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# Irrefutable Conjectures. A Review of William H. Baxter and Laurent Sagart, Old Chinese. A New Reconstruction

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## IRREFUTABLE CONJECTURES\*

A Review of William H. Baxter and Laurent Sagart, *Old Chinese. A New Reconstruction* (Oxford: Oxford University Press, 2014)

(Based on a Talk Given at “Recent Advances in Old Chinese Historical Phonology,”  
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*Ut rerum omnium, sic linguarum instabilis conditio.*  
Du Cange, *Glossarium mediae et infimae Latinitatis*, Introduction.

*The book under review summarises and develops many decades of painstaking research in the early history of the pronunciation of the Chinese language. It is the result of the collaboration between two influential linguists. An examination of the methodology deployed in this book and the philological evidence it is based on reveals very serious shortcomings of many kinds that invite further discussion. For example, the very nature of Bernhard Karlgren’s contribution to the field is misconstrued as being concerned with phonology, when in fact Karlgren was a vociferous opponent of phonology throughout his long life; there is a complete failure to problematise and properly consider the very concept of “Old Chinese”: the literature on Dialectology of Old Chinese is never considered; the analysis of derivation by tone change is quite inadequate; the discussion of first Old Chinese first person pronouns is basically ill-informed. Most importantly, the methodology is unacceptably conjectural throughout.*

KEYWORDS: *Old Chinese, Phonology, derivation by tone change, Historical Linguistics*

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\* During the long period of gestation and revision of this article I have profited from professional friendly advice from Wolfgang Behr (Zürich), Robert Blust (Hawaii), Stephen Colvin (London), Redouane Djamouri (Paris), Paul Goldin (Pennsylvania), Yaroslav Gorbachov (Chicago), Jacques Guillaume (Paris), Zev Handel (Seattle), Nathan Hill (London), Victor Mair (Pennsylvania), Alain Peyraube (Paris), Axel Schuessler (Waverly, Iowa), Christian Schwermann (Bonn), Hans van Ess (München), Alexander Vovin (Paris), Rudolf Wagner (Harvard), and especially for crucial editorial as well as sinological assistance from Jens Østergaard Petersen, Copenhagen. Of course, all misapprehensions that remain in this review are entirely my own and are not to be blamed on any of my learned colleagues and friends.

## ABBREVIATIONS

B&S	Baxter and Sagart
<i>Handbook</i>	William H. Baxter, <i>A Handbook of Old Chinese Phonology</i>
IE	Indo-European
IPA	International Phonetic Alphabet
MC	Middle Chinese
<i>New Reconstruction</i>	Baxter – Sagart, <i>Old Chinese. A New Reconstruction</i>
OC	Old Chinese
PIE	Proto-Indo-European

I take solace from the fact that no one will suspect me of being specialised in Chinese phonological history. As some may know, I am much less of a phonologist than a student of Chinese *lianhuan tubua* 連環圖畫 (bandes dessinées, “comic strips”). I am certainly not to be taken seriously on the niceties of phonological reconstruction. All I can offer here are some queries by a profane outsider. Many of these queries are perhaps based on misunderstandings, and will be easily answered. I shall stand corrected, chastised, gladly!

I have greatly enjoyed learning from Bill Baxter for a very long time. I even invited him to one of the haunts of Prince Trubetskoj, in beautiful Prague, to encourage the production of something that should be called “Phonologie chinoise pour les nuls,” in congenial collaboration with Professors Ulrich Unger, Pan Wuyun 潘悟雲, Axel Schuessler, David Sehnal, and Lukáš Zádřapa. As you shall see presently, we have always had a lot of things to discuss. The present “communication” is a public continuation of that ongoing conversation, “only for the curious and inquisitive,” inspired and informed by a *magnum opus* that deserves to be taken seriously in every detail, and in more than a three-page docile review.

In the rare picture book *Dì jiàn tú shuō* 帝鑒圖說 (Illustrated Primer for the Perusal of the [Míng dynasty] Emperor) I hit upon the following passage:

三年遠方重譯而至者七十六國，商道復興

Within three years, from distant parts, seventy-six states for whom repeated translation was necessary joined the Shāng (dynasty) and the Way of the Shāng flourished.<sup>1</sup>

This is, of course, not evidence on the Shāng; only about Míng dynasty perceptions. However, there is some earlier evidence that ancient China was a multilingual society. Concepts of translation were well-known even in early pre-Buddhist literature.<sup>2</sup> Translators were informally known in Han times by terms like *shérén* 舌人 (tongue men).<sup>3</sup> The public office of a “translator” or “interpreter” was recognised already in the early blueprints for an idealised Chinese bureaucracy. Often, it would appear that the translation was not between the (often mutually

<sup>1</sup> Tokyo National Diet Library exemplar, dated 1611, vol. 1, p. 27a.

<sup>2</sup> Behr 2004 provides a useful detailed survey on this matter.

<sup>3</sup> *Guóyǔ*, p. 63, note 12.

incomprehensible) Chinese dialects, but for communication with “barbarians” speaking different languages.<sup>4</sup> In *Zuǒzhuàn* 左傳 we thus hear of translation between Chinese dialects like that of the states of Qín 秦 and Wèi 魏:

秦伯師于河西，魏人在東，壽餘曰：「請東人之能與夫二三有司言者，吾與之先。」

The earl of Qín took post with a force on the west of the Hé, and the men of Wèi were on the east. Shòu Yú [of Qín] then said, “Let me beg the company of some man from the east who will be able to speak with those officials [from Wèi], so that I may go before with him.”<sup>5</sup>

The languages of the states of Qí 齊 and Wú 吳 were held to be mutually incomprehensible – quite possibly one might argue that the speech of Wú should be regarded as “the speech of the Yí and the Dí barbarians” (*Yí Dí zhī yǔ* 夷狄之語), i.e., not as a concurrent dialect of Chinese.

夫齊之與吳也，習俗不同，言語不通。

As for Qí and Wú, their customs are not the same and their languages are mutually incomprehensible.<sup>6</sup>

Old Chinese must be seen in the context of the dialects and adjacent languages with which it is known to have coexisted and with which it must have interacted. But as a written medium it would appear that Old Chinese reigned supreme. There were no competing literary languages in Old Chinese times. The *Chūcí* 楚辭 (Songs of the South) are written in what looks like classical Chinese. What spotty evidence we have on the dialects and languages coexisting with Old Chinese has been collected by a student of Lǚ Guóyáo 魯國堯, Huá Xuéchéng 華學誠, in his *Zhōu Qín Hàn Jīn fāngyán yánjiū shǐ* 周秦漢晉方言研究史 (*Huá Xuéchéng* 2007). Lǚ Guóyáo, in his preface, holds this book to be of crucial importance for the definition and study of Old Chinese. I fully agree. I do not understand how Baxter and Sagart (hereafter: B&S) can discuss ancient dialects without showing any awareness of the best recent literature on the subject.

#### ON THE VERY NOTION OF “RECONSTRUCTION”: A MINOR TERMINOLOGICAL NOTE

Wáng Lì’s 王力 preferred term for what B&S call “reconstruction” is the considerably less assertive *nǐcè* 擬測 (guess by surmising), and also the modest *nǐchéng* 擬成 (establish by surmising). You might think this terminological observation is an irrelevance that would only occur to an outsider unfamiliar with the discourse. But far from it! As background information on this, I must add that although Wáng Lì never seemed to me remarkable for the quality of his English, he clearly read the language quite well.

<sup>4</sup> *Guóyǔ*, p. 62.

<sup>5</sup> *Zuǒzhuàn*, Duke Wén 13.2, pp. 595–596.

<sup>6</sup> *Lǚshì chūnqiū* 呂氏春秋 23.3, p. 1552.

And it is very clear that he sensed in the English “reconstruct” an assertiveness that comes out in the word *chóngjiàn* 重建. He explicitly distanced himself from such crude discourse in connection with the kind of tentative reasoning that is all one can hope for in *nǐ gǔyīn* 擬古音 (guessing old sounds), as the Chinese generally tend to put it. Wáng Lì preferred his own emphatically unassuming *nǐcè*:

古音的擬測是以音標說明古音的系統。這些音標只是近理的假設，並不是真的把古音“重建”起來。

The *nǐcè* (lit: “hypothetical guesswork”) on old pronunciations use phonetic symbols to explain the system of old pronunciations. These symbols are no more than approximate hypotheses, they do not really “reconstruct” the old pronunciations.<sup>7</sup>

This is not just a case of self-deprecatory politeness. It is a matter of civilised intellectual *humilitas*, with a touch of scholarly “self-denial.”

#### ON THE VERY IDEA OF THE “PREINITIAL” \*C

I leave aside the fact that \*C is not preinitial but initial wherever it occurs. In their conceptual confusion B&S invite us to call certain postinitial elements “initial.” I am advised by a sturdy enthusiast of their *New Reconstruction* that “initial” in the *New Reconstruction* needs here to be read not as “the first element in a word,” but as “the first consonant in a root.” If that were so, all “preinitial” material would have to be in the form of prefixes to roots. But the “Root Structure” is formally defined as including “preinitial” elements in Fig 3.1 on p. 50 of the *New Reconstruction*. However that may be, it is logically very hard to be preinitial – as it were – without what one precedes in the process ceasing to be initial enough to be preceded as an initial ... There is sound reason to avoid logical dissonance in analytic terminology. And in matters of logic, there is no safety in numbers or in hosts of acolytes.

This must be enough about mere terminology. Let me turn to matters of conceptual substance. B&S write: “Onsets with tightly attached unidentified preinitial \*C” (p. 168). In some onsets, a preinitial consonant must be supposed but cannot be identified because it has been lost in all the pronunciations under consideration. The preinitial \*C is to “be thought of as either a stop or \*s” (p. 168). This is all I could find by way of a definition in the book about the nature of “preinitial” \*C. But I am still looking for further information.

One surprising piece of information that I did find is that this: \*C is not only a “preinitial” but a “presyllable”: “... in any case, we must reconstruct \*C.rəp, with a presyllable of some kind” (p. 307). But it is \*Cə that is a “presyllable,” whereas C is tightly attached and does not constitute such a “presyllable.” “Presyllables” require a vowel. The conceptual confusion is manifest.

Moreover, the phrase “unidentified preinitial” is clearly ambiguous. It can either refer to

- a certain initial constant that is the same in all cases, but the identity of which is uncertain, or

<sup>7</sup> Wáng Lì 1964, p. 62.

– an unidentified consonant that may be different from case to case.

At a crucial and highly controversial abstract point, the *New Reconstruction* seems here simply ill-defined and confused. For example, as interested in the study of Chinese rhetorics we are not told whether there is or is not alliteration between two words beginning with \*C. This would be supremely interesting to know.

There is yet another detail of little real substance but not without conceptual interest: the “presyllable” \*Cə does have what is often called an epenthetic vowel the exact quality of which is purely conjectural and certainly needs to be bracketed according to the B&S system (see p. 8). It needs to be bracketed in order to acknowledge that the exact quality of this vowel as ə is unsupported by any direct or indirect evidence. Nothing whatsoever proves or suggests that ə should be preferred to i, for example, or indeed any other short minor epenthetic minor vowel. (What makes this point substantially unimportant is the fact that we all really do not care which vowel it was. My point is only about conceptual clarity. Or rather: missing conceptual clarity.)

The conjecture of the highly abstract “preinitial” \*C or \*Cə matters a great deal for the *New Reconstruction*. The proposal of these “preinitials” is closely connected with “cognates” in non-Chinese languages that would be phonologically impossible to propose without “preinitial” \*C. On the other hand, if “preinitial” \*C were introduced on the basis of these presupposed cognate relations, then to say that “preinitial” \*C provides any support whatsoever for this initial presupposition is one of those entirely circular arguments.

Here is the motivation adduced for \*C in B&S:

Onsets like \*C.p<sup>(ʰ)</sup>- are reconstructed primarily to account for cases where Middle Chinese voiceless obstruents correspond to Vietnamese spirantized initials (v- [v], d- [z], gi- [z], g- [ɣ]; r- [z] in the case of \*C.s-) with high-register tone, ...” (p. 168)

This leads to another question.

### OLD SINO-VIETNAMESE

The language that B&S carelessly refer to simply as Vietnamese is clearly not Vietnamese. One might imagine that it was Sino-Vietnamese, but it was not (my Sino-Vietnamese dictionary gives completely different readings than those quoted in the *New Reconstruction*). They clearly mean to refer to the very important phenomenon of pre-Tang Old Sino-Vietnamese.

B&S write elsewhere:

Sino-Vietnamese readings are not directly relevant to the reconstruction of Old Chinese, and neither are the borrowings made in the later period, but the borrowings of the earlier layer, characterized by the tone correspondences in Table 2.111, are relevant. *Neither period is entirely homogeneous in terms of sound correspondences.*” (p. 35, my emphasis)

The last phrase bodes ill: for if there are no good sound correspondences, how are we going to be sure of what is and what is not a loan?

Anyway, now that we have successfully determined that the reconstructed spirantized initials neither belong to Vietnamese, as claimed, nor to Sino-Vietnamese, but actually to Old Sino-Vietnamese, the obvious question that arises is this: What are B&S's sources for the Old-Sino-Vietnamese reconstructions they quote? Who decided on how exactly these words were to be pronounced? Did B&S perchance homogenise the results of the reconstructions they base themselves on?

One suspects that B&S might have taken their examples from relevant work of Michel Ferlus which they quote further down on another matter. But no primary or secondary sources are given in the *New Reconstruction* for the crucial table 4.62, listing “Vietnamese spirantization of voiceless obstruents in tightly attached clusters.” Are the eight examples given all there are? Is the sound correspondence regular throughout the Old Sino-Vietnamese system?

A brief reference to [Mài Yún and Hú Míngguāng 2010](#) would have guided the reader to a very useful historical survey leading also to the primary sources. Reference to [Wáng Lǐ's Hàn Yuè yǔ yánjiū 漢越語研究 \(1948, many times reprinted\)](#), would have enabled the reader to compare B&S's data and reconstructions with an indispensable source on the subject. The magisterial and methodical survey of current developments in Sino-Vietnamese studies is [Hashimoto 1978](#). Referring to this would allow the reader to gain a perspective on the history of the study of Old Sino-Vietnamese.

[Phan 2013](#) takes issue with Hashimoto's account on pp. 48–174, mainly in a chapter entitled “Defining Early Sino-Vietnamese.” There is no need to copy here Phan's bibliography: a short selection of relevant material unmentioned by B&S must suffice here to show how they might have encouraged their readers to enter into a well-informed dialogue on the subject under discussion:

Benedict, Paul K., “An Analysis of Annamese Kinship Terms” ([Benedict 1947](#)).

Đào Duy Anh, *Chữ Nôm. Nguồn gốc, cấu tạo, diễn biến* (Chu Nom. Origins, Formation, and Transformations) ([Đào Duy Anh 1979](#)).

Ferlus, Michel, “Problèmes de la formation du système vocalique du Vietnamien” ([Ferlus 1997](#)).

Hashimoto Mantarō, “Current Developments in Sino-Vietnamese Studies” ([Hashimoto 1978](#)).

Mài Yún 麥耘 and Hú Míngguāng 胡明光, “Cóng shǐshí kàn Hán-Yuè yīn” 從史實看漢越音 ([Mài Yún and Hú Míngguāng 2010](#)).

Maspero, Henri, “Quelques mots annamites d'origine chinoise” ([Maspero 1916](#)).

Nguyễn Tài Cẩn, *Nguồn gốc và quá trình hình thành cách đọc tiếng Hán Việt* (The Origins and Process of Development of Sino-Vietnamese Readings) ([Nguyễn Tài Cẩn 1979](#)).

Phan, John Duong, *Lacquered Words. The Evolution of Vietnamese under Sinitic Influences from the 1st Century B.C.E. through the 17th Century C.E.* ([Phan 2013](#)).

Pulleyblank, Edwin G., “Some Notes on Chinese Historical Phonology” ([Pulleyblank 1981](#)).

Tryon, Ray, *Sources of Middle Chinese phonology. A Prolegomenon to the Study of Vietnamized Chinese* ([Tryon 1979](#)).

Wáng Lì 王力, *Hàn Yuè yǔ yánjiū* 漢越語研究 (Wáng Lì 1948).

It would make all the difference to everyone whether a given IPA interpretation of Old Sino-Vietnamese or a phonological reconstruction of a Proto-Vietnamese word had been formulated and found reliable by an authority on the order of an Henri Maspero or an André Haudricourt, or whether it may be a creative conjecture, for example, of someone who might have aspired to relate Old Sino-Vietnamese with maximally regular homogeneous sound correspondences to Old Chinese, as it was being reconstructed by B&S.

There can be no doubt: the critical reader needs to be told what B&S's sources were for Old Sino-Vietnamese. One needs to know to what extent these sources were rewritten and homogenised by B&S. For good reasons we need to be reassured that no wishful conjectures have gone into the reconstruction of Old Sino-Vietnamese – or of any of the other relatively inaccessible languages and proto-languages they quote, like Proto-Vietic, Proto-Viet-Muong, but also Proto-Sinitic, Proto-Sino-Tibetan, Proto-Tibeto-Burman, Proto-Min, Proto-Hmong-Mien, Proto-Hmongic, Proto-Mienic, Proto-Bodo-Garo, Proto-Hakka, Proto-Kra, Proto-Kam-Sui, Proto-Tai ... Some, if not all, of these constructs presumably remain controversial even when – for lack of recalcitrant scholarly manpower – they are as yet uncontested. But B&S present data about many of these simply as *faits accomplis*, to be accepted on their own authority, rather than on the basis of primary or secondary evidence.

#### THE ETYMOLOGY OF SHÌ 勢

B&S try to establish a potentially interesting relation of morphological derivation between shè 設 and shì 勢. For this derivation to work by the methods of the *New Reconstruction* one needs to establish

- that there was in fact no such difference in the initial consonant configurations, and
- that there was in fact no such difference in the main vowel of the words.

And B&S do come up with a pair that looks very neat indeed:

設 shè < syet < \*ŋ<sup>o</sup>et “set up”

勢 shì < syejH < \*ŋ<sup>o</sup>et-s “circumstances, setting”

The argument for there being the same initial \*ŋ<sup>o</sup>- is interesting: it appears that the word 設 was written with antecedents of 執 yì < ngjiejH < \*ŋet-s “to plant.” This – of course – would be excruciatingly difficult to prove, since the meaning “set up” is so close to the meaning “to plant.” How could one ever prove that when 執 means something very much like 設 there is not a phonetic loan here, but a semantic derivation? How could one ever prove that by anything more than subjective intuition or *Sprachgefühl*? Let us assume that Qiú Xíguī, adduced by B&S in this connection, had sound and entirely objective and testable reasons for holding that antecedents of 執 are commonly used to write the word 設 in oracle bone inscriptions, and that this



then does deserve to be called not a plausible conjecture *ex auctoritate*, but an objectively verifiable “discovery.” B&S write:

This discovery allows us not only to identify the initial consonant of (設) shè as \*ŋ°- but also to recognize its etymological relationship to 勢 shì < MC syejH ‘circumstances, setting’. (p. 30)

At least three fundamental objections arise:

Firstly, one would need to be instructed how \*ŋ- “allows one to identify the initial consonant” \*ŋ°. If there is an explanation it is certainly necessary to provide it at this point. We need to be told why the initial reconstructed has to be \*ŋ°- and not \*ŋ. (The argument from the *qùshēng* 去聲 derivation of 勢 from 設 would, of course, be circular in the usual way.)

Secondly, one would like to know if B&S seriously wish to maintain that the use of a character A with a reading X to write another word B licenses one to draw definite conclusions on how exactly to reconstruct the pronunciation of B. Anyone who has ever looked at phonetic loans in oracle bone inscriptions (and also bronze inscriptions) knows that many plausible cases of phonetic loans are often uncomfortably distant.

Thirdly, since we are so meticulous about the initial (where variation in phonetic loans is notoriously great), why do we hear nothing about the (morphological?) final \*-s in \*ŋet-s “to plant”?

The establishment of the reconstructions of the pronunciations of the elegant pair 設 \*ŋ°et “set up” / 勢 \*ŋ°et-s “setup” is not at this point based on any compelling evidence (which is not to say that the two words are unrelated!).

Accepting now, for the sake of the argument, the above reconstructions, the conclusions B&S draw are nothing less than philosophically disastrous. Basing themselves on the Mathews dictionary (of all things!) in matters of Old Chinese lexicography they write:

From the connection with 設 shè < \*ŋ°et “set up,” we can see the common thread: 勢 shì is basically the way things are set up. It can refer to *the way nature has set things up*, including such things as terrain and weather ... (p. 30, my emphasis)

Nature, in this account, is construed as an agent that goes about setting things up. Until the end of the 19th century that was a way of semi-personifying nature that was commonly read into Chinese philosophical discourse. B&S intimate that their etymology is of philosophical conceptual importance. If they had consulted any of the literature on the subject,<sup>8</sup> I think they might think otherwise. The phrase *zìrán zhī shì* 自然之勢 (the natural constellation of things) is generally taken as a constellation that is natural, “so of itself,” and not a constellation that was created or “set up” by some metaphysical agent Nature.

B&S are obviously most welcome to disagree with this way of taking things. But that would need more evidence than one failed translation from Hán Fēi’s 韓非 book. In the phrase *wú suǒ wéi yán shì zhě* 吾所為言勢者 they do not even try to

<sup>8</sup> See Röllicke 1996, Jullien 1992, and Harbsmeier 2010.

interpret *wéi* 爲 and translate simply as if it were not there: “The setup (\*ŋ°et-s) of which I am speaking ...” (p. 30). As it happens, *Hán Fēizǐ* 韓非子 40 contains much that is directly relevant to B&S’s basic idea, but they certainly write as if they had never seen that chapter.

### THE CASE OF *SHUǐ* 水 (WATER; RIVER)

For Middle Chinese 水 we have citing from the Internet site *Yùndiǎn wǎng* 韻典網 (<http://ytenx.org>):

Gāo Běnhàn 高本漢 (Bernhard Karlgren): ɕwi

Lǐ Róng 李榮: ɕjui

Wáng Lì 王力: ɕwi

Shāo Róngfēn 邵榮芬: ɕiu

Pulleyblank: ɕwi

Zhèngzhāng Shāngfāng 鄭張尚芳: ɕyiu

Pān Wùyún 潘悟雲: ɕ<sup>w</sup>ui

We can add to these:

Schuessler 2007: świ<sup>B</sup> (PMin form: \*tśui<sup>B</sup>)

Baxter 1992: sywijX

For OC, on the other hand, I have come across, from scholars using very largely the same primary sources, the following readings:

Wáng Lì: ɕiei

Lǐ Fāngguì 李方桂: hrjidx

Baxter 1992: hljij?

Zhèngzhāng Shāngfāng: qh<sup>w</sup>ljil?

Pān Wùyún: qh<sup>w</sup>lji?

By general acclaim we should now apparently all readily accept a new version:

B&S: \*s.tur?

In the *Shījīng* 詩經 (Book of Odes), 水 definitely has to be reconstructed so as to rhyme with the following, as reconstructed in the *New Reconstruction*:

- *wéi* 唯 \*<sub>G</sub><sup>w</sup> ij (< \*<sub>G</sub><sup>w</sup> uj?) (Ode 104)
- *dì* 弟 de jX < \*l<sup>h</sup>əj? (Ode 92).

As we shall see, it is also taken by some to rhyme with

- *sūn* 隼 \*<sub>s</sub>[ur? > \*<sub>s</sub>[un? > swinX (Ode 183)

## WHAT RHYMES WITH WHAT?

Regarding rhymes, yet another question looms large and is never faced squarely within its comparative context in Baxter 1992 or the *New Reconstruction*: what do we know about the general nature of the phonetic similarity that was required for something to count as a rhyme? We have plenty of Chinese poetry written by poets who certainly did not speak Middle Chinese, but who aspired to rhyme according to Middle Chinese standards. The traditional rhyme is well attested in many languages, even those without rhyme books. How do we know that such traditional rhyming was not practiced by rhymers in the Warring States and before?

What exactly is the conclusion, then, that we may draw from X regularly rhyming with Y? If we reconstruct on the assumption that there was no traditional rhyme, then – by a tempting circular argument – our reconstructions will confirm that there was exact rhyming, at least to the extent that we have been able to impose the regularity through our methodology. In fact, of course, nothing is being proved. One’s method is begging the question. We are begging exactly the kind of question about Shakespeare’s pronunciation that Helge Kökeritz 1970 was trying to answer, with such entertaining philological care.

Similarly for the closeness of relations of isoglosses or cognates between Chinese and related languages. If we reconstruct Old Chinese on the assumption that Tibetan and other so-called “Sino-Tibetan” languages are directly relevant to this reconstruction in the first place, then our reconstructions will abundantly and consistently confirm that there are such close relations, at least to the extent that we have been able to impose our “comparatist” methodology. But, of course, we will have proved nothing. Essentially, what we presuppose and impose by our methods is what we get in our results.

Similarly for the recurrent phonetic elements. If we reconstruct Old Chinese on the assumption that the recurrence of a phonetic element licenses certain claims of phonetic or phonological similarity, then our reconstructions will abundantly and consistently confirm that there are such phonetic or phonological similarities, at least to the extent we have been able to impose our methodology. But in fact, of course, no scientific hypothesis is being proposed here: all we have is *circulus vitiosus vulgaris*, to parody Sir Karl Popper’s often Teutonic magisterial manner of putting things. What you methodologically presuppose is what you get. (And this is psychologically akin to, but logically quite distinct from, what I have called the wide-spread “wishful conjecture” which is akin to plain wishful thinking, *xiéyīn* 叶音 style,<sup>9</sup> and which never enters any cycles or circle of pretended argumentation. I will return to this below.)

<sup>9</sup> This technical term is also written 協音 and 諧音.

Similarly for the interpretation of the suffix *-s*. If we reconstruct Old Chinese on the assumption that *-s* has certain semantic functions and that a coda *-s* that has no such functions is thereby shown not in fact to be a suffix at all, we shall then find that there is great regularity in the functions of *-s*, at least to the extent that we have been able to impose our ingenious methodology.

The only convincing direct evidence for the pronunciation (of the rhyme only) for 水 is in the Ode 104,<sup>10</sup> where 水 \*s.tur? would have to rhyme with 唯 \*G<sup>w</sup> ij (< \*G<sup>w</sup> uj?) and in Ode 92, where it would have to rhyme with 弟 dejX < \*l<sup>ʰ</sup>əj?. With the reconstruction of 水 as \*h(l)juj? in for Ode 92 and 104 (Baxter 1992: 617 and 623) this worked reasonably well. But then there is another case where the rhyming is with 弟 in Ode 183 (Baxter 1992: 660), but at the same time there is a puzzling rhyme with 隼.

According to B&S,

(586) 水 \*s.tur? > sywijX > shui “water, river” pMín \*tʃyi B (as if from “water; river” OC \*tur?)

rhymes with

(1130) 隼 \*[s] ur? > \*[s]un? > swinX > sǔn ‘hawk, falcon’

but at the cost of the rhymes with 唯 \*G<sup>w</sup> ij < \*G<sup>w</sup> uj? and 弟 dejX < \*l<sup>ʰ</sup>əj?.<sup>11</sup>

The Qing philologist Jiāng Yǒugào 江有誥 (d. 1851) handles the rhyming by pulling an unattested phonetic reading *yīn xī* 音璽 for 隼 out of his methodological hat, and claiming, by what I can only call a wonderfully explicit case of an output-based proposal, that in *shuǐ yǔ sǔn dì xié* 水與隼弟協 “*shuǐ* harmonises with *sǔn* and *dì*.”<sup>12</sup>

Even in Qing times, of course, there were those who denied there was any rhyme whatsoever here, for example Fāng Yùrùn 方玉潤 (1811–1883); in his *Shījīng yuánshǐ* 詩經原始, he lists the rhymes as 海十賄止四紙通韻.<sup>13</sup>

Wáng Lì, a staunch opponent of harmonising output-motivated conjecture, also refuses to reconstruct a rhyme in this instance, suggesting the following plain rhyming pattern:

沔彼流水，朝宗于海 (xə):

馱彼飞隼，载飞载止 (tjə)。

嗟我兄弟，邦人诸友 (hiuə)。

<sup>10</sup> That is, if we refuse to follow Karlgren’s emendation of 唯 to 隹 (1950, pp. 67, 267), which B&S do not find worth discussing. But B&S actually rarely mention this kind of problem. For the important question of variants in rhyme position see the essential Yuán Méi 2013 (p. 160, variants 遭 and 遭) and for a Western perspective Martin Kern 2003 and 2005. Incidentally, in their discussions of loan characters, B&S disregard Karlgren’s extensively argued and very philological “Loan Characters” while carefully referring to the notoriously indiscriminate Gāo Hēng 1989.

<sup>11</sup> *Shīsānjīng zhùshū*, vol. 5, p. 229 glosses: *sǔn xī yīn fǎn* 隼息尹反, as all the other ancient phonetic glosses would have us predict.

<sup>12</sup> Jiāng Yǒugào 1973, p. 57.

<sup>13</sup> Fāng Yùrùn 1986, p. 374.

莫肯念乱，谁无父母 (mə)? (之部)

沔彼流水，其流汤汤 (sjiang):

馱彼飞隼，载飞载扬 (jiang)。

念彼不迹，载起载行 (heang)。

心之忧矣，不可弭忘 (miuang)。 (阳部)<sup>14</sup>

B&S assume that Fāng Yùrùn, Wáng Lì, and also Hán Zhēngróng 韓崢嶸 (Hán Zhēngróng 1995, p. 233), as well as the rhyme-focussed Huáng Diǎnchéng 黃典誠 (Huáng Diǎnchéng 1992, p. 234) had it quite wrong, all of them. To be sure: B&S may indeed well have it right. But the rhyming pattern is far from being self-evident or uncontested. As Wáng Xiǎn 王顯 points out in the introduction to his singularly helpful *Shījīng yùnpǔ* 詩經韻譜 (2011, not mentioned in the *New Reconstruction*), justifying each one of one's assignments of *Shījīng* rhymes by itself would demand much more space than the 485 pages of his book.

I might have taken B&S's reasoning based on the *Odes* rhymes here much more to heart if they had at least begun to face the philological issues concerning the rhyming schemes in their primary source.

Discussing Ode 183, B&S write (p. 253):

Karlgren's original reason for reconstructing \*-r was the presence of various kinds of contacts between words of the traditional 微 Wēi and 文 Wén rhyme groups. For example, there are rhymes between the two groups, as in this sequence from Ode 183.1–183.2:

(1007) Ode 183.1–183.2:

水 shuǐ “water” < sywijX (微 Wēi)

隼 sǔn “hawk” < swinX (文 Wén)

A little later in the *New Reconstruction* (p. 295), we do get the necessary path of derivation for the phonology:

(1130) 隼 \*[s] ur? > \*[s]un? > swinX > sǔn “hawk, falcon”

準 \*tur? > \*tun? > tswinX > zhǔn “water level”

水 \*s.tur? > \*s.tuj? > sywijX > shuǐ “water; river”, pMín \*tšyi B” (p. 295, my emphasis)<sup>15</sup>

<sup>14</sup> Wáng Lì 1989, vol. 6, p. 297.

<sup>15</sup> Here B&S might perhaps usefully have considered – and perhaps at least explicitly dismissed – Göran Malmqvist 1962, pp. 107–120. (As we just saw, Karlgren reconstructs \*šjwər and Dǒng Tónghé 董同龢 reconstructs \*x̣jwed.) His arguments in that article have a bearing on my

What is needed here, to harmonise the rhymes, is an additional rule which B&S state as follows:

水 *shuǐ* sywǐjX < \*s.tur? (*dialect: \*-r > \*-j*) ‘water; river’” (p. 361, my emphasis)

The argument here is clear. But I nowhere find an extended and dated systematic series of examples that shows

- exactly in what context \*-r becomes \*-j,
- at exactly what time,
- in exactly what later dialect, spoken exactly when and exactly where, and thus relevant to the provenance of the crucial textual evidence from *Shījīng* times.

Certainly no such evidence is forthcoming on pp. 265ff. where these matters are under discussion.

Until I learn about such precisely dated relevant systematic sets of examples, I cannot regard the rule (*dialect: \*-r > \*-j*) and the above reconstruction for 水 as a falsifiable scientific hypothesis. There is no sufficiently detailed or compelling set of evidence to disagree on. There are only isolated suggestive examples. Examples or arguments that suggest otherwise are not given. I repeat: for all I know, there may indeed have been such a dialect. There may indeed also have been such a rule. Certainly! The problem is that we have no evidence about such dated and located general sound correspondences between dialects.

None of these conjectures on dialect features for which we need independent systematic sets of evidence can be explicitly refuted on the basis of explicit evidence to the contrary. But this is simply because there is so little relevant and early explicit evidence on pronunciation, and especially on details of dialect, as everyone sadly concurs. Conjectures on early dialect developments are manifestly immune to refutation. But sound scientific conjecture should live dangerously, actively risking counter-evidence.

The treatment of the rhyming in Ode 183 in Chinese tradition reveals some interesting attitudes towards such a wishful conjecture. Jiāng Yǒng 江永 (1982, p. 9) does acknowledge this as a rhyme, and Jiāng Yǒugào (1973, p. 57) seems to conjecture a reading *xǐ* 璽 for the offending character *sǔn* 隼 (if I understand this correctly: one hardly believes one’s eyes ...) – and he is followed in this conjecture by Chéng Jùnyīng 程俊英 and Jiǎng Jiànyuán 蔣見元 (1996, vol. 2, p. 527). Gù Yánwǔ 顧炎武 (1982, p. 121) puts things a little more transparently, agreeing that 水 *yǔ sǔn xié* 與隼協 “harmonises with 隼 ‘falcon’”, having just glossed 隼 as *zhī shuǐ fǎn* 之水反! Gù Yánwǔ thus constructed the kind of irrefutable “scientific hypothesis” that I would prefer to call a “wishful conjecture,” or when trying to be more up to date and less professionally profane: “an output-driven proposal.” The one and only argument for this kind of harmonising conjecture is that it would be nice for the rhyme if it were true.<sup>16</sup>

suggestion that Archaic *shàng* [sic] *shēng* 上聲 had progressive distribution of breath (crescendo volume) coupled to rising tone. This is also the main reason why Göran Malmqvist refuses to accept Pulleyblank’s (and Baxter’s) idea of the glottal stop as a marker of *shàngshēng*. B&S might also usefully have referred to the entirely relevant Sergey Yakhontov (1966, p. 15) on 水,

For a critical account of the important traditional concept of *xiéyīn* see Baxter (1992, pp. 150–153), but since then we also have the exhaustively detailed survey by Wāng Yèquán 汪業全, in his *Xiéyīn yánjiū* 叶音研究 (2009) which provides extremely useful statistics of *xiéyīn* assignments of pronunciations, and provides an extensive bibliography on the subject. It seems to me that the *xiéyīn* tradition still leaves its direct and indirect traces, even in the treatment of Ode 183 by modern scholars from Lù Zhìwéi 陸志韋 in his important *Shī yùnpǔ* 詩韻譜 (1948), to Wáng Xiǎn and his *Shījīng yùnpǔ* 詩經韻譜 (Wáng Xiǎn 2011). Wáng Xiǎn (2011, p. 354), thus “reconstructs” for 隼 a reading \*syéi which happens also to be his reconstruction for 水, and Lù Zhìwéi (1948, p. 67), stipulates a reading \*t’iwěd for 隼 which also happens to be his reconstruction for 水.

One will probably never know how, exactly, “irregular rhymes” were handled in ancient China. Not any more than we know about how Shakespeare intended his irregular rhymes to be handled.

For German I have found a splendid example of the explicit *xiéyīn* rhyming style:

In tütschen landen dapfer Iüt,

Die warheit redten alle tzyt,

Als du hast all dein tag gethon; (standard Middle High German and Early Modern German: gethân, getan, etc.)

Far hin, got geb dir ewig lon. (Brant 1508, *Bescheidenheit*, final comment)<sup>17</sup>

#### THE FOUR MIDDLE CHINESE FROGS AND THE SYSTEMATISING CHARACTER OF GUǎNGYÙN 廣韻

The “Appendix of reconstructed forms” (p. 364) invites us to believe that Middle Chinese has a rich homonymous vocabulary on frogs:

1. 電 wā ‘wae < \*q<sup>w</sup>re (MC -ae for -ea) “frog”: 55, 100, 127; see also wā < ‘wea, wā < hwae, wā < hwea
2. 電 wā ‘wea < \*q<sup>w</sup>re “frog”: 55, 100, 127; see also wā < ‘wae, wā < hwae, wā < hwea

<sup>16</sup> For a synopsis of rhyming tables for the *Shījīng* by seven authors, including Duàn Yùcái 段玉裁, Kǒng Guǎngsēn 孔廣森 and Wáng Niànsūn 王念孫, see Xià Xīn 1966, which is missing both in the *Handbook* and in the *New Reconstruction*.

<sup>17</sup> No sensible hypothesis concerning the pronunciation of *gethân* or of *lon* will, of course, be drawn on the basis of this rhyming practice. Although I will confess that I found exactly the same rhyme on the first page of *Johann Fischart’s Flöbhatz, Weibertratz, Ehezuchtbüchlein, Podagrammisch Trostbüchlein sammt zehen kleineren Schriften; Thomas Murner’s Vom Lutherischen Narren, Kirchendieb- und Ketzerkalender ...*, ed. J. Scheible (Stuttgart 1848). Johan Fischart (1546–1590), was the famous German translator of Rabelais.

3. 鼃 *wā hwae* < \**m-q*<sup>wf</sup>*re* (MC -ae for -ea) “frog”: 55, 100, 127; see also *wā* < ‘*wae*, *wā* < ‘*wea*, *wā* < *hwae*
4. 鼃 *wā hwae* < \**m-q*<sup>wf</sup>*re* “frog”: 55, 100, 127; see also *wā* < ‘*wae*, *wā* < ‘*wea*, *wā* < *hwae*

There appear to be four Middle Chinese words with identical meanings, but of course there actually are only two Old Chinese words, since 1 and 2, as well as 3 and 4, have identical Old Chinese reconstructions. For Old Chinese there are therefore only two words to discuss. For Middle Chinese the notion of *fāngyīn* 方音 (local differing pronunciations of the same etymon) might have come in useful here. These four Middle Chinese reconstructions look very much not like four words, but like four reconstructed *fāngyīn*. Unfortunately the concept of *fāngyīn* is nowhere found useful by B&S.

In *Guǎngyùn* 廣韻, there are two relevant characters (and words) discussed, one of which, 蛙, is disregarded in our *New Reconstruction*.<sup>18</sup>

Táng Zuòfān 唐作藩 (1982, p. 132), has both characters, but unfortunately he makes no distinction between them and treats them simply as allographs. It may very well turn out that Táng Zuòfān is ultimately right that there is but one word here, but according to *Guǎngyùn* he is certainly wrong. For in *Guǎngyùn*, 鼃 is defined as a subspecies of 蛙. On the other hand, we are told that the hypernym 蛙 is indeed sometimes pronounced like its hyponym 鼃.

We have, according to the (imperfectively) systematising *Guǎngyùn*:

*Wā*<sub>1</sub> 蛙, *wū guā qiè* 烏媯切.<sup>19</sup>

*Guǎngyùn* glosses this as *hámá shǔ yě* 蝦蟆屬也 “subspecies of the *hámá* (tailless amphibia [frogs and toads]).”

*Wā*<sub>1</sub> 蛙 has an alternative reading *wū guā qiè* 烏瓜切.<sup>20</sup>

So much for *wā*<sub>1</sub> 蛙 (which is nowhere referred to in the *New Reconstruction*). I turn now to *wā*<sub>2</sub> 鼃.

*Wā*<sub>2</sub> 鼃, *wū guā qiè* 烏媯切.<sup>21</sup>

According to *Guǎngyùn* it is a variant form of 蛙 (*tóng wā* 同蛙).

The semantic gloss in *Guǎngyùn* is *wā shǔ yě* 蛙屬也 “subspecies of *wā*.”<sup>22</sup>

<sup>18</sup> Incidentally, the other, 鼃, is disregarded in E.G. Pulleyblank’s, *Lexicon of Reconstructed Pronunciation* (Pulleyblank 1991). Such is the state of the art in our field.

<sup>19</sup> *Guǎngyùn*, p. 95.

<sup>20</sup> *Guǎngyùn*, p. 95, p. 171.

<sup>21</sup> *Guǎngyùn*, p. 96. Zàng Kèhé 2008, p. 2065 contains a slightly varied *fānqiè* 反切 spelling: a text referred to as *Míngyì* 名義 (copy of text taken to Japan in 804, first printed 1114, less systematising) reads: 鼃, 胡媯切.

<sup>22</sup> *Shuōwén* 說文 defines: 鼃, 蝦蟆也。圭聲。This Françoise Bottéro and I (Bottéro and Harbsmeier 2016) would read not as “means hama,” but as “is a (kind of) hama.” *Guǎngyùn* thus understands the semantics in *Shuōwén* exactly as we do.



It has the alternative reading *hù guā qiè* 戶媯切.

So we have four spellings – and some confusion about what to make of the interconnections between whatever words they are used for.

Bernhard Karlgren writes in his *Analytic Dictionary* (1923, p. 156): “Phonetically very curious are the cases 蛙, 蛙, 鼃.” Very curious indeed, I should say, since 圭 does not have a pronunciation like that of 鼃 at all. Karlgren is puzzled. In the spirit of B&S one could simply autobiographically report that “we reconstruct” a second pronunciation which gives something like modern *wā*, because such a pronunciation would explain a series of characters including 哇, 娃, 窪, 漥, 劫, 鞋, 蛙, 往, 佳, 街, 蛙, 崖 – and our 蛙 and 鼃.

Certainly it would be nice if there were such a pronunciation of the character 圭. The claim that there was such a pronunciation is unassailable, since absence of evidence is indeed no evidence for absence. The conjecture remains attractive and irrefutable. No amount of absence of evidence of such a pronunciation could ever count as evidence of such absence. The conjecture is – in this sense – immune against refutation. The *New Reconstruction* is full of such immune – but sometimes attractive – conjectures. My response is “Ja! Schön wär’s!” (“Yes! And nice it would be! If it were true”).<sup>23</sup>

In the matter of the frogs, I conclude that B&S never begin to address the philology of their four Middle Chinese frogs which are but the mechanical output of their methodological procedures. The rationalising distinction made in *Guǎngyùn* between the two kinds of amphibious creatures, hyponym and hypernym, they never even consider. The distinction in pronunciation between the two graphs they just list and never begin to discuss. They never get to discuss anything like the possibility that we may have not four different (more or less) synonymous words for frog, as their list would have us believe, but perhaps just one word mirrored through various *fāngyīn*. These *fāngyīn* made it into *Guǎngyùn* – and disappeared again to leave us in the end with – indeed – two homographs 鼃 and 蛙.

## AFFIXATION AND MORPHOLOGY IN OLD CHINESE

Gordon Downer, “Derivation by Tone-Change in Classical Chinese” (1959) distinguishes the following main semantic effects of derivation by tone change in Old Chinese:

- A. Basic form verbal, derived form nominal
- B. Basic form nominal, derived form verbal
- C. Derived form causative
- D. Derived form “effective”
- E. Derived form with restricted meaning

<sup>23</sup> This kind of case, where a homogeneous set of characters would suggest an unattested reading of the phonetic constituent, must be distinguished from the phenomenon of *fēi shēng* 非聲, where there is no such homogeneous set and no relevant similarity in pronunciation between the phonetic constituent and the complex character it is said to be the phonetic constituent in. This very important and wide-spread phenomenon of *fēi shēng*, being uncomfortable to generalising theory, is nowhere mentioned in the *New Reconstruction*.

- F. Derived form passive or neuter
- G. Derived form as adverb
- H. Derived form used in compounds

Downer adds (p. 271): “Many other uses of the *chiuh-sheng* derivation might be noticed.”

Downer (1959, pp. 269–270) presents a selection of 14 *qùshēng* derivations where he detects no change of meaning, and where I would diagnose *fāngyīn* variation. Since Downer’s article, modern technology enables us to work out the evidence of *Jīngdiǎn shìwén* 經典釋文 usage in much greater detail. Moreover, we are able to add to the *Jīngdiǎn shìwén* evidence a wealth of phonetic glosses from such sources as the digitised *Shísānjīng zhùshū* 十三經注疏.<sup>24</sup>

What such a more detailed survey of the -s suffix shows is the quite general preponderance of cases where that suffix is able to have a function F as well as the converse of the function F, as when it can change a noun into a verb and conversely a verb into a noun as – for example – in the functions A and B in Downer’s list.<sup>25</sup> In spite of its methodological and empirical shortcomings, Downer’s article on derivation by tone change represents one of the finest achievements of British Sinology through the centuries, and it is entirely relevant to B&S’s stunningly perfunctory attempt at an interpretation of that suffix.

Of the *qùshēng* readings without meaning change, only one case is registered by B&S: the word is simply listed under two pronunciations (p. 372), without any discussion.

囿 yòu hjuwH < \*[G]wək-s ‘park, garden’: 44; see also yòu < hjuwk

囿 yòu hjuwk < \*[G]wək ‘park, garden’: 230; see also yòu < hjuwH

In the case of 錫 / 賜 (p. 333),

賜 cì sjeH < \*s-lek-s ‘give’: 51

the alternative reading from *Jīngdiǎn shìwén*, carefully recorded by Downer, is disregarded.

The remaining important cases of common words with tone change without recognisable change of meaning (淡, 壽, 互, 尉, 搖, 閉, 迭, 匈) are simply disregarded by B&S.

<sup>24</sup> For some ten years we have been waiting for the publication of Zōng Fúbāng’s 宗福邦 phonological sequel to his incomparably useful *Gùxùn huìzuǎn* 詁訓匯纂 (2007), the *Yīnyùn huìzuǎn* 音韻匯纂. Six years ago, Professor Zōng assured me personally the phonological volume was almost ready for publication. And when this will be published it will radically improve the working conditions for those of us who like to relate our modern theoretical findings to traditional Chinese perceptions. But already in today’s *Gùxùn huìzuǎn* B&S should have surveyed traditional glosses on derived forms systematically before deciding on their own systematising renderings.

<sup>25</sup> For details on this particular case of V/N and N/V see Xiè Wéiwéi 2012, pp. 88–95.

Omitting the reference to Downer's article in the bibliography is merely one of Saint Thomas Aquinas's *peccata negligentiae*. But failing to take account of any of the subtle reflections in

Mei Tsu-lin (Mei Zǔlín) 梅祖麟, "Si shēng bié yì de shíjiān céngcì" 四聲別義的時間層次" (Mei Tsu-lin 2000),

Sūn Yùwén 孫玉文, *Hànyǔ biàndiào gòucí yánjiū* 漢語變調構詞研究 (Sūn Yùwén 2000),

Zhāng Zhōngtáng 張忠堂, *Hànyǔ biànsēng gòucí yánjiū* 漢語變聲構詞研究 (Zhāng Zhōngtáng 2012),

Xiè Wéiwéi 謝維維, *Hànyǔ yīnbiàn gòucí yánjiū* 漢語音變構詞研究 (Xiè Wéiwéi 2012).

is a great deal more serious.<sup>26</sup> For example, Mei Tsu-lin does attempt a detailed chronological study of derivation by tone change, and he also presents a spirited challenge to Downer's classical article which is based so predominantly on *Jīngdiǎn shìwén*. Mei Tsu-lin importantly engages in a critical examination of the reliability of the attribution of derivation by tone change to Old Chinese.<sup>27</sup> Sūn Yùwén has by far the most extensive bibliography of Chinese material on his subject and goes in exhaustive detail on the attestation of each and every of the 100 derivations he examines, thus facing the challenge by later skeptics like Gù Yánwǔ who suspected that many derivations were unattested for the earliest times and were but late formations by analogy to some early cases. Zhāng Zhōngtáng, with an important long preface by Sūn Yùwén, pays systematic attention to derivation by initial consonant change that is directly relevant to B&S's book.

Let me consider some examples of what are registered as words with the suffix -s in the *New Reconstruction*. How, for example, do we know that it is not part of the most uncertain root, as in the following:

二 èr nyijH < \*ni[j]-s "two": 110

Note that there are dozens of syntactic functions of this word, all with the "suffix" -s. What is marked by the -s suffix in the case of *sān/sàn* 三 remains unmarked in *èr* 二. This leads me to another important generalisation on the functions of the suffix -s: whatever meaning change is marked by the suffix -s is amply attested to occur unmarked by any suffix.<sup>28</sup>

<sup>26</sup> Wolfgang Behr draws my attention to some recent relevant works on derivation: Wáng Yuèting 2011 and 2014; Bì Qiānqí 2014.

<sup>27</sup> I note that on p. 309 of his detailed article Mei Tsu-lin makes favourable mention of the achievements of August Conrady 1896 and he naturally finds it important to report on Zhōu Fǎgāo 1972, pp. 9–96, which has a singularly useful index as well as a splendid history of the study of the phenomenon of derivation by tone change up to Zhōu Fǎgāo's own time.

<sup>28</sup> The first generalisation was the tendency for the suffix -s also to mean the converse of whatever it is demonstrated to mean that I have mentioned above.

There clearly was and very much remains a need to distinguish the suffix -s from the non-morphemic final consonant -s. In one example it is even suggested that the only certain thing is the suffix -s:

罽 jì kjeH < \*[k](r)[a][t][-s] “a kind of woolen fabric”: 196

I would suggest we have every reason to rewrite this in the spirit of B&S as follows:

\*[k](r)[a][t][-s]

And this is *not* a joke.

The use of brackets to indicate tentativeness of reconstruction becomes misleading when such brackets are not applied to many of the least reliable reconstructions, such as the vowel ə after reconstructed pre-initial written Cə-.

To sum up, there are several tendencies that need to be accounted for in the discussion of the suffix -s:

- The tendency for -s to mark meaning changes that are also attested to occur unmarked
- The tendency for -s to mark meaning changes in one direction as well as in the reverse direction, as when the -s is shown to mark a change from verbal to nominal but also conversely from nominal to verbal
- The absence of clear criteria for morphemic versus non-morphemic -s, as in the morphological -s in *sān* 三 “thrice” versus non-morphological -s *èr* 二 “twice”
- The fairly common presence of morphemic -s without apparent change of meaning as observed, but by no means exhaustively documented, by Downer

B&S declare the distinction between *wáng* 王 and *wàng* 王 to be one between a noun “king” and a verb “be king” (p. 59). *Zádrapa* 2011, whom they nowhere mention, would apparently agree with them. But the *Jīngdiǎn shìwén* glosses on 王 alone suffice to show that the distinction is not between nominal and verbal function, but between actor versus action.<sup>29</sup> The matter is of fundamental importance for understanding Old Chinese grammar. Let me labour my point a little by use of a few representative examples.

First of all, Lù Dēmíng 陸德明, the author of *Jīngdiǎn shìwén*, unlike B&S, noticed eleven contexts where commentaries differ on the question whether 王 should be read in the level tone or in the falling tone.<sup>30</sup> For anyone seriously

<sup>29</sup> Compare the truly memorable title by Émile Benveniste, *Noms d'agent et noms d'action en indo-européen* (1948).

<sup>30</sup> It is evident to the careful reader of *Jīngdiǎn shìwén* that Lù Dēmíng was already something of a systematiser himself. Mei Tsu-lin seems to have noticed this and justly drew attention to the importance of going beyond the *Jīngdiǎn shìwén* to those glosses that were unaffected by Lù Dēmíng's enthusiasm for derivation by tone change. In later times, the enthusiasm for derivation by falling tone became so dominant and produced so many spurious late falling tone derivatives that Gù Yǎnwū developed an exaggerated skepticism towards the whole phenomenon of derivation by tone change. *Sūn Yùwén* 2000 comes to a particularly painstaking philological rescue of our

interested in derivation by tone change, these cases must be of prime importance for the precise delimitation of the phenomenon. But the problem of this linguistically most revealing oscillation in the sources are nowhere mentioned in the *New Reconstruction*. Here is a particularly revealing (but somewhat late) case of ambiguity. It proves a point central to our argument:

非天私商而王之

于況反，或如字。(尚書 • 咸有一德):<sup>31</sup>

Clearly, adherents of the level tone reading took *wáng* 王 to function as a transitive verb “treat as a king” marked out as such by an object-pronoun object. (Some might regard this as still a noun, but used as a verb, but that does not need to concern us here. We are concerned with grammatical function in context.)

Another of these cases shows that, as a grammatical matter of course, on the so-called “nominal” reading *wáng* 王, the word is taken as transitive and verbal even without any pronominalised object:

今又王齊王: “Yet you treat the king of Qí like a king.”<sup>32</sup>

今可以王齊王而壽黔首之命 “Now, if by treating the king of Qí as a king, I could prolong the lives of the black-headed people ...”<sup>33</sup>

I knew Chén Qíyóu personally and I imagine he would not be one to forget in his edition to note if anyone respectable had proposed a falling tone for these two cases of 王. But there must remain an element of doubt whether the ancient reading really was *rú zì* 如字. However, *yòu* 又 “on the other hand (?)” and *kěyǐ* 可以 “can” are clearly followed by a verb, which in this case is apparently the so-called “nominal” *wáng* 王 (king).

Here are a few instances of *wàng* 王 being used nominally, from the texts glossed explicitly as having the *qùshēng* reading in *Jīngdiǎn shùwén*:

是故至孝近乎王: “He who is perfectly filial approximates to *be king*.” (禮記 • 祭義)<sup>34</sup>

Here, *wàng* is the nominal object of the transitive verb *jìn* 近. It is *nominalised*, and that nominalisation does not trigger a loss of the verbalising suffix. At the same time, some would say it is *zhūdòng*, *míngwùhuà* 主動, 名物化 (an active verb, nominalised). Yes: it derives from an action word. But nominal it is, in function. Just as in “a good read” the word “read” derives from a verb, and “read” is undoubtedly basically not a noun but a verb. But “read” is no less nominal for that.

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precious -s suffix. So have many others. However, none of these rescue operations release us from the duty to determine – for each and every case – whether it might or might not be a late product of a movement that may be usefully compared to the German “Rettet den Genitiv!”

<sup>31</sup> *Shísānjīng zhùshū*, vol. 2, p. 258.

<sup>32</sup> *Lǚshì chūnqíú* 21.5, p. 1474.

<sup>33</sup> *Ibid.*, see also *ibid.*, 13.7, p. 728 for an exactly similar case.

<sup>34</sup> *Shísānjīng zhùshū*, vol. 15, p. 1540: 乎王, 于況反; my emphasis.

義與信，和與仁，霸王之器也（禮記·經解）<sup>35</sup>

霸王之器也<sup>36</sup>

Here *bàwáng* 霸王 modifies *qì* 器. And the old commentator glossing this in the falling tone clearly read this as “an instrument for ruling as a despot or king,” not just as the perhaps more tempting “an instrument of a despot or king.” The issue here is not whether they were right or wrong. The point is that the *qùshēng* derivation was taken to create not a verb but a noun.

The collocation *bàwáng* 霸王 is important for us because it illustrates a crucial issue that is avoided in the *New Reconstruction*: The word *bà* 霸, reconstructed as \*p<sup>h</sup>rak-s, is credited with an -s suffix even before it is verbalised. This illustrates that the distinction between non-morphemic and morphemic final -s is essential. Precisely the change in meaning that is marked by -s in *wàng* 王 is unmarked in *bà* 霸 “rule as a tyrant.” I am ready to demonstrate that this pattern is general: whatever change of meaning that is marked by -s can also be shown to occur without such marking. You will be relieved to know that I am not proposing to enter into this demonstration now, for the meaning changes marked by -s are many, and widely varied.

成周之王功，毛詩·豳風·狼跋·鄭箋<sup>37</sup>

繼文王之王業<sup>38</sup>

成此王功，毛詩·周頌·昊天有成命·鄭箋<sup>39</sup>

內秉王心，尚書·西伯戡秦<sup>40</sup>

四王之王也: “As for the Four Kings being/becoming kings > when the Four Kings were/became kings, ...”<sup>41</sup>

Note that *zhī* 之 is a nominaliser here. What it nominalises here is *wàng* 王.

Of course, all “nominal” *wàng* 王 must be regarded as nominalised and be explained in terms of an embedded verb *wàng* unmarked for nominalisation. As I have shown in Harbsmeier 1983–1985, the same is true for *wáng* (king) which can be derived from an underlying classificatory verb as in *wáng yě* 王也 (is a king) or in *wáng zhě* 王者 (he who is a [true] king) and *wáng yě zhě* 王也者 (as for a king) along the lines of McCawley 1970 and Bach 1968. Nouns and verbs

<sup>35</sup> *Shísānjīng zhùshū*, vol. 15, p. 1600: 王，徐于況反。For the reference of 徐 see Luó Chángpéi 1984, pp. 28–34. The reference is to the early glosses by Xú Miáo 徐邈 (171–249) and not to those of the much later Xú Yuán 徐爰 (394–475).

<sup>36</sup> *Zuǒzhuàn* Duke Mǐn 1, *Shísānjīng zhùshū*, vol. 17, p. 347: 王，於況反。

<sup>37</sup> *Shísānjīng zhùshū*, vol. 5, p. 626: 王功，于況反。

<sup>38</sup> 毛詩·大雅·下武·鄭箋, *Shísānjīng zhùshū*, vol. 6, p. 1228 王業，于況反。See also *Shiji* 史記, 張儀列傳, where 王業 is similarly glossed.

<sup>39</sup> *Shísānjīng zhùshū*, vol. 6, p. 1524: 王功，于況反。

<sup>40</sup> *Shísānjīng zhùshū*, vol. 2, p. 307: 王心，于況反。

<sup>41</sup> *Zuǒzhuàn*, Duke Chéng 2, *Shísānjīng zhùshū*, vol. 17, p. 803: 之王，于況反。

are both predicates. So are proper names. Logical analysis leaves no doubt on that. But they are very different kinds of predicates. This is what one needs to sort out conceptually.

Each single one of the assignments of derivation by tone change needs a critical review of the date of its sources on the one hand, and of the philological detail of its interpretation on the other. In both these tasks Mei Tsu-lin 2000 and especially Sūn Yùwén 2000 would need to be consulted for the wealth of data they present and analyse in a well-argued way.

Affixation by -s, according to B&S (p. 346), makes unaffixed *jìn* 近 (near) into a transitive “be near to (vt)”:

近 *jìn* gj+nH < \*s-N-kərʔ-s “be near to (v.t.)”: 54, 118–119, 142, 387 n42; see also *jìn* < gj+nX

近 *jìn* gj+nX < \*N-kərʔ “near”: 118; see also *jìn* < gj+nH

However, for example in the common idiom *jìn zhě* 近者 (those who are close to [one]) *jìn* is unaffixed and semantically transitive. The gloss “near” is not a syntactic analysis but a (pretty poor!) translation for a verb. It is surprisingly difficult to be “near” without thereby being near *to something*. Nearness is conceptually transitive even when its object is understood. To say that the -s makes *jìn* transitive is to misunderstand the concept of “transitivity” – as if this were a simple matter of the presence or absence of an object. But in “John is easy to please” the “please” is as transitive as in “John is eager to please.” The transitivity of “please” does not depend on the presence of an overt object after “please.” And note that in “John is eager to please” John is not eager to please just anyone. Pleasing himself would be irrelevant. The understood object is “others.” Similarly, what is marked by -s is not transitivity but the explicit presence of an object after *jìn*.

One might have suspected that what is marked by the -s (or *qùshēng*) is not transitivity as such, but agency. Then all non-agential transitive uses should remain unmarked. But this does not turn out to be the case. The old sound gloss recorded for the following non-agential use has the derived form:

邊伯之宮近於王宮: “The palace of Biān Bó is close to the Royal Palace.”<sup>42</sup>

Cases like this settle the case against the idea that -s marks agency.

Having commented on *jìn* 近 in several places, one might have thought that the case of *yuǎn* 遠 would also be analysed by B&S to confirm the pattern of the case made for *jìn*. After all, *yuǎn* is glossed phonetically exactly 140 times in *Jīngdiǎn shìwén*, if I am not mistaken, but the *qùshēng* form – duly noted, of course, in Downer 1959 (as no. C 22) – does not make it into the *New Reconstruction*. Incidentally, Downer takes the derived form as specifically causative, neglecting putative (*bù yuǎn qiān lǐ* 不遠千里 is not causative!) and other uses. Even Homer nods. But the point that matters to me here is that the semantics of each and every case of *qùshēng* derivation has to be studied in detail. What is needed are careful semantic

<sup>42</sup> Zuǒzhuan, Duke Zhuāng 19, *Shísānjīng zhùshū*, vol. 16, p. 300: 附近之近.

and syntactic analyses. Anything less is not helpful but deeply misleading in the context of the reconstruction of Old Chinese morphology.

Apart from the suffix *-s*, the semantics of the affixes discussed in the *New Reconstruction* are not sufficiently well-defined or well documented on the basis of a sufficiently wide range of compelling ancient Chinese primary evidence and clearly relevant ancient phonetic glosses for me to want to discuss them here.

#### CHRONOLOGY OF DOCUMENTATION AND RECONSTRUCTION IN TERMS OF PROTO-SINITIC INFLUENCE

It is one thing to try to reconstruct affixation for OC texts so as to bring Chinese in line with affix-prone surrounding languages with which Chinese is probably related. It is quite another thing to show that this reconstructed affixation was a trace of more abundant affixation at earlier stages of the language. As far as I can remember, at this late stage of my life, leading linguists have long taken it for granted that this is what happened, notably in Bernhard Karlgren's famous essay on "Le proto-chinois, langue flexionnelle" (1920). In my generation it was clear that the more affixes one diagnosed the more successful one could hope to be taken to be as a student of the language.<sup>43</sup> Such is the state of the art. And the *New Reconstruction* marks a giant step forward in this direction.

By far the strongest evidence we have for affixation in OC is for the suffix *-s*. A question arises about the nature of this suffix (as about all other suffixes). Was it, as Downer certainly claimed, a receding trace of an earlier stage of OC than that from ca. 450 BC onwards, so that there would be expected to be an abundance of varied derived readings in the oracle bone texts from the 13th century onwards, becoming less at the time of the *Shījīng* and the parts of the *Shūjīng* datable to before ca. 450 BC, and declining further in the literature we have from ca. 450 BC onwards? Or was it rather the inverse, that there is a striking growth in the use of characters that would tend to illustrate what was determined in later phonetic glosses to be the result of processes of affixation, with very little of it in oracle bone inscriptions and the old *Shūjīng*, and statistically incomparably more per bamboo strip in Later Warring States literature?

Redouane Djamouri is the only one to have engaged in a painstaking investigation of the philological evidence on this question of the historical statistics of affixation in Old Chinese. In his quiet way he has tried to demonstrate that the clear evidence of affixation in the language of oracle bone and bronze inscriptions is meagre indeed,<sup>44</sup> that affixation in the *Shījīng* and the old *Shūjīng* is strikingly limited, even when one takes account of the limitations of corpus size. In his 2008 presentation he concludes:

<sup>43</sup> Apart from the *New Reconstruction* project, we have notably Maspero 1930, Benedict 1972, Pulleyblank 1973 and 1991, Mei Tsu-lin 2000, Sagart 1999, LaPolla 2003, and Schuessler 2007. More could be added. But it seems significant there could be no proliferation of Chinese authors in the list.

<sup>44</sup> Consider also Ken-ichi Takashima's thoughtful conclusion concerning the interpretation of oracle bone inscriptions: "Shāng Chinese, when viewed from larger perspectives, is still in a rudimentary stage of decipherment" (Takashima 2015).



In the light of this historical evidence, it is reasonable to conclude that the Shang and early Zhou documents do not provide us with data allowing us to claim that derivational morphology was already attested in the language of that period.

As I have shown, the semantic alternations cited as evidence for the derivational morphology reconstructed for OC are in fact clearly attested only from the Springs and Autumns period on (and this mainly in transmitted documents).

In its early development, the Chinese writing system shows the equivalence: one character = one morpheme. It is only with the later emergence of the affixed forms that the level of transposition became to a certain extent polymorphemic (one character encoding more than one morpheme).<sup>45</sup>

None of this, of course, proves that such affixation was somehow implicit in ways that make it impossible to document, but nonetheless present. I am reminded here again that absence of evidence is not evidence of absence: “Just because one cannot see the morphology in oracle bone inscriptions does not mean it was not there.” I quite agree. But, somehow, I feel on safer ground when discussing the morphology of a fairly extensively attested language, when I can actually see some of the expected evidence for it, and do not have to only imagine it. To generalise on footnote 1 in Bloomfield 1925: “The usual processes of linguistic analysis are not suspended on any continent.”

To be concrete: a great deal of further research is needed to make Djamouri’s conclusion stick. We would like to know, for example, whether action-王 and action-君, agential/transitive 近 and agential/putative 遠, nominalised 難, and the many other examples of this sort abound in oracle bone inscriptions and bronze inscriptions, become less in the *Shījīng* and the old *Shūjīng*, and are more sparsely evidenced in the Later Warring States literature. We also need to pay attention to affixation where the written forms involve different characters, as for example in 田 (field) versus 田 / 佃 (hunt), 威 (authority) and 畏 (stand in awe of) and many, many others. To take one example of many possible ones, if there is much 畏 (stand in awe of; fear) contrasting with *wēi* 威 in oracle bone inscriptions, bronze inscriptions and the *Shījīng*, that is quite as much to the point as an occurrence of 王 *wàng* (rule as king) with regard to our question of affixation.

What we need to test is not only the quantitative historical development of the whole gamut of the well-attested suffixation with -s. But this is a very good starting point. The suffix -s is by far the most well-attested one in OC phonology. It is certainly not a matter of modern analytical conjecture only. From the emergence of phonological glossing in China in Eastern Hàn times onwards, *qùshēng* derivation has been commented upon in some detail. In all other affixation we need to address the question why, if it was so fundamental, none of the very sharp traditional Chinese philologists right down to the 20th century, ever got wind of it, and why even in this beginning 21st century so many highly clued-up non-Westernised linguists still will have none (or very little) of it.

<sup>45</sup> Djamouri 2008, p. 13.

In the *New Reconstruction*, none of such philological fieldwork seems even conceived of as relevant. But the matter is potentially of prime typological importance. As Djamouri says:

If we assume that the language of the Shang inscriptions belongs to the Sinitic branch and as such belongs to the ST [i.e., Sino-Tibetan] family, we are led to the conclusion that the loss of the PST [Proto-Sino-Tibetan] derivational morphology had already occurred in the Shang language.<sup>46</sup>

I believe this matter merits further philological attention. Djamouri's thesis is not a matter of irrefutable and unconfirmable conjecture. Every one of the affixes proposed by B&S, insofar as they are sufficiently precisely defined to be testable on the basis of persuasive evidence, should be tested against their attestation in Shàng and Early Zhōu excavated and transmitted literature. And as Sir Karl Popper should have put it (if he has not): one has made a useful scientific conjecture to the extent one has specified the precise conditions of its conceivable refutation.

A philological conjecture ceases to be indulgent wishful thinking to the extent it is seen to live dangerously, risking neat refutation by compelling evidence; and the supporting evidence for one's conjectures is valid only to the extent it is accompanied by especially detailed attention to the kind of evidence one wishes one had never seen, as Angus Graham has put it to me, quite unforgettably.<sup>47</sup>

Here is an example: When claiming that \*ŋ<sup>s</sup>aj? 我 said to derive from \*ŋ<sup>s</sup>a 吾 by suffixation it is entirely in the spirit of Graham to consider carefully two uncomfortable considerations:

There are those who would suggest that while “\*m<sup>s</sup>”, though not attested in any Asian language, does seem to be pronounced in Ubykh, the constellation “\*ŋ<sup>s</sup>” might appear to some of my learned linguist colleagues to need surgery in the articulatory organs to become pronounceable, and is in any case not attested anywhere else.

More importantly, as Sagart 1999<sup>48</sup> recognises, *wú* 吾 (I, my; we, our; one, French *on*), is unattested for many hundreds of years during which *wǒ* 我 (we) was already a common or garden word on bones and bronzes, happily coexisting with the equally common but semantically contrasting *yú* 余 (I). This may not make the decisive difference, but it needs to be discussed. On bones and bronzes *wǒ* 我 does not contrast with the much later and always unemphatic, non-contrastive and grammatically restricted *wú* 吾 (I) which remains absent not only in the old *Shūjīng* but also in the old *Shījīng*.<sup>49</sup>

<sup>46</sup> Djamouri 2008, p. 13.

<sup>47</sup> Private communication, ca. 1969.

<sup>48</sup> Sagart 1999, p. 135, footnote 2, finds it important for his argument that *wú* 吾 is “basically a singular form.” Unfortunately, I am not sure what it is to be “basically a singular form” in Chinese, but not so much unlike standard classical Chinese *wǒ* 我 (I, my; we our; one, one's) and in contrast with *yú* 余 (I, my) which is the standard first person pronoun commonly used by rulers for themselves in oracle bone inscriptions, *wú* 吾 is quite commonly used to mean (we; our) as well as (one, one's). Thus, *pace* Sagart 1999, p. 144, footnote 2, *yú* 余 is attested much earlier than *wú* 吾.

<sup>49</sup> The one occurrence of the graph 吾 in the old *Shūjīng* is explained by Qián Zōngwǔ 錢宗武 (2004, p. 4), as follows: “吾”是後人改竄，古本作“魚。”“吾 is a rewriting by later men (scribes). The old version had 魚.” For further discussion see Sagart 1999, *passim*.

Given all this: why not indeed call \*ŋ<sup>ʰ</sup>a 吾 a late reduced form of \*ŋ<sup>ʰ</sup>aj? 我? Maybe this is not the right answer.<sup>50</sup> But the failure to bring up the problem of the chronological dissonance all is profoundly disturbing.

First person pronouns are central parts of a language. The matter of *wǒ* 我 and *wú* 吾 illustrates nicely the profound change of the Chinese language during the first half of the first millennium BC. Any purely structural interpretation of the relation between the two words that does not take into account the chronology of their emergence in the language is seriously deficient. One can still claim, as in the case of morphology, that though one does not see it in our sources, *wú* 吾 must really have been present – orally, but unwritten. This would be irrefutable, but – shall we say – insufficiently supported by observed evidence.

### KARLGREN’S “PHONOLOGICAL RECONSTRUCTION”

There are those of us who imagine that the distinction between phonology and phonetics is basic and absolutely crucial. B&S are strident in their criticism of Bernhard Karlgren: “But the inadequacies of his phonological reconstruction made it difficult to identify the patterns involved” (p. 4). We are here informed that Karlgren produced a phonological reconstruction of Old Chinese, and that this phonological reconstruction was inadequate. Well, I am sorry to say: Bernhard Karlgren never showed any interest in phonology. As my friend Göran Malmqvist put it over some white wine: “Karlgren was intellectually allergic to phonology.”

B&S might have noticed Karlgren’s title “A Compendium of Chinese *Phonetics* in Ancient and Archaic Chinese” [my emphasis], which they do mention. They might have paid attention to pp. 336–337 of this work:

In short the “phonemic” language description is often one-sided and oversimplifying. It is my conviction that it will soon have seen its best days and that new currents will dominate in linguistics which do more justice to the infinite richness of every living language.

As Henry Henne 1955 took care to point out in his brief review of Samuel Martin pioneering work on Old Chinese phonology, *The Phonemes of Ancient Chinese* (Martin 1954):

Bernhard Karlgren’s *Études sur la phonologie chinoise* (3 vols, 1915–19) is a landmark in sinological linguistics. To students of the history of the Chinese language as well as to students of modern dialects it is a *sine qua non*. As to Karlgren’s use of the word “phonology,” it should be noted that the term must be taken in its “pre-Troubetzkoyan” sense. Accordingly Karlgren is primarily concerned with a detailed description of Ancient Chinese phonetics.<sup>51</sup>

Søren Egerod 1955 states very clearly in no less a journal than *Language*:

<sup>50</sup> On this question, see Kennedy 1956.

<sup>51</sup> Henne 1955, p. 119.

Most of the decisive reconstruction work was done in pre-phonemic days, the chief results being embodied in Bernhard Karlgren's *Études sur la phonologie chinoise* (1915–26), and Henri Maspero's *Le dialecte de Tch'ang-ngan sous les Tang*, *BEFEO* 1920. [...] The first phonemic discussion of the phonetic values previously arrived at was undertaken by Y.R. Chao in his "Distinctions within Ancient Chinese," *HJAS* 5.203–33 (1941). It is of paramount importance, for the understanding of this whole problem, to make clear the exact nature of the reconstructed transcriptions. [...] On this level of investigation all of the entities represent so many x's and y's, since our information has hitherto been derived from Chinese characters, not from phonetic symbols. Only the *modern dialects* will provide material for reconstructing their actual value. Before attempting to supply any phonetic values we can work out a complete system of formulary transcription, in which each entity is represented by a number or an arbitrary letter. By replacing the formulae with reconstructed sound values we arrive at a phonetic transcription."<sup>52</sup>

Y.R. Chao's disciple Søren Egerod himself did engage in phonological analysis in his important doctoral thesis on Min dialect (*Egerod* 1956, not mentioned in the *New Reconstruction*). Karlgren refused to examine that dissertation, declaring himself incompetent to discuss the "phonemes" therein mentioned. B&S are, of course, leading experts in matters of phonology. But in this book they write as if they were quite uninformed about the basic issues in the history of their own discipline.<sup>53</sup> Let it be repeated, then, that book titles notwithstanding Prince Nikolai Trubetzkoi (1890–1938) came too late for Karlgren's creative phase.

Y.R. Chao's easily most important article on "The Non-Uniqueness of Phonemic Solutions of Phonetic Systems" from 1934 states the crucial generalisation on philology with exemplary clarity:

In reading current discussions on the transcription of sounds by phonemes, one gets the impression of a tacit assumption that given the sounds of one language, there will be one and only one way of reducing them to a system of phonemes which represent the sound system correctly. Since different writers do not in fact agree in the phonemic treatment of the same language, there arise then frequent controversies over the "correctness" or "incorrectness" in the use of phonemes.

The main purpose of the present paper is to show that given the sounds of a language, there are usually more than one possible way of reducing them to a system of phonemes, and that these different systems or solutions are not simply correct or incorrect, but may be regarded only as being good or bad for various purposes.<sup>54</sup>

<sup>52</sup> *Egerod* 1955, p. 470.

<sup>53</sup> *Zhōu Fǎgāo* 1972, p. 281 reports in some detail how Dǒng Tóng hé 董同龢, like Karlgren, was violently opposed to phonemic analysis.

<sup>54</sup> *Chao* 1958, p. 38. Sanford Schane, in his much later much contested article "On the Non-Uniqueness of Phonological Representations" (*Schane* 1968) duly acknowledges his debt to Y.R. Chao's impressive much earlier arguments concerning specifically the case of the reconstruction of Chinese in a general context of historical linguistics.

Y.R. Chao says it all very clearly, but this non-uniqueness of phonological solutions has an interesting parallel in W.V.O. Quine’s “indeterminacy of translation” – however, neither Quine’s “indeterminacy” nor Chao’s “non-uniqueness” encourage or even condone any abandoning of the important tasks of phonological and semantic analysis.

It has been pointed out to me that in dwelling on these bygone classics I indulge in “ritual ancestor worship.” I am unrepentant. Chao 1958 and Egerod 1970 had important and substantial points to make. It seems to me that B&S refuse to consider seriously enough the non-uniqueness of their solutions to the problems they undertake to solve in their book.

Attributing phonology to Karlgren is *not* a matter of sloppiness of terminology. It manifests a deep conceptual confusion that persists and profoundly affects also the subject to which I must now turn: the distinction between reconstructing and transcribing Middle Chinese.

### “TRANSCRIPTION” VERSUS “RECONSTRUCTION”

Karlgren thought that the pronunciation of Middle Chinese was something that needed to be reconstructed in a painfully long and controversial process, on the basis of a range of historically diverse and not even contemporary sources. Pān Wùyún and Zhāng Hóngmíng’s 張洪明 recent account of *Qièyùn* 切韻 is also in terms of reconstruction, not transcription:

According to the systematical analysis on the *fanqie* and the principles for classifying rhymes in *Qieyun*, depicted previously, the finals of *Qieyun* can be reconstructed as shown in Table 6.4.<sup>55</sup>

B&S repeatedly insist that their version of Middle Chinese phonology is a transcription. This is not just untrue, it is also quite plainly inconsistent with B&S’s own explicit account of their procedures, as we shall see below.

Søren Egerod, speaking of the reconstructed elements of Middle Chinese puts it bluntly: “Only the modern dialects will provide material for reconstructing their actual value.”<sup>56</sup>

B&S should take up this challenge to their claim that they are transcribing and not reconstructing Middle Chinese. This might clarify the very things they are doing. For a lingering doubt remains that what they mean by “transcribing” is not really what the rest of us understand by that word.

The name Shǒuwēn 守溫 – if any such person ever existed – did not in any case become famous in Late Tang and early Five Dynasty times for just transcribing 36 initials (he turns out to have been wrong about these anyway). He became famous for sorting them out from very messy earlier primary sources, and then for arranging them “alphabetically,” in the spirit of Sanskrit phonology and in the Sanskrit “alphabetic” order, so that from their arrangements certain conclusions about the phonetic values of the elements in the schema can be drawn.<sup>57</sup>

<sup>55</sup> Pān Wùyún – Zhāng Hóngmíng 2015, p. 87.

<sup>56</sup> Egerod 1955, p. 471.

It is agreed that even Shakespeare's pronunciation needed to be reconstructed and the very substantial evidence there is for performing this reconstruction has been found radically insufficient for the task, if we are to believe the classic work in the field by the Swedish scholar from the Uppsala school of philology, Helge Kökeritz, *Shakespeare's Pronunciation* (1953): "No magic formula exists by means of which we can single out the eye rhymes in Shakespeare."<sup>58</sup> The splendidly versatile David Crystal wisely admits that some arbitrary decisions have to be made if one wishes to try to perform Shakespeare in his own reconstructed language. Describing the pronunciation of Shakespeare is *not* a mere matter of transcription; it is a matter of highly problematic reconstruction. *Et simulariter de Seribus*.

Following Y.R. Chao, B&S say they neglect phonetic distinctions which are differentiated as phonetic distinctions in their sources, but which are in complementary distribution and therefore phonologically irrelevant. In so doing they are exactly NOT "transcribing" any Chinese symbols. Which symbols, anyway, could they have been transcribing for the ubiquitous initial glottal stop in syllables that might seem to begin with an open vowel, for example?

B&S's "transcription" of Middle Chinese presents itself as a notational variant of the reconstruction in Baxter 1992. In this book we read the following:

Moreover, words with Karlgren's *-e-* and words with Karlgren's *-ä-* appear to rhyme freely with each other in poetry of the Middle Chinese period. The following pair illustrates these vowels as reconstructed by Karlgren:

(16) 先 *xiān* 'first', Karlgren's *sien* (my *sen*)

(17) 仙 *xiān* 'an immortal', Karlgren's *sjän* (my *sjen*).

All evidence appears to indicate that these two words actually had the same main vowel in Early Middle Chinese, and differed only in the preceding medial; accordingly, I write them *sen* and *sjen*, respectively.<sup>59</sup>

Here, as in so many other places, Baxter explicitly replaces Karlgren's phonetic reconstruction with an attempt at phonological reconstruction. He openly demonstrates that what he is doing is refusing to transcribe what Lù Fǎyán 陸法言 (*fl.* 581–617) registered in the spirit of the famous dictum from the introduction to his dictionary, preserved in *Guǎngyùn*:

*póu xī háo lí, fēn bié shǔ lèi* 剖析毫釐，分別黍累。

<sup>57</sup> Paul Nagel notes carefully the Chinese as well as the European pre-history of his endeavour of reconstruction: "Auf europäischer Seite ist die Sprache des Ts'ieh-yün [切韻] nach Vorarbeiten von Kühnert, Volpicelli, Schaank und Pelliot nun vor einigen Jahren von Maspero und Karlgren rekonstruiert worden" (Nagel 1941, p. 103).

<sup>58</sup> Kökeritz 1970, p. 33. Compare also Dale Coye 2003. For a sumptuously documented history of the rhyme and related poetic devices in the Indo-European languages I warmly recommend Calvert Watkins 1995, especially pp. 5–96. Most of this discussion depends entirely on the kind of detailed knowledge of the sound patterns of these attested languages that B&S are trying to supply for Old Chinese.

<sup>59</sup> Baxter 1992, pp. 28–29.

“to split hairs and separate tiny grains”<sup>60</sup>

Thus using Lù Fǎyán’s principles one would transcribe a word like the Russian *zhit’* жить (to live) as *zbyt’* жьть, rhymes with *bit’* бить (to knock) not withstanding. Rhyme goes nowhere to prove phonetic identity or even closeness. It is clear that Lù Fǎyán and his highly clued-up friends knew this remarkably well. B&S often reconstruct throughout their book as if they did not.

According to Pān Wùyún and Zhāng Hóngmíng, in Middle Chinese we have one phoneme /a/ with three allophones a, ɤ, ɑ, one phoneme /e/ with three allophones ɛ, E, e, one phoneme /ə/ with two allophones ɨ, ə, one phoneme o with four allophones ɔ, o, ɤ, ɒ.<sup>61</sup> Lù Fǎyán and his friends, as I understand them, would have aimed to distinguish all such allophones, even when they may have noticed that these were never semantically distinctive (or in complementary distribution) and also did not seem to affect rhyming behaviour.

One may very well want to agree with B&S that one should postulate a glottal stop for syllables without any other initial consonant. But where is that Middle Chinese technical linguistic term which one is “transcribing” and which in the AD 601 or later Middle Chinese sources specify the glottal stop they postulate in all these syllables that might appear not to have an initial consonant?

Moreover, for the initials, the “transcription” of Middle Chinese is notoriously based on the Late Middle Chinese rhyme tables on which Baxter writes:

The stage of the language represented by the rhyme tables (Late Middle Chinese) differs somewhat from the language of the *Qìyùn*; but the rhyme tables, if carefully used, are still very useful in *reconstructing* Early Middle Chinese, and much of their terminology is applicable to the Early Middle Chinese stage.<sup>62</sup>

E.G. Pulleyblank concurs:

Though the rhyme table language and the *Qieyun* represent different dialects that are not in the same direct line, they go back to a common ancestor, and, in general, their phonological categories are compatible.<sup>63</sup>

But to call a “transcription” what is based on a conflation of primary sources that apparently concern even “different dialects that are not in the same line” and are merely compatible, is profoundly unsettling to my idea of reliable reconstruction of a language.

<sup>60</sup> *Guǎngyùn*, p. 16; cf. Luó Chángpéi 2008, vol. 7, pp. 59–61. By the way: it is awkward that the *New Reconstruction* hides away the reference to *Guǎngyùn* under the name of its modern editor, Yú Nǎiyǒng, where only insiders would look for it, and also that it does not refer the student to the excellent typeset edition edited by Cài Mèngqí 蔡夢麒 (2007) which provides an alphabetical index, as well as a reconstruction of the Middle Chinese pronunciation of each one of the 25,335 characters in the book. It would also be useful to know that the beautiful third printing (2011) of Zhōu Zǔmó’s edition from 1960 has some indices that none of the others have and concentrates on essential textual criticism (*Guǎngyùn jiàoběn*).

<sup>61</sup> Pān Wùyún – Zhāng Hóngmíng 2015, p. 88.

<sup>62</sup> Baxter 1992, p. 41, my emphasis.

<sup>63</sup> Pulleyblank 1984, p. xiv.

B&S claim that they “transcribe” the analysis of Middle Chinese that they find in sources as far apart as 601 and 1007 – and well beyond. Thus, the Middle Chinese they describe is itself the result of a convenient assumption that the *Qièyùn* and the *Guǎngyùn*, put together for good measure with the *Yùnjìng* 韻鏡 (first printed in 1161), taken together, can be used to reconstruct one unitary phonological system, that of Middle Chinese. Maybe the assumption is necessary, *faute de mieux*. But it remains an assumption, a working hypothesis.

In any case, if there is one thing all seem to be sure of, it is that Middle Chinese is a language that no one person or group of persons ever spoke as their mother tongue. The introduction to *Qièyùn*,<sup>64</sup> together with the relevant section of *Yánsì jiāxùn* 顏氏家訓, “Yīncí piān” 音辭篇, are enough to assure me of that. And it remains a philosophical as well as an historical mystery to me that a variety of Chinese that never existed in the form it was described should have been able to exercise such a remarkably fine-grained and pervasive control over the development of all modern dialects (except on the Mǐn dialects). None of my teachers has been able to dispel this profound mystery over the many decades. Nor have any of them been able to explain the historical process by which all the other varieties of Chinese spoken throughout the empire petered out without significant historical trace, giving way to a dialect that was never spoken by anyone.

#### DO LANGUAGES GROW ON TREES? ON THE METAPHOR OF THE PHYLOGENETIC TREE

The methodological importance of the phylogenetic metaphor comes out in an interesting diagram suggested as a possible alternative in [Handel 2010](#) (p. 3):

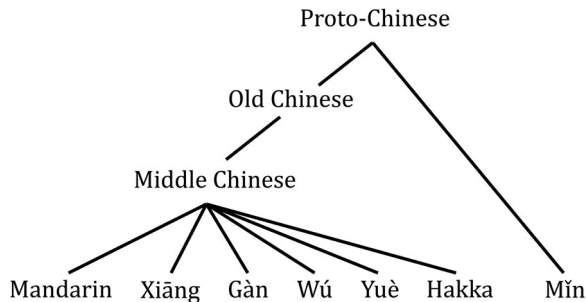


FIG 1. Ancestral Mǐn and Old Chinese as Sister Languages.

By attaching Mǐn as a daughter to Proto-Sinitic, the deviant phonology of Mǐn becomes quite irrelevant to the reconstruction of Old Chinese. What will be affected is Proto-Chinese only. And even Proto-Chinese would only be affected to the extent that Proto-Mǐn, reconstructed on the basis of 19th century observations may be taken to provide plausible evidence on a sociolinguistic configuration around the second or third millennium BC. This involves two assumptions: firstly, that the relevant features of Mǐn did not change significantly during those millennia, and

<sup>64</sup> [Bái Dízhōu 1931](#), p. 185: “*Guǎngyùn* 廣韻 has 3,875 syllables, *Jìyùn* 集韻 has 4,473, that is 598 more syllables!”



secondly, that these relevant features were not an unpredictable effect of language contact and bilingualism in multi-ethnic colonial Fújiàn.<sup>65</sup>

Now, if interaction with neighbouring languages, or other types of language contact (such as the arrival in the community of people speaking a different language), can have as much influence on what a language looks like as its historical roots (Colvin 2007, p. 2), then we get a very different tree with some messy structural features of great historical importance: the daughterless languages on the one hand and the horizontal interaction on the other:

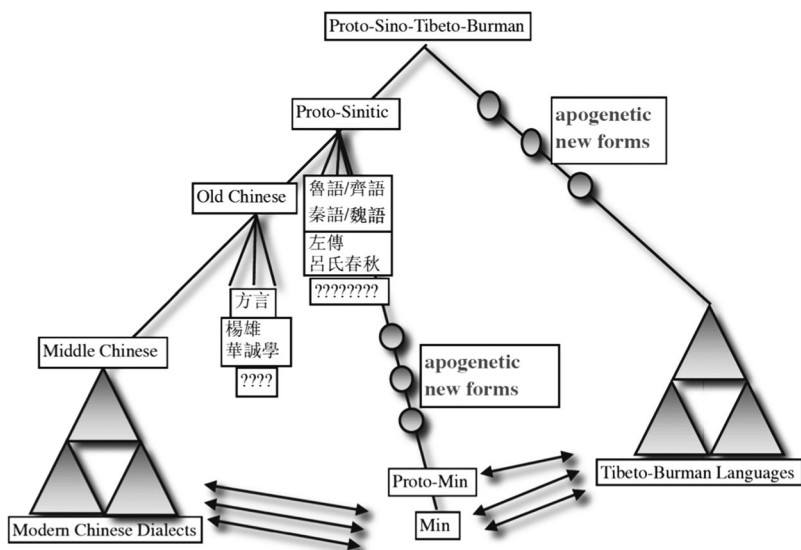


FIG 2. Enriched Experimental Diagram of the Ancestry of the Chinese Language. By the author.

Charles Darwin – a passionate searcher for and recorder of disconfirming evidence of his own theories if ever there was one<sup>66</sup> – came to make a distinction in his scheme of cladistics between what has come to be called the ordinary branching “cladogenetic” creation of new forms versus the uncomfortably problematic *non*-branching “apogenetic” new forms which he marked with dots on the derivation line (see Archibald 2014, figure 4.5, p. 133).

“Apogenetic” new forms (and I do wish I could find a better term!) mark clusterings of cladistic characters in an historical development one is tempted to discuss in terms of a punctuated but unilinear descent with modification. Darwin’s circles in his diagram draw the timelines within which new forms punctuate developments.

<sup>65</sup> For a useful account of the ethnic complexities and colonisation of Fújiàn Province see Bielenstein 1959, and for the more general ancient background the immensely readable Maspero 1925.

<sup>66</sup> The story goes that Darwin kept a notebook in which he forced himself to take written note of all apparently disconfirming evidence to his current theories. I have never been able to confirm this entirely plausible story, but in the *New Reconstruction* I certainly find very little of such a passion for the careful examination of countervailing arguments.

There is no denying that the metaphorical transferral of phylogenetic diagrams to the case of the Indo-European language family continues to be helpful as a simplification. Also, Handel's diagram (see Fig. 1) has helped me a great deal to understand the basic argumentative structures involved. But for the case of Chinese, as for other languages, this metaphorical transferral obfuscates a host of radical differences between phylogenetic life tree relations on the one hand and historical relations between languages on the other. We need to focus on a number of such problems, in particular:

### 1. The Non-unitary Nature of Languages

We have no reason to assume that there ever existed an “Old Chinese” that was a unitary sociolinguistic entity conforming pretty exactly to one and only one structural system: what is presupposed is a kind of linguistic essentialism. Sociolinguistic entities will normally contain and indeed consist of structurally distinct coexisting variants and dialects. As I understand biological species, these are not quite like such sociolinguistic entities. Elephants do differ from each other, all of them. But they qualify as elephants because they share exactly those features that define the species “elephant.” Elephanthood is not a variable constantly changing convention, but defined by a fairly stable set of genetic structural features which is fairly well-defined and describes a unitary set – as long as elephants survive in the evolutionary process, that is.

### 2. Horizontal Interaction through Dialect and Language Contact

Massive migration combined with wide-spread multilingualism and extensive language contact will have interfered with the regularity of phylogenetic developments straight across typological genealogical language groups.<sup>67</sup> The genetics of the elephant is not constantly affected by that of the lions or tigers in his vicinity. But that is exactly the kind of thing that is happening all the time with typologically distinct natural languages. From ancient times onwards, this is manifest from historical sources.

### 3. Continuous Deep Structural and Perhaps Even Typological Historical Change within One Given Language

One notes that “Old Chinese” as defined in the *New Reconstruction* explicitly includes the language of the early parts of the *Shūjīng* which differs from classical Old Chinese in fundamental ways.

There is no need to go into any detail at this point. The differences between what Max Uhle calls pre-classical Chinese in his thesis differs on core matters of grammar and of the lexicon from later forms of Chinese. They are not a matter of reconstruction or conjecture. What one finds are not minor dialect differences, but profound structural contrasts (whether these were or were not of a typological nature, I shall leave aside for the moment). There certainly cannot be one fine-grained grammar of the language of Hǎn Fēi and that of the old *Shūjīng*. Nor is there any reason to believe that there was anything like one fairly stable fine-grained

<sup>67</sup> See Egerod 1983.

phonological system these profoundly different historical varieties of Chinese had in common. Unlike elephants, lions and the like, languages undergo continuous structural interaction.

All this reduces the transparent helpfulness of the phylogenetic tree metaphor within the development of Old Chinese and Sino-Tibeto-Burman.

And if one looks at an enriched experimental diagram of the sort I have contrived above, some unsettling additional historical puzzles will naturally arise: If Middle Chinese was just a congeries of elements from a wide variety of local dialects at the time – as explicit paratexts of the time demonstrate it was<sup>68</sup> – how could it possibly become the only formative influence on all of the non-Mǐn dialects? Why can all non-Mǐn dialects today be derived only from that one construct of Middle Chinese, whereas all other dialects, which are in fact recorded in early dictionaries, had no effect on anything? This is a very real and concrete historical puzzle or paradox. It is not a technical question of professional phonological analysis.

One would like to be able to imagine the historical scenario by which one host of dialects simply fizzled out without affecting Middle Chinese in any way, and by which then that mixed construct “Middle Chinese,” which tries to describe a language which everyone agrees was never spoken by anyone at any time, had this overwhelming impact which made it the genetic historical source of all modern observed non-Mǐn dialects.

What we do find in Greek is the convergence of a rich variety of dialects to *koinē* Greek, which developed into Byzantine and the Modern Standard Greek. And this was understood even long before Carl Darling Buck’s *Greek Dialects* (Buck 1928). We do not find in the Greek case convergence of dialects then to be followed by another wave of radical divergence to the point of incomprehensibility. But that is what a broad consensus of Chinese historical linguists appears to want us to believe occurred in China.

Broad consensus among the professionals does, however, not by itself constitute an argument in favour of that consensus. What I am curious to know is not what kind of consensus there is between whom, but what the compelling historical and philological evidence is in favour of that consensus. The narrative we have so far does not seem plausible.

## PROTO-SINITIC

B&S write:

Furthermore, since the language of the earliest Chinese texts appears to be very close to the common ancestor of all attested varieties of Chinese, it is difficult at this stage to make a meaningful distinction between Old Chinese and Proto-Chinese (= Proto-Sinitic). As a practical matter, we use the term “Old Chinese” in the narrow sense to refer to the earliest stage of Chinese that we can reconstruct from Chinese evidence, and we consider evidence from any Sinitic language (including Chinese loanwords to other languages) to be relevant to its reconstruction.<sup>69</sup>

<sup>68</sup> See particularly Branner 2006.

<sup>69</sup> Baxter – Sagart 2014, p. 2. For pertinent discussion of the notion of a proto-language see Pulgram 1961.

The language of the *Shījīng*, which is what Karlgren was trying to reconstruct, is, of course, already fundamentally different from the earlier stages of the language as attested in the oracle bone and bronze inscriptions. B&S base their reconstructions, like their predecessors, on the rhymes attested in texts that date some six hundred years after the earliest recorded evidence on Old Chinese, oracle bone inscriptions. The stage of the language which they actually base their reconstruction on is very neatly distinguishable from that of the earliest attested stage of the language. Any historian of classical Chinese syntax is well aware of this.

B&S disregard the vast differences between the oracle bone inscription language and the bronze inscription language on the one hand and the language of the earliest *Shījīng* on the other, but even the differences between the early *Shūjīng* and the *Shījīng* are striking enough:

1. *zhě* 者 and *yě* 也 are among the most important and ubiquitous structural features of classical Chinese. Both are absent in the old *jīnwén* 今文 parts of the *Shūjīng*.
2. *Qí* 其 is the next most important structural particle in classical Chinese. Where we should expect *qí* 其在 the old *Shūjīng*, we often get *jué* 厥 (199 times), versus 其 (202 times). *Shījī* replaces the vast majority of *jué* 厥 in its quotations with *qí* 其. The language has undergone a profound change regarding a core word. See Bodman 1948 and the much more detailed accounts in Qián Zōngwǔ 1996 and Ken-ichi Takashima 1999.
3. The copula *wéi*, indifferently written 唯 / 維 / 惟, is ubiquitous in the old *Shūjīng* in grammatically quite unique ways that have been analysed in a masterful detailed thesis supervised by Georg von der Gabelentz on “pre-classical Chinese grammar”: Max Uhle 1880. See Qián Zōngwǔ 1996, pp. 16–18 for a less sophisticated analytic survey of the data.
4. Extensive use of bi-directional phonetic loans (*shuāngxiàng jìjièzì* 雙向假借字), where X can be used for Y and also Y for X, are particularly frequent in the old *Shūjīng*. See Qián Zōngwǔ 1996, pp. 14f.

The differences are many more than these. They are truly staggering. And yet, B&S find “it is difficult at this stage to make a meaningful distinction between Old Chinese and Proto-Chinese (= Proto-Sinitic)” (p. 2). They treat the *Shījīng* as direct sources for Proto-Chinese (= Proto-Sinitic). It is abundantly clear throughout the *New Reconstruction* that its authors have great difficulty making this crucial distinction between the theoretic construct “Proto-Chinese” (= Proto-Sinitic) on the one hand and on the other hand the historically and archaeologically attested cultural artefact “Old Chinese.”<sup>70</sup> More about this later.

At this point I have two comments:

<sup>70</sup> For a more detailed comprehensive account of the grammar of the old parts of the *Shūjīng*, see Qián Zōngwǔ 2004. For the no less relevant and no less well-documented case of the language of the oracle bone inscriptions, and for the related particle *huì* 叀, see the very careful thesis by Redouane Djamouri (1987), pp. 235–363, for which Uhle’s important work has not been accessible. An inspiring magisterial general survey of oracle bone inscription grammar remains Chén Mèngjiā 1988 (preface 1955), pp. 85–134. The literature is vast. See Zhāng Yùjīn 2001 which provides an excellent bibliography.

Firstly: B&S are, of course, not reconstructing anything close to the earliest attested form of Chinese when they base their reconstruction on evidence in the *Shījīng*.

Secondly: what they base their reconstructions on – according to their own explanation – does not represent any unitary phonological system. B&S write:

A limitation in the use of rhymes as evidence is that only a minority of Old Chinese words are actually attested as rhymes. Rhymed passages in newly discovered documents can resolve some questions, but even if a word is attested as a rhyme, it may be difficult to reconstruct its pronunciation if there are too few examples. In part this is because *the poems originate at different times and places and do not all reflect the same phonological system*.<sup>71</sup>

By “newly discovered documents” one presumes that B&S do not wish to refer to newly discovered printed texts but excavated texts from the fourth century onwards, by now already at least 900 years later than the earliest inscribed bones. Moreover, for these B&S assume that originating at different times and places they “do not all reflect the same phonological system.” This raises the following issue: since the received and transmitted printed literature also contains texts that originate at different times (in fact over a period of some 700 years), and in different places, why are we entitled to assume that these transmitted and not excavated texts reflect the same phonological system, when we are not entitled to assume the same thing for excavated texts? And if a difference is to be made between excavated and transmitted texts, how are we to judge those texts which survive both in transmitted and in excavated forms? Is the phonology of the excavated *Lǎozǐ* possibly non-“Old Chinese” parochial, and may it fail to reflect the same “Old Chinese” phonological system that the transmitted “Old Chinese” text does?

We note that B&S use evidence ranging over a long period as directly relevant to details in their fine-grained reconstruction of their Old Chinese. Now, suppose that, a few thousand years hence, someone offered a detailed reconstruction of the phonology of some “Olde English” basing themselves on plausible details which they find in texts ranging from Beowulf to Virginia (Woolf).<sup>72</sup> That is just the kind of thing that is on offer when B&S set out to explain to us the phonological system of Proto-Chinese basing themselves directly for this reconstruction on evidence from the oracle bone inscriptions to the *Guōdiàn* 郭店 manuscripts dating from somewhere in the late fourth or early third century BC, and on sources beyond this. In particular, they speak of extremely detailed rhyme distinctions (speaking about previous scholars who investigated Old Chinese rhyming): “Over time, their analysis gradually became more fine-grained, but recent research makes it clear that it was not fine-grained enough” (p. 24). But given the sparse and chronologically as well as dialectally disparate nature of our direct sources, are such fine-grained aspirations realistic?

B&S’s approach to “Old Chinese in the narrow sense,” i.e., Proto-Sinitic, seems to me to be a supremely confident great leap backward from Bernhard Karlgren’s

<sup>71</sup> Baxter – Sagart 2014, p. 25, my emphasis.

<sup>72</sup> See Minkova 2014 for the complexity of developments during that period.

judiciously modest delimitation of the object of his phonetic reconstructions as the language represented by the *Shījīng*, the rhyming patterns of which had been such crucial evidence for his and Chén Lǐ's 陳澧 admirable endeavours. Nothing suggests, and absolutely nothing confirms, that a phonologically stable fine-grained Proto-Sinitic phonological system can be reconstructed on the basis of oracle bone inscription texts, rhyming bronze inscriptions, and the *Lǚshì chūnqiū*. Moreover, since the *Shījīng* does not pretend to represent any one unitary dialect system even for the *Shījīng* itself there was not any one well-defined system for that time to even misconstrue.

I am reminded at this point of what my benefactor Zhū Déxī 朱德熙 pointed out regarding “Modern Standard Chinese” as a well-defined object for study. On the august occasion of his honorary doctorate in Paris (where I was unfortunately absent) he has been reported to me as having declared to the dismay of his public: “L’objet n’existe pas! Plus on entre dans ses détails, plus on départ de sa réalité.”

## OLD CHINESE

I will now turn from Proto-Chinese to Old Chinese.

Zev Handel defines OC as follows: “Old Chinese is generally considered to be the Chinese language as spoken in the first half of the first millennium BC, and reflected in the rhyming patterns of the *Shi Jing* [Book of Songs or Book of Odes] and in the phonetic elements of early Chinese characters” (Handel 2003, p. 544).

As Bernhard Karlgren was refreshingly well aware, the exact relation between “the Chinese language as spoken” at the time, and the language as documented in the writings of the period is very much open to question. Karlgren carefully tried to identify grammatical features that were limited to direct speech in our texts.

B&S subscribe to a broader definition of the term “Old Chinese”: “We use the term ‘Old Chinese’ in a broad sense to refer to varieties of Chinese used before the unification of China under the Qin 秦 dynasty in 221 BCE” (p. 2).

To start with, it would have been useful to specify what kinds of uses we have records of. To what extent do our records provide plausible direct evidence of the spoken language with a well-defined unified pronunciation and phonology?

Next, I diagnose here a serious historical category mistake. The *political* date of the unification is of no particular significance for the *linguistic* history of the time in this case. Very wisely, Ernst Pulgram is able to link political events like the advent of Charles the Great in his scheme to the emergence of Romance textual evidence. But he is concerned not with political history, but with the state of our sources on written vernacular. Not so B&S. D.C. Lau’s 劉殿爵 masterful study, *Huáinánzǐ yùndú jí jiàokān* 淮南子韻讀及校勘 (Lau 2013), published only recently, long after his death, certainly suggests that 139 BC would be a somewhat less inadequate date to opt for, since the rhyming in that book seems almost entirely compatible with that in *Lǚshì chūnqiū* which we are asked to date 249 BC. Sergey Yakhontov wisely refrains from conflating linguistic history dates with political ones in his book on the Chinese language (which was not found worthy of inclusion in B&S’s bibliography): “Древнекитайский язык этого времени— с V в. до н. э. по II в. н. э.—можно назвать классическим” (“Old Chinese of this time – from the 5th

century BC to the second century of our era — can be called classical”).<sup>73</sup> Likewise, Pān Wùyún and Zhāng Hóngmíng avoid conflating political history with linguistic history in settling on a later date for the end of Old Chinese: “Middle Chinese (from c. 5th century CE to 12th century CE) phonology, derived from Old Chinese (from c. 1700 BCE to 1st century CE) and evolving into Modern Chinese (from c. 15th century CE to present).”<sup>74</sup> But for Pān and Zhāng Old Chinese also covers more than 1,600 years. One notes that unlike B&S, Pān and Zhāng judiciously place the beginnings of Old Chinese long before its attestation, as any historical linguist would certainly be wont to do.

Consider again, for a moment, the notion of “the fine-grained phonological system of the Olde English language (8th to 20th century), based on evidence from Beowulf (8th to 11th century) to Virginia Woolf (20th century).” If English words were written with Chinese characters for all morphemes, then a few thousand years hence some over-confident linguist might indeed be tempted to write such an account of “the fine-grained phonological system” of the pre-1945 English language. I submit that such a linguist would be engaging in a project that is not only hard to realise, but ill-conceived in the first place.

#### OLD CHINESE VERSUS PROTO-CHINESE

Unlike Indo-European (a real form of speech current on the Eurasian or Anatolian grassy steppes a few millennia ago), the reconstructed Proto-Indo-European (hereafter PIE) “parent language” is not a concrete historical fact. Rather, it is a very recent hypothetical construct or an abstract scheme, a set of formulae, from which the properties of the extant daughter languages can be derived – more or less successfully. Our notion of PIE is constantly updated. IE is moved around by chronological (and geographical) arguments, but basically remains as old as it ever was. On the basis of PIE one can glean some IE, as the hyper-confident August Schleicher did so successfully and amusingly in his fable written in his version of PIE.<sup>75</sup>

Using Louis Hjelmslev’s stilted but very precise prose in *Omkring sprogteoriens grundlæggelse* (1943), one would insist that IE is a *semiotic*, PIE is a *metasemiotic*. My favourite case, the Old Novgorod Dialect from the Russian Middle Ages, is wonderfully comparable to that of some Mǐn sub-dialects. Now, a Proto-Old-Novgorod posited on the basis of some modern North-West Russian dialects would be a construct one may or may not wish to indulge in, and which some would want to declare to be an untrammelled theoretical *fantaisisme*. But anyone who declares Old Novgorod itself a vain figment of the imagination would have to contend with the fact that this dialect is actually rather well-defined and well attested, and magisterially and exhaustively documented and analysed on no less than 878 pages in Andrey Anatol’evich Zaliznyak 1994.

Let us now turn to the well-documented case of Old Chinese. Take the magisterial statement in the opening pages of B&S’s book: “Furthermore, since the language of

<sup>73</sup> Yakhontov 1966, p. 7. See, particularly, the judicious and meritorious translation by Jerry Norman of the phonological part of this book in Yakhontov 1978–1979 and also Norman’s influential other translations from Yakhontov’s work, Yakhontov 1968 and 1970.

<sup>74</sup> Pān Wùyún – Zhāng Hóngmíng 2015, p. 80.

<sup>75</sup> See Lehmann – Zgusta 1979.

the earliest Chinese texts appears to be very close to the common ancestor of all attested varieties of Chinese, it is difficult at this stage to make a meaningful distinction between Old Chinese and Proto-Chinese (= Proto-Sinitic)” (p. 2).

Now if “Old Chinese” is intended in the broader sense in which I fear the unsuspecting reader might be excused for taking it, then we have here an egregious conceptual category mistake. It was Stephen Colvin who has made this point most succinctly and precisely: “Old Chinese is a cultural artefact and Proto-Chinese is a fairly dodgy construct.”<sup>76</sup> This category mistake tends to reify a theoretical construct by conflating a theoretical construct with a complex attested cultural historical phenomenon. This reading would not qualify as a scientific mistake. It would be mere cognitive and conceptual confusion.

But perhaps B&S do not mean anything as appallingly conceptually confused as this. Perhaps they do intend “Old Chinese” here to be used in their narrow technical sense, referring to “the earliest stage of Chinese that we can reconstruct from Chinese evidence.” But in that case B&S will have offered us something perilously close to a plain tautology. Since the earliest stage of Chinese we can reconstruct for Chinese is defined as being exactly Proto-Chinese (= Proto-Sinitic), they would be telling us that they find it “difficult at this stage to distinguish between Proto-Chinese and Proto-Chinese.” A proto-language can never be any earlier than the earliest stage one can reconstruct for it, exactly because the proto-language is the earliest stage of a language we can reconstruct for it.

And yet, what if B&S are not intending any of these two readings of their statements? What if what they intend is to posit two historical phenomena, one attested in “the earliest Chinese texts,” the other imagined as a concrete manifestation of the Proto-Chinese (= Proto-Sinitic) system they reconstruct as the common ancestor of all later varieties of Chinese. Now this does make sense. And if true it would be important if these two could be shown by compelling evidence to coincide.

But, quite justifiably, B&S make precious little use of the oracle bone inscriptions, which they date from 1250 BC, while Qiú Xíguī (2000, p. 29) places their origin in the fourteenth century BC. Their phonological reconstruction is crucially based on the rhymed *Shījīng* that is some seven or eight hundred years later than “the earliest texts we have.” Of course, these oracle bone inscriptions throw relatively little light on matters of phonology, certainly not enough to reconstruct the phonological system underlying them. David N. Keightley puts it in his inimitably spirited way: “The inscriptions tell us so little about their sound that ‘the problem of the pronunciation of Shang graphs’ has been declared ‘nigh insurmountable’.”<sup>77</sup> Thus, here again, though potentially interesting, this interpretation does not accord with their stated programme.

I suggest it might have been helpful not to send the unsuspecting reader speculating on what exactly is meant by “Old Chinese,” and on how such “earliest stages” might be comparable to but different from exactly what kind of “proto-languages.”

Reification of constructs (like the inventories that constitute a proto-language) lies at the very heart of large parts of the *New Reconstruction*. It is for this reason that this matter deserves much more attention than such terminological atrocities as “pre-

<sup>76</sup> Private communication, 2015.

<sup>77</sup> Keightley 1985, p. 67. See also *ibid.*, pp. 67–70 for detailed discussion of the state of the art as he saw it in 1978 and Serruys 1974.



initials,” and “post-finals” which one may dismiss as mere logical infelicities of no consequence for the substance of the work.

B&S are reconstructing something as a “proto-language” for which Huá Xuéchéng has discussed not only a wide range of *fāngyǎn*, but also a number of mutually incomprehensible dialects, as we have seen in my opening remarks, see Huá Xuéchéng 2007 and now Wáng Zhípíng – Mèng Péngshēng – Zhāng Jié 2014. Now a proto-language must be reasonably unitary and must not have sister languages, since if it had such sister languages or dialects, the real proto-language would be the parent proto-language that all these dialects, including the “proto-language,” should be construed to derive from. So, if the language reconstructed by B&S does have mutually incomprehensible dialect sisters of the same Sinitic family, as Huá Xuéchéng and Lǚ Guóyáo would have us believe, it follows that B&S have not reconstructed any Proto-Chinese (= Proto-Sinitic).<sup>78</sup> Nothing, of course, prevents B&S from disagreeing with the late evidence we have on early dialects. But in order to do so, they would have to engage Huá Xuéchéng in a detailed philological discussion of the rich relevant evidence he provides. One must remember that beyond the cases where the *Shījīng* is explicitly mentioned in comments on dialect, there are also all the cases where words placed geographically as dialectal and not part of *sifāng tōngyǔ* 四方通語 (current speech everywhere) do actually get used in the *Shījīng*.

## THE CHRONOLOGY OF PROTO-LANGUAGES

We trace back records of Old Chinese to the middle of the second millennium BC. We can only trace back Old Tibetan to the 7th century AD, and many other Sino-Tibetan languages to no earlier than the 19th century or even the early twentieth century. When we reconstruct a proto-language for a language attested only in the 20th century, or in the 7th, is our construct relevant to the historical explanation of the linguistic situation in second millennium BC? Are we entitled to assume for any reconstructed proto-language that no deep systemic changes relevant to our explanation occurred in the course of those 3,000 years?

As Karlgren (1931, p. 25) was quick to point out against his competitor Sir Walter Simon: What we need to compare is not classical Tibetan with Old Chinese, but at the very least Proto-Tibeto-Burman with Proto-Chinese.

But as András Róna-Tas (1985, pp. 98–112) was a little slower to insist, also against Karlgren: The *décalage* or distance of time even for Proto-Tibetan and Proto-Tibeto-Burman is so large that it becomes unclear exactly how much a reconstruction based on data from the 7th century AD can tell us about a relationship that may or may not have obtained in the 3rd millennium BC (and probably long before that). Like Karlgren, Róna-Tas 1985 is deeply worried that the relationship being so distant in time, the number of theoretically conceivable cognates becomes unmanageably vast, very

<sup>78</sup> For a neat set of dialect words in the *Shījīng* see Huá Xuéchéng 2007, pp. 181–182. There is one beautiful case which Guō Pú 郭璞 actually disputes the over-enthusiastic claim of local usage in the *Fāngyán* 方言: The word at issue is *shū* 姝 “beautiful,” as in 《詩·邶風·靜女》: 靜女其姝。《鄘風·干旄》: 彼姝者子。《方言》j. 1 has this: (好) 趙魏燕代之間曰姝。郭璞注: 亦四方通語。Guō Pú comments: “This is surely current usage everywhere!” Observations on localisms and dialects were not simply taken on authority by a congregation of submissive commentators in ancient China.

much beyond the 338 cognates postulated by Sir Walter Simon in his famous “Habilitation” (1929) (which goes unmentioned in the *New Reconstruction*). What we have is a very uncomfortable “embarras de richesse” of opportunities for theoretically possible cognate claims with a corresponding penury in compelling standards for the proof or plausible confirmation of the same claims. As Róna-Tas puts it very mildly: “Es ist viel leichter gemeinsame Isoglossen zu finden als eine klare Abstammungsrelation.”<sup>79</sup>

I am reminded that the gap in time between Old Chinese and the Tibeto-Burman does not matter one iota. And it would indeed not matter one iota if Proto-Tibeto-Burman could be reconstructed (without circular reference to Old Chinese) in such a way that it related with Old Chinese following plausibly general sound laws. Here is the decisive question as I see it: Do we, or don't we, know enough about the systematic regular relationship between Tibetan and Chinese to determine that the Tibetan of the 7th century, which underwent considerable sound changes at all times we know of it, i.e., from the 7th to the 21st century, has undergone no such fundamental systemic changes during the preceding two thousand years? The Lhasa dialect should not be treated as if it simply were the Tibetan language, of course. But that dialect changed radically from the 7th century AD until today. Some other dialects changed less. Was it the Lhasa dialect that was typical of the unrecorded development of pre-attested Tibetan or was it some other dialect that saw little change the last 1,300 years? We cannot tell. And the question is crucial because such changes would be relevant to our interpretation of the systematic historical relation between Tibetan and Chinese. And, of course, if we reconstruct Proto-Tibetan the way Proto-Chinese is reconstructed, i.e., with reference to its assumed relation to other languages to which we wish to relate the language systematically, we would have one of those vicious circles that everyone is wary of.

Sure enough there are languages only known from recent evidence, but confidently asserted to represent very ancient states of affairs where the time gap does not matter one iota. For example, we know the Nuristani languages only from late 19th century evidence, but these small languages from isolated inaccessible mountain areas, studied by the Norwegian scholar Georg Morgenstierne, show every sign that they have not undergone the general changes that I understand are quite broadly attested for early-attested Indo-Arian and Iranian language groups. Thanks to Morgenstierne and others we know that Nuristani languages stopped very early undergoing the systematic changes that the other major subgroups of Indo-Iranian did continue to undergo. We have reason to speak of the position of Nuristani thousands of years ago, because the links to what has been independently reconstructed as Proto-Indo-Iranian from a wide variety of early-attested languages, are sufficiently compelling and systematic – and are said to be fairly consistently present in surviving modern Nuristani languages. Geographical isolation combined with coherent systematic evidence from early attested languages make the case for Nuristani languages not only well attested but also plausible.<sup>80</sup>

In the case of Tibet one might argue that in ancient times communications were difficult in that part of the world. But the all-important difference between Nuristani

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<sup>79</sup> Róna-Tas 1985, p. 109.

languages and Tibetan is that we have no sufficiently pervasive and systematic relations between the Tibetan language and what we know of Old Chinese to be able to claim that the language was unchanging, as certain other languages (and not just Sanskrit!) are well attested to have been.

The case of Mǐn is quite different in even more decisive ways. There is no geographic isolation of the speakers. On the contrary, demographic movements, cross-typological and cross-dialectal language contact must have been extensive in the area, through the ages, if we may trust our historical sources. Many of the late systematic changes to Middle Chinese, such as changes in the initial consonant cluster system and the tonalisation of the final -s suffix, have indeed applied to the Mǐn dialects. But it can be argued that a few have not. But the decisive difference remains this: Proto-Chinese itself can not be reconstructed on the basis of any systematic relations to a wide range of other sufficiently richly early-attested and clearly distinct daughter languages of Proto-Chinese.

For Proto-Tibetan and Proto-Tibeto-Burman the case remains contested.<sup>81</sup> For historically less richly documented languages, the case is only much worse. It is hard enough to arrive at the proto-language constructs for these. But it is almost sheer guesswork when one has to make the crucial decisions on which radical changes and what “new forms” (in Charles Darwin’s terminology) did or did not occur to those hypothetical proto-systems in the course of the millennia, where the only well-documented language of the region, Chinese – no matter how one decides to reconstruct it – underwent such a range of well-documented truly profound changes before the time of the attestation of Tibetan. The important thing is that these were changes that Middle Chinese by itself would never have allowed us to predict, but which were no less real for that. What we are asking is where the evidence is that Tibetan, during that same period, did not undergo any changes that would be relevant to the question of a genetic relation between Chinese and Tibetan.

Suppose, for a moment, we had no sources on Old Chinese, and that we were to reconstruct Proto-Chinese on the basis of Middle Chinese. The result would be predictably and profoundly wrong because it would be systematically oblivious of the profound changes that emerge from our primary evidence from the *Shījīng* in the first millennium BC. Already Chén Lǐ and Bernhard Karlgren demonstrated that much. How are we so sure that the reconstruction of Proto-Tibetan based on those very same Middle Chinese times does not yield the same kind of profoundly wrong results regarding that distant past?

Suppose again we reconstructed Proto-Chinese on the basis of modern Chinese dialects. If we are lucky, we might to some extent approximate Middle Chinese and declare it to the Proto-Chinese. If we are very lucky, that is. But today we know very well, thanks to the helpful work done in works like *Grammata Serica*

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<sup>80</sup> For patriotic Norwegian reasons, I have taken the example of Nuristani. But, of course, languages like Lithuanian, more close at hand, would demonstrate the same thing. And that Lithuanian case is of some special interest in that there seems to be no question of any geographic isolation or inaccessibility of the Lithuanian terrain as an important factor.

<sup>81</sup> See DeLancey 2011. And one may be forgiven for suspending one’s judgement by a judicious form of *epochē* even in the case of Hittite as Indo-European. It is not as if the Indo-European phylogenetic tree was impeccable and only our Sino-Tibetan phylogenetic system problematic.

*Recensa* and Baxter’s 1992 *Handbook*, that we would be making some pretty substantial mistakes about the phonological situation in the early first millennium BC. In reconstructing the proto-languages for those languages that are only documented for little more than a hundred years we are doing exactly this: we project back onto the distant past of three thousand years ago a state of linguistic affairs that is only known to have existed a few decades ago, except when there is a wide range of highly regular systematic correspondences between the recent state and a well attested ancient state that is then shown to be demonstrably preserved in modern times.

Such cases are known to exist. But in the absence of such a wide range of highly regular systematic correspondences between the recent state of Tibetan and other more recently attested languages on the one hand and Old Chinese on the other, all this begins to feel like a “House of Proto-Cards.” A recent work of Nathan Hill (2012) may put some of this to rest for the case of Tibetan, for all I know. This is for the specialists to decide. But Nathan Hill is nowhere mentioned in the *New Reconstruction*, no more than the great Walter Simon. And their results are nowhere discussed or problematised in the *New Reconstruction*, as far as I can see. And if there is one thing that everyone can agree on, it is that the case of the pronunciation of classical Chinese is not like that of the very special case of Sanskrit, which would seem to have subsisted with only fairly unimportant *fāngyīn* variations from around 600 BC right down to modern times.

### FĀNGYÁN 方言 VERSUS FĀNGYĪN 方音

The presence of *fāngyán* 方言 (local words) in Old Chinese is of great importance for a proper understanding of the nature and the social context of the language. The modern concept of *fāngyán* (dialect, usefully discussed already in Baxter 1992, p. 7) must be carefully distinguished from the entirely different traditional concept of *fāngyán* as used by Yáng Xióng 揚雄 in his eponymous book. *Fāngyán* in Yáng Xióng’s sense, and in classical Chinese generally, includes only those phenomena where a dialect has a word (or etymon, to be more precise) that is not part of the *koinē*. However, for a proper understanding of the nature of the reconstruction of “Old Chinese” the concept of *fāngyīn* 方音 (local pronunciations) has long been recognised as even more important. Remarkably, the term *fāngyīn* is completely absent and the important literature on the dialects in Old Chinese times is almost completely disregarded in the *New Reconstruction*.

B&S are acutely aware of the presence and the importance of dialects. All the more surprising that they disregard the main sources we have on these dialects. Let me retell some of Master Lǚ Guóyáo’s 魯國堯 eminently useful summary of the Old Chinese dialect evidence assembled by Huá Xuéchéng (2007, pp. 4ff):

Ēryǎ 爾雅 (3rd–2nd c. BC?) localises 131 words.

Xǔ Shèn 許慎 (ca. 58 – ca. 135) in his *Shuōwén jiězì* 說文解字 references more than 50 words from Chǔ 楚, 45 from Qín 秦, 33 from Qí 齊, 20 from Zhōu 周, and 17 from Rǔnán 汝南.

Líu Xī 劉熙 (born ca. 160) in his *Shì míng* 釋名 lists 40 dialect words from 19 different locations.

- Wáng Yì 王逸 (fl. around 117), in his *Chǔcí zhāngjù* 楚辭章句 mentions 22 dialect words, 2 from Qín and the rest from Chǔ.
- Hé Xiū 何休 (129–182), in his *Chūnqū Gōngyángzhuàn jiègu*, mentions 30 dialect words; 3 notices concern grammar.
- Zhèng Xuán 鄭玄 (127–200) was not particularly interested in localising speech. Nonetheless, we do find references to 67 dialect words in his commentaries.
- Gāo Yòu 高誘 (fl. around 205–212), in his commentaries on *Lǚshì chūnqū* and *Huáinánzǐ* mentions 25 dialect locations in connection with 84 words.
- Lù Jī 陸璣 (fl. 3rd c.), in his *Máoshī cǎomù niǎoshòu chōngyú shū* 毛詩草木鳥獸蟲魚疏 refers to 80 dialect words.
- Guō Pú 郭璞 (276–324), in his commentary on *Mù tiānzǐ zhuàn* 穆天子傳 mentions 5 dialect words, in his commentary on *Shānhǎijīng* 山海經 9, and in his commentary on *Ēryǎ* 爾雅 19.

Could one imagine that during the years 1200–221 BC there were no *fāngyīn*, as the relatively constant rhyming patterns from the 9th down to the 3rd centuries BC might seem to suggest? Can one imagine a phonologically consistent standard *yǎyán* 雅言 (dignified speech) that prevailed unchanged throughout the empire during the ten centuries before that empire even existed? If this were the case, then it might indeed make sense to reconstruct the phonology of such a “dignified speech,” what already Yáng Xióng called *sifāng tōngyǔ* 四方通語 (common language of all the regions; *Fāngyán* 1.3), and what Guō Pú went on to refer to as *Zhōngguó sifāng zhī tōngyǔ* 中國四方之通語 (the common language in all the regions of the Central States; *Fāngyán* 3.49). But the crucial point remains that what is *tōngyǔ* 通語 need not have *tōngyīn* 通音, i.e., a unified standard pronunciation with the same phonological system. Shanghai Mandarin (i.e., the Mandarin spoken in Shanghai, as opposed to the Shanghai local dialect) has a different phonology from Peking Mandarin. There are a neatly different Mandarin *fāngyīn* in Shanghai. (We can leave aside the question of *fāngyán* [dialect words] in Shanghai Mandarin.)

Such *fāngyīn* have been taken for granted by many for “Old Chinese.” Bernhard Karlgren may not have agreed with Lín Yǔtáng 林語堂 (1899–1976), but his students were certainly reading him eagerly. For Lín Yǔtáng was not only a very fine writer on linguistics, he also had the a broad humanistic horizon in his writings on this subject as on everything else. He wrote not as a linguist, but as an intellectual. His writings on language are conveniently assembled in Lín Yǔtáng 1994, vol. 19 (missing in B&S’s bibliography). There is very useful basic information to be garnered here, of the kind that is sorely missed in the *New Reconstruction*:

大概古音家可分二派。一派是承認方音的，像顧炎武江永、孔廣森、張行孚、一派是不承認方音的，像錢大昕、段玉裁、張成孫等。

By and large the old specialists in old pronunciation can be divided into three schools: one school acknowledges *fāngyīn*, as do Gù Yánwǔ and Jiāng Yǒng, Kǒng Guǎngsēn, Zhāng Xíngfú; one refuses to recognise *fāngyīn*, as do Qián Dàxī, Duàn Yùcái, and Zhāng Chéngsūn.<sup>82</sup>

Lín Yǔtáng shares Yú Yuè's enthusiasm for Zhāng Xíngfú 張行孚 (1875–1908), one of a whole set of very impressive set of short-lived phonologists of late Qing times, and quotes from his *Shuōwén fāyí* 說文發疑 (Doubts raised by *Shuōwén*) 1, 35b–36a on the same page:

然必合韻、江、錢三家之說，知古韻所以不能強合者，皆方音為之。

And so in combining the accounts of Gù (Yánwǔ), Jiāng (Yǒushēng) and Qián Dàxī one understands that the reason why one cannot force the ancient rhymes into regular correspondence is all the doing of the local pronunciations.<sup>83</sup>

Lín Yǔtáng maintains:

古之有方音不異於今，不須申辯。

That the ancients had their local pronunciations just as we have today needs no argumentation.<sup>84</sup>

It may need no argumentation, but it badly needs to be pointed out in view of modern reconstructions of “Old Chinese.”

如認明上古用字不離方音，則材料隨地可以發現。

Once one understands that the ancient Chinese *use of characters* is inseparable from local pronunciations, then one can find [relevant] material all over the place.<sup>85</sup>

The phenomenon of *fāngyīn* is hardly touched upon anywhere in the long text of the *New Reconstruction*. And yet the problems posed by *fāngyīn* might have played a crucial role in the definition of Old Chinese. Lín Yǔtáng, who studied Chinese linguistics in Leipzig, is in fact nowhere mentioned by B&S. The literature on *fāngyīn*, meanwhile, is considerable, but Huá Xuéchéng, *Zhōu Qín Hàn Jīn fāngyán yánjiū shì* (2007) is the indispensable reference work that no one studying Old Chinese would want to miss, just as Dīng Qǐzhèn 丁啟陣, *Qín Hàn fāngyán* 秦漢方言 (1991) is valuable.

Yú Yuè (1821–1906) writes in his introduction to one of the works on *Shuōwén* by Zhāng Xíngfú (who flourished before 1881 when he published a very beautiful critical edition of the *Shuōwén* dictionary):

古人無韻書，則詩之韻各隨其方音而殊矣，後人乃欲於數千年後為古人釐定一韻書，何怪其勞而無功乎

The ancients had no rhyme books, so that the rhymes in poems would each differ according to the local readings [of characters]. When later people, then, several

<sup>82</sup> Lín Yǔtáng 1994, p. 14.

<sup>83</sup> *Ibid.*

<sup>84</sup> *Ibid.*, p. 21.

<sup>85</sup> *Ibid.*, [my emphasis].

thousand years *post festum*, desired to provide a fine-grained rhyme book for the ancients, what wonder that theirs was an effort without [convincing] result.<sup>86</sup>

What may appear irregular is explained by the natural variations of *fāngyīn*. What gets registered by modern *yùnsū* 韻書 for Old Chinese as tolerance of rhyming across rhyme groups may at times also be a manifestation of features of the author's or compiler's *fāngyīn* variation affecting not only in initials but also in rhymes. The *shuāngshēng* 雙聲 is then understood by Yú Yuè as at least partly motivated by a locally distinct *fāngyīn*. And, as I said, the *fāngyīn* may indeed cut across phonological categories much as the pronunciation of 是不是 does in Wú dialects. *Tōngyùn* 通韻 (rhymes across rhyme categories) can and must then be studied as possible evidence of *fāngyīn* where they do occur. Of course, B&S are no doubt aware of all this. But such analyses cannot be adequately studied in a book which never even mentions the term *fāngyīn* nor any of the literature on *fāngyīn*.

### THE PROBLEM OF THE NON-UNIQUENESS OF PHONOLOGICAL RECONSTRUCTIONS

For outsiders to Chinese phonology it is tempting to acquiesce in the *New Reconstruction*. If it was found good enough for Oxford University Press and for the congregation of practitioners of the art of reconstruction according to B&S, it should be good enough for the rest of us. One is tempted to acquiesce in it and get on with one's work, quoting it – like everyone else – whenever we need a reconstruction for a pre-Qín text. There is comfort in this, and a comforting sense of the progress in science. We progress from Baxter 1992 to Baxter and Sagart 2014.

But there are intellectual dangers in this. Consider the book by He Mingyong and Jing Peng, *Chinese Lexicography. A History from 1046 BC to AD 1911* (Oxford University Press 2008). The mild Françoise Bottéro has recently demonstrated how this very tempting and lavishly produced work is nothing less than a scholarly nightmare, replete with substantial and basic factual mistakes throughout.

Now one book that until further notice I do consider as a fairly helpful up-to-date guide on matters of historical linguistics is Don Ringe's book from 2013, *Historical Linguistics. Toward a Twenty-First Century Reintegration* (co-authored with Joseph F. Eska). Ringe has a broad perspective on things, and one of his interests is the reconstruction of the enigmatic “Nostratic,” which I find fascinating. Don Ringe boils down his objections to a certain way of reconstructing Nostratic with some delicate precision when he writes: “Baxter biases his method very heavily in favor of whatever hypothesis is being tested.”<sup>87</sup> In the *New Reconstruction* this kind of bias is present throughout the book: The evidence considered is the evidence favourable to the reconstructions proposed.

Axel Schuessler's comprehensive proposals are not even found worthy of a summary explicit dismissal. He is mentioned three times, for his dating of the palatalisation of velars during the Hàn, and for nothing else. Thus the *New Reconstruction* simply refuses to relate to the current state of the art of reconstruction as summarised by the most systematically productive scholars in their field. Maybe Schuessler is

<sup>86</sup> Yú Yuè, *Shuōwén fāyí*, “Xù” 序, pp. 589f.

<sup>87</sup> Ringe 1998, p. 186. Compare also Bomhard 1984.

not always right, maybe he is often wrong on basic points. But as a very major authority on the reconstruction of Old Chinese his methods and results ought to have received proper attention.

The competing analyses of James Matisoff, whose extensive work over many decades on Sino-Tibetan linguistics is directly relevant to the reconstruction of Old Chinese, are also nowhere found worthy even of being summarily rejected. Again, the issue is not whether Matisoff had it all wrong, but whether it is good scientific method to completely disregard the leading competing approaches and results in one's field.

Modern science is a conversation. And in a conversation one must listen carefully and justify one's results as preferable to those of one's predecessors and contemporaries. And by the way, one would like to know whether there really is nothing to learn from or to disagree with on a reasoned basis in Ulrich Unger's extensive works on the reconstruction of Old Chinese. But this absence is not surprising since practically all research written in German is – for some reason – absent in B&S's bibliography.<sup>88</sup>

Let me ask with Plato: Is the *New Reconstruction* of Baxter and Sagart *doxa*, made more or less plausible, or is it *epistēmē*, demonstrated to be true or at least demonstrably preferable to the best existing alternatives? Are we given conjectures and proposals motivated by certain relevant considerations or are we presented with more or less compelling evidence to show that the *New Reconstruction* is preferable to its many predecessors and contemporary competitors? *Epistēmē* requires not only that one argues for one's solution but also that one systematically considers the best available competing arguments for plausible alternative solutions, and above all that one explains in detail how one arrives at one's conclusions.

Autobiographic reports of the kind “we reconstruct as follows,” encountered throughout, do not amount to argumentation. They do not help. B&S have not been able to avoid this autobiographic style and to show how they have reached their conclusions very systematically: far too often they tacitly dismiss a plethora of alternatives suggested by learned scholars from the PRC, Taiwan, Hong Kong, Japan, Russia, Germany, France, and the USA. We are left in the dark why Axel Schuessler is to be hardly discussed, why Pan Wuyun is to be disagreed with on pre-intials and many other things, etc.

Let me take the subject of astronomy as my example: Even the great science of the stars of the sky establisheth not the Heavenly Mansions. We may work on our philological stars, but have we then any Heavenly Mansions, complete?

Is what we can philologically ascertain not mere scattered adumbrations of what may or may not ever have been a well-defined and fine-grained unitary phonological system? Are there not many ways of construing those Heavenly Mansions? Do we not, perhaps, have a non-uniqueness of solutions of problems, not only in phonological reconstruction?

In reconstruction, there is not only the frequent effect of non-uniqueness of solutions, there is also the all-important and oft-unmentioned “incompleteness principle”: In principle, we can only reconstruct that part of an ancestral language which has reflexes or effects on sufficiently well-defined daughter languages.

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<sup>88</sup> Sagart 1999 does mention some of Unger's works in his bibliography.



But there is not only language death. There is also linguistic feature death, i.e., the obliteration in all daughter traditions of points of phonology, morphosyntax, lexical meaning, etc. The result is that these points of grammar (or “characters,” as they are called in cladistics) are no longer recoverable. Thus, we would not even be able to guess at the Latin *ablativus absolutus* as opposed to the Greek *genitivus absolutus*, for example. Not to speak of the famously arcane Latin form of the *supinum*, which does, however, pop up in Old Church Slavonic and Vedic and is thus not quite limited to Latin.

In a polemical and refreshingly unfashionable article, Ernst Pulgram pointed out an uncontroversial fact concerning the incompleteness of reconstruction:

For example, if one were to reconstruct the proto-language of the modern Romanic languages, ignoring for the sake of the experiment that it is available in the form of ancient Latin of one kind or another, one could neither reconstruct the entire Latin vocabulary as we know it to have existed (a number of Latin words are not continued in any Romanic dialect), nor could one, from the evidence of the living Romanic dialects, reconstruct a language of more than three cases, or guess the existence of deponent verbs, or discover that at least one kind of Latin, the Classical Latin of metric poetry and possibly prose, had significant vocalic quantity, and so forth.<sup>89</sup>

What I would suggest now is that in phonology a similar “incompleteness principle” applies. Even if one accepts highly conjectural “wishful reconstruction” as acceptable, the number of homophones remains considerable. The number of homophones is much higher than I would expect in a natural language. With stricter standards on conjectural reconstruction, the situation would become even much more extreme. The likelihood that a reconstructed language made more distinctions than we can determine on the basis of any compelling evidence from daughter languages seems to me overwhelming.

## PHILOLOGICAL QUIBBLES

### 1. Epigraphy: Type versus Token 1

B&S claim that the graph 聞 is unattested in OC times.

Thus {聞} wén ‘hear’ refers to the word now written as 聞, not the character “聞” itself; in the Old Chinese period, {聞} wén was written in a variety of ways, but “聞” was not one of them. (p. 8)

One wonders why they did not consult any standard handbooks on this. *Tāng Yúhuì* (2001, p. 788), has clear counter-evidence to their claim. The Warring States derivation of the graph is also duly recorded in *Lǐ Xuéqín* 2012, p. 1048. B&S are, obviously entitled to disagree and insist that none of the graphs are Old Chinese graphs, or that none are “versions of” what they thoughtlessly call “the character ‘聞’.”<sup>90</sup> But their naive use of the normally innocent phrase “the character ‘聞’” fails to take account of the well-known basic problems around type, token,

<sup>89</sup> Pulgram 1961, p. 19.

graphemes and the typology of allographs in epigraphy. Suffice it to say that here we can and must attempt a reasonably well-defined analysis of what it takes for a graph to count as “the same character” as “the character ‘聞’.”

## 2. Decontextualisation: Type versus Token 2

The *Gōngyángzhuàn* 公羊傳 defines the force of expressions in context. It says what a given *token* of expressions are taken to mean in a given context. This text does not say what words as such, or as a *type*, mean.<sup>91</sup> Here is an example adduced by B&S:

而者何？難也。乃者何？難也。曷為或言而，或言乃？乃難乎而也。

Here is my translation:

What is indicated by 而？ It is that it is difficult. What is indicated by 乃？ [Also] that it is difficult. Why does it in one case say 而 and in the other case 乃？ It is because (也) with 乃 the difficulty [indicated] is greater than with 而.

B&S first mispunctuate the last complex question as follows:

曷為或言而？ 或言乃？ 乃難乎而也。

And they go on to mistranslate the whole passage as if it was about the meanings of word types:

“What does ‘而 [ér]’ mean?” “There was a difficulty.” (p. 72)

Let me spell this out properly: the word-type *ér* 而 does not have the dictionary meaning “there was a difficulty.” And the commentary claims nothing of the kind. A linguistically sensitive translation must respect the nature of commentarial discourse about the force of expressions in context as opposed to a general de-contextualised attribution of meaning to words as such. Göran Malmqvist has tried to do this in his pioneering translations of the *Gōngyángzhuàn* (1971). The de-contextualisation of meta-discourse is, as it happens, a major event in the early history of Chinese linguistics. I can vividly imagine how my disagreement with B&S’s translation of the *Gōngyángzhuàn* here will sound trifling. But the fact is that this disagreement touches upon a crucial development within early Chinese linguistics which demands of us a high degree of philological sensitivity to structural detail.

<sup>90</sup> For the Yúnmèng 雲夢 manuscripts one should now consult the sumptuous heavyweight edition Chén Wěi 陳偉 (ed.), *Qín jiǎndù héjí* 秦簡牘合集 (2014), in 6 huge vols. I find these volumes summarise relevant scholarship in a magisterial way and provide exhaustive bibliographies. One very much regrets, though, that none of the precious materials so well presented and annotated, are translated. From an epigraphic point of view, as from the phonological/grammatical point of view, the political date 221 BC does not in fact mark any important general break in the Qín epigraphic tradition, as far as I can tell. (Ulrich Lau, a leading specialist in Qín legal texts, concurs in this assessment.)

<sup>91</sup> For relevant discussion on the significance of the type/token distinction, see the eminently readable Wetzel 2009.

### 3. Translations

Very little Chinese is translated in the *New Reconstruction*. It is disconcerting how many of the few translations that there are, are imprecise. One more example might illustrate what I mean:

(1060) 今兗州人謂殷氏皆曰衣。

Nowadays the people of Yǎnzhōu 兗州 all pronounce the family name 殷 Yīn [\*ʔər] as 衣 Yī [\*ʔ(r)əj]. (Xǔ Wéiyù 2009, p. 356)

But the commentator Gāo Yòu was very far from claiming that Yǎnzhōu was populated by one particular people. The correct translation has to be something like “Nowadays people in Yǎnzhōu, when referring to the family name Yīn all pronounce this like 衣.”

### 4. Graph Analysis

B&S write: “recall that 炎 yán is phonetic in 熊 \*C.[ŋ]<sup>w</sup>(r)əm> hjuwng > xióng ‘bear (n.)’” (p. 314).

But there is no 炎 at all to recall in 熊. On the contrary, there is only 火. On the other hand, Xǔ Shèn’s 許慎 dictionary plausibly suggests that this 火 should be taken as an abbreviated form standing for an assumed phonetic constituent 炎. That is a very different state of affairs, which is indeed relevant to the discussion. The shoddiness of philological discourse of this sort is too common for comfort in the *New Reconstruction*. Take the following definition of a prefix:

Prefix \*k- added to verbal roots derives nonfinite forms of the verb that can be used as nouns: (p. 57)

I understand nothing of this, as it stands. The ultimately all-important sections on the semantics of affixation as a whole do give an impression more of hasty summary than of any careful philological analysis and documentation of meanings and meaning relations. As this review goes to the press I find that George Starostin (2015, p. 389) has arrived at a similar conclusion: “... in my opinion, if further progress is indeed to be made, certain methodological principles should probably be amended. Possible recommendations would include a far more rigorous approach to issues of comparative semantics and grammatical derivation in the case of ‘word-families’...”; I can only heartily agree. There is no need to go into detail on this point.

### 5. Philology

For the important *Yīqièjīng yīnyì* 一切經音義, B&S list the meritorious edition Zhōu Fǎgāo 1972, failing to mention the comprehensive work on this crucial source by Xú Shíyí 徐時儀, *Yīqièjīng yīnyì sān zhǒng jiàoběn hékǎn* 一切經音義三種校本合刊 (2008). None of this would be more than marginally irritating if they did not, instead of addressing a good edition, quote this:

祇神、上顯堅反、上顯堅反、考聲云、胡謂神為天、今開中人謂天神為祇也。

祆神 [“Xiān deity”]: the first [word] is pronounced 顯堅反 [MC *x(enX) + (k)en = xen*]. The *Kǎo shēng* 《考聲》 says: the Iranians call deities 天 [heaven]; nowadays people within the pass [i.e., in modern Shānxī and Gānsù] call heavenly deities 祆 [MC *xen*]. (p. 114)

which involves a gross mispunctuation with a highly problematic translation of the generic term *hu* 胡 as specifically “Iranian” from the young Albert Dien in 1957 in an important passage from that book. The fact that *hú* can be shown to refer to people who are in fact Iranian is not proof that the word was a specific term for “Iranian.” At the very least, readers of the *New Reconstruction* might be invited to consult the *Taishō Tripitaka* text in No. 2128 T54, p. 0551a (j. 37) (which – sadly – is also notorious for its pervasively faulty punctuation ...).

## 6. Semantics

B&S gloss *hǎo* 好 as “good,” but Bernhard Karlgren’s gloss “good (Shī)” for *hǎo* in *Grammata Serica Recensa* was not made in one of the finer lexicographic moments of his long career. Neither his students, nor his students’ students, have followed him in this definition of the basic meaning of *hǎo*. The meaning “good” is not the basic one for *hǎo* 好. The word underwent a profound meaning change in colloquial Chinese. And the interpretation of *hǎo qiú* 好逑 as “good mate” in Ode 1 is disingenuously puritan. Her “splendid beauty” is surely an embarrassingly great deal more relevant to the poem. Nor is *hǎo gē* 好歌 “a beautiful song” congenially interpreted as a good song (*pace* Xiàng Xī 1997, p. 223). Glosses along the semantic lines of *měi* 美 (handsome, splendid) are pervasive in the ancient literature, and Xǔ Shèn in his *Shuōwén* dictionary has the graph-based gloss *měi* 美 (beautiful), not *shàn* 善 (good, excellent). B&S could have confirmed what I say here if they had consulted the 91 traditional glosses conveniently lined up in the indispensable handbook by Zōng Fúbāng, *Gùxùn huìzǎn* 故訓匯纂 (2003). But that handbook is nowhere mentioned in the *New Reconstruction*, in spite of the fact that it would have been crucial for determining the old and traditional semantic glossing of words, for example, of derivations by tone change, as in *hào* 好 (like; like to; be prone to).

## CONCLUSIUNCULA

Reconstructions of all kinds need to be taken *cum grano salis* (with a pinch of salt), no matter how little those who have cooked up those reconstructions feel there is any need for it. Mindful of all this, thoughtful reconstruction tends to declare itself tentative. In the case of the avowedly tentative and hypothetical reconstructions of the *New Reconstruction* of Old Chinese it seems to me these reconstructions ask for something more like a table spoon of salt.

In a judicious review of Baxter 1992, William Boltz commends the achievements of the book and ends up with a magisterial statement about the future of the subject:

Progress beyond this, now, especially with respect to the still largely intractable problem of initial consonant clusters, their nature and their devolution, not to mention the question of the ultimate genetic affinity of Chinese, which in spite of the apparent confidence of adherents to a Sino-Tibetan hypothesis, remains unsettled,

will depend on a willingness to apply somewhat less tested and more experimental methods and to consider less conventional kinds of data.<sup>92</sup>

These were wise words that have been borne out by more recent developments. [Baxter 1992](#) was not only very useful, also for many of us who are not professional phonologists. But it also promised difficult tasks ahead.

There are those, like the seasoned and devoted phonologist W. South Coblin, who, having spent many decades working hard on the reconstruction of Old Chinese phonology, had enough of it, threw up their arms and declared a non-conjectural detailed reconstruction of Old Chinese simply impossible and hence pointless.

There are also those, like the seasoned linguist Roy Andrew Miller (1924–2014), who came to find the current trends in the reconstruction of Old Chinese of his time so pervasively flawed, and the congregation of loyal professionals, *nemine contradicente*, so wondrously docile, that the field was beyond repair, hence best disregarded.

There are those like Søren Egerod (1923–1995), who seemed convinced that what we can hope to reconstruct is not the complete sequence of phonological segments, perhaps even with their suprasegmental features. What we can make more or less plausible informed guesses on, according to him, are various phonological features that were probably part of the pronunciations of classical Chinese words.

There are those like Axel Schuessler, who materially disagree on a very large number of the arguments made in the *New Reconstruction* and complain that alternative professional proposals are insufficiently considered, if at all, and that (even more seriously) much of the reconstruction is presented as pure conjecture without sufficiently substantial evidence and without consideration of counter-evidence ([Schuessler 2015](#)).

There are those like Dah-an Ho 何大安 (from Academia Sinica), generally a very mild scholar, who finds pervasive serious fault<sup>93</sup> with the *New Reconstruction* and ends up in drawing radical personal conclusions about the authors of the book which I would rather present in Chinese: 其中所犯的某些錯誤，如果說的嚴重的話，甚至會令人對作者的基本訓練與學術忠誠喪失信心。當然，我們不希望出現這樣的結局。<sup>94</sup>

But there are also those like Wolfgang Behr who are inclined to acquiesce in B&S's reconstructions as the "state of the art," the best that we have until we all manage to agree on something even better.<sup>95</sup>

Finally, there are those like the linguist Pamela Munro, who are delighted with the unanimous decision of her committee of the Leonard Bloomfield Book Award of the

<sup>92</sup> [Boltz 1993](#), pp. 185–207, this quotation on p. 207.

<sup>93</sup> 1. 對語言事實的欠尊重對證據效力的不了解 2. 對運用古文字資料以及閱讀古籍所需基本功的不足 3. 對借詞的曲解對文獻的不忠實 4. 對新知的茫昧歷史語言學觀念和方法論的落後。  
[Ho Dah-an 2016a](#), p. 27.

<sup>94</sup> *Ibid.*

<sup>95</sup> A position stated at the Workshop "Linguistic Approaches to Premodern Chinese Literature" held at the University of Zürich in October 2015 (<http://www.asienundeuropa.uzh.ch/de/aboutus/bulletin/2016/reconstructions.html>, accessed 14 October 2016).

Linguistic Society of America to select the *New Reconstruction* for their distinguished prize.

Now I must confess that I do not belong to any of these kinds. It is true enough that the classical Chinese language has been learnt and taught remarkably well for centuries and millennia without any oxymoronic reference to preinitials, post-finals, and pre-pre-initial tightly or loosely bound pre-syllables. And it is important to keep this in mind as one tries to make progress in the field of classical Chinese philology. But the fact remains that the sound world of Old Chinese will always be indispensable for the study of classical Chinese texts. Homoiophony (similarity of pronunciation that is taken to license *tōngjiǎ* 通假 [phonetic loans] or the old diagnosis of *shēng zhī wù* 聲之誤 [writing mistake because of similarity of pronunciation] in textual criticism, etc.) is crucial for the responsible transcription of excavated literature as well as for textual criticism of all kinds of Chinese writings. The delimitation of the kinds of differences that are permissible within the range of homoiophony is of absolutely crucial significance for methodology in Old Chinese textual studies. The more we lower the threshold of what may be declared to be homoiophonous and the more phonetic and phonological differences we permit among homoiophones, the less plausible – let alone compelling – our arguments from homoiophony will become.<sup>96</sup> The more tolerant we are in matters of homoiophony, the more we approach the “fröhliche Wissenschaft” or “gaia scienza” of a Paul K. Feyerabend (1924–1994), where “anything goes” – until it is brought down.

Consider the exceptional phonetic tolerance exhibited in the list of characters with which *héng* 衡 is held to have the *tōngjiǎ* relation as established on the basis of authoritative editions of excavated literature by Bái Yúlán 白於藍 in his singularly convenient *Zhànguó Qín Hàn jiǎnbó gǔshū tōngjiǎzì huìzǔǎn* 戰國秦漢簡帛古書通假字匯纂 (2012):

1. pǐ 疋 (p. 211), 2. yú 虞 (p. 238), 3. hè 賀 (p. 305), 4. suǒ 索 (p. 459), 5. hèng 橫 (p. 715), 6. liàn 練 (p. 800).

Here is a less extreme *tōngjiǎ* set for yǔ 與:

1. yǐ 以 (p. 12), 2. yè 冶 (p. 13), 3. yú 餘 (p. 193) 4. yú 予 (p. 194), 5. yé 邪 (p. 195), 6. yā 牙 (p. 196), 6. yè 夜 (p. 450).

These sets are not typical, but they do illustrate the homoiophonic liberties that are being taken in authoritative editions of excavated literature in the interpretations of graphs even when there is no graphemic motivation for the claim that the *tōngjiǎ* “is a loan for” relation. In general, the importance of homoiophony for loaning relations between characters is manifest throughout Bái Yúlán’s useful 1,965-page handbook, successor to the 423-page version from four years earlier. Thus “licensed” *tōngjiǎ* liberties grow exponentially, every year, and by current practices of economic analysis we should expect many thousands of new pages within the coming decades.

<sup>96</sup> Here, of course, *Karlgren 1963–1967* remains indispensable, though not always reliable.

My concern here is that any judgement on these crucial matters of homoiophony will at all times have to rely on the best available reconstruction of the sound system of the language. Compare the case of economics. One may be excused for concluding that economists have failed us egregiously. And yet, we cannot for that reason want to do without them altogether. We need economists' analyses, no matter what our objections are to their definitions, their methods and even the reliability of their results. So there we are: we need reconstructions, no matter how unreliable these may prove to be.

And, of course, there is not only homoiophony: alliteration, homoioteleuthon, assonance, sustained assonance, principles of euphony (in spite of the fact that the Xià dynasty was taken to precede the Shāng, we often have Shāng Xià 商夏 and only rarely the chronologically more plausible Xià Shāng 夏商), homorganic euphonic linkage between words (as in 甲兵 where the link between the two syllables would appear to be something like “-p p-”, and where we only rarely find the non-homorganically linked 兵甲): these are some of the many all-important rhetorical devices that dominate ancient Chinese verse and also ancient Chinese prose composition. Their proper study depends on some view on the pronunciation of classical Chinese.

Here again, I still take solace from G.K. Chesterton's otherwise also dangerous *apperçu*: “Whatever is really worth doing, is worth doing badly.” In short: We must continue do our best even when what is our best will – quite demonstrably – continues to be never good enough. But phonology is too important to be left to just one congregation of cooperative (largely consenting) professional phonologists. There is a need for critical philo-logical hereticism – or at least for straightforward logical heterodoxy.

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## CHINESE ABSTRACT

### 評論《上古漢語構擬新論》(OLD CHINESE. A NEW RECONSTRUCTION)

《上古漢語構擬新論》是兩位頗具影響力的當代語言學家白一平和沙加爾多年合作的最新成果。該書詳細地綜述與梳理了清代以來學界關於漢語早期語音史的研究，並在此基礎上以新方法論提出了上古漢語構擬的新方案。然而，該書的方法論以及構擬中涉及的訓詁學證據仍存在諸多值得商榷的嚴重問題。例如書中認為高本漢在上古漢語領域的貢獻主要體現在音系學方面，這實際是一種誤解，因高氏本人終其一生都極為反對所謂音系學；書中對「上古漢語」的定義帶有很大的方法問題；對上古漢語通過變調破讀派生新詞這一現象的分析不夠充分；有關上古漢語第一人稱代詞的部分，也未全面地佔有論述所必需的文獻訓詁證據。由於缺乏可靠的論據，兩位作者的方法論有可能成為無法驗證的假設，而這正是全書最根本的問題。

關鍵詞：上古漢語，音韻學，變調派生詞，歷史語言學

## NOTES ON CONTRIBUTOR

Christoph Harbsmeier, professor emeritus of Chinese at the University of Oslo, is now an adjunct professor of Chinese Literature in the University of Copenhagen. His main interest is in the philosophy of language, grammar and conceptual history, all in a comparative perspective. His books include vol. 7.1 of Joseph Needham, *Science and Civilisation in China, Language and Logic* (1998) and *Aspects of Classical Chinese Syntax* (1981). His most recent book, co-authored with Françoise Bottéro, is *Chinese Lexicography on Matters of the Heart. An Exploratory Commentary on the Heart Radical in Shuo wen jie zi* (Paris 2016).

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