tib 117) usw." (p. 132),—in a word, in precisely the phonological contexts of occurrence predicted by the 1966 formulation. At the same time, the occurrence in these same Turfan-Sammlung documents of instances of *i* that are not covered by the 1966 allophonic formulation should occasion no surprise, since as already pointed out, this in and of itself fits in quite well with the nature of the allophone, and indeed only serves to underscore the essentially sporadic, impressionistic, and unpredictable nature of all allophonic notation.

But at the same time that one is gratified to be able to point out the way in which the 1966 formulation for the allophonic role of *i* in Old Tibetan texts has been validated by Dr. Taube's work with the fragments in the Turfan-Sammlung in the DDR, one is also regretfully compelled to comment on certain aspects of other work along these same lines, of fairly recent date, for which we are in debt to Dr. Tor Ulving (hereafter U).¹²

U reaches the conclusion that "we have to resign to the view that the 'reversed *i*-sign' is merely a graphical variant of the ordinary *i*-graph, con-

ditioned [*sic*—understand as ‘influenced by,’ in the sense of ‘in imitation of’?—RAM] by the employment of two *i*-graphs in the Indic prototype of the Tibetan script” (p. 215). But the key passage in U’s conclusions, and the line that indeed fortunately obviates the necessity for refuting most of his 1972 contribution, is found immediately preceding the line just cited: “I do not see that the *i*-graph writings can be reduced to any system based on the assumption of their symbolizing a special vowel quality” (*loc. cit.*). By writing “. . . reduced to any system . . .,” U clearly demonstrated that he has missed the point, and at the same time conveniently identifies for us just what the point is that he has missed. There never was, nor is there now, in all of this, any question of “reduction to a system.” Systems are matters of phonemes, not of allophones or phones. With a notation, particularly with a partial and sporadic notation, reflecting and based upon allophones, we do not expect to encounter thoroughly consistent, wholly predictable systems toward which we may “reduce” our data—nor do we find one here. Any allophonic notation, ancient or modern alike, is a question of the keenness of the ear of the recorder, and is subject to the limitations imposed by the constant necessity for making hard choices between whether to note a feature of pronunciation—assuming that one has really heard it—or not to note it, in which case one sincerely hopes that one has not. The entire process is well described as an infinitely intertwined set of connected continuities and gradual gradations that constantly shade and fade into one another; it is the variety of linguistic problem for which we expect multiple, non-unique solutions; and in this last in particular, we are hardly disappointed in the evidence available from our early Tibetan documentary and epigraphical sources. U has missed the point of all this.

At the same time, one cannot help but admit that in a certain limited sense, it is indeed fortunate that U’s paper provides us with such clear evidence of his misunderstanding of the theoretical basis for this entire question, since otherwise his arguments would be difficult to refute. They would otherwise be difficult to refute for two reasons; one, his arguments are much belabored by a number of internal contradictions; two, he proceeds on the basis of a set of private assumptions concerning details of the pronunciation of early Tibetan that are clearly and obviously gratuitous.

Under the first of these two rubrics, a full inventory of U’s internal contradictions would not only tax our readers’ patience but also far exceed our own limits of energy, time, and publication-space; fortunately, a representative sample of the evidence will suffice.

Internal contradictions of argumentation and methodology severely limit the utility of U’s remarks from their outset. Thus, at the very same time that he stresses “the fact [*sic!*] that we know next to nothing about the Tibetan dialects of the early times,” (p. 208, note 1), U simultaneously scores us in the following terms: “Miller reveals the astonishing fact that he evidently takes no account at all of the spoken language hiding between the text . . .” (p. 212). Similarly, we are charged with studying “simply juxtapositions of syllables without the slightest consideration of their func-

tioning in the living spoken language" (p. 215). Now as a general rule of thumb, in philology as well as in life, either one can do something, or at least attempt to do it, or one cannot; but it hardly seems fair thus to be scored for not attempting what U first of all points out (p. 208, note 1) is in his own view impossible. All this seems not only internally contradictory but also rather less than fair, particularly since the 1966 formulation was based in its methodology upon just what U mistakenly claims was not "taken . . . account of at all," i.e., upon an attempt in so far as our sources and knowledge today allow it, to study the orthographical idiosyncracies of Old Tibetan texts in the light of the evidence that we do have available for what the nature was of the spoken language that must lay behind them. Nor were many such attempts made in the literature generally, prior to 1966. Even the metaphor of "spoken language hiding behind the text" with which U assails us (p. 212) is taken over directly from our own 1966 formulation, where we noted how "the facts of the living language have more than once broken through [the] best-intentioned orthographic defenses" (p. 257).

Nor are the internal contradictions of U's rebuttal limited to his approach to our 1966 formulation; they literally stud every passage of his own new contribution as well. First we are told that "in the modern Lhasa dialect . . . vowel assimilation takes place only when there is a close connection between the syllables involved, as between base and suffix, or the constituents of a disyllabic compound word" (p. 209). But shortly thereafter it is categorically claimed, but with no supporting evidence advanced at all, that "*lha myi* 'gods and men' [is] hardly the type of collocation where vowel assimilation operates" (p. 213). As we shall note further below, the precise nature of this "connection," resp. "close connection" that U professes to be able to identify intuitively within the morphology and syntax of early forms of Tibetan is extremely nebulous; but at the very least, and giving the entire issue all possible and due benefit of doubt, if we are looking for "a close connection between the syllables involved, as between . . . the constituents of a disyllabic compound word," might not a word such as *lha myi*, *lha myi*, *lha mi* be a likely candidate? On what grounds has it simply been excommunicated, and *why* is it "hardly the type of collocation where vowel assimilation operates"? Little more need be said, and nothing more need be added, in the face of arguments as capricious and volatile as these.

Second, there is the rather more serious issue of U's gratuitous assumption of concrete details concerning the pronunciation of Old Tibetan. Fresh from having warned us that "next to nothing is known about the Tibetan dialects of the early times from which his [i.e., RAM's] documents stem" (p. 208, note 1),¹³ U next proceeds himself to a wholly amazing display of data concerning intimate internal details of the pronunciation of just those same dialects—data to which he alone appears to be privy, and data presented without a shred of philological evidence. The detail that most of U's unwarranted claims and statements in this connection are disarmed, if not defused, by a judicious sprinkling of qualifying "probable"s and "pro-

bably"s, and similar disclaimers throughout his contribution, is sadly offset by his penchant for advancing other claims not so qualified with the most emphatic terms available, e.g. "surely," "out of the question," and "inconceivable," to list some of his favorite epithets. Again, only a sampling of this egregious gratuitousness is possible, but it will suffice.

Thus, U writes, "it surely makes a great deal of difference to our interpretation of a collocation of morphemes whether we have to do with *mi* the negative or *mi* 'man,' or *la* the particle or *la* 'mountain pass' " (p. 212): but *how* can we be this "sure"? "It is not probable that a numeral was so closely bound to this preceding noun that its vowel was affected" (p. 212): who established these probabilities, and on what grounds? "*lha myi* . . . [is] hardly the type of collocation where vowel assimilation operates" (p. 213): *how* do we know this? "It is out of the question that the *a*-vowel of the unstressed particle *la*—there is no reason to believe that it was otherwise at the period concerned—should influence the vowel of a following word with which it is not in any respect closely connected" (p. 213): *why* is it "out of the question," *how* can anyone be this certain about stress in a language none of us has ever heard (or ever will!), and *what* is meant or implied by "connection"? "It is inconceivable that the vowel of the negation was assimilated to the preceding particle . . ." (p. 213): inconceivable to *whom*, and upon what grounds? ". . . the 'ablative' particle *nas*, itself an unstressed morpheme hardly capable of influencing the vowel of the following particle" (p. 214): *how* do we know it was unstressed, and *who* can tell what it was or was not capable of in Old Tibetan? "This particle was in all probability unstressed then as it is now, and it is very unlikely that its weak vowel would have assimilated the *i* to *ĩ* . . ." (p. 214): no comment necessary. ". . . the few instances . . . where vowel change would be theoretically possible—a few closely knit constructions . . . lose their power of conviction" (p. 215): who knows what is or is not "theoretically possible" in any language, and what precisely defines a "closely knit construction," also how are the constructions cited in this connection a whit different from *lha mi* which is "hardly the type of collocation where vowel assimilation operates"? ". . . it is out of the question . . ." (p. 216): no comment.

Collation of U's many references to "connection," resp. "close connection" or "closeness" as a quality or linguistic criterion obtaining between forms in Old Tibetan throws but little light upon what he intends to mean by these terms. The criterion he thus invokes is apparently yet another of those several intimate details about Old Tibetan to which U alone in all the world is privy. About the only coherent pattern that identifies itself under this rubric appears to be rooted in translation into English: Old Tibetan forms that may end up being translated as English nouns (*la* 'mountain pass,' *mi* 'man') seem in U's system automatically to be assigned to a linguistic category that is somehow differently weighted from the category to which belong forms that must be translated otherwise, or forms that being "particles" do not translate very well into English, or into any Indo-

European language, in the first place. One wonders what the Old Tibetan speakers responsible for our texts would have made of this nice distinction.

Still, it would hardly be correct to leave the impression that there is nothing to admire in U's contribution. One cannot, for example, help but admire the confidence that U exudes in his own privy sources for information concerning the details of the pronunciation of Old Tibetan. After U has, for example, told us flatly that he can point to "syllables which in the living speech of the time cannot in any way have constituted such closely knit entities as are the condition for vowel assimilation to operate" (p. 217), how can one possibly reply, except to offer what must necessarily appear to be, particularly in the face of such aplomb, the entirely lame excuse that unlike U, one has not heard "the living speech of the time," and so one cannot say nearly as much about its details as U apparently can, and surely does. Most of us would count ourselves fortunate to have at our disposal descriptions of living Tibetan languages encompassing phonetic descriptions of the order of precision, particularly with respect to stressed and unstressed syllables, that U seems to have at his command for the "living speech" of the Old Tibetan period. Nor can one cease to envy U for controlling an ancient language so securely that he is able to state flatly and firmly what is and is not "out of the question" or "inconceivable" for occurrence in that language; few of us can say as much for any language, even modern languages we know, speak, and use daily.

Buddhism teaches us that all outer appearances are illusory, that everything having forms is impermanent. No one doubts that this is so; still, this noble doctrine can only cause practical difficulties if we apply it quite literally to certain mundane encounters, and on its strength, e.g., refuse to remove ourselves from out of the path of a speeding train or motor-car. Illusory and impermanent though the vehicle in question may be, we had still better get out of the way when we see it coming. Linguistics teaches us that terminology is essentially trivial, nothing more than names that the linguist assigns arbitrarily, and that as a consequence one linguistic term is always quite as good, or as suitable, as another, so that it does not really matter just what a given linguistic entity or phenomenon is called; and there is also little point in arguments about whether something ought properly to be called this or that in linguistic discourse. Again, no one doubts the truth of this, but that still does not mean that it is a very good idea to push the principle involved to its fullest limits, e.g., to refer to as 'nouns' words in a given language that have inflections for tense, aspect, or voice, or as 'verbs' words involved in genitive, accusative, or ablative case-relations, etc.

Reading U teaches us that in many respects, the question of the arbitrary nature of linguistic terminology is rather like the question of the reality of the speeding motor-car bearing down upon us in the road: again, experience shows that an important gap must here be recognized to exist between theory—or belief, if one wishes to put it that way—and practice. Though linguists are supposed to understand that all linguistic terminology

is arbitrary, often they either do not so understand, or even understanding it, they forget it, and as a result draw misleading conclusions simply on the basis of their over-literal understanding or interpretation of an arbitrary term that someone else has happened to employ. This is always unfortunate.

In the present instance, serious misunderstanding appears to have occurred on U's part simply because the 1966 formulation of the allophonic role of the Old Tibetan *i*-graph was then expressed using the term "vowel harmony." It is now clear that the introduction of that term in order to categorize the evidence available for a number of rather varied sets of assimilatory shifts within the vocalisation of Old Tibetan, at least in so far as we (who lack U's privy sources for the intimate details of Old Tibetan pronunciation, alas!) have evidence for these assimilatory changes at work in the texts, has proved to be the quite innocent source for much misunderstanding, and even the origin of serious scandal. All this has been as unfortunate as it has been unnecessary. Even as we move ourselves out of the path of the speeding motor-car, we ought never to forget, this is not real: no forms are permanent, no linguistic terms are absolute.

Another, related, and equally unfortunate misunderstanding has centered about the relevance of citing non-Tibetan forms as somehow providing what U most recently refers to as "illustration [of] the general principle . . ." of this or that historical linguistic change (e.g., U, p. 204, footnote 2). Earlier U had objected most strongly to our citing Middle Chinese forms, as possibly exhibiting significant historical parallels for developments within Tibetan, preferring instead to illustrate the same point himself with citations from Old Nordic.¹⁴ To this a likely reply seemed to be that while "[t]he dangers of citing Chinese or for that matter any non-Tibetan evidence in this connection are only too apparent through all the discussion of the problem . . . the controlled introduction of Chinese evidence . . . attempted in 1955 still seems to be preferable both as theory and as practice to the only model for these developments which [U] offers, 'the *a*-umlaut of the type well known from Indo-European, e.g. Old Nordic *horna* < **huma*'. Surely Chinese is at least as much to the point here as Germanic."¹⁵ U has now replied that we "quite overlook[ed] the fact that these IE forms were not compared with any *forms* in Tibetan, but were only meant to illustrate the general principle of *a*-umlaut." (U, p. 204, footnote 2).

The gratuitous assumption that there exists, somewhere in the world, a "general principle of *a*-umlaut," apparently to be understood, from U's lines, as a natural tendency of human speech, is one fraught with so many theoretical and methodological perils that it would be impossible even to begin to list them here. For the present, it must suffice to point out that calling a given phenomenon in a given language a "general principle," even if the language is Indo-European, does not necessarily make it so.

Second, and more importantly, we have here to face up somehow to the entire issue of whether or not superficial historical parallels from non-

related languages are ever relevant in the type of discussion here underway. Linguistic history is still history. Simply because something may be shown to have happened once, somewhere, to someone, at some time in history, does this really make it any more (or for that matter, any less) probable that the same thing also happened independently elsewhere, at another time and place, to others? One wonders. It has been suggested that "Chinese is at least as much to the point here as Germanic;" why? Because of the existence of a considerable body of evidence that points to the validity of the assumption that, even though not genetically related,¹⁶ Chinese and Tibetan are nevertheless still both languages that belong to a common "linguistic area,"¹⁷ and also because of the secondary assumption that parallels in details of linguistic development shared between members of a common linguistic area are indeed significant for the consideration of the history of the languages in question. This is why Chinese data is probably relevant to the history of Old Tibetan phonology, and *vice versa*; it is also why data from Old Nordic and other Indo-European languages is not—unless we are prepared to put Indo-European also into the same "linguistic area" with Chinese and Tibetan.

Third, there is the problem of linguistic terminology, which need not detain us much longer, and has already confused the total issue far more than was ever necessary. Since "vowel harmony" apparently is so prone to such serious misunderstanding, e.g. by U, it was an innocent but apparently also a bad choice of terms. We ought rather to look for a less misleading term, even though no linguistic term is ever so perfect that someone somewhere will not be misled by its employment. But particularly keeping in mind the importance of the concept of the linguistic area in advancing a discussion such as the present one, we might now suggest replacing "vowel harmony" in the 1966 formulation with the term "*i*-breaking," thus identifying, in terms of a common linguistic area, the Tibetan phenomenon in question with the generally thus-designated and well-attested phenomenon known to us from most of the Altaic languages, particularly (but by no means exclusively) from Mongolian.¹⁸ The term "*i*-breaking" is quite accurately descriptive of what was at the heart of the 1966 formulation that has caused U such difficulties: an assimilatory shift of an [i] allophone of /i/ in the direction of [I] when found in the immediate phonological context of a preceding or following /a/.¹⁹ Of course it remains to be seen whether thus changing the name of the thing helps to reduce the misunderstanding that has surrounded it; but one may hope so.

The familiar and often-cited examples of this "*i*-breaking" as it is observed, e.g. in the Mongolian languages are not only, without serious question, rather more to the point in all this than examples from Old Nordic ever can be; they also provide significant and useful parallels for the Old Tibetan data, and at the same time they are especially valuable for the light that they incidentally throw upon the unrealistic nature of many of U's assumptions, particularly upon his often reiterated pronouncements to the effect that this or that sequence is "hardly the type of collocation where

vowel assimilation occurs," or that "it is out of the question that the *a*-vowel of the unstressed particle... should influence the vowel of a following word," or that "an unstressed morpheme [is] hardly capable of influencing the vowel," etc., etc. As already pointed out above, none of these *ex cathedra* rulings may be questioned, much less fairly criticized, because we are never told upon what data they are based; only U knows, and he does not tell us. But essentially underlying all these claims it is not difficult to identify the *a priori* assumption that in any language, including Tibetan, an unstressed vowel cannot, and never could, have been "responsible" for assimilatory shifts of the "vowel harmony" variety.

How does this assumption fare when measured alongside the established facts of the history of the Mongolian languages? Not very well. The Mongolian languages always have their stress (which Poppe calls *expiratorische Druck* or *Akzent*) on the first syllable, unless the second syllable is long or a diphthong; and this is a feature that they inherited directly from the Proto-Altaiic linguistic unity.²⁰ Nevertheless, it is exactly and precisely an unstressed *a* in the second syllable that triggers the "i-breaking." In other words, if we apply U's *a priori* assumption, his "general principle," to the Mongolian languages, then we would be able to "prove" that the "i-breaking" seen in such sets as Khalkha *maχχə* 'flesh, meat,' = Mo. *miqan* 'id.,' or Urduš *šara* 'yellow' = Mo. *sira* 'id.,' along with hundreds of other examples,²¹ could not and never did take place: but they did. So U's "general principle" and his *a priori* assumptions about what an "unstressed vowel" can or cannot do simply do not hold up against the evidence of the history of languages from the same linguistic area, no matter how neatly they may fit into the history of certain of the Indo-European languages.

Similarly, and this time restricting ourselves solely to Tibetan materials, one could, by applying U's *a priori* assumptions, "prove" that there never took place the fronting of the stem vocalisation before the addition of the diminutive morpheme in *-Cu*, changes studied and documented two decades ago in painstaking detail by our colleague Uray.²² But these shifts also certainly did occur; we have the evidence in the texts that they did. In other words, it is really very risky to postulate what is "unlikely" or "inconceivable" in linguistic history. Just as in all history, so also in the history of languages, it is sometimes apparently just the most unlikely, the most inconceivable things that actually do turn out to happen.

Further it might be noted that, quite like the Old Tibetan allophone sometimes written in our texts with the *i*-graph, the Mongolian (and other Altaiic) "i-breaking" is a phenomenon that also began as a peculiarity of pronunciation, an allophonic, sub-phonemic feature of the languages in question. Sometimes the assimilatory shifts involved "went all the way," resulting finally in phonemic, i.e. in genuine "linguistic change," but sometimes they did not. And this in turn is why Poppe writes of the "i-breaking," that it "is a peculiar feature of spoken [Mongolian] languages. This does not mean, however, that it occurs there in all cases. On the con-

trary, there are numerous cases in which the vowel **i* regularly remains as such and, when becoming another vowel, it does so independently of the vocalism of the following syllable. . . . In numerous cases the 'breaking' occurs, . . . but there are numerous inconsistencies."²³

Here we have an excellent account of what is exactly the same situation as the one that we observe in the epigraphic evidence for the Old Tibetan allophonic pronunciation of /i/ as something like [I], and sometimes written with the *i*-graph; we can also understand from this passage, and from the parallels with the "i-breaking" suggested above, why it is fatuous to continue to look for absolute rules, or total consistency, in the scribal employment of this graph in our Old Tibetan texts. Consistency is of the nature of the phonemic principle, and of writing systems based on that principle; inconsistency is of the nature of phonetic writing, with its sporadic, always less-than-rigorous notation of auditory differences on the sub-phonemic level. Once this is clear, then it also becomes clear just how and why scribal inconsistencies may themselves be of value, providing as they do a means for dating more than one text, at least within terms of a relative chronological sequence;²⁴ but even more important, perhaps, is the conclusion that they do not, in and of themselves, invalidate the 1966 formulation for the allophonic role of the Old Tibetan *i*-graph.

With this, we have done more than simply attempt to explore, at least in a preliminary fashion, some of the major implications of the concepts of phone and phoneme, and particularly some of the ways in which these essential—and essentially—Indic concepts of linguistic science relate to the problems of the orthography of our Old Tibetan texts, even though that alone, to be sure, is quite worth doing, and hence has here been worth our time to undertake. Nevertheless, quite over and above this immediate consideration of enhancing our understanding of the phonological dimensions of Old Tibetan and its orthography, there remains another quite separate but intimately related domain of problems: the eventual development of a more rigorously methodological approach to the question of Tibetan text-criticism than anything that has been up to now at our disposal. The exploitation of our newly enhanced understanding of the principles upon which the Old Tibetan orthography was based, in particular of its sporadic employment of allophonic writings for certain of the vowels, toward the ultimate ends of text-criticism remains one of the most important of all possible applications of these studies, and as such ought not to be lost sight of, even if the purely linguistic aspects of these problems were not of themselves of value and interest. Well over two decades ago, Li Fang-kuei remarked quite accurately that "a critical apparatus for the edition of ancient Tibetan texts has not been available."²⁵ Nor may we expect that one will ever become available, until we ourselves become better equipped to operate in the realms of Old Tibetan phonology, phonetics, and orthography. Viewing the question in this way, we soon perceive that whether Sprigg mistakenly believes that the Tibetans were ignorant of the phoneme, or even if he erroneously suggests that the phoneme is a modern

American invention, or whether or not Ulving understands the difference between a phonetic and a phonemic approach to orthography, or recognizes the guises in which both of these are likely to reveal themselves in a given text, are all actually matters of but little moment, *except* in as far as such positions interfere with the progress of Tibetan studies, particularly by interposing quite unnecessary blocks of stumbling into our path of progress toward the ultimate development of a rigorous methodology of text-criticism for early Tibetan texts and documents. When they do this, then regretfully it becomes necessary to remove such obstacles from the path of our science as expeditiously as possible. For until at least the preliminary steps of this work of clearing away such misunderstandings of the relationship between linguistic entities and orthographic realities have been accomplished, Tibetan studies will remain, as they already have too long been, quite at the mercy of the most naive approach imaginable to the nature of these texts, and to the solution of their problems—even culminating at times in the genial suggestion that what one should do with a difficult text is simply to rewrite it into something easier to understand, on the grounds that “the printing blocks have frequently worn out and been remade, offering opportunities for mistakes to creep in.”²⁶

When in 1808 Friederich von Schlegel wrote what has been called “[t]he manifesto of the Romantic tradition in Indian studies”,²⁷ he published it under the impressive title of *Über die Sprache und Weisheit der Inder*. Such a resounding title would surely not be in favor today, but it may nevertheless give us pause for thought; perhaps, in studying all these things, we today are too quick to separate *Sprache* from *Weisheit*; but the Indians did not, and neither did the Tibetans. We need not necessarily propose the restoration of the Romantic tradition in Indic studies, or in Tibetan studies, in order to be reminded that, after all, one of the essential reasons for studying any of these things, Indic or Tibetan alike, can only derive from a conviction that they *do* in one manner or another embody *Weisheit*. The Tibetans, like their Indic masters, knew much that we today can only slowly and painstakingly recover, and probably never master. But the way to begin is hardly to assume, out of hand, that they knew less than we do about all these matters, especially when there is so much concrete evidence to the contrary. The Tibetan grammarians *did* know and understand the concept of the phoneme: it is only in London today that the same idea seems poorly comprehended. The scribes responsible for the Old Tibetan documents embodied in their orthographic practice not only the phonemic principle but also a certain degree of sub-phonemic notation of pronunciation variations: again, what was apparently clear enough to them seems obscure only in certain European academic circles. In linguistics, as in all other fields of Tibetan studies, we will do well to begin from an initial assumption for the prior existence of *Weisheit* on the part of those whose cultural traditions and monuments we are bold enough to attempt to study, rather than gratuitously masking the achievements of others in our own ignorance. “As Bharṭṛhari himself puts it, the Goddess of Learning does not

smile on those who neglect the ancients."²⁸

NOTES

1. R. A. Miller, *Studies in the Grammatical Tradition in Tibet*. (Amsterdam Studies in the Theory and History of Linguistic Science, III, Studies in the History of Linguistics, Vol. 6). John Benjamins B. V., Amsterdam 1976, hereafter = *SGTT*. Other abbreviations: J. F. Staal, ed., *A Reader on the Sanskrit Grammarians*. MIT Press, Cambridge 1972 = *RSG*.
 2. This is even true, *mutatis mutandi*, of the Chinese writing system, though a demonstration of this claim would unfortunately take us too far afield here.
 3. R. K. Sprigg, Review of *SGTT*, in *BSOAS* 41.184-85 (1978).
 4. Likely to be overlooked, but in its own way a most useful contribution to the history both of the concept and of the term phoneme is Youn-han Kim, 'The Origin of the Phoneme Theory,' *Eoneohag, Journal of the Linguistic Society of Korea* 3.47-60 (1978), a valuable article that will repay the search necessary to locate it in most libraries. It would, for example, have spared Sprigg not only his mistaken assumptions but also his embarrassingly inaccurate generalizations.
 5. These issues are treated fully elsewhere; see my paper 'Phone, Phoneme and Graph in the Old Tibetan Grammarians,' *AOH* 34.153-162 (1980).
 6. As we have shown elsewhere ('The Far East,' in Thomas A. Sebeok, ed., *Current Trends in Linguistics*, Vol. 13, *Historiography of Linguistics*. Mouton, The Hague & Paris 1975 pp. 1213-64), the case of China provides evidence for a tradition of linguistic science also growing out of Indic origins but less closely associated with them than was that of Tibet—but the case of China was also, as it happened, one in which phonemics was pursued and refined to the eventual (and unfortunate) total neglect of phonetics.
 7. Quoted in *RSG*, p. xiii.
 8. R. A. Miller, 'Some Problems in Tibetan Transcriptions of Chinese from Tun-huang,' *Monumenta Serica* 27.123-48 (1967; published 1969).
 9. *Op. cit.*, p. 134, with Plate C, which allows easy verification of the epigraphical evidence at issue.
 10. R. A. Miller, 'Early Evidence for Vowel Harmony in Tibetan,' *Language, Journal of the Linguistic Society of America* 42.252-77 (1966).
 11. M. Taube, 'Die Tibetica der Berliner Turfan-Sammlung,' *Altorientalische Forschungen*, V, *Schriften zur Geschichte und Kultur des alten Orients* (Akademie-Verlag, Berlin, 1977), pp. 123-44. But Taube's valuable contribution is to be slightly corrected at p. 132, in
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as much as we did not, in 1966, undertake to study the evidence on this question preserved in the Tun-huang versions of the Tibetan Rāmāyaṇa, not then having the necessary materials conveniently at hand. Recently these texts have been most carefully edited by J. W. de Jong, 'The Tun-Huang Manuscripts of the Tibetan Rāmāyaṇa Story,' *Indo-Iranian Journal* 19.37-88 (1977), where at last all the relevant epigraphical evidence is marshaled, and only waiting for someone to investigate.

12. Tor Ulving, 'Tibetan Vowel Harmony Reexamined,' *T'oung Pao* 58.203-17 (1972).
 13. We are also accused by U, p. 215, of having "ransacked a number of early manuscripts, largely of the 9th and 10th centuries, discovered in the Tun-huang caves and other Central Asian sites." But English 'ransack' means two different things, and it is not clear which of these two is the sense of U's allegation. If he meant 'ransack' in the sense of 'to search through and carry away all valuables in, to pillage,' the charge is false; if he meant 'ransack' in the sense of 'to search every part of,' then one can only ask, what in the world is wrong with that?
 14. Tor Ulving, 'Umlaut in Tibetan Numerals,' *T'oung Pao* 47.75-88 (1959).
 15. *Language* 42.275 (1966).
 16. We take this position here and for the present simply as the working hypothesis that appears to us best to reflect the present state of comparative research in this area.
 17. Colin P. Masica, *Defining a Linguistic Area: South Asia* (University of Chicago Press, Chicago 1976), *passim*.
 18. N. Poppe, *Introduction to Mongolian Comparative Studies*. (Mémoires de la Société Finno-ougrienne, 110.) (Helsinki 1955), p. 36 ff.
 19. *Language* 42.263 (1966).
 20. N. Poppe, *Vergleichende Grammatik der altaischen Sprachen*. (Harrassowitz, Wiesbaden 1960), p. 143.
 21. Poppe, *Introduction to Mongolian Comparative Studies*, p. 39 ff. There is also a considerable literature on a parallel "i-breaking" phenomenon in the other Altaic languages, including Japanese and Korean, some of the more important items from which are cited in *ZDMG* 126.*69*, note 13 (1976).
 22. Uray Géza, 'A Tibetan Diminutive Suffix,' *AOH* 2.183-219 (1952).
 23. Poppe, *Introduction to Mongolian Comparative Studies*, p. 34.
 24. See *Language* 42.273 (1966) for specific examples.
 25. *T'oung Pao* 44.5 (1956).
 26. A. Wayman, *Journal of the American Oriental Society* 78.33, footnote 27 (1958).
 27. Thus, *RSG*, p. 49.
 28. J. Brough, 'Theories of General Linguistics . . .,' reprinted in *RSG*, p.
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402. Bhartṛhari (*ca.* 7th century AD), is the author of “the earliest extant work specifically devoted to the philosophy of grammar,” his *Vākyapadīya*.
