The Sino-Tibetan Hypothesis

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The Sino-Tibetan hypothesis postulating a genetic relationship between Chinese and Tibetan is surveyed, with special attention to the methodological and other scientific shortcomings of a recent attempt to compile a "handlist" of putative Sino-Tibetan lexical comparisons that further purports to substantiate that hypothesis. The hypothetical nature of all postulated early linguistic relationships is stressed; precisely like Indo-European or Altaic, Sino-Tibetan too always remains a hypothesis that may be argued for or against more or less convincingly, but it can never be "proven" to have existed. Phonological correspondences ("sound laws") are important evidence that support a postulated linguistic relationship; but to claim that they "prove" such hypotheses is completely to misunderstand their operation, as well as to mistake the powers of the comparative method of the Neogrammarians. Especially is this true when, as in the recent "handlist" that purports to "prove" the Sino-Tibetan hypothesis by constructing "sound laws", the formulations in question actually prove not to be worthy of the name, consisting rather of mere teleological "reconstructive exercises" in which quite imaginary forms of more and more complex configuration are postulated virtually at will, with almost no regard for documentable data. The paper concludes that, for the present at least, available evidence for a Sino-Tibetan linguistic relationship is weakened rather than enhanced by the *petitio principii* fallacy inherent in the "handlist"; it also studies in some detail the evidence of the Tibeto-Burman numerals, where the simplest (and hence also the most plausible) hypothesis to account for the data points toward early borrowings of these words from Chinese into nearby Tibeto-Burman languages, rather than toward a hypothesis of genetic relationship among all these languages.

The historical-linguistic problem of a Sino-Tibetan hypothesis—i. e., the question of whether or not the considerable number of Tibetan languages and dialects and the even larger number of Chinese languages and dialects known today all represent later, changed forms of a now-lost earlier linguistic unity to be identified as "Sino-Tibetan" (ST)—is of scientific interest for many reasons. The linguist is interested in this problem because ultimately the linguist must always be concerned with the historical filiation of the materials with which we work, even if we might wish at times to be exclusively occupied with synchronic matters. Sooner or later even the most determinedly synchronic approach to linguistic issues becomes involved, *faute de mieux*, in problems of history, and hence also in questions of earlier, now-lost stages in the development of the observed data that we have before us. The historian, in particular the prehistorian, is avidly concerned with these questions because only the study of earlier linguistic unities can advance our control of the history of man significantly back into the past, beyond that always relatively shallow horizon afforded by written records, even in those relatively rare portions of the world where genuinely early written records do exist. And even the historian of science can hardly help but find these matters of interest and concern. Tracing the postulation of earlier linguistic connections, and in particular studying those facets of such postulations that directly bear upon the recovery ("reconstruction") of earlier linguistic unities ("proto-languages," *Ursprachen*) is of enormous value for anyone interested in how we go about learning what we know about man and his earliest history. The story of the discovery of the Indo-European (IE) linguistic unity, which came about in large measure as a response to the stimulus afforded by the "discovery" of Sanskrit by European scholarship in the eighteenth century, also provides a significant introduction to much of the subsequent development of Western thought over a wide range far transcending the narrowly linguistic. In more than one sense, then, to study earlier linguistic relationships in Europe, and also to study the study of such relationships, is to study a representative cross section of intellectual currents.

Nor should what is thus true of Europe be any the less true of Asia. All the above concerns, common as they are to the history of earlier linguistic unities, ensure that a consideration of the ST hypothesis offers a valid topic for the historian as well as for the linguist. But the particular case of ST offers a number of additional features of intellectual interest, over and above those that normally distinguish the study of any earlier linguistic unity. And it is these that particularly commend its scrutiny to us at the moment, quite apart from the obvious manner in which the study of this problem may pay honour to the memory of the distinguished scholar to whom this volume is dedicated, a man who devoted much of his long career to the painstaking historical study of Chinese and Tibetan alike.

The ST hypothesis (as defined in our opening lines) at the same time does and does not bear significant resemblances to the well-known hypothesis of an original IE linguistic unity. At the outset, an important point that one finds too seldom emphasized in the literature must be stressed. This is the fact that IE *too* is a hypothesis. *All* truly early, "now-lost" linguistic unities permanently and necessarily remain on the level of hypothesis. Some are relatively well explored, others less adequately documented. Some, like IE, have in the course of their study been so bolstered by such a great panoply of convincing evidence embracing linguistic matters of detail and minutiae that few informed observers are any longer sceptical of the soundness of the hypothesis. Others continue to be talked about in general terms but chronically seem to lack the specifics of detail that might otherwise render them more convincing.

But whether well-documented or not, whether convincingly set forth or not, each and every early linguistic unity, all "proto-languages," are hypotheses pure and simple. By definition, they left no written records. By definition, they passed out of existence early in the history of man. By definition, they must always remain on the level of the hypothesis that cannot be verified. No one now living has ever known anyone who spoke IE, or even heard it being spoken. Proto-languages may be postulated, and even recovered, in ways that are convincing; but none of them can be proven to have existed, anymore than the linguistic relationships that they presuppose can ever be proven.

This essential level of the hypothesis is course one of the principal elements that ST and IE have in common. Both are hypotheses; both necessarily will remain so. And in the case of both, what is most important is not whether they have been proven—because they cannot—but instead how convincing is the evidence that may be brought to bear upon each individual hypothesis.

But it is precisely upon this particular issue that the history of the study of IE and ST diverge sharply and significantly. With the European "discovery" of Sanskrit, the hypothesis of an IE proto-language was soon thereafter quite adequately formulated, even as early as Sir Wm. Jones' famous lecture of 1786. Virtually all that followed—and to be sure, much was to follow—was merely a matter of filling in missing details. Nevertheless, what must be stressed is that this initial insight of Sir William and his contemporaries was primarily stimulated by correspondences in matters of morphological details among what we now generally recognize to be the cognate IE languages, not by phonological correspondences ("sound laws"). It was the impressive parallels to be found between the paradigms in the classical languages of Europe, which they already knew by heart, and the paradigms in Sanskrit, which they had just now discovered, that fired the imagination of the early European comparativists, leading them inexorably toward the conclusion that beneath this mass of detail somehow and somewhere there lurked the existence of a "now-lost" linguistic unity. A realization of the importance of sound correspondences, upon the basis of which the so-called Neogrammarians would erect their assumption of regular sound change as the principal factor in the history of human language, came a full century later. The systematic comparison of the IE languages, and hence also the first detailed postulation of an IE unity, was begun in 1816 by F. Bopp in his *Über das Konjugationssystem der Sanskritsprache*, and brought to a comprehensive level in his *Vergleichende Grammatik des Sanskrit,* *Zend, Griechischen, Lateinischen, Litthausichen, Gothischen und Deutschen* of 1833. Not until 1876 did A. Leskien write of the *Ausnahmslosigkeit der Lautgesetze.*

It is extremely important to keep this chronology of earlier IE studies in mind in our scrutiny of the present status and possible validity of the ST hypothesis. In IE, it was the significance of morphological anomalies for the hypothesis of an earlier linguistic unity upon which attention first focused. It was early understood that if these anomalies meant anything at all, they must represent the common inheritance by the various languages from some common, "now-lost" original. It was out of this insight that the concept of sound laws developed, slowly and with considerable difficulty.

But nowhere along this chain of scientific development and scholarly progress was there any idea of ever using sound correspondences to "prove" earlier linguistic relationships. Linguistic relationships were initially hypothesized on the basis of morphological evidence. Sound correspondences were then extrapolated from within the lexical materials that constituted the morphological evidence. And it was these sound correspondences that then, in their turn, substantiated a further, but entirely secondary and separate, assumption of regular sound change.

In too much historical-linguistic literature today the direction of this chain of linguistic (and scholarly) events has been misunderstood. Frequently it is even reversed, as if it were necessary to begin with the establishment of sound correspondences in order to identify the lexical and morphological materials that generally appear to argue for genetic relationships between languages, rather than the other way around.

In the specific case of ST, this today quite prevelant misunderstanding of how it is that one goes about first postulating, then studying the history of earlier linguistic unities has turned into a particularly massive block-of-stumbling for many investigators. It is not difficult to see why this has happened. By and large, the languages commonly grouped together today under the ST rubric lack precisely those varieties of morphological anomalies that first gave impetus to the insights, then later facilitated the detailed studies, of the pioneer Indo-Europeanists. As a consequence, most workers attempting to substantiate the ST hypothesis have not only almost exclusively concerned themselves with extrapolating putative sound laws from suspected cognates—they have mostly also gone the one step further of assuming, even if only tacitly, that if such sound laws can be extrapolated, the genetic relationship of the languages concerned will have been proven. This misunderstanding of methodology and concatenation of fallacies, as we shall see in more detail below, mostly account for the present unproductive crisis that continues to hamstring ST comparative studies.

Meanwhile, and as it were almost at the other extreme of the historical-linguistic spectrum from IE, there are also significant lessons to be learned about how to approach ST from a brief consideration of another postulated early linguistic unity, Altaic. Almost everyone agrees about IE; hardly anyone agrees about Altaic. Without question, Altaic remains the single most disputed such concept ever yet proposed by scholarship. The hypothesis that the Turkic, Mongolian, and Tungus languages (including Manchu), and possibly also a number of peripheral Far Eastern languages including Korean and Japanese are all later, changed forms of an earlier Altaic is long-standing in the literature. But challenges to this position have also long been made, and in recent decades they have accelerated both in frequency and in shrillness.

Many investigators are simply unwilling to admit that any non-IE peoples, and in particular the Turks, Mongols and Tungus speakers of Central and Littoral Asia, could possibly be the heirs of a historical-linguistic continuum that would in so many ways parallel the linguistic history of "proper Europeans." This is, of course, essentially nothing more than a racist approach to the question; and as such it need concern us here no longer, except to decry both its motivation and its implications in the strongest possible terms.

Other aspects of this "anti-Altaicist" sentiment are somewhat more creditable. It cannot be denied that certain elements of the arguments that have been advanced against the Altaic hypothesis have their own *raisons d'être*. In the Altaic languages morphological anomalies of the sort so strikingly exploited by the Indo-Europeanists are not absolutely absent, but they are not plentiful either. With very few exceptions, and unlike IE, the Altaic languages do not have nominal or verbal paradigms studded with deviant formations that veritably cry out for historical explanation because their traces survive in different descendant languages vastly separated from one another in time and space.

The Altaic hypothesis, as a consequence, turned initially not to morphology but to syntax for its first impetus. The original idea that Turkic, Mongolian, Tungus, Japanese and Korean are all later changed forms of an original "now-lost" linguistic unity was mainly rooted in the observation that the overall syntactic structures of all these languages are strikingly parallel. Even today one can in many cases translate virtually morpheme-by-morpheme from one of the Altaic languages into another. Lexical correspondences, including correspondences between bound morphemes, require more acumen to recognize—so much so that their existence has often been deprecated when not downright (and incorrectly) denied. The closeness in lexicon and syntax exhibited by all these languages is qualitatively different from the closeness revealed by the morphological anomalies of IE. Nevertheless, it exists; it is a phenomenon that must somehow be accounted for; and the Altaic hypothesis is the best way in which to account for it.

Those who persist in challenging the Altaic hypothesis must, to the contrary, account for these parallels by a thesis of wholesale borrowing. They profess to see the Mongolian peoples borrowing virtually all their language from the Turks, and the Tungus speakers theirs in turn from the Mongols. Or sometimes portions of these borrowing scenarios are reversed, if that seems convenient in order to fit any particular case at issue. Seldom if ever addressed is the cardinal question of *why* any of these peoples should have had to rely upon one another for words that concern virtually all the most ordinary, non-technical frames of semantic reference—body-parts, climate and geography, activities of everyday life, etc. And this is not even to mention the question of what, in that event, the Mongols spoke before they borrowed their language from the Turks, etc., etc. Borrowing among the various Altaic languages of course frequently took place, as it did among the IE languages. But advocates of this assumption of enormously wide-scale borrowing among the Altaic languages, so wide-scale as to rule out the possibility of their genetic relationship to one another, themselves run the risk of the old fallacy of deriving one attested language from another, much as it was common to derive Latin from Greek before the discoveries of Sir Wm. Jones. That particular proposition is now well forgotten in IE circles. In Altaic studies it not only survives but is frequently made the subject of independent rediscovery.

Nevertheless, the progress of the Altaic hypothesis does resemble that of IE in one important respect. Just as the systematic postulation of IE effectively began from the study of morphology, with Bopp's work on the conjugation systems of the various languages, so also was Altaic comparative linguistics effectively founded by an important monograph about word-formation, G. J. Ramstedt's *Zur Verbstammbildungslehre der mongolisch-türkischen Sprachen* published in 1912. In this remarkable work of scholarly pioneering, Ramstedt demonstrated convincingly that the details and anomalies of secondary verb-stem formation in Turkic and Mongolian argue strongly for the genetic relationship of these languages, as do also the data available on the same topic from the Tungus languages. (Of the latter only small amounts of the evidence were available to Ramstedt; subsequent publications by a number of scholars working with more recently available materials have now filled in this gap in the picture.)

But at this point, the history of Altaic diverges sharply from that of IE. Bopp's work became the keystone of subsequent studies; Ramstedt's was—and still is—almost totally forgotten. Rather than building upon the sound morphological foundations that Ramstedt's 1912 monograph provided for their field, Altaicists have instead too often dissipated their energies in feckless speculation about the possibility of widespread borrowings in all directions rendering the hypothesis of an original Altaic linguistic unity unnecessary. In the vast anti-Altaistic literature of the past several decades Ramstedt's original morphological formulations are seldom if ever mentioned.

Does what we are able to learn from these necessarily brief sketches of the history of scholarship in other linguistic unities such as IE and Altaic tell us anything about the problem of the ST hypothesis? Indeed it does. In ST we have an earlier linguistic unity apparently postulated despite, rather than because of, the morphological evidence of the languages concerned. The structural facts of Tibetan on the one hand and of Chinese on the other have meant that in the study of the ST hypothesis, morphology has played virtually no role at all. ST has had nothing along the lines of Bopp or of Ramstedt; one must only mention the possible exception (actually an "exception that proves the rule") of A. Conrady's *Eine indochinesische Causativ-Denominativ Bildung, und ihre Zusammenhang mit den Tonaccenten* of 1896; but at best, in point of materials as well as in method, this monograph does not bear comparison either with Bopp or with Ramstedt.

As a consequence, ST has not enjoyed even the possibility for evolving sound correspondences out of the solid foundation of lexical materials already demonstrated to point in the direction of genetic relationship by reason of the prior confrontation of their morphological anomalies and the resultant identification of deeper (and older) regular patterns underlying superficial (and newer) differences. Rather, workers in ST have typically plunged directly into comparative phonology, trying to identify cognate lexical items solely on the basis of semantic congruity and overall phonetic similarities, quite independently of morphological considerations. Little wonder that their results have mostly been unconvincing, once we understand how sharply their approach has diverged from the classic methodology developed in and elaborated for the study of other and similar problems in the history of human languages, especially from that nurtured in IE studies.

Indeed, given the nature of both Tibetan and Chinese morphology, one can only be astonished that it has ever been suggested that these two languages—actually and more accurately, these two great language families—are genetically related. Nothing in the morphology of either points in the direction of such a hypothesis. The syntactic structures of both are totally dissimilar; so also are their overall phonological patterns. We are left with the strong impression that mere geographical proximity, together with certain lingering adumbrations of the somewhat involved and generally rather special political relationship that has long existed between Tibet and China, are mainly at the basis of the formulation of this hypothesis, rather than any pure and simple linguistic considerations.

Meanwhile, as we have seen, Altaic studies have all but shipwrecked themselves upon the shoals of loanwords. Instead of evolving overt phonological criteria for identifying borrowed forms, their very existence in Altaic has been used as an argument against the Altaic hypothesis itself. But by contrast, in ST studies even the possibility of loans between Chinese and Tibetan has scarcely ever been entertained. The merest similarity in sound and sense between one word in Tibetan and another word in Chinese has typically been seized upon as evidence for genetic relationship, while the possibility of borrowing has remained virtually unexplored. Yet surely their long history of geographical proximity, along with the centuries of social, religious and political contacts between the Chinese and the Tibetans, would imply the existence of a considerable stock of lexical borrowings in both directions.

The immediate relevance of the above sketches of IE and Altaic to the problem of the ST hypothesis may also readily be verified by the study of two recent comprehensive contributions to ST studies, Benedict 1976 and Coblin 1986. The former is a long and extremely detailed study that attempts to contain some of the major damage sustained by its author's approach to ST as a result of the critical reception of Benedict 1972. It is mainly useful for its citations of earlier work in the field (though one searches here in vain, as also in Benedict 1972, for even a mention of Conrady's 1896 monograph!), but also for its authoritative summation of the mainstream of ST work as it has stood up to a decade ago; its exploration, and especially its critical evaluation in terms of our above remarks, must be left to the reader. The latter monograph is somewhat different, and deserves more detailed scrutiny.

Coblin straightforwardly describes both his work and his conclusions in the following important passage; "I am not a Sino-Tibetanist, and as of at least 1975 I was by no means convinced of the validity of the Sino-Tibetan Hypothesis. I have, however, carefully observed developments in this field during the past decade; and the aggregate of evidence assembled by those working in it has now converted me to the view that Chinese and the Tibeto-Burman languages must have descended from a common proto-language. ... The object of the present work has been to collect what for me were the most convincing of these materials into a single list and to arrange this list in a clear and convenient form, with indexes which make the information in it easily accessible" (Coblin 1986. 7-8). On all these scores Coblin is as good as his word. His study is admirably clear, explicit, and convenient, his data well indexed and easily accessible. Easy as a consequence is a theoretical and methodological evaluation of his sudden conversion to the ST hypothesis, to which we now turn.

Coblin begins by suggesting that "[i]f the Sino-Tibetan correspondences ... are systematic, then it should be possible on the basis of them to reconstruct Sino-Tibetan proto-forms from which Chinese and Tibeto-Burman reflexes can be derived by regular rules" (8). Unobjectionable at first glance, and ostensibly correct enough so far as it goes (even though the statement represents a dangerously close approach to the tautological), it is hardly informative to point out that systematic correspondences can be restated as "regular rules" (are there *irregular* rules?), since the latter is after all simply a reversal of the former.

But then Coblin advances into an even more circular and a far more dangerous arena: "The format of presentation is to take the reconstructed Sino-Tibetan system as a starting point and then to explain in detail the subsequent developments of the various elements in the system. ... This format is adopted because of convenience of presentation. ... The importance of the reconstructive exercise lies not in the detail of this or any other reconstructed system but in the fact that the exercise can be successfully carried out, regardless of theoretical convictions or orientations" (8).

Even as a totally theoretical proposition entirely isolated from all specific linguistic data, this last allegation raises serious questions; but when we consider its implications for the study of the ST hypothesis, its problems assume major proportions. The fact that a "reconstructive exercise" can be "successfully carried out" hardly argues for the historical truth of the formulation embodied by that reconstruction. The operation may be successful, but the patient may still die. Yet surely it is the historical truth of such matters that is ultimately the goal of all "exercises" in historical linguistics, reconstruction included. But it is when we confront Coblin's theoretical position with the facts of the data that he actually manipulates, and see with what reconstructed forms it is that he undertakes, and according to himself "successfully carries out" his "reconstructive exercise" that we must loose all confidence in the historicity of his results, and as a consequence also loose all confidence in the validity of his conclusions.

This is primarily because in performing his ST reconstructions Coblin has lost sight of the fact that what one writes in such forms are in every instance supposed to be symbols representing earlier linguistic entities, things that were once there, even if probably in other forms than we now have them in the attested languages and written records, but nevertheless and for all that, something real, something historical, and something demonstrably true—above all else, something, not nothing. But far too many of Coblin's reconstructed phonological units prove upon closer inspection not to be something: they are nothing. His ST forms typically consist of long strings of "reconstructed" phonemes most of which eventually merge into zero in the later attested languages. We are asked seriously to believe in sequences in which, e.g., ST **bk-* > O[ld] C[hinese] **k-* but TB **bk-*; ST **dg-* > OC **g-* but TB **dg-*; ST **dγw-* > OC **gw-* but TB **d-w-*; ST **gs-* > OC **s-* but TB **gs-*; ST **sgw-* > OC **gw-* but TB **sg-*, etc. (18 sqq.). Far too often, as these and many others of his "rules" will show, Coblin is working not with something but with nothing; and so the way in which his "exercise can be successfully carried out" also tells us nothing.

Given the assumption that many different things all turn into nothing, then there is no absolute upper limit to the number of different "reconstructions" that may be postulated in order to bring this Tibetan form into apparent historical congruence with that Chinese form. If **bk-* > *k-* and *bk-*, then why not **dbkwj-* > *k-* and *bk-*? Then with Coblin one may write strings of initial consonants and semivowels pretty much at will, merely by assuming that in the case of Tibetan most of these elements turn into zero, apart from the few that survive in order to account for the shape of the Tibetan form (because that is what we are looking for), while in the case of Chinese most of them also turn into zero, except that another and usually different one survives and thus accounts for the shape of the surviving Chinese form (which we are also looking for). This is not the method of reconstruction of the Neogrammarians. Confronted with the consonantal correspondences in words like Lat. *que*, Skt. *ca*, Gk. *té*, and Phryg. *ke* 'and', the Neogrammarians reconstructed IE **q͜ue*, where the consonant **q͜u* regularly accounted for the multifarious reflexes of the initials, depending upon the language and the vocalism involved. Operating like Coblin, they would have written something along the lines of **ktcea*; similarly for the root of Lat. *quis, quid*, Skt. *cid*, Gk. *tís, tis*, Osc. *pís, pid*, they reconstructed IE **qui–*, not **ptkisd*. Coblin shows us a glassbead game to be played with marks on paper; it has nothing to do with history, even less with any hypothesis of historical connection between the languages with whose words it is played. That it can be "successfully" carried out tells us nothing except in as far as it testifies to the ingenuity of the player of the game, in this case Coblin.

Partly because of Coblin's apparent unfamiliarity with the essentials of the Neogrammarians' technique of historical reconstruction, and partly also due to his basic misunderstanding of the role of "regular sound correspondences" in the consideration of problems in the historical relationship between languages, he has ended up deeply entangled in the elementary logical fallacy of *petitio principii*, also known as "begging the question." This is the fallacy in which a premise is assumed to be true without warrant, most often (as in Coblin's case) by implicitly taking for granted that which actually remains to be proven. The multi–consonanted and semi–vowelled "reconstructions" that he writes for his ST are implicit, gratuitous assumptions that appear to demonstrate his proposition. But they do not and cannot accomplish this, because time and time again the sole *raison d'être* for their shapes, particularly for their consonantal and semi–vowel configurations, is that same original proposition that they are being invoked to demonstrate.

If one grants that any but a very limited number of individual units in a reconstructed phonology can be tolerated to merge with zero in some of the attested languages, and particularly if with Coblin one grants that the majority of such reconstructed units not only can but did become zero in most cases, then quite literally anything is possible and anything can and does happen. Longer and longer initial sequences are written. More and more of the elements thus hastily assembled are "explained" by mergers with zero. More and more disparate forms can in this fashion be "historically related." But it is all *petitio principii,* because that which remains to be demonstrated has been implicitly taken as true, and is itself the sole body of evidence adduced in the argument for the validity of the initial premise.

Coblin is by no means the first *amateur* of such matters to fall headlong into this, the most serious of all the traps for the unwary who persist in venturing into Neogrammarian territories. Ever since 1976 Nishida Tatsuo in Japan has been attempting to "prove" the genetic relationship of Japanese with Tibetan in almost precisely the same fashion, writing a series of quite circular, thoroughly *petitio principii* reconstructions of forms that he argues show a historical relationship between these two languages. Just as in the case of Coblin, Nishida has been seduced into his erroneous conclusions by his misunderstanding of the theory and practice of linguistic reconstruction; also as in the case of Coblin, the superficially neat, self-contained, and internally consistent system that he has erected for this purpose proves upon even cursory inspection to be no more than a pack-of-cards, an elaborate display of the *petitio principii* fallacy at its most fallacious.

One hesitates to cite specific examples from among Coblin's ST reconstructions that document in detail how he has transmogrified the reconstruction technique of the Neogrammarians into a veritable self-parody of itself, if only because one hesitates to give further currency to his postulations: once written down and published, and especially once cited, a linguistic form, even forms as patently spurious as Coblin's reconstructions, unfortunately tend to have a life of their own—not to mention a kind of linguistic Gresham's Law, in which bad forms drive out good ones. Nevertheless, some specific examples must obviously be given, and these we shall mostly select from among the lower numerals.

Several cogent reasons govern this choice. Numerals are widely regarded as providing prime lexical evidence for linguistic relationships (even though the numerals too, like all other lexical elements, may be and frequently are borrowed). With the numerals we are mostly free of problems of semantic equivalence. And most important for our present problem, certain obvious formal similarities between the Tibetan and Chinese lower numerals have long played a significant role in convincing many otherwise cautious observers of the validity of the ST hypothesis.

Norman, for example, writes that "〔a〕 Chinese-Tibeto-Burman affinity is unassailable, " and in evidence prints a list of 24 ST comparisons that in his view "by itself is virtually sufficient to establish a genetic link between Chinese and the other languages given" (1988. 13, 12, and his Table 1. 2). Within that list, six of the lower numerals ('1', '2', '3', '5', '6', '9') constitute the single largest semantic category, accounting for a quarter of the whole, as against 4 body-part terms, 3 animal names, 2 pronouns, 2 verbs ('kill', 'die'), 2 sensory adjectives ('bitter', 'cold'), 2 ecological-environmental nominals ('sun/day', 'tree/wood'), and 1 grammatical element (the list also contains 3 words, 'name', 'year', and 'poison', that that for over half a century have been widely regarded as old cultural-loans from Chinese into Tibetan, cf. H. Maspero, *JA* 222, 1933, 79 n. 1). Thus we see quite clearly the major role that the numerals play in all this, even in a highly selective, extremely cautious and rigidly winnowed list of possible ST comparisons such as Norman's. What then, we must now ask, has Coblin for his part done with these numerals?

The evidence may most conveniently be exhibited in the following table, extrapolated from Coblin 1986 *passim.* and *svv.* :

	ST	OC	MC	WT	WB	NC
'2'	**gnyis	*njidh	*ńźi-	gnyis	hnac	èrh
'3'	**gsum	*səm	*sâm	gsum	sùm	sān
'4'	**bɫyid	*sjidh	*si-	bzhi	lè	ssŭ
'5'	**lngaγ	*ngagx	*ngwo:	lnga	ngà	wǔ

'6'	***dljəkw*	**ljəkw*	**ljuk*	*drug*	*khrauk*(1)	*liù*
'7'	***shnjis*	**tshjit*	**tshjet*	*bdun*	*hnac*(2)	*ch'ī*
'8'	***priat*	**priat*	**pwǎt*	*brgyad*	*hrac*(1)	*pā*
'9'	***dkwjəɣw*	**kjəgwx*	**kjəu:*	*dgu*	*kùi*	*chiǔ*

Notes to Table: (1) missing in Coblin, *svv.* (2) *sic* in Coblin 131, see further below. The numerals '1' and '10' have not been included in the Table or discussed in the present study since their historical-linguistic problems, if properly treated, would exceed the space available.

In the table above STand O(ld) C(hinese) are entirely Coblin's, his evidence that his "exercise can be successfully carried out." M(iddle) C(hinese) is on a different level, representing in the main a consensus of the past seven decades of international study on Chinese historical phonology. (Nevertheless, its forms always require, as above, the '*' which is missing from their citations in Coblin.) W(ritten) T(ibetan), W(ritten) Burmese, and N(ew, i.e. modern) C(hinese) are matters-of-fact.

A glance at the table immediately reveals that one of the significant ways in which the phonological patterning of Tibetan differs from that of Chinese is its phenomenon of prefixation. WT words like *gsum* '3' or *bzhi* '4' simply do not look like attested forms in any variety of Chinese. In the case of these words we may with confidence separate the *g-* and *b-* as prefixes thanks to the internal evidence of the numeral paradigm (cf. WT *gcig* '1', *bdun* '7'); and much the same can also be done for many (if not all) initial clusters in a large number of other words.

In their search for ways in which to relate Chinese to Tibetan students of early Chinese phonology have frequently seized upon the fact that the Chinese script has preserved internal evidence for a certain number of long-lost initial consonant clusters in many Chinese words. This has frequently seemed to be a good way in which to bridge the substantial gaps that separate Chinese from Tibetan phonology. Unfortunately, even when the Chinese script does appear to indicate that a given word probably had an earlier complex consonantal initial, in most cases it is all but impossible to come up with anything out of the Tibetan materials remotely resembling the sequence to be reconstructed from Chinese.

What Coblin now has done, typically with the numerals but also throughout all other sectors of the lexicon as well, is to focus major attention upon the generally accepted fact that Chinese did originally have many words with initial consonant clusters. But simultaneously he has quite forgot that in order to recover those clusters for Chinese we must have evidence from Chinese. In his haste to write early Chinese forms that will "look like" Tibetan, and hence seem to substantiate the ST hypothesis, he has virtually abandoned the serious quest for these consonant clusters from within Chinese itself. Instead he has simply written them for his putative OC forms on the basis of the same alleged Tibetan cognates with which he would compare them—in a word, he has worked out to its uttermost fallacy the *petitio principii*, taking as a known and as a given that same unknown that he is attempting to solve.

To be sure, WT *drug* '6' does look a little like Chin. *liù* '6', especially when we accept the general consensus that modern *liù* goes back to earlier **ljuk*. But what can be done with the initials? The Chinese script does seem to indicate that this word once had a somewhat more complex initial in Chinese than it has had for centuries since; but the initial cluster that may be recovered from the script is **gl-*, not the **dl-* that the Tibetan would seem to call for (cf. Benedict 1976. 170b: "the well attested initial *gl-*"). What to do? Coblin simply cuts the Gordian knot, ignoring everything that we have learned of Chinese historical phonology over the past decades, and writes ST **dljəkw* '6', which then according to his "rules" regularly yields WT *drug* and "OC **ljəkw* '6' " (133 and *passim.*). His only justification for writing the **dl-* initial in his ST form is because that is the phonetic configuration for which he is looking in his eagerness to accommodate WT *drug* into his "system." And since in that same "system" any number of phonological constructs may be allowed simply to become zero, he finds no problem in establishing a secondary rule by which ST **d-* in such a position "becomes zero in Chinese, " etc., etc., so that it all works out: "the exercise is successful."

But in actual fact of course it does *not* all work out. The initial **g-* of the **gl-* that the Chinese script tells us most likely existed in the earliest Chinese word for '6' is nowhere explained historically by any of this. The same analysis could be performed upon all the other ST numerals reconstructed by Coblin. They tell us nothing about the history of Chinese, and almost as little about the history of Tibetan. Above all, they do not enhance our confidence in the hypothesis of an original ST linguistic unity.

That all earlier systems of linguistic relationships are necessarily hypotheses has been stressed several times over in the above. But hypothesis does not mean playing tennis-without-a-net. Nor does it mean that one is at liberty (as Coblin has felt himself to be) simply to write postulated forms without any methodological basis, apart from the desire, natural enough in itself, to find what one is looking for.

Nor does the fact that in all problems of long-range linguistic relationships we necessarily deal with hypotheses mean that all hypotheses are equally valid. If that were so, then there would be no reason to score Coblin for writing such circular and teleological forms as ST **dljekw* '6'. But historical reconstruction is not a game; and all hypotheses of genetic relationship are not equally plausible.

Norman is in good company; no one can glance at the Tibetan and Chinese numerals without wondering if there is some historical connection between these two sets of forms: the only question is, *what* historical connection? What hypothesis shall we frame that will best explain what that connection may have been? Several are possible. We may with Coblin assume that both sets of forms go back to forms in an original ST common language. On the level of hypothesis this is unobjectionable, and many have chosen that explanation. The problem arises when we attempt to validate this hypothesis by citing materials and evidence that will render it convincing. As we have seen, that is far from easy to do, particularly because of the WT prefixes; and in the case of Coblin, the attempt has badly miscarried. The forms that he writes for ST, and from which he would derive the attested forms, lead us nowhere. This does not mean that his hypothesis is wrong. But it certainly does not argue at all convincingly in its favor.

Even if it were not flawed by its indulgence in the *petitio principii* fallacy, Coblin's hypothesis would still be highly questionable simply because it is highly complex. It is not by any stretch the simplest historical scenario that may be postulated in order to accommodate his data. In historical linguistics the choice between contending hypotheses must always proceed according to the elementary but essential principle: when more than one hypothesis is available, the simplest is the one to select. One *could* "explain" all the IE interrogatives as selective, *ad hoc* simplifications of **ptkisd*, and state "rules" that describe how this form was simplified one way in this language, another way in that; but one does not. One elects the simplest hypothesis, and reconstructs IE **qu̯i-*.

In the case of the Tibetan and Chinese numerals there is another, and for the principle of simplicity also a more desirable hypothesis over and above that of genetic relationship from a now-lost original ST. This is the hypothesis that the numerals (and probably also most of the rest of the putative ST "common vocabulary") were borrowed from Chinese into TB, resp. into Tibetan, at an early period, but already well enough along into the history of the Chinese language so that at the time of their borrowing their Chinese originals had already lost whatever initial consonant clusters they may once have had in the earlier stages of Chinese.

In other words, even though the earliest form of Chinese '6' may have been **gluk* or the like, by the time that this word was borrowed into the neighboring non-Chinese languages it had already become simply **luk* or **l̯iuk* in Chinese. Accordingly, it was Chinese forms of these later, simplified shapes that were borrowed into TB, not only for '6' of course but for the other lower numerals as well. Only later, and after their borrowing into TB, did these borrowed numerals acquire TB prefixes within TB itself. The TB prefixation process by which this took place is a historical-morphological operation concerning the semantic configurations and constraints of which we are still very much in the dark, despite decades of speculation. The original function of the process appears to have been to provide overt morphophonemic marking for smaller or larger semantic categories, e. g., *m-* for nouns with reference to human body-parts, *s-* for animal terms, *d-* for seasons of the year, *b-* for perfective verbs, *s-* for intensive-factitive verbs, and like. Reconstructing the original TB prefixation system has been rendered difficult not only by subsequent shifts within the semantic categories concerned, and by secondary phonetic changes that have frequently obscured the identity of the earliest prefixes, but also by failure of students of the problem to consider non-IE semantic categories such as obligatory possession (inalienability) *vs.* optional possession (alienability), that may also very likely have played a part in the phenomenon.

At any rate, and unlike virtually everything else with which we have been concerned here, imperfectly understood though its details may be, the TB prefixation is *not* a hypothesis, it is an easily verifiable matter-of-fact. This means that when we propose its early operation as a solution for the historical-linguistic problems presented by a comparison of the Tibetan and Chinese numerals (and also for most of the residue of the "common ST vocabulary") we are not attempting to resolve the inadequacies of one hypothesis by invoking another: we are—and this is all too rare in ST—at last and finally having recourse to something that may definitively be demonstrated as actually having happened in history.

The TB prefixation was not identical, for a given root, at all times and places throughout TB. Sometimes one language employed one prefix, sometimes another used another. All this may easily be directly documented from attested materials, something of prime importance in a field such as this where too often imagination and fantasy have run rampant. The existing literature on this question is far from definitive; but even in its present state (e.g., Benedict 1972. 103 *sqq.*) it adequately indicates the principal ways in which this prefixation operated. Our sole innovation in the present lines is to propose that it also operated, in much the same fashion, with borrowed roots as well, specifically with the lower numerals that TB borrowed from Chinese.

Old and frequently unreliable for matters of precise detail though they are, the citations for the TB languages in the *Linguistic Survey of India* (Vol. III, Calcutta, 1909) will serve adequately for this purpose, and it is to those data that we turn, citing specifically in connection with the fate of the numerals *vis-à-vis* the TB prefixation phenomenon, forms from the following languages: G(aro), *LSI* 135; K(achin), *LSI* 204; Ko(lhreng), *LSI* 239; H(iroi)-L(amgang), *LSI* 248, and B(anjogi), *LSI* 227, along with Gy(arung), not numbered in the *LSI* but appearing there as the first in its "Tibetan Group," 227*a sqq.* (We omit vowel-diacritics both from language-names and from citations, as irrelevant to the present problem, and we respell the velar nasal of the *LSI* as *ng* throughout.) Let us see what happened to the numerals in these representative TB languages, always keeping in mind that additional forms of similar purport could easily be cited from several other languages.

For '2', against WT *gnyis*, Ko and H-L both have *ki-ni*, and Gy has *ka-nais*, forms that fit in well with WT and its prefix. But not so B, where '2' is *pi-ni*. Obviously this language did not inherit the velar prefix of WT *gnyis* as did the others; and we shall find this to be true throughout the numerals, where B universally has prefixed an element with an initial labial. Whatever happened here in the history of TB, it did not happen early enough for all the languages to inherit the velar prefix of WT *gnyis*.

Similarly for '3'. WT *gsum* is reflected well enough in Gy *ka-sam* and (probably) in G *gi-tam*; but K has *m^a^sum* and B has *pa-tum* for '3'. Once more, however and whenever WT acquired its prefixed *g-* in *gsum* '3', the process did not transpire early enough in the history of these languages for the velar prefix as such to be universally inherited. The numeral '4' shows an even greater diversity. WT *bzhi* '4' may well have its prefix *b-* echoed in G *bri*, and that too may perhaps account for the labial prefixes in K *m^a^li*, Ko *milli*, and H-L *pilli*. But then one must wonder in the case of B *pi-li, pa-li* '4' whether the labial is the labial of WT *bzhi*, or the labial that this language has prefixed to all its lower numerals without exception. The numeral '5' is even more striking. The prefix of WT *lnga* '5' finds a recognizable reflex in none of the languages we are inspecting: Gy has *kung-ngan*, G *bonga*, K *m^a^nga*, Ko *ra-nga*. This last form in *r-* we might be inclined to identify with the WT *l-* prefix, but this is not as simple a matter as it might appear, in the light of H-L *paranga* and B *panga*. Again, each language appears to have gone virtually its own individual way in this matter of prefixation; unvarying are only the roots (the **sum* of '3', the **nga* of '5', etc.); and as a consequence these roots themselves all begin to look more and more like simple (and relatively late) borrowings from Chinese.

The demonstration could easily be continued throughout the lower numerals, but it need not be further belabored. When none of the TB languages has any prefixational element directly or obviously reminiscent of the *l-* of WT *lnga* '5', and when indeed only a few have anything at all, viz. *r*, that can even with violence be forced into this WT pattern, what possible reason can there then be to postulate, with Coblin, an initial **l-* cluster for the ST prototype form from which he proposes to "derive" WT *lnga*, Chin. **ngwo*:, and all the other words for '5' —and yet that is exactly what he has done in writing "*ST **lngaγ* '5' " ! To say that this is a case of the cart driving the horse is to put it mildly: it is simply carrying over the

WT form, distorted by the addition of a final $^{**}\gamma$, into the vastly remote construct of ST—and then working backward from that formulation into the attested forms of the present by the convenient mechanism of letting most things in the reconstruction become zero—not to mention ignoring all the languages in which '5' appears quite without a liquid prefix, forms such as *bonga, panga* and the like.

Even within WT itself we have documentary evidence that the same kind of noun prefixation that we observe for the numerals took place sometimes differently in different varieties of the language. The WT words for some of the seasons of the year, WT *dgun* 'winter', *dbyar* 'summer, ' and *dpyid* 'spring' permit the easy identification of a prefixed *d-* in this set. But in a sense the evidence of these WT words is somewhat deceptive, since the prefixation may be shown to be more regular in this variety of the language than it must have been in the proto-language of which even WT is another, later changed form. Some of these Tibetan terms for the seasons were borrowed some time ago into the Altaic language known as Monguor; there we find Mgr. *rëGul* 'winter'. This form shows that in the variety of Tibtan from which Monguor borrowed the word in question the word for 'winter' was not identical with WT *dgun,* but instead had a prefixed *r-*, something of the order of **rgun,* a form that, for that matter, also occurs in at least one early text, even though in the WT canon it became replaced by *dgun* (further details in *Language* 44, 1968, 153). In other words, the present-day neat set of WT forms for 'winter', 'summer', and 'spring', all with prefixed *d-*, turns out to be rather later, and also rather more regular, than the original situation in the proto-language underlying WT, where at least one of these words once had prefixed *r-*, not prefixed *d-*. We have above already mentioned WT *ming* 'name'. This can hardly be other than an early loan from Chin. *ming* 'name', an original, important, and characteristic Chinese cultural and technical term widely borrowed throughout Asia. Coblin would reconstruct "ST ***mying*"; but it is obvious that this borrowed term, like those for the numerals, also acquired a prefix in some portions of the TB linguistic domain subsequent to its borrowing, as shown by such forms as WB *ǎmañ* 'name', Rangkhol *erming*, and Magari *armin* 'id.' (Benedict 1972. 31). Note also that here in effect Coblin himself covertly recognizes the same independent prefixation process that we postulate, when he writes "TB **r-ming*" (111). Similarly for such "basic vocabulary" elements as the word for 'eye'. Coblin reconstructs "ST **myikw*, " "OC **mjəkw*, " both little more than graffiti-disfigurements of Old Tib. *myig*, WT *mig* 'eye'. Whether (as seems likely) an old loan from Chinese, or an original TB root, the form underwent varied prefixation, B *kemit*, H-L *a-mit*, Gy *tai-myaik* (Benedict 1972. 84: *tĕmńǎk*), Miri *əmik*, Lepcha *ǎmik*, Kom *ka-hmit* (*LSI* 240); nor is the evidence of the earliest Tibetan written records uniform on this score, a document from Turfan preserving Old Tib. *dmyig* 'eye' with prefixed *d-* (Berliner Turfansammlung 24, B 4, published by M. Taube 1980 and reported in Róna-Tas 1985. 124). The same thing happened in all these words, and hundreds of others, that happened to the numerals.

Tacit acknowledgement of the principle here proposed—i. e., that the numerals, along with many other forms in TB, underwent different varieties of prefixation at different times and different places, hence also that this prefixation, especially in the very late canonical forms in which we find it in WT, cannot possibly serve as the basis for writing "ST reconstructions" from which Chinese forms are then irrationally to be derived by deletions and mergers with zero—is frequently to be found in the earlier secondary literature on the ST hypothesis, even in work from the pens of some of the most convinced proponents of the ST linguistic unity. For '5' Benedict reconstructed "TB **l-nga*$\sim$**b-nga*" (1972. 31, 94). If Benedict's symbol '$\sim$' in this and so many other of his postulations means anything at all, it can only mean that he regarded it impossible to reconstruct a single TB form for '5' with a specific initial cluster. Some of the attested forms require **l-*, others require **b-*. This is just what we have seen in the evidence gleaned above from the *LSI*.

In other words, Benedict himself, the most able champion of a ST unity, does not postulate the form for TB that would be required to substantiate Coblin's chimeric "ST ****lngaγ** '5' ". Nor are we here merely seizing upon an isolated or unrepresentative accident of symbolization. Benedict quite clearly sets forth his views on the role, and history, of the prefixes in the numerals (1972. 94–95). He describes precisely what we have already seen here: how the word for '5' sometimes has **l–*, "but prefixed **b–* ... is much more generally represented, " while in the case of '6' and '9' "prefixed **d–* is well attested ... but note replacement of **d–* by *k–*"—or as it might better be put, note prefixation not of *d–* but rather of *k–*. Throughout Benedict 1972 one finds awareness of the problem of the TB prefixation; and one searches in vain for any justification for writings such as Coblin's ***lngaγ* '5', ***dljəγw* '6', ***dkwjəγw* '9' and their ilk.

Bodman 1980, another vigorous champion of ST, is even more direct and to the point on this issue. Confronting the Chinese word for '6' with various TB forms, he concludes that one can only reconcile the totality of the lexical evidence by reconstructing an ultimate proto-form **C–ruk* '6', in which **C–* "stands for an undermined stop prefix" (1980. 73). From this formulation it is only a short step to our position. There is no reason to burden the oldest form in Chinese with any initial cluster in the word for '6'. If indeed there was such a cluster, it was **gl–*. But since such a cluster is nowhere represented in TB, it is possible only to assume that different initial consonants were prefixed in the various TB languages at different times in their history, and *after* the borrowing of '6' from Chinese. Bodman, for his part, emerges from his own consideration of the historical problems of this word for '6' with the following succinct statement: "... it is equally possible, and simpler, to posit different TB prefixes for the TB subgroups" (1980. 73). It would be difficult to put the matter better or more deftly, except perhaps to add that, because all this is a question of historical linguistics, the simpler solution is always the one that is to be preferred.

Thus far we have concentrated on Coblin's failures to cope with the phonological and morphological dimensions of his ST reconstruction attempts. But historical linguistics must also take due cognizance not only of the sounds but also of the sense of the forms it compares. On this level also, i. e., on that of semantics, Coblin frequently departs from the elementary methodological strictures of the Neogrammarians. Limits of space preclude complete documentation of his failures on this score, but by conservative estimate at least three-quarters of his Chinese-Tibetan comparisons fall short of the mark as soon as we consider their semantic dimension. A few representative examples must suffice.

Long ago R. Shafer (*Introduction to Sino-Tibetan*, Part 1, Wiesbaden: 1966. 46, 58) compared WT *bka* 'order, commandment' with Chin 歌 **ka* 'song', focusing entirely upon the superficial phonetic similarity between the two forms while entirely ignoring the plain fact that the semantic dimension of the comparison is impossible: in their meaning the two words have no common term whatsoever, and never did. From the pages of Shafer this sorry comparison found its way into Benedict (1972. 18), intact except for the introduction of one further serious error. Shafer had added to this spurious comparison a WB word that he wrote *tśa-ka*\ 'word(s)'. This Burmese form in 1972 Benedict miscopied, writing *tsǎ-ka* and ignoring the tone-mark; and it is now this second-hand, incorrectly copied version of this same WB word that once more appears, together with all the rest of this absurd comparison, in Coblin 1986. 162. Actually, both Shafer's and Benedict's morphological analysis of this Burmese form was also incorrect, but this also Coblin has taken over intact: the final *-ka* in WB *čəka*: 'word(s)' is a suffix to a root *čə-*, a noun-formant with parallels elsewhere in the language, and as a consequence surely not to be taken here as the root itself. In other words, quite apart from the misspelling and miscopying, there is nothing in the Burmese word even on the phonetic level to compare with the Chinese and Tibetan forms at issue, even if those forms themselves had any common semantic element—which they do not in the first place! But this old "comparison" by Shafer dies hard: still unwilling to admit the total absurdity of the whole matter, Benedict has recently taken us to task for having "overlook[ed] the significant Lahu gloss 'sing'" in Benedict 1972. 187 fn. 487 (Benedict 1976. 168a). We did not overlook it, we merely chose to dignify it with silence, since all it says is "(JAM notes meaning 'sing' in Lahu)". Interesting, if true; but hardly linguistic data with which anyone can work. Does it mean that Lahu has a verb *ka-* 'to sing'? And even if so, how does that bridge the gap between 'order, commandment' and 'song', not to mention resolving the orthographic and morphological muddles of the Burmese citations?

When not in this fashion simply passing on yet once more the earlier blunders of others, Coblin for his own part shows an approach to the semantic dimension of historical linguistics characterized by sweeping displays of a particularly vivid imagination. Chin. 宮 **kjung* 'palace' is compared with WT *khongs-pa* 'inside' (98); Chin. 㢋 **tshje* 'attack an enemy from the flank' with WT *gcal-ba* 'spread, lay out' (139); Chin. 呂 **ljwo:* 'spine' with WT *gra-ma* 'awn; fishbones; lattice frame'. Similar examples of infamous allofamy could easily be extended. They place Coblin's semantics on the same level of unconvincing improbability already documented for his phonology and morphology.

Other objections should and eventually must be raised to Coblin's "ST." Considering the enormous time-depth that a true ST linguistic unity would necessarily embrace, his materials comprise too great a total number of examples. Indeed, the sheer bulk of his comparisons and the wide attestation of his putative cognates both argue eloquently for old loan-words as explaining those (relatively few) of his examples that are immune to criticism on other levels. Bird 1982 has sobering data on IE in this connection. J. Pokorny's *Indogermanisches Etymologisches Wörterbuch* (Bern &

Munich: 1959, 1969) registers 2044 roots. But among this enormous total, only a single IE root is attested in all fourteen IE subgroups! B. Collinder was fond of pointing out that the number of words common to modern Swedish and modern Greek is so small that no one would ever, on that basis alone, have suggested that the languages are related. The more of Coblin's comparisons one can accept, the more it all begins to look not like genetic relationship but rather like several successive strata of old borrowings.

The single most attractive feature of this proposed hypothesis of borrowing, especially for the numerals, is that it no longer makes it necessary to conjur up unsubstantiated forms like Coblin's ***dljəkw* '6', much less to foist such teleological constructs off onto Chinese. But that is not all that argues in its favor. Even more important, the hypothesis that the TB numerals were borrowed from Chinese fits in well with our knowledge of early linguistic history in much of Asia, at the same time that it expands and enlarges upon that same history.

Everyone knows that the Chinese decimal system was long an object of wonder and envy on the part of most of China's neighbors. Even the most avid advocates of ST now generally admit that the Chinese numerals were early borrowed into the Thai languages. We all know that they were also early borrowed into Japanese and into Korean. In both these last instances the Chinese loans proved so popular that they have driven the pre-existing native Japanese and Korean numerals virtually into oblivion. What wonder, then, that the Chinese numerals should also early have been borrowed into the TB languages on China's early borders? With this hypothesis we do more than rid the literature of teleological reconstructions and imaginary forms for non-existent words; we open the way to an enhanced understanding of what actually happened in human history, particularly human history as revealed in the sociolinguistic relationships between China and its early neighbors.

As with so much else in life, eventually most of this comes down to a matter of choice: it depends on what variety of linguistic history one prefers. We may with Coblin write "ST ***bɨyid* '4' ", and then try to relate Chin. 四 **si-* to this strange concoction. But in that case, we must not forget that Nishida for his part "reconstructs" a "ST **bdli* '4' ", from which form he is equally prepared to derive not only the Chinese and Tibetan words in question, but Old Jap. *yö* '4' as well (1978. 291-2)! Or, we may view the Tibetan, and most of the TB, numerals as old loans from Chinese. In that case, we may postulate the following historical scenario. The word for '4' was borrowed into many (but not all!) of these neighboring languages from a Chinese original that had something of the shape **li*, shown, e.g., by WB *lè*, Sunwar (*LSI* 113) *le*, Taungtha (225) *li*, Takpa (277a *sqq.*) *pli* (already with the labial prefix!), and similar TB forms. Shortly thereafter, within Chinese itself, the initial of the Chinese word for '4' shifted from **l-* to **s-* because of intraparadigmatic analogy with the word for '3', Chin. **sâm* (much the same variety of paradigmatic analogy that, separately, accounts for the prefixed *g-* on WT '1', '2'. and '3'). And it was this secondary, analogically altered Chinese form that was subsequently borrowed into the direct ancestor language(s) of WT. There this form borrowed from Chin. **si-* became *bzhi* thanks to secondary voicing of its borrowed initial (subsequent to its automatic sibilization $\underline{s} > \underline{\check{s}}) > \underline{\check{z}}$ following the purely Tibetan prefixed *b-* (cf. the same process in T'ukumi (*LSI* 171) *mezhe* '4'). Arguing in this manner, we may recover much of the history of the Chinese numerals, both in China proper and among her neighbors. But so long as we dabble with chimeras such as Coblin's ***bɨyid* or Nishida's **bdli*, we learn nothing.

As with the numerals, so also with many other items in Coblin's "handlist." They point, if anywhere, to early cultural borrowing. They have semantic reference to activities or concepts that are impossible to associate with the enormously remote period of ST, but make quite valid sense when interpreted as relatively late loans from Chinese. Examples include 'varnish' (156) and 'work leather so as to make it soft' (146, where for that matter

Chin. 艮 **njän* is attested in this sense only once in a single Han lexicon, and so like too many of Coblin's Chinese citations, is hardly a convincing cognate); we have already noted above similar difficulties involved with assuming a genetic relationship for words meaning 'name,' 'harvest/year', and 'poison'.

Coblin's insistence upon the "success of his exercise" as a vindication of the ST hypothesis requires one final remark. In his comparison of the numerals, he has mistakenly copied out the WB words for '2' (correctly, *hnac*) and '7' (correctly, *khuhnac*), and enters the same form, *hnac* for both '2' (154) and '7' (131) in his "handlist". But *mirabile dictu*, Coblin's reconstructions, including his rules for deriving Chinese from his ST postulations, work quite as well with these entirely spurious forms as they would have done with the real thing: even though his data are manifestly erroneous, his "exercise" is still "successful." Surely this one fact tells us all that we need to know about the "handlist," its methods, and its conclusions.

Civil law recognizes the principle of estoppel. If one acquiesces in silence to error or injustice for too long one runs the risk of forfeiting future rights of redress. Perhaps scholarship too ought to recognize estoppel. Otherwise the lifework of such devoted and remarkable scientific workers as Professor Li Fang-kuei may well be obscured by a welter of unsubstantiated reconstructions decked out in constellations of asterisks. Certainly the least part of the homage due such great men is to call attention to the perilous state in which several of their scholarly specialities stand today, notably the problem of the Sino-Tibetan hypothesis.

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