

## **A Recent Contribution to Sino-Tibetan Linguistics (Review Article)**

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### **1. Introduction**

In 2002 the Institute of Linguistics (Preparatory Office) of Academia Sinica published as part of its Language and Linguistics Monograph Series two volumes of collected articles by Professor Hwang-cherng Gong 龔煌城, distinguished member of the Institute and internationally acclaimed specialist in Sinology and Linguistics. The first of these, *Collected Papers on Tangut Philology*, deals with the Tangut or Xīxià 西夏 language.<sup>1</sup> The second is entitled *Collected Papers on Sino-Tibetan Linguistics* and is devoted to a rather wide range of topics in the Sino-Tibetan field exclusive of the Tangut specialty per se.<sup>2</sup> It is the latter collection which is the subject of the present review.

The collection is comprised of three sections. The first of these may be considered the true nucleus of the work and is made up of nine papers dealing with Sino-Tibetan comparative studies. The second contains three papers on a twelfth century form of Chinese in Tangut transcription. The final section is devoted to Tibeto-Burman studies and is made up of three papers on different subjects in that field. Our review will begin with the second and third sections and then move on to the much longer first one.

### **2. Early northwest Chinese in Tangut transcription**

Early northwest Chinese of the late Tang and Five Dynasties periods is unique in that its pronunciation has been preserved in extensive alphabetic records, primarily in Tibetan script and to a lesser extent in Brāhmī, Uighur, etc. From a slightly later period

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<sup>1</sup> 西夏語文研究論文集, Language and Linguistics Monograph Series Number C2-1, Taipei: Academia Sinica, 2002.

<sup>2</sup> 漢藏語研究論文集, Language and Linguistics Monograph Series Number C2-2, Taipei: Academia Sinica, 2002.

there are Tangut transcriptions. These differ from those just mentioned in that Tangut, like Chinese, was written with a logographic script. Thus, any attempt to learn how Tangut was pronounced faces problems similar to those which confront historical linguists working in Chinese. Professor Gong (hereafter G) is, as indicated above, a specialist in Tangut philology and is therefore ideally qualified to study materials where Tangut characters have been used to transcribe Chinese syllables. The three papers reproduced here, which are numbers 10, 11, and 12 of the full collection, date from 1981-1995. The first deals with Chinese syllable initials (1981),<sup>3</sup> the second with various questions concerning the finals (1989),<sup>4</sup> and the third with the full phonological restoration of the finals (1995).<sup>5</sup> The techniques for teasing sound values out of this material are intricate and complex. One wonders how well anyone who is not himself a Tangutologist (which the present reviewer assuredly is not) could competently assess the result. Nevertheless, it does seem possible to make some general observations on points where there is unlikely to be much in the way of controversy among specialists.

Not surprisingly, the language reflected in this material has all the earmarks of a later stage of the same ones found in the earlier Tibeto-Chinese northwest texts. For example, traditional nasal initials may be realized as nasals or as prenasalized voiced stops, according to rather subtle and complex conditioning factors. And like the most evolved Tibeto-Chinese varieties, it has lost the traditional *zhuó* 濁 (breathy voiced/murmured) series of obstruent initials, which have then been replaced under all tones by aspirates, leading to complete merger with the inherited aspirates. It has also evolved beyond the Tibeto-Chinese type in certain ways. For example, it has lost all final stops in the entering tone. And it has lost all final nasal consonants, while retaining residual vowel nasalization in most cases.

In the end, the material presented in these three papers allows us to continue the story begun in the Tibeto-Chinese corpus and extend it a century or so later. G also expresses the hope (p.371) that it may someday be possible to trace this development down to the modern dialects of the area where these earlier language types were spoken (i.e., almost certainly the Gansu Corridor). In the opinion of the present reviewer, this may never be possible. For it would seem that the modern dialects of that area are really for the most part intruders from farther east, brought in during large-scale migrations of Ming and Qing times. At most, we may hope to find in the modern Corridor dialects some residual substrate features which reflect much older forms of Chinese.

The entire section ends with a very useful synoptic table in which G's hypothetical or reconstructed forms for this type of "Tangut Chinese" are derived from *Qièyùn* 切韻

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<sup>3</sup> 十二世紀末漢語的西北方音（聲母部分）。

<sup>4</sup> 十二世紀末漢語的西北方音（韻尾問題）。

<sup>5</sup> 十二世紀末漢語的西北方音韻母系統的構擬。

System (QYS) forms. This type of presentation, which is used in all three papers, has the advantage that it allows readers to make connections between the early northwest material and the traditional phonological framework. And perhaps this is all it is intended to do. But since this is not explicitly stated here, it raises an interesting question. Bernhard Karlgren believed that the QYS represented the dialect of the city of Cháng'ān 長安, which was next-door to the Gansu Corridor and was possibly a first cousin to the type of language spoken there. It is unlikely that anyone today adheres to Karlgren's position. Instead, those who think the QYS is based on some actual form of Chinese, or is in some way to be associated with such actual forms, generally tie it to the speech types of one or more of the major metropolitan areas of east central China in the late Nánběicháo 南北朝 period. And so we may ask, what evidence is there that any form of Chinese spoken in the Gansu Corridor in the twelfth century is descended from the speech of the east central area represented in the QYS? But, as indicated above, it may be that the connections drawn by G with the QYS system are not intended to be taken literally. One is left to wonder about it.

In closing this section, it is worth noting that in 1994 there appeared in Peking a book-length study of much the same material dealt with by G in his papers (Li 1994). Readers who are interested in the subject may like to consult this and compare its conclusions with those obtained by G.

### 3. Tibeto-Burman studies

The final three papers deal with disparate Tibeto-Burman topics and date from 1977-82. The first article is a study of Written Tibetan (WT) -y- as it behaves in minimal pairs such as g-yog "servant" versus gyog "crooked" (1977).<sup>6</sup> The conclusion is that Tibetan graphic g-y- should be interpreted as underlying \*g-ɣy-. Further early clusters such as \*s-ɣy- and \*r-ɣy- are then proposed, and diminutive infix functions of -y- are suggested and illustrated. The paper is interesting and thought-provoking.

The second work in this section deals with Ākē 阿科 or Akö, a Loloish language of Burma belonging to the Lolo-Burmese branch of Tibeto-Burman (1982).<sup>7</sup> It was originally known only from a word-list published in 1900. G did fieldwork on this language in 1980 and 1981, working with an informant living near Kaohsiung in southern Taiwan. In the paper he provides a detailed discussion of the phonology, framed in terms of the Chomsky and Halle *Sound Patterns* feature system. This is then followed by comparisons with Akha, Burmese, and WT. The language appears to be similar to, though distinct from, the better known Akha.

<sup>6</sup> 古藏文的 y 及其相關問題。

<sup>7</sup> 阿科話的音韻系統及其來源。

The final paper of the group is a discussion of three Proto-Tibeto-Burman (PTB) case particles, i.e., genitive \*-ʔi, dative-locative \*-a, and dative-locative \*-du (1989).<sup>8</sup> Curious similarities are noted between these particles and similar ones found in Altaic. It is suggested that Tibeto-Burman has borrowed these from Altaic at an early period.

#### 4. Comparative Sino-Tibetan

This section, comprising the first nine papers in the book, is clearly the heart of the collection and represents the primary focus of G's research efforts and interests. For all of his scholarly work, including the Tangut studies published in the companion volume of collected papers, is ultimately directed toward the study of Proto-Sino-Tibetan (PST) and, in particular, the way in which Chinese has evolved from this ancestral speech form.

One perceives in Sino-Tibetan comparative linguistics today a particular "topography", which, interestingly enough, arises directly and exclusively from the way particular researchers view early Chinese. For there is really relatively little difference of opinion among Sino-Tibetanists about the handling of Tibeto-Burman materials as comparanda at the level of Sino-Tibetan comparison. This is true even when these individuals are themselves active Tibeto-Burmanists and disagree with other specialists on matters within Tibeto-Burman. It is, in the end, one's handling of Chinese that shows where one stands in the Sino-Tibetan field at large.

The topography alluded to here embraces two major promontories of conviction, divided by a wide gulf. On one side stands the Archaic Chinese or Old Chinese (OC) system developed by F. K. Li in the 1960's and published in 1971. This system is in a sense descended from that of Bernhard Karlgren, while at the same time being quite different from it in many respects. For Li's purpose in devising his new system was to examine the weaknesses in Karlgren's work and make changes, fundamental if necessary, wherever Karlgren's system was either internally contradictory or inadequate to account for data found in Chinese sources of the pre-Han period. But, where Karlgren's ideas seemed free of these weaknesses, they were in general to be retained.

Across the gulf lies a very different set of ideas and approaches to the phonological structure of early Chinese. For they deal with all three major components of the Chinese syllable, i.e., initials, finals, and tones, in ways which differ fundamentally from those seen in the work of Li and those who have followed his ideas. The roots of this different set of views on OC seem to lie in the work of the Russian linguist and Sinologist, S. E. Yakhontov, which first became known outside Russia from a paper given by him in Moscow in 1960 and formally published in 1963. Yakhontov's work quickly became

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<sup>8</sup> "Case Postpositions in Tibeto-Burman Languages".

known in the West, perhaps most widely through the early writings of E. G. Pulleyblank. Later, N. C. Bodman and his student, W. H. Baxter, continued work along these lines, yielding Bodman's seminal comparative article "Proto-Chinese and Sino-Tibetan" in 1980 and Baxter's full codification of the new OC phonology in 1992. Today, adherents of this great complex of ideas are prominent in Russia, North America, and increasingly also in China and Europe.

All scholars are of course individuals, and it is not our intent to imply that these two major ways of thinking about early Chinese constitute formal schools whose members march in lockstep. But, with a few notable exceptions, even a cursory glance at a work on OC, or at the OC component of a work on PST, will tell the reader which side of the gulf is home to the writer in question. Where, then, does G reside? He is clearly on Li's side. But he is by no means a slavish upholder of orthodoxy. For he has in fact paid numerous visits across the abyss and brought back ideas from there which Li never espoused or would have accepted. In reading G's papers, the way in which this has been done will be of particular interest to us here.<sup>9</sup>

The papers cover a span of twenty years (1980-2000) and show a remarkable degree of internal consistency. G has hardly ever changed course or backtracked in his journey. He has set his course and followed it.

1. The first paper is entitled "A Comparative Study of the Chinese, Tibetan, and Burmese Vowel Systems" (1980). In this work G adopts F. K. Li's OC vowel system, which is summarized as follows (p.3):

Vowels:	i	u	Vocalic Clusters: iə ia ua
	ə		
	a		

The same system is discussed again in the fifth and ninth papers, and it is used *passim* in all the articles in this section. It is of course quite different from the six-vowel system posited by the opposing camp, and there has been much debate between the two

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<sup>9</sup> It should be noted that the present reviewer once worked actively in the field of Sino-Tibetan comparative studies. That involvement ended nearly two decades ago, if one considers that publication time for both articles and monographic works in the field often amounts to at least three or four years. Assuming, then, that the statute of limitations on youthful adventures should have expired in more or less twenty years, we shall claim neutrality here as regards the ideas to be examined. For it is not our intent to carry a brief for either of the major approaches alluded to above. Instead, we shall adopt the position of an outside observer whose primary concern today is with Chinese of much later periods than those covered by the conventional expression "Old Chinese".

sides on this point. We shall not involve ourselves in that discussion here. Instead, we shall merely give our own impressions of G's system as it relates to certain of our own current concerns. What immediately catches the eye here is the expression "vocalic clusters", a term which is not often encountered in linguistic descriptions of Chinese. It is clearly an adaptation of Li's own "vowel clusters", which he specifically introduced as the English equivalent of Chinese fùhé yuányīn 复合元音 in his 1971 article. When used in the descriptions of non-sinitic languages, the term "vowel cluster" is normally synonymous with "diphthong" and/or "triphthong". If Li meant it to be understood otherwise, he did not say so. Probably, his vowel clusters were meant to be taken as diphthongs. Now, diphthongs are usually classified into phonetic types, depending on which of their constituent elements is more prominent or sonorous. Falling or descending diphthongs show prominence in the first element, rising or ascending diphthongs in the second. The term "level" has been used to describe cases where stress continues evenly through the entire diphthong (Gleason 1961:255). It is possible that Li envisaged diphthongs of this type here, though again, he did not elaborate.

Diphthongs are sometimes interpreted differently by phonologists. For example, [au] in English *house* is phonetically diphthongal, but different phonologists have described it variously as a single phoneme, as a vowel plus another vowel, or as a vowel plus a semivowel or glide. G's own comments on this matter as it affects OC are as follows (p.84), "All these vowel clusters behave as one syllable, as it is assumed, that each Chinese character stands for one syllable." It is uncertain what is really intended here, for we must wonder what it means to say that vowel clusters behave as syllables. But in an immediately preceding line G makes a clear and unequivocal statement: "As for Li's vowel clusters /iə/ and /ia/, they are different from /jə/ and /ja/..." And herein lies a significant difficulty for observers who are familiar with modern spoken forms of Chinese or with alphabetically recorded varieties of the language from pre-modern periods. For in known forms of Chinese, concatenations of a high vowel followed by a lower one are nearly always realized phonetically as glide plus vowel. This rule is so pervasive that field dialectologists hardly ever explicitly transcribe glides in their recordings of Chinese dialects. When one sees a written form [kian], one automatically reads it as [kjaŋ], etc. To find a form of Chinese where this is not the case would be rather unusual. But to find one where both [kian] and [kjaŋ] were present, were **capable of interriming**, and were **in phonemic contrast** would be, quite simply, astounding. Now, perhaps there are places in the world where such things occur. But to find it in **Chinese** would be unheard of. And we must remember here that it has in fact not actually been found. It has, on the contrary, merely been hypothesized as a solution to certain textual problems. But to propose it in this context is really no less surprising than to introduce ejectives and clicks into discussions of earlier Chinese consonantism.

In a world where all things are theoretically possible, this must by definition also be so. But our inclination would always be to look for any other “more Sinitic” alternative before taking refuge in such a peculiar redoubt. Now, in all fairness, the idea that entities like a “kjan and a “kian could co-exist, rime, and contrast in Chinese was in fact already suggested nearly a century ago by no less a luminary than Karlgren himself, but, let us never forget, purely as a **theoretical solution** to problems in the interpretation of the rime tables. And thence did it quickly pass into the lore of the field, with hardly a murmur at the time from anyone except perhaps Y. R. Chao, a person who, significantly, had spent a great deal of time actually listening to various types of Chinese (Chao 1941a:62, 1941b:212-15). We today, after so many further decades of listening to even more types of Chinese, should stand back and look at what has really been suggested here. It is, in a word, astounding.

2. The second paper is entitled “On the Reconstruction of Certain Old Chinese Initials in the Light of Sino-Tibetan Comparison” (1990; republished: 1994).<sup>10</sup> One of the most striking differences between F. K. Li’s system and those of the other side is that, while Li derives later Chinese l- from earlier \*l-, the opposing side believes that it came from \*r-. Attendant on this latter theory is a complex of other ideas, e.g., that later initial zero followed by a strong palatal glide (the traditional yùsì 喻四 initial) derives from \*l-, that in certain graphic-phonetic series \*l- also yielded later d-, that later z- derives from \*l- clusters in certain contexts, etc., etc. In this matter G crosses the gulf and joins Li’s opponents. In the present paper he does not speak of the \*l- > d- question, but in the fifth paper (p.86, n.19) he adopts essentially that view, deriving d- from earlier \*l- plus a nasal element, which he says is “like ‘a (a-chung) in WT”. And in the seventh he deals with this nasal in somewhat more detail, along with certain other prefixes. We shall mention it again below in that connection. Here, then, G has left behind Li’s system; and so his interpretations can accordingly be viewed as a sort of bridge between the two major approaches to OC in this matter.

3. The third paper, “On the Reconstruction of Old Chinese Liquid Finals in the Light of Sino-Tibetan Comparison” (1993),<sup>11</sup> deals with the reconstruction of OC \*-r and \*-l. Li had originally reconstructed a final \*-r in certain syllables which in later Chinese had open finals. G concludes on the basis of comparisons with Tibetan that some of these \*-r’s were really \*-l’s. But further comparisons suggest to him that both final \*-r and \*-l sometimes also yielded later Chinese -n, without any detectable conditioning factors. G concludes that this was due to dialect mixture and envisages different OC dialects, some

<sup>10</sup> 從漢藏語的比較看上古漢語若干聲母的擬測。

<sup>11</sup> 從漢、藏語的比較看漢語上古音流音韻尾的擬測。

of which had \*-r/\*-l and some \*-n. We are not told when, where, or by whom these different dialects were spoken, or what other evidence there might be about them. For the moment, at least, their existence would seem to be an outgrowth of the present problem.

4. “The First Palatalization of Velars in Late Old Chinese” (1994), deals with a problem which has vexed the field for much of its history. Karlgren chose essentially to ignore it. Everyone else has had to tackle it, and the attempts have produced a wide variety of solutions. The problem, in a nutshell, is that certain Middle Chinese (MC) palatals appear to derive from OC velars, for unknown reasons. OC phonetic series point to velar initials in the problematic cases, and in some instances the archaic Mǐn dialects still show velars in such etyma today. Stray examples of this type are also found in other southern dialect groups. But no conditioning factors are immediately evident. Early solutions involved position sets of velar-like or palato-velar consonants as origins of the palatals in such cases. Later approaches more often build into OC medial features which would then have conditioned the palatalization. Li posited clusters containing the medial complexes \*-rj-, e.g. \*krj- > \*tśj-, etc. G’s solution is similar to this. He proposes OC initial complexes of the type \*klj-, \*khlj-, etc. as origins of the palatals. And systematically this works. However, those who are interested in Chinese of later times will be inclined to worry about the actual mechanics of the proposed process, in light of what we know about the language as it existed after the OC period.

As regards the actual process involved here, G says (p.74) that “the first palatalization took place before -lj-.” Taken literally, this can be interpreted as follows:

\*klj- > tślj- > tśj-

where -lj- is the conditioning factor for palatalization. We would then envisage a hypothetical stage, immediately after palatalization, where the change has occurred and the conditioning configuration is still in place. Later, the conditioning factor is lost, leaving the palatals as reflexes of the earlier velars. However, this scenario is a less than convincing one. For the fact is that we have in late Han and Six Dynasties materials a number of Buddhist transcriptional examples which show unpalatalized velars still in place, and in all such instances the Chinese syllables in question transcribe Indic syllables consisting of velar + vowel. For example, from the mid-sixth century Nanking area we find in the transcriptions of Sanghabhara:

指 tśi: for Sanskrit ki. Cf. G (p.72) 旨 \*kljid > tśi:

只 tśje: for Sanskrit ke. Cf. G (loc. cit.) 枳 \*kljig > tśje:



And for the northwest area of the same period we have from Jñānagupta the form 𑖀 standing for Skt. ke and kye, etc. So an intermediate stage such as *tślj-* would seem to be out of the question here.

But the fact is that G probably did not have such a scenario in mind in the first place, for on p.75 of the article he elaborates as follows: “In the case of Old Chinese, when -l- yodized before -j-, it caused the palatalization not only of initial velars, but of labials as well.” Now, yodization is described in the phonetics handbooks as a sound change or other process resulting in [j].<sup>12</sup> And, taken literally, this would suggest in the present instance that we get *\*klj- > kjj-* as our intermediate stage. But what would this mean in real phonetic and syllabic terms? How are we to make sense of it? A double glide is an anomaly in a syllable-based phonology like that of all known forms of Chinese. And, on the other hand, an automatic reduction to a single glide (i.e., -jj- > -j-) would yield homophones of the syllable types that did **not** palatalize, such as 技 *gje:*. So, in the end, we are left in a quandary here.

Now, as a legacy of the English usage of Karlgren, the term yodization is sometimes employed by sinological linguists as a synonym for palatalization. Should this be considered here? To read G’s statement this way means that earlier *\*l* in configurations such as *\*klj-* has become a palatal lateral, i.e., [ʎ]. But, viewed from the standpoint of the entire process, this yields *\*klj- > kʎj- > tśʎj- > tśj-*. Which leaves us no better off than we were above when we arrived at the hypothetical form *tślj-* as an intermediate stage.

Can we not, as a last resort, retreat to a version of the old position that *\*klj-* had yielded an intermediate palato-velar stage of some sort, i.e., *\*k̠-* [c], which was then used by early medieval transcribers to represent Indic *k*? Unfortunately, this will not do, for then we cannot explain why the transcribers would have chosen such an inexact representation when they had other, perfectly good, Chinese velar-initial syllables available for their purposes, syllables which they did in fact often use. The upshot of all this is, then, that while G’s formulation *\*klj- > tśj-* appears at first glance to make perfectly good diachronic sense, it turns out that we cannot envisage how it actually happened, given the textual evidence we have about the intermediate stages of the process. In the end our conclusion must be that, in order to be adequate, any palatalization hypothesis must give us a post-OC **unpalatalized** stage in which plausible conditions for palatalization are actually present in the finals.<sup>13</sup>

5. The fifth paper is entitled “The System of Finals in Proto-Sino-Tibetan” (1995). In

<sup>12</sup> See, for example, Peter Mathews, *The Concise Oxford Dictionary of Linguistics*, Oxford: Oxford University Press, 1997, p.408.

<sup>13</sup> For an effort to do exactly this, see Schuessler (1996).

the introduction, the author tells us, “The purpose of this paper is to set up the system of finals in Proto-Sino-Tibetan on the basis of a comparison of four classical languages in this family, i.e., Old Chinese, as reconstructed by Li (1971), Written Tibetan, Written Burmese, and Tangut as reconstructed in Gong (1993b).”<sup>14</sup> Here G adopts the vowel system already laid out in the first paper. And he also retains Li’s (and Karlgren’s) medial glide -j-, often called “yod” by Sinologists. In recent years there has been much criticism of this yod and its use in reconstructing earlier stages of Chinese phonology. The tendency has been to try and get rid of it in one way or another. G proves to be a strong champion of it, arguing that Tangut has inherited it virtually *in toto* from PST, thereby substantiating its existence. Now we must await the reply of the opposing camp, who are of course honor bound to tell the rest of us what they think about this, and to do so **in detail and with the same rigor** G himself has exercised in handling the Tangut data. This is an unenviable task, since it means they must undertake to penetrate for themselves the arcana of Tangut philology. We await their reply with interest.

A point where Li’s OC system differs sharply from those of the opposing camp is in his reconstruction of the final stops \*-g, \*-d, and \*-b in syllables where the other side has open finals, vowels + \*-s, and/or voiceless obstruents + \*-s. Karlgren also reconstructed stops in a number of the same finals, and he explicitly characterized them as voiced. In this connection, G (p.94) states that “Li (1971) has final voiced consonants” here. And this has also frequently been said by opponents of Li, who have in turn criticized these “voiced” consonants. But what did Li really say about these things? Let us read and find out (Li 1971:25):

.....這類的韻尾輔音我們可以寫作 \*-b, \*-d, \*-g 等。但是這種輔音是否是真的濁音，我們實在沒有什麼很好的證據去解決他。現在我們既然承認上古有聲調，那我們只需要標調類而不必分辨這種輔音是清是濁了。

“.....These final consonants can be written as \*-b, \*-d, \*-g, etc. However, we do not actually have much satisfactory evidence for deciding whether these consonants were true voiced sounds or not. For now, since we recognize Old Chinese as having had tones, we must only mark the tone classes and need not distinguish whether these [final] consonants were voiceless or voiced.”

Surely, surely, not statement ever made by any human being could be clearer than this. It would perhaps have been more convenient both for Li’s defenders, and for his attackers, had he adopted an absolute position on the voicing question. But the fact is that he did not. And no power on earth can ever change that now. Those who claim that

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<sup>14</sup> Gong (1993b) is identified in the references as: “Xixiayu yunmu xitong de nice”, Manuscript.

these sounds were voiced must do so entirely at their own risk, without appealing for support to Li's authority. And those who wish to attack this position should set their sights on those who actually hold it, rather than manufacturing a straw man at which to fling their missiles.

An interesting feature of G's final system is final \*-s, which he posits for syllables having later Chinese qùshēng 去聲. This \*-s is of course widely accepted among those who work on OC, having been first introduced by André Haudricourt in 1954 and gained ground steadily since then. Li did not dispute its possible existence in pre-OC stages of the language, but resisted its introduction into OC itself. G has parted from Li here and joined the other side. However, unlike others who reconstruct \*-s after vowels, liquids and the voiceless obstruents, G posits a series of voiced stop + \*-s clusters. Syllables of this type in his system generally correspond to vowel + \*-s and obstruent + \*-s syllables in the systems of other scholars. G also has voiceless obstruent + \*-s finals. The difference between the types, e.g., between a \*-gs and a \*-ks, is then determined for the most part by Sino-Tibetan comparative evidence.

6. The sixth paper is entitled, "The Chóngniǔ Question viewed from the Standpoint of Sino-Tibetan Comparison" (1997).<sup>15</sup> This is a study of the historical origins of the notorious chóngniǔ 重紐 ("double knot") final pairs, which are placed under different niǔ 紐 "small circles" (< "knots; knots used as buttons on traditional Chinese garments") in the lexica of the QYS. The separate finals in these pairs are also separated in the Sòng rime tables, being placed in the third and fourth rows (i.e., děng 等, conventionally called "divisions" or "grades" in English) respectively. Gong's conclusion, shared with certain earlier researchers and based on comparative and other evidence, is that syllables of the Division Three type originally had a medial -rj- combination, while those of the Division Four type did not. This view, with many individual variations and permutations, is fairly widely held today. G (p.126) cites with apparent approbation the idea of certain scholars, based on medieval Chinese transcriptions of Sanskrit, that MC may have had an r-like medial element in Division Three chóngniǔ syllables. This view has rather far-reaching implications for the syllable structure of whatever actually spoken forms of Chinese are assumed to underlie the transcriptional evidence. But since questions of this type are not the real concern of G's collected papers, we shall not pursue them further here.

7. The seventh paper is entitled "The Question of Old Chinese Prefixes viewed from the Standpoint of Sino-Tibetan Comparison" (2000).<sup>16</sup> The work deals in the main with

<sup>15</sup> 從漢藏語的比較看重紐問題（兼論上古 \*-rj- 介音對中古韻母演變的影響）。

<sup>16</sup> 從漢藏語的比較看上古漢語的詞頭問題。

three reconstructed prefixal elements: \*s-, \*r-, and \*N-. Of these, \*N- catches the eye, because an upper case transcription of this type usually implies an abstraction of some sort. As it turns out, this N- is borrowed from a paper by Chang and Chang (1977) who use it to transcribe the Tibetan letter 'a-chung and then extend it to reconstructed forms. G states that, following Chang and Chang, it represents “a nasal” (一個鼻音); but this is the extent of what he has to say about its actual nature. So we must turn to Chang and Chang to learn more about it. To begin, we should note that Tibetan 'a-chung is of course not a “sound” as such but rather a grapheme, so the question then becomes, what sound did it represent in WT when used in pre-consonantal position? And about this there has been much discussion. Chang and Chang clearly belong to the school which, on the basis of comparative study of the modern Tibetan dialects, feels the 'a-chung was added to other consonants to show that they were prenasalized. So, in a sense at least, Chang and Chang seem to have viewed it as a symbol for prenasalization. Maybe this is also how G conceives of it. Prenasalization is by definition homorganic with following consonants, so N- would be a good symbol for such a thing. And, as G makes quite clear here, his OC \*N- at the Sino-Tibetan comparative level corresponds to 'a-chung in Tibetan. In fact, in this paper he actually transcribes Tibetan 'a-chung as N- in the same way Chang and Chang do in their article. So far so good. But there is a problem. In G's reconstructed system we have a combination \*Nl- (i.e., \*N- plus \*l-). And this leads us to wonder whether such a thing as a prenasalized lateral really occurs in human language, especially in word initial position. Recently, Ladefoged and Maddieson (1996:118-19) have made some relevant observations on this. They say, “If we consider just those consonants produced with the pulmonic airstream, then the observed partially nasal consonants fall into only four classes. These are prenasalized stops (including affricates), prenasalized fricatives, prestopped nasals, and, perhaps, prenasalized trills. In all but prestopped nasals, the nasal portion of the segment occurs before the non-nasal part. **We do not know of any cases of prenasalized lateral or central approximants**, nor do we know of any types of segments other than prestopped nasals in which the nasal portion follows the non-nasal portion” (emphasis added: WSC). Now, it is unwise precipitously to discount a reconstructed entity simply because one does not oneself know of any cases of such a thing elsewhere. And it is certainly not our intent to do that here. But the fact that Ladefoged and Maddieson do not know of such a thing anywhere in the world is troubling. We would like for G to have given us some examples of it, and specifically in word initial position. This is not just fuss and pickiness on our part. G's \*Nl- is fundamental to his system, since it accounts for why his \*l- there can yield later d- in certain etyma. It is worrisome that the mechanism proposed for it is not known to occur in human speech. Alternatively, one might suppose that G's \*N- is really a separate consonant rather than prenasalization. But if so, what consonant? If it were a

plain consonant, he would surely have simply written it as such. The matter remains perplexing.

8. The eighth paper is entitled “Reconstruction of r and l Clusters in Old Chinese and Proto-Sino-Tibetan” (2001).<sup>17</sup> Clusters in -r- and -l- are of course posited by many scholars who work in the field of early Chinese. G’s particular formulations reflect the fact that he has joined those who derive later Chinese l- from earlier \*r-, etc. See paper no.2.

9. In “The Development of Finals from Proto-Sino-Tibetan to Old Chinese and Proto-Tibeto-Burman” (2000)<sup>18</sup> G gives the reader a full summary of his ideas on the field of Sino-Tibetan comparison. Though the paper is devoted specifically to the finals, his views on the initials are reflected in each of the comparative sets he discusses. In the previous papers listed here, he has for the most part allowed the ancient languages, Written Tibetan, Written Burmese, and to a lesser extent Tangut, represent the Tibeto-Burman side. But in the present article PTB is used, cited in the well-known reconstruction of Paul K. Benedict. And here, for the first time, we encounter numerous full PST reconstructed forms. Previously, the discussion was conducted mainly in terms of OC and the changes and additions to be made in Li’s OC system. Thus, the present paper offers us a complete picture of PST as G conceives of it and shows how it articulates with the Chinese and Tibeto-Burman daughter languages.

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In closing this section on G’s Sino-Tibetan comparative work, it is worthwhile to think about the enterprise as a whole. As already mentioned, the ninth paper cites numerous PST forms along with their OC and Tibeto-Burman reflexes. If we compare these, an interesting fact emerges. For it turns out that G’s PST and OC have the same forms at essentially all points! The only real exception to this seems to be a pair of PST finals in \*-ul and \*-jul, which correspond to OC \*-ən and \*-jən (p.238). Now, as noted above under our discussion of the third paper, OC is conceived of as having had different dialects, some of which had final \*-l where others had final \*-n. And the etyma where this dialect difference obtains are precisely those which show the PST \*-ul > OC \*-ən development. So, by rights we should be able to substitute OC \*-əl and \*-jəl on p.238 without doing any injustice to the coherence of the table. This step would bring PST and OC within a hair’s breadth of total identity. Now, if OC and PST are for all

<sup>17</sup> 上古漢語與原始漢藏語帶 r 與 l 複聲母的構擬。

<sup>18</sup> 從原始漢藏語到上古漢語以及原始藏緬語的韻母演變。

intents and purposes identical, in that we have no real way to distinguish between them, then this must inevitably affect our view of the Sino-Tibetan family as a whole. The usual view is that OC is a backward projection of MC and is the sole representative of the Sinitic branch of Sino-Tibetan. WT, WB, and Tangut, on the other hand, are pre-modern descendants of PTB, which represents the Tibeto-Burman branch. But if OC and PST are the same thing, then that changes the picture and should be acknowledged in our model for the family. In fact, it becomes redundant to use two different names for this one entity. We should choose one and set the other aside. Here, however, we shall for the sake of clarity simply combine them as OC/ST. Now, what implications does this have for the scheme as a whole? We must conclude that OC/ST is directly ancestral to MC, WT, WB, and Tangut, which are now parallel daughters of OC/ST. We should recall that OC/ST is not a true common system, reconstructed by strict application of the comparative method to the daughter corpora. It is rather a “customized” common system, where all comparative results have been passed through a sieve of strictures derived from the analysis of early (i.e., pre-MC) Chinese texts, to wit, rime categories, graphic-phonetic series, etc. Put another way, reconstructed OC/ST forms must account not only for development of the parent language to the daughters but also for the philologically established categories derived from early Chinese texts. In a real sense, then, MC, WT, WB, and Tangut are all descended from something which shows complete agreement with Zhōu period graphic and riming conventions. So, if in the end we choose PST as the name of this entity, then, logically, PST is a Sinitic language, to the extent that Zhōu period texts are viewed as Chinese texts. If one prefers to avoid this result, then one must call the language not PST but OC and then assume that WT, WB, and Tangut are, like MC, daughters of OC.

Speaking as an outside reader, another interesting observation arises from a comparative perusal of the nine papers. Examining the four major types of comparanda, i.e., MC, WT, WB, and Tangut, we find that the two alphabetically recorded ancient languages are cited in their orthographic forms, with no phonetic interpretation added. On the other hand, forms from the two logographically written ancient languages, i.e., MC and Tangut, are spoken of as “reconstructions”. But only the Tangut forms are starred in the customary manner of reconstructions. Why is this so? We are not told. All written texts are imperfect representations of their underlying languages. And all of them are consequently subject to phonetic interpretation. In WT, the interpretation of the graphic forms has been considered fairly straightforward. Only the letter 'a-chung, the combination g-y- (studied by G himself), and to a much lesser extent the final letters -b, -d, and -g, have been subject to substantive differences of phonetic interpretation. On the Burmese side one thinks, for example, of the extensive debate on the graphic combination usually transcribed as *ui*. But none of these phonetic interpretations are

conventionally called reconstructions in the literature. MC is another matter. Here the script is logographic, and the study of pronunciation has therefore centered on rime books and rime tables. These have been used to identify initial and final syllabic units and sub-units, which are in certain respects analogous to the units represented by alphabetic graphemes in languages like WT and WB, and for that matter also in Latin and Greek. The phonetic values of these MC units have then been determined by many of the same methods used to interpret the alphabetic scripts, i.e., modern pronunciation in later members of the same language group, loan forms into and out of the scrutinized language, etc. Perhaps this is why the MC forms are not starred. Perhaps the only reason for calling them reconstructions today is that Karlgren used that term for the process when he began his researches in MC. What about Tangut? Here the present reviewer dares not speak. To him, the methods employed to determine Tangut pronunciation **look** a great deal like those applied in Chinese, i.e., use of rime books to identify units, filling in the values of the units by comparison with parallel forms in other languages, etc. But, only fools rush in where angels fear to tread. We must leave Tangut to the Tangutologists. What strikes us, though, as outside readers, is that the current terminology used in this field of Sino-Tibetan studies is anomalous when juxtaposed with that of the field of comparative and historical linguistics as a whole. It is not just idiosyncratic. It is actively peculiar. And behind the terminology lies, of course, the methodology. Do anomalies in the former hint at problems in the latter? Our time grows short. Let us leave well enough alone.

## 5. Conclusions

The collection under review is in a word a monumental achievement. It reflects remarkable erudition and dedication of a sort we may all envy and should all emulate. It lays before us a fully-drawn statement of the author's approach to the field of Sino-Tibetan comparative linguistics, which can now serve as a fruitful and instructive guide to all, whatsoever their particular theoretical persuasions may be. To those who wish to begin their studies in the field, it can serve as a textbook. In particular, its careful tracing of the birth and subsequent developments and filiations of particular theories and views, all supported by exact references to the original sources, will be an invaluable guide to readers. And to those who hold alternate or opposing views, it sets a high standard for future discussion. Any reply to the author's positions must now be framed with the same care and rigor he has himself exercised in propounding them.

From Professor Gong the field has received a gift of inestimable value, the fruits of a lifetime of dedication to Sinology and Sino-Tibetan comparative linguistics. In closing, we offer him both congratulations and thanks.

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