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RESPONSE TO HO DAH-AN

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ABSTRACT

Ho Dah-an's 2016 review of our book *Old Chinese: A New Reconstruction* (2014) contains little discussion of the book's main themes or proposals: he focuses instead on "errors" which, according to him, "reflect the outdated concepts of the authors and the insufficiency of their basic training." In this response to Ho's review, we consider his discussion of these "errors." On two points (discussed below), he has actually found errors (a faulty interpretation of an entry in the *Shuōwén jiězì* and one incorrect citation), which we are glad to be able to correct. Neither materially affects our conclusions. The other "errors" that Ho mentions are not errors at all: in most cases they reflect basic misunderstandings on Ho's part: of our book, of other sources, and of basic principles of historical linguistics. Since some of these misunderstandings relate to general issues in the reconstruction of Old Chinese, we attempt here to correct these misunderstandings in order to set the record straight.

KEYWORDS

Old Chinese reconstruction Rhyme groups Final coda Loanwords

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Ho Dah-an's 2016 review of our book *Old Chinese: A New Reconstruction* (2014; henceforth B&S) contains little discussion of the book's main themes or proposals: He focuses instead on "errors" which, according to him, "reflect the outdated concepts of the authors and the insufficiency of their basic training." On two points (discussed below see sections 4 and 6 below), which we are glad to be able to correct. Neither error materially affects our conclusions. The other "errors" in most cases reflect basic misunderstandings on Ho's part: of our book, of other sources, and of basic principles of historical linguistics.

We should say at the outset that our "basic training" is primarily in historical linguistics. As we emphasize in our book (B&S 2014: 4–6), the linguistic reconstruction of Old Chinese is an ongoing process; it also involves many disciplines, and we are not so foolish as to think that it can ultimately be successful without the participation of scholars whose grasp of many other fields—including early Chinese literature, paleography, and the study of neighboring languages—surpasses our own.

B&S departs in significant ways from traditional approaches to Old Chinese reconstruction. From the time of Bernhard Karlgren to the end of the 20th century, most research on Old Chinese was based on three main kinds of evidence: (1) The Middle Chinese readings of characters preserved in rhyme books, rhyme tables, and other sources; (2) the rhymes of the *Shījīng* 詩經, and (3) the information about early pronunciation furnished by the Chinese script, in the form of phonetic compounds and loan graphs. Our book attempts to make progress by considering a broader range of evidence:

1. In reconstructing word onsets we use evidence from modern dialects, especially the Mǐn 閩 group, which preserve distinctions that were lost in the Middle Chinese written sources. This kind of evidence has only rarely been taken into account in Old Chinese reconstruction.¹

2. For rhyme evidence, traditional reconstructions have generally relied on an analysis of Old Chinese rhyming based on the work of Chinese scholars of the Qīng 清 dynasty (1644–1911) rather than dealing directly with the *Shījīng* rhymes themselves. This tendency is especially dramatic in Ho's review: He claims that our reconstruction conflicts with the Old Chinese rhyme evidence, but does not cite a single example to support this claim. Our reconstruction is indeed in conflict with the traditional analysis

of Old Chinese rhymes, but we contend that it is consistent with the rhyme evidence itself. Our analysis builds on the results of Baxter (1992), which intensively reexamined the *Shījīng* rhymes and identified rhyming distinctions that were overlooked in the traditional analysis (see section 1 below). Ho Dah-an shows no familiarity with this earlier work or, for that matter, with actual *Shījīng* rhyming.²

3. In using evidence from the writing system, earlier work relied largely on the characters of the script that has been standard in China for the last two millennia. But this is problematic, since a significant number of these characters are of late origin and reflect the phonology of a period later than Old Chinese. Because of recent discoveries in China, it is now feasible to use the scripts of genuine pre-Qín 秦 documents to reconstruct pre-Qín phonology, and we do so where possible.

4. Several language families that were in contact with Chinese from an early period have a significant number of very early loanwords from Chinese, which reveal distinctions of which Middle Chinese and modern dialects show only traces. These loanwords sometimes confirm the impression (derived from the complexity of Proto-Min initials, and from analysis of the script) that Old Chinese word onsets were much more complex than those of Middle Chinese.

An additional feature of our book is its emphasis on morphology. Old Chinese has many sets of words that are similar in both sound and meaning. Where possible, we analyze such sets as having a common root with different affixes, and attempt to assign functions to these affixes. Here is an example of such a set, with our reconstructions and morphological analysis:

- (1) 樹 *shù* < *dzyuX* [遇合三上麌禪]³ < *m-toʔ “plant (v.); place upright”
 樹 *shù* < *dzyuH* [遇合三去遇禪] < *m-toʔ-s “tree”: cf. Proto-Min *džhiu C⁴
 拄 *zhǔ* < *trjuX* [遇合三上麌知] < *t<r>oʔ “prop up, support (v.)”
 柱 *zhù* < *drjuX* [遇合三上麌澄] < *m-t<r>oʔ “pillar”: cf. Proto-Min *dhiu B, Proto-Kra *m-tʂu A “pillar” (Ostapirat 2000: 232), Proto-Hmong-Mien *ɲæu “pillar” (Ratliff 2010: 80)

The root implied by these forms is *toʔ, perhaps “to support.” The

*m- prefix in the verb *dzyuX* < *m-toʔ “to plant; place upright” is responsible for the initial voicing in Middle Chinese and voiced aspiration in Min; we take this *m- prefix to be a morpheme indicating volitional action by the subject. The noun *dzyuH* < *m-toʔ-s “tree” is nominalized by the suffix *-s: “something planted.” The verbal form *trjuX* < *t<r>oʔ “prop up, support (v.)” has an *<r> infix indicating distributed action (as a roof is supported by more than one pillar). In *drjuX* < *m-t<r>oʔ “pillar,” *m- is a prefix that derives instrumental nouns, separate from the *m- indicating volitional action. (The presence of an original nasal is supported by the Proto-Kra and Proto-Hmong-Mien forms, which we take to be early loans from Chinese.) Hypotheses like these can be evaluated by seeing how much order they bring to the attested vocabulary of Old Chinese.

Our book proposes a reconstruction system for Old Chinese that is intended to account for this broader range of evidence. We emphasize that all reconstructions are hypotheses whose predictions should be continually tested against all available evidence, and revised or abandoned as necessary (B&S 2014: 4–6). We do not claim to have “proved” that our reconstructions are correct; in fact we believe that, strictly speaking, such proofs are impossible in historical linguistics. But explicitly formulated hypotheses can be useful even if they turn out to be wrong (Chomsky 1957: 5).

Ho Dah-an largely ignores these main points of our book. Here we consider one by one the “errors” Ho claims to have found in our book.

1. “ONE RHYME GROUP WITH MANY VOWELS”

Ho Dah-an claims that our reconstruction “deviate[s] from received truths” because it implies that words with different main vowels can regularly rhyme with each other. Speaking of the *Shījīng* and other pre-Qín texts with rhymes, he says, “The rhyme groups obtained through analyses of these texts obviously provide robust and reliable evidence” (p. 177); furthermore, “sinograms belonging to the same rhyme group must share the same final or main vowel. This statement should be self-explanatory and unshakable” (p. 178). He goes on (pp. 178–182) to cite examples from poetry in various languages to establish this principle.

His whole eight-page discussion of this issue is beside the point, because we entirely agree that normal rhyming requires identical main

vowels. When we reconstruct *-an, *-en, and *-on in the traditional 元 Yuán rhyme group, it is not that we believe that *-an, *-en, and *-on rhyme with each other; rather, we argue that in the poems of the *Shījīng*, *-an, *-en, and *-on should be regarded as three separate rhyme groups which the traditional analysis erroneously combined into a single group. (This is why we consistently refer to rhyme groups like 元 Yuán as “traditional” rhyme groups).

It is startling that Ho failed to understand this central point of our argument. We would have thought that it was clear to any careful reader of our book, from passages like these:

“[T]he distributional arguments sketched above for reconstructing *-an, *-en, and *-on with different vowels (and similarly for *-ən and *-un) also suggest hypotheses to check against the corpus of Old Chinese rhymes. In other words, rather than reject the six-vowel reconstruction because it disagrees with the traditional rhyme categories, we can test the predictions of this reconstruction against the Old Chinese rhymes themselves to see if they reveal previously unnoticed rhyming distinctions. Using a probabilistic method for testing hypotheses about rhyming distinctions, Baxter (1992) showed that in fact, the predictions of the six-vowel reconstruction are correct: our reconstructed *-an, *-on, and *-en do indeed rhyme separately, as do *-ən and *-un; and the same is true of the rhymes with other codas. These are facts about Old Chinese rhyming that neither the traditional rhyme categories, nor the reconstructions of Karlgren and Li that are based on them, can account for.” (B&S 2014: 207)

“[T]he existence of the distinctions among *-an, *-on, and *-en is well supported by rhyme and xiéshēng evidence: see Baxter (1992: 370–389).” (B&S 2014: 273)

“[T]he traditional analysis puts both *-ən and *-un in the single rhyme group 文 Wén. In fact, the rhyming distinction between *-ən and *-un is unusually clear: the only rhyme that mixes them appears to be in Ode 248.5 (see Baxter 1992: 425–431).” (B&S 2014: 288)

The development of this approach can be roughly sketched as follows. The distribution of initials and finals in Middle Chinese strongly

supports the hypothesis that some early stage of Chinese had a six-vowel system. This was proposed by Bodman (1971); detailed arguments are presented by Baxter (1977: 159–168, 1992: 235–253—relying in part on Jaxontov 1960) and in our book (B&S 2014: 198–211). But it was noticed from the beginning that the six-vowel hypothesis appeared to conflict with traditional analyses of Old Chinese rhyming, because (for example) it requires that 葛 *gé* < *kat* [山開一入曷見], 潔 *jié* < *ket* [山開四入屑見], and 奪 *duó* < *dwat* [山合一入末定], all assigned to the traditional 月 Yuè rhyme group, should be reconstructed with different main vowels, as *-at, *-et, and *-ot respectively. In principle, there were three possibilities:

1. Perhaps the six-vowel hypothesis was correct for the *Shījīng* language, and the traditional rhyme groups are also correct. This would imply that, for example, *-at, *-et, and *-ot were allowed to rhyme even though they have different main vowels. Ho Dah-an portrays us as holding this position.

2. Or perhaps the six-vowel hypothesis was correct for some earlier stage of Chinese, and the traditional rhyme groups are correct for the language of the *Shījīng*: but by the time of the *Shījīng*, the language had undergone sound changes that made its rhyming consistent with the traditional analysis, and also consistent with the principle that normal rhymes require identical main vowels. For example, perhaps the *Shījīng* language had undergone changes like *-et > *-jat and *-ot > *-wat, so that all words in the traditional 月 Yuè group would have the rhyme *-at. This approach is reminiscent of Lǐ Fāngguì's reconstruction (1971), where his “*-iat” generally corresponds to our *-et, and his “*-uat” often corresponds to our *-ot.

3. A third possibility was that the six-vowel hypothesis was correct for the *Shījīng* language, and that rhyming did (normally) require identical main vowels, but that the traditional analysis overlooked a number of important rhyme distinctions. For example, perhaps the traditional 月 Yuè group should actually be divided into three separate rhyme groups, *-at, *-et, and *-ot, which the Qīng philologists failed to distinguish.

Bodman and Baxter never seriously considered option 1, but in the 1970s and 1980s they had not done the detailed analysis of *Shījīng* rhymes that would be required to support option 3, so for the time being they stayed with option 2; Bodman (1980) used the term “Proto-Chinese” for the earlier

stage in which (for example) *-ot had not yet diphthongized to *-wat.

But Baxter (1992) undertook a detailed statistical analysis of the *Shījīng* rhymes, and, using a statistical procedure for deciding whether two groups of words *A* and *B* are separate rhyme groups, found that the rhyming distinctions predicted by the six-vowel system were actually confirmed, supporting option 3.⁵ But the analysis in Baxter (1992) shows that rhymes involving different vowels are much less frequent than would be expected if the traditional rhyme groups are correct. Bái Yīpíng (2019) showed that the evidence for the rhyming distinctions among *-at, *-et, and *-ot (all in the traditional 月 Yuè group), as we reconstruct them, or between *-ən and *-un (both in the traditional 文 Wén group), is much stronger than the evidence for the distinction between the 脂 Zhī and 微 Wēi rhyme groups, proposed by Wáng Lì ([1937] 1958) and widely accepted by most researchers (including us, with some modifications). Starostin (1989) had already reached conclusions about rhyming similar to Baxter (1992), though without the statistical arguments.


If Ho Dah-an is not aware that we support option 3, then there are large parts of our book which he didn't understand or didn't read. The principle that Old Chinese rhyming normally required identity of main vowels occurs again and again in our arguments. For example (as argued in B&S 2014: 207–209), in Ode 106.3 of the Mao *Shī* (齊風·猗嗟), all the rhyme words must be reconstructed with *-on (or *-o[n]⁶) except for 反 *fǎn* < *pjonX* [山合三上阮非] in the fifth line, which must be reconstructed with *-an? because of its rhymes elsewhere:⁷

(2) rhyme words in Mao *Shī* 106.3:

	MC	OC
	變 <i>ljwenX</i> [山合三上獮來]	*[r]on?
	婉 <i>'jwonX</i> [山合三上阮影]	*[ʔ]o[n]?
	選 <i>sjwenH</i> [山合三去線心]	*[s]o[n]ʔ-s
	貫 <i>kwanH</i> [山合一去換見]	*k ^ʰ on-s
→	反 <i>pjonX</i> [山合三上阮非]	*Cə.pan?
	亂 <i>lwanH</i> [山合一去換來]	*[r]ʰo[n]-s

At first glance, this appears to be an irregular rhyme according to

our reconstruction (assuming, as we do, that rhyme normally requires identical main vowels). But in an alternate version of the text, attributed by the *Jīngdiǎn shìwén* 經典釋文 to the Hán 韓 *Shī*, the rhyme word in the fifth line is not 反 *fǎn* but 變 *biàn* < *pjenH* [山開三去線幫] < **pro[n]-s*, which would be a regular rhyme according to our hypotheses (Huáng 2006: 154). The argument that 變 *biàn* was more likely to be the earlier reading was already given in Baxter (1992: 364–366), but has recently been supported by newly discovered pre-Qín texts. The Shànghǎi Museum text “Kǒngzǐ *Shī* lùn 孔子詩論” (Mǎ Chéngyuán 2001: 34, 151–152), a Warring States text on bamboo strips, quotes this very line on strip 22, and writes the relevant rhyme word as

(3) 

Lǐ Jiāhào (1979) showed that this graph is used in Warring States documents to write the word

(4) {弁} *biàn* < *bjenH* [山開三去線並] < **C.[b]ro[n]-s* “cap.”⁸

This word {弁} *biàn* itself rhymes as *-*o[n]-s* in Ode 102.3, and this character also used as a loan for 變 *biàn* (our **pro[n]-s*). Our argument is that although 反 *fǎn* < **Cə.pan?* in this stanza appears to be an exception to the principle that syllables must have the same main vowel to rhyme, the irregularity results from a late change in the text: in an earlier version, the rhyme word was {變} *biàn* < **pro[n]-s* “change (v.),” with an **o* as expected, consistent with our reconstruction—and with the principle that rhyme involves identity of the main vowel. If we really believed that *-*an* could regularly rhyme with *-*on*, then Ode 106.3 would raise no problems, and our argument would make no sense.

Mǎ Kūn (2018) makes a similar observation on what appears to be a rhyme between 和 *hé* (our **cʰoj*) and 靡 *mí* (our **m(r)aj*) in the received version of the *Zhōu yì* 周易 (《☱ 中孚》 *Zhōngfú*): The Mǎwángduī silk version has 羸 *léi* < *ljwe* [止合三平支來] < **[r]o[j]* instead of 靡 **m(r)aj*,

which suggests that the rhyme was originally a regular rhyme in *-oj.

Examples such as these not only support the six-vowel hypothesis; they also show that the six-vowel hypothesis can serve as a heuristic to identify problematic passages in early texts. Again, if we believed that different main vowels could rhyme, as Ho Dah-an claims that we do, there would be nothing peculiar about 反 *fǎn* in Ode 106.3 or 靡 *mí* in the *Zhōu yì* passage just cited (see B&S 2014: 207–211).

That Ho misunderstood our views on this basic point is especially clear in this passage:

Even one of the authors Baxter once remarked, in a self-critique, that “It is not unusual that the Old Chinese *et rhymed with *it (*e and *i both being front vowels); it is rather unusual that *-at rhymed with *-it” (2010: 176) [i.e., Bái 2010: 176].⁹

Nothing in Bái (2010) constitutes a “self-critique.” The passage referred to has to do with the question of what vowel to reconstruct in {設} *shè* < MC *syet* [山開三入薛書] “to set up.” In the six-vowel system, the MC syllable *syet* could be reconstructed with either *-at or *-et. But there is paleographical evidence to support *-et in {設}: As Qiú Xīguī ([1985] 2012) showed, {設} *shè* was written in oracle bones and other early documents with a character ancestral to 執 *yì* < MC *ngjiejH* [蟹開三去祭疑, 重紐四等]—which in our reconstruction can only be reconstructed as **ɲet-s*, with the front vowel *e, because it has the division-IV *chóngniǔ* 重紐 final *-jiejH* (B&S 2014: 204). Words written with the same phonetic element, like words that rhyme, normally have the same main vowel, so the fact that {設} *shè* is written with 執 *yì* < **ɲet-s* supports reconstructing {設} *shè* with *-et, rather than *-at. For this reason, we reconstruct {設} *shè* as **ɲet-s*.

As for rhyme evidence, 設 *shè* rhymes only once in the *Shījīng*, in the following passage from Ode 220.1:

(5) Ode 220.1 (《小雅：賓之初筵》):

鐘鼓既設 設 *shè* < MC *syet* [山開三入薛書] “set up”

舉觴逸逸 逸逸 *yìyì* < MC *yit-yit* [臻開三入質以] “back and forth”

“The bells and drums are set up;

guest and host toast each other back and forth”

Now in the traditional analysis, 設 *shè* < *syet* belongs to the traditional 月 *Yuè* rhyme group, while 逸 *yì* < MC *yit* belongs to 質 *Zhì* (our *-it); this rhyme is a case of “月質合韻 *Yuè Zhì héyùn* (月 *Yuè* and 質 *Zhì* rhyming together)”, and the two rhyme words must be reconstructed with different main vowels, no matter whose reconstruction one uses. But since *e and *i are both front vowels, it is easier to explain the rhyme if we reconstruct 設 *shè* < *syet* with *-et (in agreement with the paleographical evidence) than if we reconstruct it with *-at. That was the point of the passage Ho Dah-an translates.

2. “RETENTION OF OLD FEATURES IN XIÀOYÌ”

Ho Dah-an’s objection here relates to a parenthetical remark in a single sentence of the conclusion of our book, where we summarize the hypothesis that the rising tone (*shǎngshēng* 上聲) arises from an OC final *-ʔ, and the departing tone (*qùshēng* 去聲) from earlier *-h, itself from still earlier OC *-s. The sentence is:

There are in fact dialects in which the consonantal elements [ʔ] and [h] are still present (e.g., Xiàoyì 孝義, in Shānxī province; see Sagart 1999b: 132 and Guō Jiānróng 1989).
(B&S 2014: 318)

Ho Dah-an attempts to argue that these features in the Xiàoyì dialect are not retentions from an earlier, toneless stage of Chinese: rather, he says that “the presence of a glottal stop is due to the concave tone [312],” and that the final [h] is due to “pragmatic emphaticity (p. 186).” Actually, whether these features of the Xiàoyì dialect are retentions or later innovations is impossible to prove conclusively one way or the other, but since they are exactly the features predicted by Haudricourt’s model of the

origin of Chinese tones (Haudricourt 1954a, 1954b), and since both hypotheses are abundantly supported by other evidence, it seems plausible that they are indeed retentions. As already pointed out by Mei Tsu-lin (1970: 89), a number of other dialects also have a glottal stop in shǎngshēng, without a concave tone: for example, the lower rising tone of Pǔchéng 浦城 dialect has the contour [54], and ends “in a clear glottal stop when occurring before pause” (Norman 1969: 39).

Seemingly without having ever listened to Xiàoyì tones, Ho confidently asserts (pp. 186–187) that the glottal accident in the middle of the shǎngshēng (as in 馬, transcribed as [maʔa³¹²] by Guō Jiànróng, the author of the monograph on Xiàoyì dialect [Guō 1989], and a native speaker of the dialect) is really “middle laryngealization” of the kind that can sometimes be heard at the dipping point in the third tone of Pǔtōnghuà. However, “middle laryngealization” is only an occasional component of the Pǔtōnghuà shǎngshēng; it hardly ever gives the acoustic impression of a glottal stop. In contrast, a glottal accident heard as a glottal stop by both Guō and Sagart, is consistently present in Xiàoyì shǎngshēng words. To illustrate, in figures 1, 2, and 3, we provide images from the computer program PRAAT (Boersma and Weenink 2022) of the spectrograms, as well as pitch and intensity contours, of three Xiàoyì shǎngshēng words in Guō Jiànróng’s pronunciation. (The actual sound files are available online at <https://stan.hypotheses.org/2032> = Sagart 2022.)

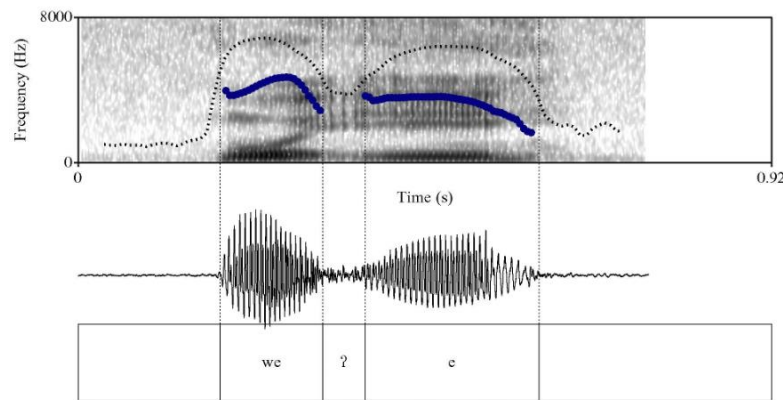


Figure 1 The syllable 委 [weʔe] in Xiàoyì: top: spectrogram, intensity (dotted line), pitch (thick line). Bottom: sound signal.

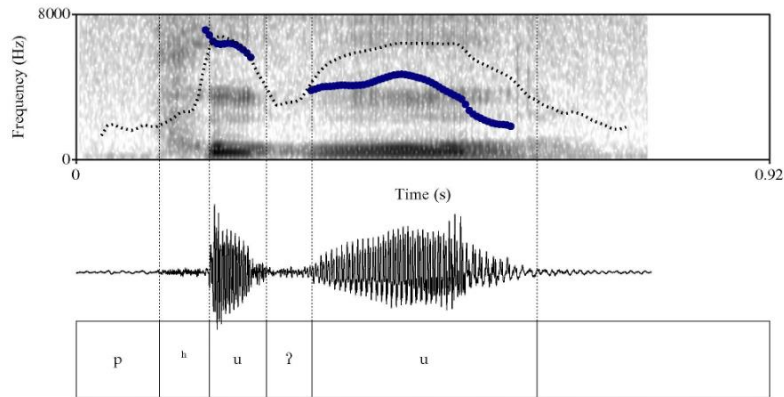


Figure 2 The syllable 普 [pʰuʔu] in Xiàoyì. Top: spectrogram, intensity (dotted line), pitch (thick line). Bottom: sound signal.

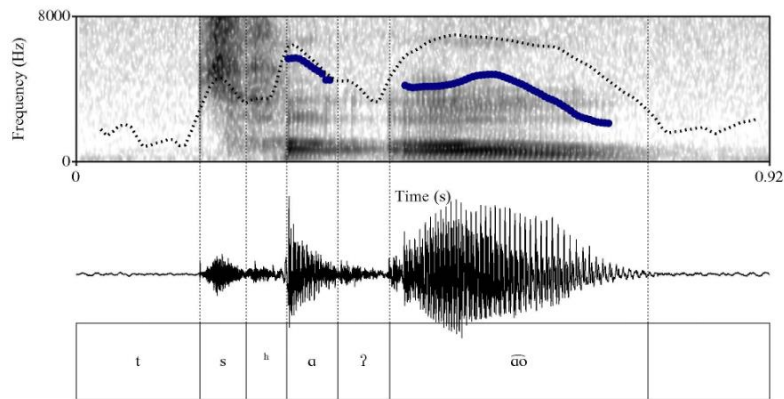


Figure 3 The syllable 草 [tsʰaʔa̯o̯] in Xiàoyì. Top: spectrogram, intensity (dotted line), pitch (thick line). Bottom: sound signal.

The signal shows a sudden drop in intensity and pitch, accompanied by interruption of periodicity for a duration of about 60–80 ms, in the middle of the main vowel, typical of a glottal stop or, in the case of fig. 1, possibly creaky phonation. Ladefoged and Maddieson (1996: 74) state that especially in intervocalic position, glottal stops are typically realized as an interval of stiff phonation (creaky voice).

As for Ho Dah-an's argument about the "pragmatic emphaticity" of the final [h], it is based on a peculiar interpretation of the original source, and reveals a basic misunderstanding of elementary phonology. He quotes Guō Jiànróng as follows:

When tone III executes a high fall, it is actually preceded by a slight rising, which can be notated as 453. *For emphatic effect* the falling part is also accompanied by slight aspiration. [去聲高降時實際上還帶一個略升的音頭，也可記為“453”；強調時，下降部分還有輕微的送氣作用。] (as quoted, p. 185; our emphasis)

Here Ho Dah-an's "for emphatic effect" translates "qiángdiào shí 強調時." This is an odd translation: a more natural interpretation would be simply "when stressed." Regardless of what determines when the aspiration occurs and when it does not, the fact that the phenomenon is restricted to qùshēng syllables means that it must depend on phonological features of those syllables, and cannot be attributed to pragmatics alone.

Ho further states that Zhū Xiǎonóng (2009) "clearly points out that Sagart mistook creaky voice for laryngealization." He is apparently referring to a passage (Zhū Xiǎonóng 2009: 8) where Zhū is criticizing Sagart (1986), in which modern dialects are used to support the hypothesis that qùshēng was at one point characterized by glottalized phonation. But the examples Zhū cites in his attempt to refute Sagart (1986) are actually examples of phonation types associated with shǎngshēng dialect reflexes, described in Mei (1970), and quoted (with approval) in Sagart's paper when discussing Haudricourt's hypothesis of a glottal stop at the origin of shǎngshēng. They are irrelevant to Sagart's hypothesis about qùshēng. Ho apparently did not read Zhū (2009) carefully enough to notice the confusion, and thus his remark has no force.

3. "USING PHONETIC TRANSLITERATION TO PROVE FINAL CODA"

This section of Ho Dah-an's review actually criticizes two points in our book: One is our use of Chinese transcriptions of foreign words to support the hypothesis that Old Chinese had a coda *-r, contrasting with *-n and *-j; the other is our reluctance to use Tibeto-Burman forms as evidence for reconstructing distinctions in Old Chinese. The second

criticism is accompanied by methodological discussions involving the history of Indo-European reconstruction. To deal with Ho's first objection, we must first present some historical background and sketch our argument for reconstructing an *-r coda.

The primary evidence in Chinese for an *-r coda in Old Chinese, distinct from both *-n and *-j, comes from some of the phenomena described in traditional Chinese phonology as *yīn-yáng duìzhuǎn* 陰陽對轉, which may be translated as “crossover alternations between vocalic-coda and nasal-coda rhymes.”¹⁰ For example, there are connections in both rhymes and phonetic series between words assigned to the traditional 微 Wēi and 文 Wén rhyme groups. With the same phonetic element:

- (6) 斤 *jīn* < *kj+n* [臻開三平欣見] “axe; catty”
 芹 *qín* < *gj+n* [臻開三平欣群] “cress”
 旂 *qí* < *gj+j* [止開三平微群] “banner; flag”

Now 斤 *jīn* and 芹 *qín* are traditionally assigned to the 文 Wén rhyme group, which is described as 陽 *yáng* because it has the nasal coda -n in Middle Chinese; but 斤 *jīn* is also the phonetic element in 旂 *qí*, which is sometimes assigned to the 微 Wēi group, described as 陰 *yīn* because it has the vocalic coda -j in Middle Chinese. Also, 芹 *qín* and 旂 *qí* rhyme with each other in Odes 222.2 (小雅·采菽) and 299.1 (魯頌·泮水). In Ode 182.3 (小雅·庭燎), we have the following rhyme sequence:

- (7) 晨 *chén* < *zyin* [臻開三平真船] “morning”
 輝 *huī* < *xjw+j* [止合三平微曉] “brilliant”
 旂 *qí* < *gj+j* [止開三平微群] “banner; flag”

There are parallel alternations between words in the traditional 元 Yuán and 歌 Gē groups, sometimes in alternative readings for the very same character:

- (8) 番 *fān* < *phjon* [山合三平元敷] “a turn, a time”
 also read *bō* < *pa* [果合一平戈幫] in the expression 番番 *bōbō*
 “martial”
 鼉 *tuó* < *da* [果開一平歌定] “alligator”
 also read *dan* [山開一平寒定]

暵 *xuǎn* < *xjwonX* [山合三上阮曉] “to dry in the sun”
 also read *xjweX* [止合三上紙曉] “fire”

Middle Chinese *-a* and *-jwe* here come from earlier **(w)aj*, so these examples also seem to reflect an early alternation of **-n* and **-j*.

Gōng Huángchéng ([1993] 2002) credits Walter Simon with the earliest proposal that words that had the coda *-j* in Middle Chinese (and words where an earlier final **-j* was lost, such as those with MC *-a* [歌開] or *-wa* [歌合]) must have had some final consonant in Old Chinese, which Simon (1929) reconstructed as a voiced fricative “*ḍ*” (i.e., [ḍ]). After initial doubts, Karlgren (1933) moved close to Simon’s idea by reconstructing final **-r* in the traditional 微 *Wēi* and 脂 *Zhī* rhyme groups, and in those words of the traditional 歌 *Gē* group that showed alternations with Middle Chinese *-n*.

It was Starostin (1989: 338–341) who first noticed that such alternations were limited to a subset of words with certain phonetic elements, including 番 *fān*, 單 *dān*, and 鮮 *xiǎn*, and that the same words tended to rhyme among themselves. He reconstructed a third coda **-r* in such words, distinct from both **-j* and **-n*, but he did not attribute the yīn-yáng duìzhuǎn phenomenon to random, irregular contacts between **-r* and **-n* words in general (as Karlgren seems to have done). Instead, he argued that OC final **-r* developed like **-n* in most dialects, but like **-j* in others. The unpredictable reflexes of OC **-r* in Middle Chinese—sometimes like OC **-j*, sometimes like OC **-n*—were explained in terms of dialect stratification: Middle Chinese had (at least) two layers, one in which OC **-r* evolved like **-n*, and another in which it evolved like **-j*. Starostin’s proposal was mentioned in Baxter (1992: 843 n. 209), but Baxter did not accept it at that time. We are now persuaded that Starostin was right.¹¹

In arriving at this solution, Starostin very clearly had the situation in Tibeto-Burman languages in mind. He knew that there exists comparative evidence for two final liquids in Tibeto-Burman, and this suggested what potential distinctions to look for in Old Chinese phonetic series and rhyme sequences. He cited examples of cognate words ending with **-r* in Tibetan and Lushai (= Mizo). But Tibeto-Burman evidence is not the ground on which he proposed that Old Chinese had an **-r* coda. That proposal was based on evidence from Chinese: a three-way distinction

he observed between final *-n, *-j, and a third phoneme, which he assumed to have been *-r, in Chinese phonetic series and rhyme sequences. This is consistent with our own statement on the use of external evidence:

- (9) It is perfectly legitimate to take hints from Tibeto-Burman (or anywhere else) when formulating hypotheses about Old Chinese.... But it would be a mistake to use Tibeto-Burman evidence to *test* hypotheses about Old Chinese. The fact that [r], [n], and [j] codas contrast in some Tibeto-Burman languages may raise the question of whether Old Chinese had a similar contrast, but only evidence from within Chinese (not excluding words in other languages borrowed from Chinese) can answer this question. (B&S 2014: 40)

Although he seems not to have mentioned Starostin's proposal, Gōng Huángchéng proposed a similar explanation four years later (Gōng [1993] 2002: 56, 58). He distinguished two historical stages of Chinese: Proto-Chinese (*Yuánshǐ Hànyǔ* 原始漢語) and Old Chinese (*Shàngǔ Hànyǔ* 上古漢語). According to him, Proto-Chinese had final *-n, *-l and *-r, inherited from Sino-Tibetan. He thought that by Old Chinese times, *-l and *-r had merged with *-n in most areas, although some regions kept them distinct. He believed that this Old Chinese diversity is at the root of this particular yīn-yáng duìzhuǎn phenomenon.

Although we admire the late Professor Gōng's contributions to Sino-Tibetan linguistics, we believe that his reasoning in this paper is flawed. He acknowledged that he found no evidence in Chinese for a distinction between final *-r and final *-l, either in xiéshēng characters or in *Shījīng* rhyming ([1993] 2002: 54–55). To reconstruct a Chinese or Proto-Chinese contrast between *-r and *-l on the basis of Tibeto-Burman evidence alone, and then to assert that Chinese *-r and *-l correspond to TB *-r and *-l, is circular reasoning. Professor Gōng's examples may well be valid Sino-Tibetan cognates, and they are probably good evidence for reconstructing final *-r and *-l in Proto-Sino-Tibetan. But they tell us nothing about how to reconstruct Old Chinese. As of now, reconstructing both final *-r and final *-l in Old Chinese is a hypothesis about Chinese for which no known Chinese evidence exists.

Ho prefers Gōng Huángchéng's theory to Starostin's, mainly

because Gōng presents more comparisons between Chinese and Tibeto-Burman languages. Now it is quite plausible that Proto-Sino-Tibetan had a contrast between final *-r and *-l, but we know that by the time of Middle Chinese, both are gone from the Chinese phonological system. So by these assumptions, both final *-r and final *-l were lost at some point between Proto-Sino-Tibetan and Middle Chinese. But no amount of Tibeto-Burman evidence can tell us when either was lost in the branch leading to Chinese. We believe there is sufficient Chinese evidence to support the reconstruction of final *-r in Old Chinese, but we have found no evidence for reconstructing a contrasting final *-l.¹²

And even if Proto-Tibeto-Burman had both final *-l and final *-r, they were not necessarily inherited unchanged from Proto-Sino-Tibetan in that branch either. In fact, Sagart (2018) argues that PST *-l and *-r were not retained unchanged in Proto-Tibeto-Burman: he proposes that Chinese words like 犬 “dog,” 水 “water,” and 唇 “lip” had *-r in Old Chinese, coming from PST *-r, but that this *-r changed to *-j after a high vowel in all Tibeto-Burman languages. Basing our reconstruction of final consonants in individual Old Chinese words on the testimony of Tibeto-Burman languages here would mislead us. So it would be an error to assume that our internally-based reconstruction of *-r clashes with Tibeto-Burman evidence. Nathan Hill documents a correspondence between our OC *-r, Tibetan -r, and Burmese zero; another between our OC *-r, Tibetan -l, and Burmese zero; another between our OC *-n, Tibetan -n, and Burmese -n; and yet another between our OC *-j, Tibetan zero, and Burmese -y (Hill 2014: 100).¹³

We do not advocate ignoring Tibeto-Burman evidence entirely; in fact we cite possible Tibeto-Burman cognates in a number of places in our book. But we must keep in mind that although reconstructions have been proposed for Proto-Sino-Tibetan and Proto-Tibeto-Burman, there is still considerable uncertainty about the phonology of both. The task of reconstructing Proto-Sino-Tibetan is ahead of us; the two pillars of Proto-Sino-Tibetan reconstruction will be Old Chinese and Proto-Tibeto-Burman. In order for any reconstruction of Proto-Sino-Tibetan to be solid, it is essential that Old Chinese and Proto-Tibeto-Burman be reconstructed independently of each other.

We now come to Ho’s criticism of our use of Chinese transcriptions

of foreign words to support the hypothesis that Old Chinese had a coda *-r. We noticed that when Chinese characters with Middle Chinese final -n are used in early texts to transcribe words with foreign [r], the same cluster of phonetic elements seems to be involved as those that show alternations between -n and -j (or -Ø from earlier *-j). Here, we cite only one example from our book; for others see B&S (2014: 260–264):

- (10) 單于 *chányú* < *dzyen-hju* [山開三平仙禪, 遇合三平虞云] < (Hàn) *dar-fīwa “Xiōngnú ruler” (from OC < *[d]ar + *_G^w(r)a)

Cf. Written Mongolian *daruḃa* “governor” (also borrowed into Persian as *dārūga* “governor,” see Doerfer [1963–1975: 1.319–1.323]. Presumably the original language was Xiōngnú; we do not mean to imply that the Xiōngnú were necessarily proto-Mongols.) (B&S 2014: 260)

Here there is independent evidence for reconstructing the phonetic element 單 with final *-r:

- (11) 單 *dān* < *tan* [山開一平寒端] < *Cə.tʰar “single, simple” is phonetic in
 鼉 *tuó* < *da* ~ *dan* < *[d]ʰar “alligator” (as in (8) above)

Examples like these had previously been explained by saying that since early Chinese had no final *-r, foreign [r] was transcribed by *-n, assumed to be the closest equivalent. If that were the case, then any word with Middle Chinese -n < *-n should be as likely as any other to be used in this fashion. But most examples seem to be confined to words written with phonetic elements that seem to show alternations between *-n and *-j. If some varieties of Chinese had final *-r down into the Hàn period, then we would expect these words with *-r to be used to represent foreign [r] at that time. That prediction is borne out: the phonetic elements of the characters involved appear to be chosen from among those reconstructed (for independent reasons) with *-r. It is true that the interpretation of such transcriptions can raise thorny problems because it is difficult to know exactly how they arose, and which languages may have been involved. We do not consider them the primary evidence for the *-r hypothesis, much less as “proof” of it. But the pattern seems too strong to be due to chance.

We now turn to Ho's methodological considerations as to how and whether evidence from sister languages should be used in reconstruction. In criticizing our point of view in (9), he draws upon Chén Zhōngmǐn (2007), in which two approaches to linguistic comparison are contrasted: Chén calls them “mesocomparison” (*céngjí bǐjiào* 層級比較, perhaps more clearly translated as “level-by-level comparison”) and “cross-comparison” (*kuàjí bǐjiào* 跨級比較, literally “level-skipping comparison”), which he advocates. What Chén (2007) calls cross-comparison actually includes two different practices:

1. skipping levels: that is, reconstructing higher levels in the family tree before the lower levels have been reconstructed; and
2. constructing a proto-language based on evidence from its sisters, not just its daughters.

Chén Zhōngmǐn illustrates his concept of cross-comparison by citing a number of examples. He quite correctly points out that many classic results of historical linguistics, such as Grimm's Law, Verner's Law, the explanation for the development of palatal consonants in Sanskrit, and the Indo-European laryngeal theory, were discovered by “skipping levels”: that is, by comparing individual languages of different subfamilies, before the ancestors of their subfamilies had been reconstructed. Thus Grimm's Law was based on comparing such individual languages as Greek, Gothic, and Old High German, before either Proto-Greek or Proto-Germanic had been reconstructed.

Ho Dah-an concludes from what we say about the use of Tibeto-Burman that we reject all “cross-comparison,” and that our methodology would thus require us to reject Verner's Law and the laryngeal theory. He concludes that our “perception and methodology ... fall behind Verner's and de Saussure's by more than a century.”¹⁴ Actually, while we object to reconstructing a proto-language based on its sisters, as noted above, nothing we say in (9) prohibits skipping levels, as should be clear to any attentive reader of our book. For example, in reconstructing Old Chinese, we make use of data from individual languages such as Cantonese or Méixiàn Hakka without reference to Proto-Cantonese or Proto-Hakka. And of course, we do not reject either Verner's Law or the laryngeal theory, nor would anything in our methodology motivate us to do so.

As another example of a classic linguistic hypothesis that our

assumptions supposedly require us to reject, Ho Dah-an discusses the Indo-European laryngeal theory. This example involves “cross-comparison” only in the sense of skipping levels, to which (as explained above) we have no objection. That is, Saussure (1879) arrived at his original hypothesis by comparing individual ancient languages like Greek, Latin, and Sanskrit, without first having reconstructed Proto-Greek, Proto-Italic, Proto-Indo-Iranian, etc. What Ho Dah-an says about the laryngeal theory is revealing:

An inspirational example to consider is the theory of laryngealization [sic]. This theory attempts to resolve the irregular correspondences during the stage of Proto-Indo-European. None of the highest level proto-languages, such as Greek and Sanskrit, could offer any internal evidence for a solution. In 1879, based on mathematical principles, Ferdinand de Saussure predicted that in the context of irregular correspondences, there might have been an element that had been lost, which he called “coefficients sonantiques” and indicated with an H, suggesting that this element might be related to laryngeals.... What the theory of laryngealization tells us is that however similar the reconstructed results are, we are never allowed to go beyond the language family for more evidence. (pp. 195–196)

In these few sentences, Ho Dah-an makes more errors than he claims to have found in our whole book:

1. The laryngeal theory involves the reconstruction of laryngeal consonants, not “laryngealized” consonants.

2. Unlike Verner’s Law, the primary motivation for the laryngeal theory was not to explain seemingly irregular correspondences between languages, but rather to account for morphological alternations between long and short vowels in roots within the same language, such as the long vowel in Greek *stē-* (from earlier **stā-*) “stand,” as in *hí-stē-mi* “I stand,” and the short vowel in *sta-tós* “stood, placed.” Another Greek example is *dō- ~ do-* “give,” with a long vowel in *dí-dō-mi* “I give” but a short vowel in *do-tós* “given.” In current notation, the reconstructions are *stē-* < **stā-* < PIE **steh*₂- vs. *sta-* < PIE **sth*₂-, and *dō-* < PIE **deh*₃- vs. *do-* < PIE **dh*₃- (Fortson 2004: 71–72). Here, **h*₂ is the “a-colored” laryngeal, and **h*₃ is the “o-colored” laryngeal; reconstructing them in this way makes these stem alternations parallel to alternations where the root contains a

semivowel like *i or *u (or other resonants, which we pass over here), as in Greek *leip-* (< *léik^{w-}) vs. *lip-* (< *lik^{w-}) in *léip-ō* “I leave” vs. *é-lip-on* (< *lik^{w-}) “I left”; or in *péuth-* (< *b^héud^{h-}) vs. *puth-* (< *b^hud^{h-}) in *péuth-ō* “I persuade” vs. *é-puth-on* “I persuaded”: In each pair there is a full-grade form with *e and a zero-grade form without *e.¹⁵

3. It is not true that neither Greek nor Sanskrit “could offer any internal evidence for a solution”; it was precisely by analyzing such internal evidence that Saussure came up with his proposals.

4. The only “mathematical principle” involved in Saussure’s argument was simple analogy, reconstructing Greek *stē- ~ sta-* and *dō- ~ do-* by analogy to *leip- ~ lip-* and *peuth- ~ puth-* (unless it counts as a “mathematical principle” that Saussure used “x” to represent an unknown element, which he does in a few places).

5. Saussure reconstructed two “coefficients sonantiques,” not one (which is why the expression is plural). (Recent formulations usually reconstruct three.)

6. Saussure wrote them as “A” and “Q” (corresponding to *h₂ and *h₃ in current notation), not “H.”

7. Saussure did not suggest that his “coefficients sonantiques” were related to laryngeals; he maintained that they were vocalic.¹⁶

8. Finally, Ho’s statement that “What the theory of laryngealization tells us is that however similar the reconstructed results are, we are never allowed to go beyond the language family for more evidence” is confused and confusing. We suppose he means that it was a mistake to identify the Indo-European laryngeals with those of Semitic (as some linguists did), because Indo-European and Semitic are different language families. If so, then his statement is irrelevant, because it presupposes that the boundaries of each language family were known in advance. Those claiming a connection with Semitic (who did not include Saussure, as far as we know) were arguing that Indo-European and Semitic did belong to the same family.

4. “午 AND 五 NOT HOMOPHONOUS”

The two words 五 *wǔ* “five” and 午 *wǔ* “7th earthly branch” are homonyms in Middle Chinese (both *nguX* [遇合一上姥疑]) as well as modern Chinese, and they have generally been reconstructed as homonyms for Old Chinese as well. But the fact that two words had the same initial in

Middle Chinese does not necessarily mean that they had the same initial in Old Chinese also. On the basis of xiéshēng and word-family evidence, we hypothesize that MC *ng-* [疑] could come not only from velar nasals *ŋ- and *ŋʰ-, but also from uvular initials preceded by nasal preinitials.

The reasoning is as follows. As a phonetic element, 五 *wǔ* < *C.ŋʰaʔ¹⁷ seems to be used only to write other words that show up with MC *ng-* [疑] (including some whose apparent Tibeto-Burman cognates also begin with a velar nasal). But some cases of MC *ng-* seem to have a more complex origin. For example, we suspect that the forms in (12) are from the same root, which we reconstruct as **g^w(r)aj*, and that MC *ng-* in this case comes from the uvular **g^w-* preceded by a nasal prefix **N-*:

- (12) 為 *wéi* < *hjwe* [止合三平支云] < **g^w(r)aj* “make, do, act as”
 偽 [*wěi*]¹⁸ < *ngjweH* [止合三去寘疑] < **N-g^w(r)aj-s* “(made up?)
 false”

Similarly, it seems likely that the MC *ng-* in 譌 ~ 訛 *é* < *ngwa* comes from a related root **q^{whs}aj*:

- (13) 譌 ~ 訛 *é* < *ngwa* [果合一平戈疑] < **[m]-q^{whs}aj* “deceive, false”
 化 *huà* < *xwaeH* [假合二去禡曉] < **q^{whs}<r>aj-s* “transform”

This reconstruction makes it possible to analyze the forms in (12) or (13) as forms from the same root, and it accords with our general strategy of trying to analyze word-family relationships in terms of roots and affixes.

Based on similar evidence, we argued in our book that 五 *wǔ* “five” and 午 *wǔ* “7th earthly branch” had different Old Chinese initials (B&S 2014: 128–130):

- (14) 五 *wǔ* < *nguX* [遇合一上姥疑] < **C.ŋʰaʔ* “five”
 午 *wǔ* < *nguX* [遇合一上姥疑] < **[m].q^haʔ* “7th earthly branch”

Our assumption is that at some point in or after the Old Chinese period, certain nasal + uvular onsets merged with the original velar nasals *ŋ- and *ŋʰ-. After that change happened, we would expect 五 *wǔ* “five” and 午 *wǔ* “7th earthly branch” to be interchangeable as phonetic elements in the script; but before that time, we would expect that they would be kept

separate. In fact, this seems to be the case (for details see B&S 2014: 128–130).

For example, previous reconstructions of 杵 *chǔ* < *tsyhoX* “pestle” (Karlgren “*t’io,” Dǒng Tónghé “*k’iag,” Schuessler “*tha?”) have been unable to account for the presence of 午 *wǔ*, presumably a phonetic element, in the character: both the MC initial *tsyh-* [昌] of 杵 *chǔ* “pestle” and the initial /k^h/ in /k^hy 3/, the word for “pestle” in the Shíbēi 石陂 variety of Mǐn (Akitani 2004: 81), are difficult to derive from an Old Chinese velar nasal initial. But it works well to reconstruct the initial as *t.q^h-:

(15) 杵 *chǔ* < *tsyhoX* [遇合三上語昌] < *t^ha? < *t.xa? < *t.q^ha? “pestle”

We assume that *t.q^h- eventually changed to *t.x- and merged with original *t^h-, becoming MC *tsyh-* [昌]. (The development in Shíbēi is plausibly *t.q^h- > *t.k^h- > /k^h-/) We have independent reasons for reconstructing preinitial *t- as part of the Old Chinese phonological system, as in 肘 *zhǒu* “elbow”:

(16) 肘 *zhǒu* < *trjuwX* [流開三上有知] < *t-[k]<r>u? “elbow”

The 寸 *cùn* element on the right of 肘 is ultimately from the phonetic element 九 *jiǔ* < *kjuwX* < *[k]u? “nine,” which was probably originally a pictograph for “elbow” (Jì Xùshēng 2010: 374–375, 991).

Another way of reconciling the MC *ng-* of 午 *wǔ* < *nguX* with the *tsyh-* of 杵 *chǔ* < *tsyhoX* might be to reconstruct the latter with “*t.ŋ̊-,” assuming a development like *t.ŋ̊- > *t.x- > *t^h-. But other words written with 午 *wǔ* as phonetic seem to have uvular connections, with *q^h- or *q^hs-:

(17) 許 *q^h(r)a? > *xjoX* [遇合三上語曉] > *xǔ* “place (n.),” which is probably related to
 所 *s-q^h<r>a? > *srjoX* [遇合三上語生] > *suǒ* “place (n.); that which”; note that the phonetic element in 所 *suǒ* is
 戶 *m-q^ha? > *huH* [遇合一去暮匣] > *hù* “to stop, to check; door”.
 Probably from the same root as 許 *xǔ* and 所 *suǒ*, we have
 處 *t.q^ha? > *tsyhoX* [遇合三上語昌] > *chǔ* “be at,” 處 *t.q^ha?-s > *tsyhoH* [遇合三去御昌] > *chù* “place (n.)”, where the phonetic element 處 also probably has a uvular initial:
 處 *q^hra > *xu* [遇合一平模曉] > *hū* (dialect development: *q^hr- > r^s- > x-) “tiger”

Here 許 $xǔ < xjoX$ could be reconstructed with a voiceless velar nasal as $*\eta(r)a?$ (as in Baxter 1992: 208—“*hng(r)ja?” in the notation there), but it would be difficult to derive the MC initial $h-$ [匣] in the phonetic element 戶 $hù < huX$ from a velar nasal.

Finally, if 午 $wǔ$ and 五 $wǔ$ really were originally homonyms, we would expect that they could be used more or less interchangeably as phonetic elements; but this seems to happen only in the later part of the Old Chinese period (see below). So our reconstructions 五 $wǔ < nguX < *C.\eta^s a?$ “five” and 午 $wǔ < nguX < *[m].q^{hs} a?$ “7th earthly branch” are plausible, and consistent with our other assumptions.

As a supposed counterexample to our claim that 午 $wǔ$ and 五 $wǔ$ were not interchangeable as phonetic elements in the early script, Ho cites parallel passages from two bronze vessels: the *Máo gōng dǐng* 毛公鼎 and the *Shī Hōng* (or *Xún*) *guī* 師甸簋. Both are dated to the late Western Zhōu period:¹⁹

(18) *Máo gōng dǐng*



Shī Hōng (Xún) guī



In both cases, the second character has 五 $wǔ < MC nguX$ “five” as phonetic. Ho Dah-an interprets the first two characters as standing for 捍禦 $hàn yù < MC hanH-ngjoX$ [山開一去翰匣, 遇合三上語疑] “defend,” and interprets the whole phrase as 捍禦王身 $hàn yù wáng shēn$, meaning “defend the king’s person.” Ho Dah-an’s point is that while in 吾 and 菩, the phonetic

element is 五 *wǔ*, in 禦 *yù* it is ultimately 午 *wǔ* (see Ji Xùshēng 2010: 130); and he concludes that 五 and 午 were interchangeable as phonetic elements as early as the *Máo gōng dǐng* (i.e., as early as Western Zhōu).


But this would be a counterexample to our hypothesis only if both 五 and 午 were used to write the same word in the early Old Chinese period. In fact, although the character 禦 *yù* is found in oracle-bone inscriptions as the name of a sacrifice, its use to write *ngjoX* [遇合三上語疑] “defend, guard” is late. The *Shuōwén* glosses the character 禦 *yù* as “sacrifice” (“祀也”), and has a separate entry for 敵 *yǐ* < *ngjoX* [遇合三上語疑], glossed as “禁也” (“to prohibit”), to which Duàn Yùcái adds this note: “敵為禁禦本字，禦行而敵廢矣” (“敵 *yǐ* is the original character for {禦} *yù* in the sense of 禁 “prohibit”; [the character] 禦 *yù* came into use, and then 敵 *yǐ* was abandoned.”) If Duàn Yùcái is right, then 吾 and 菩 in the *Máo gōng dǐng* and *Shī Hōng (Xún) guǐ* write {敵} in the sense of “to guard, protect, prohibit,” consistent with our hypothesis; we would predict that the character 禦 came to be used in this sense only after the onset of 午 *wǔ*, originally *[m].q^hs-, had become a velar nasal *ŋ^s-.²⁰





Ho Dah-an also denies that 午 *wǔ* and 五 *wǔ* wrote different words and represented different things. Relying in part on Zhāng Shìchāo (2011), he says:

“[B]oth 乂 and 𠄎 mean crisscrossing. One depicts the crossing of chopsticks; the other depicts the crossing of silk threads. Chopsticks are hard in substance, so they are written with straight strokes, and their crisscrossing becomes 乂. Silk threads are soft, so they are written with curved strokes, and their crisscrossing becomes 𠄎. 乂 and 𠄎 are actually the same word. Their shape is determined by the hardness or softness of the associated substance. Since they are the same word, their pronunciation is also the same. That their pronunciation was both indicated with 疑古 [as a fānqiè spelling, i.e. MC *ngi* + *kuX* = *nguX*] in *Guangyun* is the most powerful internal evidence.”²¹ (p. 201).

However, Zhāng Shìchāo’s view, quoted in Ho’s footnote 26 and translated by Ho in his text, is actually rather different from Ho Dah-an’s: Zhāng argues that 乂, used in early texts for {五} *wǔ* “five,” originally represented the idea of “crossing,” and its use to write “five” is a loan use.

Once 𠂇 came to be used for “five,” then the character 午 (supposedly a homonym of 五) was used instead of 𠂇 as a loan for the word meaning “crossing.” Zhāng does assume that 五 *wǔ* and 午 *wǔ* were homonyms, or at least similar in pronunciation, but he agrees with most paleographers that the character 午 *wǔ* was originally a pictograph for {杵} *chǔ* “pestle,” and he does not say that 五 *wǔ* and 午 *wǔ* were originally the same character, much less the “same word.”

Ho Dah-an concedes that “there are still quite a few scholars of the opinion that 午 originally means 杵.” He is certainly right about this.²² But this is, he says, because they are unaware that the character originally represented “the crossing of silk threads,” and at a certain stage of the development of the script, took on the form , which happened to resemble a pestle; it was only then that it came to be used to write {杵} *chǔ* “pestle”—a fact that was “never made known publicly by previous scholars.”

In fact, in paleographic documents, the character 午 occurs in two main graphic shapes: /  and . A form like the latter is found already in a late Shāng bronze inscription; it is usually considered a graphic development from the earlier form .²³ The clearest evidence that this form represents a pestle is its presence in early forms of the character for {舂} *chōng* “to pound with a pestle,” as in the form below, which depicts two hands holding a pestle over a mortar (臼 *jiù*). This form appears in a mid-Zhōu bronze inscription:²⁴

(19)



Both 午 and 五 occur very frequently in early materials, often in the same inscriptions, and as far as we know they are always clearly distinguished.

In this section Ho Dah-an does find one genuine error in our book, when we say that 五 *wǔ* “five” is a sound gloss in the *Shuōwén*’s entry for 午 *wǔ* “7th earthly stem” (p. 207–208): The relevant passage from the *Shuōwén* is given below; our reconstructions of the relevant items are given below the quotation:²⁵

- (20) 午：𠄎也。五月陰气午逆陽，冒地而出
 午 [*nguX*] means “oppose” [𠄎 *nguH*]. In the fifth [五 *nguX*] month, the dark qì opposes [午 *nguX*] and goes against [逆 *ngjaek*] the bright, covers the earth, and comes out (Dīng Fúbào [1931] 1976: 6639b).
 午 *nguX* [遇合一上姥疑] < *[m].q^haʔ “seventh earthly stem”
 𠄎 *nguH* [遇合一去暮疑] < *ŋ^sak-s “oppose.”
 五 *nguX* [遇合一上姥疑] < *C.ŋ^saʔ “five”
 逆 *ngjaek* [梗開三入陌疑] < *ŋrak “go against”

Here, 𠄎 *wù* < MC *nguH* is clearly a sound gloss for 午 *wǔ* < *nguX*, and we took 五 *wǔ* < *nguX* in the gloss to be a sound gloss also. (Sound glosses like this are frequent in *Shuōwén* entries.) But as Ho points out, our interpretation of 五 *wǔ* was wrong: nearby entries in the *Shuōwén* show that the “fifth month” here is part of a numerical pattern that includes nearby entries for other earthly stems (*dìzhī* 地支): The earlier entry for the sixth earthly stem 巳 *sì* refers to the fourth month, and the later entry for the eighth earthly stem 未 *wèi* refers to the sixth month. The reason we overlooked these nearby entries is that in our database, *Shuōwén* entries for different words are stored in different records, thus becoming detached from their immediate context; and we failed to notice this.

However, the error does not affect the point we were making, which was just that although we reconstruct 五 *wǔ* and 午 *wǔ* differently for early Old Chinese, they had probably become homonyms (or near homonyms) “at least by the time of the *Shuōwén*” (B&S 2014: 130)—a fact that is already sufficiently clear from the double phonetic in 𠄎 *wù* < *nguH* [遇合一去暮疑]. In fact, the use (cited by Ho Dah-an) of the character 𠄎, with the same two phonetics, in the Qín stone drums (*Shí gǔ wén* 石鼓文) to write {吾} probably indicates that the relevant sound change had already happened, at least in certain areas, several centuries earlier.²⁶ Finding this mistake does nothing to refute our reconstruction of 午 as *[m].q^haʔ for an earlier stage of Old Chinese.

5. “CALLING A HORSE A DEER”

We reconstruct “deer” as follows:

- (21) 鹿 *lù* < *luwk* [通合一入屋來] < *mə-r^sok “deer”

This example is cited in two places in our book: the first place is example (95) (p. 56, in chapter 3), which we reproduce here:

- (22) 鹿 *mə-r^sok > *luwk* [通合一入屋來] > *lù* “deer”; cf. Bùyāng 布央 /ma 0 lək 8/²⁷ “deer” (Lǐ 1999: 199)

Ho Dah-an spends eight pages attempting to refute our reconstruction of 鹿 *lù* “deer” and to argue that it reflects incompetence on our part, if not deliberate exclusion of contrary evidence. His main arguments are: (1) that the Bùyāng form /ma 0 lək 8/ (phonetically [ma⁰ lək¹¹]) is our only evidence for reconstructing *mə- in this word; (2) that the Bùyāng form must be a loanword from Zhuàng 壯 (a Dai = Tai language also spoken in the region where Bùyāng speakers live) because of the [11] tone on [lək 11]; (3) that the first syllable [ma 0] is a prefix added as an innovation within Bùyāng, and thus cannot be borrowed from Chinese. He speculates about why we would not recognize these facts: perhaps we read Lǐ Jīnfāng (1999) in a “rough and incomplete manner,” or perhaps we cited it selectively, ignoring the parts that do not support our ideas.

As for the first point, Ho Dah-an says this about our example (95), quoted in (22) above: “According to the style of the entire book, this example means that, the reason for reconstructing an *mə- prefix for “deer” is because there is a Chinese loanword /ma0/ in the Bùyāng language” (p. 211).²⁸ Later he says: “Since from example (95) this [i.e. the Bùyāng form] is the only piece of internal evidence, the authors must have regarded it as de facto direct proof” (p. 212).²⁹

These statements are false. The main discussion of the *mə- prefix is in Chapter 4 (“Old Chinese onsets”), not chapter 3 (“An overview of the reconstruction”), where the quoted example (95) occurs. As explained in chapter 4 (B&S 2014: 178–180), the reason for reconstructing *mə- in 鹿 *lù* < *mə-r^sok “deer” is that 鹿 *lù* is one of several words where some Mǐn dialects have an initial /t/ or /d/ corresponding to Middle Chinese initial *l*- [來]. We gave four examples of this correspondence (including this one) on p. 179. We propose that such cases reflect OC *mə.r- or *mə.r^s-, where the *mə. is lost in Middle Chinese, giving initial *l*-; but that in Mǐn, the reflex appears to be Norman’s Proto-Mǐn softened initial *-d-. The Bùyāng word offers possible support for this hypothesis, since its form agrees with

what our system already suggests, based on the Mǐn dialect data. As far as we know, previous reconstructions of Old Chinese have not attempted to account for the Mǐn forms that have initial /t/ or /d/ corresponding to Middle Chinese initial *l*- [來]. Several of these words have xiéshēng or other evidence of an initial Old Chinese *m- before the *r- or *rʰ- that is the source of MC *l*- [來], as shown in (23):

- (23) 鯉 *lǐ* < *liX* [止開三上止來] < *mə-rəʔ “carp (n.)”: cf. Zhènqián 鎮前 /ty 5/, Yǒng’ān 永安 /ti 4/, Jiàn’ōu 建甌 /ti 4/ (Norman 1996, 36), Shíbēi 石陂 /di 5/ (Akitani 2004: 79)
- 埋 *mái* < *meaj* [蟹開二平皆明] < *m.rʰə ~*mə.rʰə ? “bury”: cf. Zhèyáng 柘洋 /tai 2/, Fú’ān 福安 /tai 2/, Níngdé 寧德 /tA:i 2/ (Norman 1977–1978: 340); Xiàmén 廈門 /tai 2/ “bury”
- 蠣 *lì* < *ljejH* [蟹開三去祭來] < *mə-rat-s “oyster”:³⁰ cf. Fúzhōu 福州 /tie 6/ (Féng Àizhēn 1998: 100); cf. the Mandarin two-syllable expression 牡蠣 *mǔlì* < *muwX-ljejH* [流開一上厚明, 蟹開三去祭來] “oyster”
- 鹿 *lù* < *luwk* [通合一入屋來] < *mə-rʰok “deer”; cf. Jiēyáng 揭陽 /tek 8/ and Bùyāng (Lángjià 郎架 dialect) /ma 0 lək 8/ “deer”³¹

Notice that both the characters 里 and 萬 are both used as phonetic elements for words with MC initial *m*- [明]. The dental reflexes in Mǐn are sporadic, probably because of dialect mixture, but they are reminiscent of the reflexes of Norman’s Proto-Mǐn “softened” voiced *-d.

Our hypothesis is that the connection of these words with initial *m*- is not a coincidence: That Norman’s Proto-Min *-d can reflect Old Chinese *mə-r- or *mə-rʰ-. The Bùyāng word may be an early loan from Chinese, borrowed at a time when the initial minor syllable was still present in Chinese; this could help account for the initial /t/ in the Jiēyáng form. This hypothesis may or may not turn out to be correct, but it suggests a direction for research: are there other examples of Mǐn /t/ or /d/ corresponding to MC *l*-, and do they also show connections with initial *m-? It was this kind of thinking that led us to the Bùyāng form: because of the apparently irregular initial in the Jiēyáng form /tek 8/ “deer,” our hypothesis led us to look for connections of 鹿 *lù* with initial *m*-, and we found a possible connection in Bùyāng. Ho Dah-an’s statement that the Bùyāng form is “the

reason” for reconstructing *mə- shows that he either did not read pages 178–180 or did not understand them.

Ho’s second argument, that the [lɔk¹¹] of Bùyāng /ma 0 lɔk 8/ must be a loanword from Zhuàng, is based on a misunderstanding of a passage from Lǐ Jīnfāng (1999: 17), which Ho translates as follows:

There are six tones in the Lángjià speech. Some prefixes are unstressed with neutral tone, which does not count as a tone. [...] Rhymes with stop codas occur only in tone 11 and 54, and a few also in tone 31. These are Zhuàng loanwords. (p. 211)

From this Ho Dah-an concludes that Bùyāng words with tones [11], [54], and [31] are *all* loanwords from Zhuàng; so he concludes that the /lɔk 8/ in Bùyāng /ma 0 lɔk 8/ [ma⁰ lɔk¹¹] “deer” must be a Zhuàng loanword.

But here is Lǐ Jīnfāng’s original text (1999: 17):

郎架話有六個聲調，部分前綴念輕聲，不計在內。[...] 塞聲韻只出現在 11、54 調上，少數出現在 31 調，是壯語的借詞。

The Lángjià dialect has six tones, not including the neutral tone that occurs in some prefixes.... Rhymes with stop codas occur only in tone 11 and 54, and a few also in tone 31. These are Zhuàng loanwords.

As Jacques (2017) points out, Ho Dah-an has parsed Lǐ Jīnfāng’s sentence incorrectly. Ho thinks the verb phrase “是壯語的借詞”(“are Zhuàng loanwords”) refers to all Bùyāng words with tones [11], [54], or [31]. This would mean that all stop-final syllables in Bùyāng are loans from Zhuàng. But Lǐ Jīnfāng only meant that words with stop codas in tone [31] are loans from Zhuàng. In fact, from Kra comparative linguistics it is clear that Bùyāng retains Proto-Kra stop codas in inherited words, and that the modern tones associated with syllables having stop codas are [54] for syllables with voiceless initials and [11] for syllables with voiced initials (Jacques 2017). This is clearly indicated, for example, in Ostapirat’s table of reflexes of Proto-Kra tones (Ostapirat 2000: 115). Tone [11] in [ma⁰ lɔk¹¹] “deer” is the expected tone for a syllable ending in a stop and beginning with a sonorant. So Ho Dah-an’s argument that [lɔk¹¹] in Bùyāng

[ma⁰ lək¹¹] must be a loan from Zhuàng because of its [11] tone is based on a hasty misreading of Lǐ (1999) that is inconsistent with basic facts about Kra linguistics.

Ho Dah-an's third argument is that /ma 0/ is a prefix in the Bùyāng form. As evidence he cites Lǐ Jǐnfāng and Zhōu Guóyán (1999: 59), which he quotes as below (p. 213 in Ho Dah-an's review):

The prefixes found in many Bùyāng words are not a retention of an earlier form but develop later in order to discriminate word meanings. What best illustrates this situation are words recently borrowed from Zhuàng or Chinese. (The syllabic pattern of prefixes are either intrinsic or of a later development.) Originally these words are without prefixes, but to avoid homophony, a prefix is added to them when they are borrowed into Bùyāng.

Here it is necessary to clarify the sense of the term “prefix” (*qiánzhuì* 前綴) as it is used by Ho Dah-an, Lǐ Jǐnfāng and Zhōu Guóyán, and generally by linguists writing descriptions of Kra-Dai languages in Chinese. On the one hand, the term applies in Bùyāng to what we (in English) call prefixes: morphemes of the shape CV(C), tonal or toneless, which occur before full monosyllabic morphemes. Examples in Lángjià Bùyāng are /li 0/- in animal names; /la:k 0/- in names of young humans and animals; /ma:k 0/ in names of fruits and other roundish things. On the other hand, these authors also use the term *qiánzhuì* to refer to a limited set of toneless and phonologically reduced syllables of the shape CV, without any detectable lexical meaning or morphological function, for instance in [qa⁰ lip¹¹] “hoof,” [ma⁰ tən³¹²] “wasp,” [ta⁰ qu⁵⁴] “turtle dove.” Lǐ Jǐnfāng himself made an important discovery about the origins of these minor syllables: He compared Proto-Austronesian disyllables such as *maCa “eye,” *manuk “bird,” and *maCəy “to kill” with Lángjià Bùyāng [ma⁰ ta⁵⁴] “eye,” [ma⁰ nuk¹¹] “bird,” and [ma⁰ tɛ⁵⁴] “to die, to kill” and insightfully noted that the Bùyāng “prefixes” in these forms continue the first syllables of earlier disyllabic words (Lǐ 1998: 195).³² In other words, they are not separate morphemes, and thus not true prefixes in the usual sense of the term.

In words without clear Austronesian etymologies, however, Lǐ and Zhōu do indeed hold the view that initial syllables were added to some

roots “to discriminate meaning”: They assume that as words eroded due to phonetic evolution, speakers selected CV syllables, apparently at random, and put them before words to make up for phonetic erosion.

We do not believe that languages randomly add meaningless syllables to their words just “to distinguish meaning” or “to compensate for sound change.” When normal phonetic evolution results in exceedingly short or confusing forms, several strategies are available to give them additional phonetic flesh: addition of (true) affixal morphemes; compounding; or replacement by a longer form through semantic shift or borrowing. Thus through regular phonetic evolution, Latin *apis* “bee” ought to give forms like [ep], [ef], [e] in French dialects; but most dialects either show replacement by a suffixed form, e.g. standard French *abeille* [abej] < Lat. *api-cula* “small bee”; or a compound like [honey-fly]; or replacement by a word originally meaning “bird” (Gilliéron 1918). No French dialect adds randomly selected phonemes or syllables.

It is moreover abundantly clear that even in words without Austronesian etymologies, Bùyāng’s pseudo-prefixes are not recent additions: Jacques (2017) worked out their correspondences in other Kra-Dai branches, finding in particular that *ma*⁰- corresponds to various labial consonants in Proto-Tai; while *qa*⁰- corresponds to aspiration in Proto-Tai if the following consonant is a nasal, and to *k- if it is *l-. So, both in inherited words with Austronesian etymologies and in those without them, Bùyāng pseudo-prefixes are inherited. But we believe that Bùyāng /ma 0 lək 8/ “deer” is not an inherited word, but a Chinese loanword. Even if natural languages randomly added phonetic material “to distinguish meaning,” it would not make sense for Bùyāng to add such material only to loanwords. Where does the /ma 0/- come from?

Our answer is that the /ma 0/- reflects Old Chinese minor syllable *mǎ-, the loosely attached form of prefix *m₃- occurring in animal names (B&S 2014: 55–56). In our Old Chinese reconstruction, some words were disyllables, with a main syllable preceded by a minor syllable of the shape /Cə/, structurally similar to what is found in Bùyāng, and very commonly in other Kra-Dai languages around the time of first contact with Chinese. The inherited minor syllables of early Kra-Dai languages were perfectly adapted to render the minor syllables (both loosely and tightly attached) of late Old Chinese.

Ho Dah-an summarizes his argument as follows:

[T]he /ma⁰ lok⁸/ in Bùyāng is borrowed from the Zhuàng *ma⁴ lok⁸*, which in turn is borrowed from Middle Chinese 馬鹿. The *ma⁴* in Zhuàng is a content word for “horse,” whereas the nominal prefix ma⁰ in Bùyāng is added only after the term is borrowed into the language. So this ma⁰ is not related to the *ma⁴* in Zhuàng, nor to the Old Chinese *mǎ-. (p. 217)

To summarize our response: (1) Having overlooked our main argument for *mǎ- in “deer,” based on Mǐn dialect evidence, (pp. 178–180), Ho Dah-an incorrectly assumed that the Bùyāng word is our only evidence for *mǎ- in “deer”; the more important evidence is the presence of a dental initial in Mǐn words for “deer,” corresponding to MC initial *l-* [來], which Ho ignored. (2) His conclusion that the Bùyāng word must be a loan from Zhuàng (and not from Chinese, as we claim) is based on a misreading of Lǐ Jǐnfāng (1999: 17), and reflects an ignorance of basic facts about the Kra-Dai languages. (3) We find it implausible that Bùyāng borrowed the Zhuàng word *ma⁴ lok⁸* but ignored the first syllable, and then added a meaningless but coincidentally similar initial syllable /ma⁰/ to avoid homophony (with some unspecified word). We consider it much more plausible that Bùyāng /ma⁰ lok⁸/ “deer” is an early borrowing from Chinese *mǎ-r⁶ok, where the /ma⁰/ syllable in Bùyāng reflects the Chinese prefix *mǎ- which we reconstructed in this word for independent reasons.

Ho Dah-an spends several pages on the Zhuàng word /ma⁴ lok⁸/ “deer” or “red deer.” His main point seems to be to deny that the first syllable is a prefix in Zhuàng: rather, he says it is the content word /ma⁴/ “horse.” He says that Zhuàng /ma⁴ lok⁸/ was borrowed from Chinese 馬鹿 *mǎlù*, literally “horse deer,” and that it refers to *Cervus elaphus*, “red deer” in English. We are unclear why this is relevant (unless it is to argue that only the second syllable of Bùyāng /ma⁰ lok⁸/ can have been borrowed from Zhuàng, and that the first syllable /ma⁰/ must have been added later). We make no claims about this Zhuàng word, and do not mention it at all in our book. It is possible that the Zhuàng word is a late loan from Chinese, as he says; another possibility, which seems plausible to us, is that, like Bùyāng /ma⁰ lok⁸/, it was originally an early borrowing from Chinese *mǎ-r⁶ok “deer,” later modified to /ma⁴ lok⁸/ “horse deer”

either by folk etymology or under the influence of the Chinese word 馬鹿 *mǎlù* “red deer.”

6. “A MOUNTAIN IS A RIVER BANK”

In this case, Ho Dah-an has actually found a second error. On page 148, in example (596), we give the reconstruction of 山 *shān* < *srean* [山開二平山生] “mountain” as *s-ŋrar, citing the *Shì míng* 釋名; and on page 391, note 76, we quote the *Shì míng* gloss for 山 “mountain” as follows, citing “(Hǎo Yìxíng et al. 1989: 1015)”:

(24) 山崖也、產生物也

But the text in Hǎo et al. (1989) is:

(25) 山產也、產生物也

However, we did not just make up or miscopy the text in (24); it was our reference that was in error. The version of the *Shì míng* text in (24) is quoted in the *Jīngdiǎn shìwén* 經典釋文 in a gloss to the following passage from *Zhuāngzǐ*:

(26) 莊子行於山中、見大木、枝葉盛茂、……
“Zhuāngzǐ was walking in a mountain and saw a large tree, with luxuriant branches and leaves....”

The full gloss in the *Jīngdiǎn shìwén* (Huáng Zhuō 2006: 789) is as follows:

- (27) 山中：釋名云、山、崖也、產生物也、說文云、山、宣也、謂能宣散氣、生萬物也。
 “山中 *shān zhōng*: The *Shì míng* says: 山 [*shān* < MC *srean*, 山開二平山生] is 崖 [*yá* < MC *ngea*, 蟹開二平佳疑] “slope”; it produces [產 *chǎn*] things. The *Shuōwén* says: 山 is 宣 *xuān* [MC *sjwen*, 山合三平仙心, “to spread”]; it means that [mountains] can spread and scatter [散 *sàn* < MC *sanH*, 山開一去翰心] 氣 *qì*, and give birth [生 *shēng* < MC *sraeng* 梗開二平庚生] to the myriad things.”

So to correct our error, we should replace the reference “Hǎo Yìxíng et al. (1989),” with “(*Shì míng* 釋名, as quoted in *Jīngdiǎn shìwén* [Huáng Zhuō 2006: 789]).”

The *Shì míng* gloss “山，崖也” (as quoted in the *Jīngdiǎn shìwén*) caught our attention because it is one of several pieces of evidence suggesting that 山 *shān* might end in *-r rather than *-n, and that it might be etymologically related to the word *yá* < MC *ngea* “slope, cliff” or “river bank” now written variously as 厓, 崖, or 涯. As already discussed in section 3 above, we follow Starostin (1989) in reconstructing an Old Chinese coda *-r, contrasting with both *-n and *-j. Final *-r developed to *-n in most dialects, but to *-j in the region in and near the Shāndōng peninsula. The main motivation for reconstructing final *-r is to account for cases where related words (at times the very same word) sometimes develop into Middle Chinese as if they had *-n, and sometimes as if they had *-j (see the full discussion in B&S 2014: 252–268). While 山 *shān* reflects the usual development of *-r to *-n, {崖} *yá* appears to reflect the dialect development of *-r to *-j:

- (28) 厓 ~ 崖 *yá* < *ngea* [蟹開二平佳疑] < *ŋ^sraj < *ŋ^srar “side of a mountain”
 涯 *yá* < *ngea* < *ŋ^sraj < *ŋ^srar “river bank”³³

Ho Dah-an asserts that 崖 belongs to the 支 *Zhī* rhyme group (our *-e); this is indeed what most reference books say, presumably because of the phonetic element 圭 *guī*, which belongs to the traditional 支 *Zhī* rhyme group (our *-e):

- (29) 圭 *guī* < *kwej* [蟹合四平齊見] < *[k]^we “jade scepter”

However, this does not oblige us to reconstruct 崖 *yá* < *ngea* with OC *-e. Neither 厓, 崖, nor 涯 occurs as a rhyme word in the *Shījīng*, and according to Lǐ Shǒukuí and Xiāo Pān (2015: 62–65), the characters 厓 ~ 崖 *yá* “side of a mountain,” and also 岸 *àn* “riverbank, shore,” are derived from an older pictograph 厃 *hǎn*, depicting a slope or cliff. The phonetic element 圭 *guī* < *[k]^we was presumably added after the final *-r had changed to *-j, and does not represent the phonology of early Old Chinese.³⁴ (The phonetic element 圭 *guī* is anomalous in any case, as characters with initial *k^w- are not usually used as phonetics to write syllables with initial *ŋ^s-.) On 厃 *hǎn*, the *Shuōwén* says:

- (30) 厃 [厃], 山石之厓巖, 人可居。象形。凡厃之屬皆从厃。厃 [厃], 籀文从干。
 “厃 is a slope [厓 *yá* < *ngea*, 蟹開二平佳疑] or cliff [巖 *yán* < *ngaem*, 咸開二平銜疑] of mountain rock, in which people can reside; it is a pictograph... In the seal script, it is 厃 [厃], with the component 干 *gān*.”

Note the presence of 厓 *yá* < *ŋ^srar, which we suspect is a sound gloss. We reconstruct 厃 and 厃 (an early form of 岸 *àn*) as follows:

- (31) 厃 [hàn] < *xanX* [山開一上旱曉] < *ŋ^sar? “cliff”
 厃 ~ 岸 *àn* < *nganH* [山開一去翰疑] < *ŋ^sar-s “shore, bank of a river”

These belong to a set of words that all have something to do with “slope,” “face,” or “cliff,” and which seem to be etymologically related:

- (32) 山 *shān* < *srean* [山開二平山生] (< *s-ŋran) < *s-ŋrar “mountain, hill”
 巘 *yǎn* < *ngjenX* [山開三上獮疑] ~ *ngjonX* [山開三上阮疑] (< *ŋran(?) ~ *ŋan(?) < *ŋrar(?) ~ *ŋar(?) “hill; hilltop”
 厶 ~ 厶 ~ 涯 ~ 崖 *yá* < *ngea* [蟹開二平佳疑] (< *ŋ^sraj) < *ŋ^srar “slope of a mountain, bank of a river”
 厶 [*hàn*] < *xanX* [山開一上旱曉] (< *ŋ^san?) < *ŋ^sar? “cliff”
 岸 *àn* < *nganH* [山開一去翰疑] (< *ŋ^san-s) < *ŋ^sar-s “bank of a river”
 顏 *yán* < *ngaen* [山開二平刪疑] (< *C.ŋ^sran) < *C.ŋ^srar “face, forehead”

We suspect that all these forms come from a root *ŋar ~ *ŋ^sar or *ŋ^srar ~ *ŋrar originally meaning “front, face,” with various semantic developments. The different forms are probably to be explained partly by morphology, partly by dialect mixture; we cannot explain them all at this time. But we doubt that their similarity in pronunciation is due to chance.³⁵ The direct and indirect evidence for our reconstructions of these words is complex; we cite only parts of it here.

As noted above, the *Shuōwén* gloss for 山 *shān* is in (33), with our tentative translation:

- (33) 山，宣也。宣气散生萬物，有石而高。
 “山 *shān* means “spread” [宣 *xuān*]; the spread *qi* 氣 scatters [散 *sàn*] and produces the myriad things. [A mountain] has rock and is tall.”

Here 宣 *xuān* “spread” is apparently a sound gloss (a semantic connection with “mountain” is implausible), and it is also a word where we can reconstruct *-r with some confidence (B&S 2014: 258), suggesting that 山 *shān* also had an *-r coda:³⁶

- (34) 宣 *xuān* < *sjwen* [山合三平仙心] (< *swan) < *s-q^war “spread (v.)”

Shījīng rhymes do not always clearly distinguish *-r from *-n, and the rhyme evidence for *-r in 山 *shān* itself is somewhat equivocal (Odes 189.1, 197.8, 305.6; the rhyme in 197.8 probably indicates *-r). However,

the word 巘 *yǎn* < *ngjen* ~ *ngjon* “hill; hilltop,” which we believe is related, does rhyme in Ode 250.2 with four other words, all of which can be securely reconstructed with *-r:

- (35) rhyme sequence in Ode 250.2 (《大雅·公劉》):
 原 *N-g^war > *ngjwon* [山合三平元疑]
 繁 *[b]ar > *bjon* [山合三平元奉]
 宣 *s-q^war > *sjwen* [山合三平仙心]
 歎 *ŋ^sar > *than* [山開一平寒透]
 巘 *ŋrar(?) or *ŋar(?) > *ngjenX* [山開三上獮疑] or *ngjonX* [山開三上阮疑]³⁷
 原 *N-g^war > *ngjwon* [山合三平元疑]

The primary evidence for reconstructing *-r, however, is cases of yīn-yáng duìzhuǎn 陰陽對轉 contacts between words with Middle Chinese vocalic codas and words with MC final *-n*, which we attribute to dialectal differences in the development of OC final *-r. This is why we considered the connection of 山 with 崖 *yá* (= {厓}) to be significant. We assume that the development of {厓} was as follows:

- (36) {厓} *ŋ^srar > (dialectal) *ŋ^sraj > *ngea* [蟹開二平佳疑]

For these reasons, we take seriously the version of the *Shì míng* text preserved in *Jīngdiǎn shìwén*. A long-standing principle of textual criticism is *difficilior lectio potior* “the more difficult reading is stronger”: The idea is that when two versions of a passage exist, the less straightforward one is probably the earlier one, because it is easy to understand why a copyist would modify a confusing passage, consciously or unconsciously, to something that is easier to understand; it is harder to understand why a copyist would replace an apparently unproblematic passage with something more difficult (see Beal 2008: 120). If the original *Shì míng* text had been “山、產也,” no copyist would find anything strange about it, and would have no motivation to “correct” the text: the two words were homonyms except for tone in Middle Chinese, and probably in Old Chinese as well (the Mandarin pronunciation “chǎn” is irregular):

- (37) 山 *shān* < *srean* [山開二平山生] < *s-ŋran < *s-ŋrar “mountain, hill”
 產 [*chǎn*] < *sreanX* [山開二上產生] < *s-ŋran? < *s-ŋrar? “bear (v.), produce”
 For the reconstructed *ŋ, note that the *Shuōwén* says the phonetic element in 產 *chǎn* is an abbreviated version of
 彥 *yàn* < *ngjenH* [山開三去線疑] < *ŋrar-s “adornment.”

The word 顏 *yán* < *ngaen* [山開二平刪疑] “face, forehead” is especially interesting because it shows both dialect developments (*-r to *-n and *-r to *-j) for the same character. Although the only reading in the *Guǎngyùn* is MC *ngaen*, the *Jíyùn* 集韻 also gives the pronunciation *ngea* [蟹開二平佳疑] for 顏, as a variant way to write 崖. Also, 顏 is said to be read like 崖 in a passage in the *Shǐjì*, juàn 29: “Hé qú shū” 《史記卷二十九·河渠書》 (*Shǐjì* 1982, 4: 1412):

- (38) …… 引洛水至商顏山下。
 ... directed the waters of the Luò river to below the face of the mountain Shāng(yán).

The commentaries on this passage read:

- (39) 【集解】服虔曰：「顏音崖。或曰商顏，山名也。」
 “*Ji jiě*: Fú Qián says: 顏 is pronounced like 崖. Some say that 商顏 is the name of the mountain.”
 【索隱】顏音崖，又如字。商顏，山名也。
 “*Suǒ yīn*: 顏 is pronounced like 崖; also with its usual pronunciation. 商顏 is the name of the mountain.”

Here *Shǐjì jí jiě* 史記集解 is a commentary by Péi Yīn 裴駟, a scholar of the fifth century CE; the Fú Qián 服虔 referred to was a scholar of the Eastern Hàn period. The *Shǐjì suǒ yīn* 史記索隱 is by Sīmǎ Zhēn 司馬貞 of the early eighth century. Whether the comments correctly interpret the *Shǐjì* or not, they show that 顏 *yán* “face; forehead” was felt to be connected to 崖 *yá* “side of a mountain.”

7. CONCLUSION

Let us sum up the status of Ho Dah-an's criticisms of our book.

1. "One rhyme group with many vowels": Without citing a single actual rhyme sequence as evidence, Ho Dah-an claims that our reconstruction requires words with different main vowels to rhyme. Our view, stated repeatedly in the book, is the opposite. This point was already explained and defended thirty years ago in Baxter (1992). Ho Dah-an failed to grasp this fundamental point of our argument.

2. "Retention of old features in Xiàoyì": Apparently without any exposure to Xiàoyì dialect and without presenting any physical evidence, Ho disputes the phonetic characterization given of the Xiàoyì shǎngshēng by Guō Jiànróng (a field linguist and a native speaker of the dialect) and by Sagart. We provide phonetic detail and imaging supporting our description of the Xiàoyì shǎngshēng. Finally, he cites a criticism by Zhū Xiǎonóng (2009) of an imagined error in a passage from Sagart (1986) which actually refers to the views of Mei Tsu-lin (1970) on shǎngshēng, not to Sagart's own views on qùshēng.

3. "Using phonetic transliteration to prove final coda": Ho Dah-an objects to our use of Chinese transcriptions of foreign words in -r as evidence for OC *-r. He ignores the fact that the Chinese material in these equations involves words which we would reconstruct with *-r for independent reasons. He also incorrectly portrays us as holding the position that one cannot reconstruct an older proto-language before having first reconstructed all the younger ones; even a cursory reading of our book would have shown that we clearly do not hold this position. Ho's methodological considerations rely on a confused account of the Indo-European laryngeal hypothesis which displays his lack of understanding of some basic facts of Indo-European historical linguistics. We do object to reconstructing a proto-language based on evidence from its sisters, because this would introduce circularity when reconstructing the higher proto-language. We do not doubt that Old Chinese *-r probably does correspond to Tibeto-Burman final liquids, at least in part; but the only way to check this hypothesis is to have an independent way of deciding where to reconstruct *-r in Chinese.

4. "午 and 五 not homophonous": We support our claim that 午 and the earliest members of its phonetic series had uvular stops in their onsets

with evidence from Mǐn dialects and with word-family evidence. We show that the word 敵 *yǔ* < *ngjoX* [遇合三上語疑] “to prohibit; to stop,” which appears written as 吾 and 菩 in two early Zhōu inscriptions, was not written as 禦 (with 午 phonetic) until later, as noted by Duàn Yùcái: this negates Ho’s argument that 午 and 五 were interchangeable phonetics already in early Zhōu. Ho’s idea that the early graphs for 午 and 五 represent a single word with the meaning “crisscrossing” conflicts with the prevalent view among paleographers that 午 *wǔ* originally depicts a pestle; for a clear and detailed account of these issues, see Mǎ Kūn (2019, 2021). Ho does correctly criticize our characterization of 五 *wǔ* as a sound gloss in the *Shuōwén*’s entry for 午 *wǔ* “7th earthly stem”: But this does not affect our main arguments at all.

5. “Calling a horse a deer”: A careless reading of our book caused Ho to miss the crucial Mǐn dialect evidence that is the basis for reconstructing a preinitial syllable in 鹿 *lù* < *luwk* < **mə-r*ok “deer.” As already noted by Jacques (2017), Ho’s belief that /lək 8/ in the Bùyāng word /ma 0 lək 8/ must be a loan from Zhuàng reflects his misreading of a sentence in Lǐ Jīnfāng (1999) and his ignorance of Kra-Dai comparative linguistics. Finally, Ho’s argument that /ma 0/ in the Bùyāng form is an indigenous prefix betrays a dim understanding of what minor initial syllables really are in Kra-Dai languages.

6. “A mountain is a river bank”: Here, Ho Dah-an has actually found a second error in our book. A passage we quoted from the *Shì míng* 釋名, a work compiled in the 2nd century CE, comes not from the received version but from a version evidently seen by Lù Démíng 陸德名 (c. 550–630 CE) and quoted in his *Jīngdiǎn shìwén*. Still, the principle *difficilior lectio potior* suggests that the version seen by Lù in the late 6th or early 7th century may better reflect the original *Shì míng* than the received version of the text. In any case, the reconstruction of final *-r in a whole family of words having to do with slopes or cliffs is supported by a variety of other evidence and arguments.

If a reviewer finds actual errors in a book, the only appropriate attitude of the authors should be gratitude. Ho did indeed find two errors, as mentioned above. We are glad to be able to correct them. Neither error substantially affects our arguments, however. (We ourselves have also taken this opportunity to point out a few additional errors.) But we have

found in Ho's 54-page review many more serious errors than he claims to have found in our 431-page book. We believe that a reviewer's primary responsibility to the reader is to carefully read the book he is reviewing; and Ho Dah-an failed to do this.

NOTES

1. An exception is Starostin (1989: 49–133), who projected the voiceless resonants (*mh- etc.) and the voiced aspirated stops and affricates (*bh- etc.) of Jerry Norman's Proto-Min reconstruction (Norman 1973, 1974) back to Old Chinese. Yang (1982) and Benedict (1987) also made some proposals to account for evidence from Min.

2. Starostin (1989) also reexamined the rhyme evidence, and independently reached conclusions that largely agree with Baxter (1992). But although Zhèngzhāng Shàngfāng's reconstruction system is similar in many ways to Starostin's and ours (especially in the vowel system), he did not challenge the traditional rhyme categories, arguing that phonological rhymes and the rhymes used in poetry are only loosely connected (Zhèngzhāng 2003: 157–158).

3. The characters in brackets after our Middle Chinese notation identify the relevant syllable in terms of traditional categories for Middle Chinese: for example, in “*dzyuX* [遇合三上麌禪],” “遇” indicates that the MC syllable we write as *dzyuX* belongs to the Yù shè 遇攝; “合” that it is considered a hékǒu 合口 syllable; “三” that it is in sānděng 三等 “division III”; “上” that its tone category is shàngshēng 上聲 “rising tone”; “麌” that it is in the Yǔ [*< MC ngjuX*] 麌 rhyme in the *Guǎngyùn* rhyme dictionary; and “禪” that the conventional name for its initial consonant is Shàn 禪 (= MC *dzy-*). We include these terms for the convenience of readers who are familiar with traditional terminology. We emphasize that notations like *dzyuX* are not intended to be phonological reconstructions of any particular variety of the Chinese of the Middle Chinese period; they are intended simply as a convenient, typable way to represent the information derived from traditional written sources such as the *Guǎngyùn*. Our notations are based on the representations of Middle Chinese syllables given in Dīng and Lǐ (1981), and they are in one-to-one correspondence with them.

4. Proto-Mǐn reconstructions are based on the correspondences in Norman (1973, 1974, 1981, 1991, and Luó Jiérui 1987).

5. Of course, any large corpus is likely to have some irregular rhymes, for one reason or another. Not all *Shījīng* rhymes are regular, even according to the traditional analysis; some involve words with different main vowels, no matter whose reconstruction one uses; the rhyme between 設 *shè* and 逸 *yì* in example (5) below is such a case. But such cases are too infrequent to be considered as regular rhymes.

6. In our notation, we put square brackets around any reconstructed sound which, given the evidence at hand, could also be reconstructed as another sound having the same MC reflex. Thus *-o[n] differs from *-on in that, in the former, we lack the evidence to guarantee that the coda was *n as opposed to *r, which usually also becomes final -n in Middle Chinese (see section 3 below).

7. For example, see Odes 58.6, 220.3, 253.5, where 反 *fǎn* rhymes with words that cannot be reconstructed with *o.

8. We follow the practice of Chinese paleographers in the use of curly brackets: “{弁}” means “the word now standardly written as 弁,” however it might be written in a particular document.

9. Ho’s English translation of Baxter’s original Chinese does not reflect what was meant, nor does it make sense in the original context. A better translation would be “For *-et to rhyme with *-it is not too strange (*e and *i are both front vowels); for *-at to rhyme with *-it is rather rare” [“上古的 *-et 跟 *-it 押韻不算太奇怪 (*e 和 *i 都是前元音); *-at 跟 *-it 押韻倒是比較少見的。”].

10. Ho Dah-an says: “Even the authors themselves also acknowledge that the Qing philologists had noticed this phenomenon” (p. 188)—the “even” suggesting that we would be reluctant to recognize the contributions of the Qīng scholars who laid the groundwork for our field. The suggestion is preposterous and offensive.

11. Starostin did not specify the geographic location of the hypothetical dialect where *-r became *-j, but we noticed that his hypothesis is supported by a number of Hàn-dynasty comments about local pronunciations: the area where *-r became *-j seems to have been the region in or near the Shāndōng peninsula (see B&S 2014: 264–268).

12. Actually it is possible that the coda in question should be reconstructed as *-l instead of *-r; there is little evidence either way on this point (B&S 2014: 195). But in any case, we know of no evidence that final *-l and *-r were distinct in Old Chinese.

13. Hill also finds evidence that a small group of words reconstructed by us with *-j have -l in Tibetan. We suspect that these are either faulty comparisons or words which on closer examination will turn out to have had *-r in Old Chinese.

14. Falling behind Verner (1877) and Saussure (1879) by a century would put us in the 1770s; we assume that Ho Dah-an meant to place us in the 1870s.

15. It was only later that Saussure noticed that the theory could explain some seemingly irregular correspondences, such as the correspondence of Greek unaspirated /t/ to Sanskrit aspirated /tʰ/ in Greek *statós*, Sanskrit *sthitás* “stood, placed,” both from PIE *sth₂-tós (in modern notation); see Szemerényi (1973: 8–9).

16. See Szemerényi (1973: 6). Szemerényi says that the third, “e-colored” element, earlier written as *E but now usually written as *h, was proposed by Möller (1879, 1880); and it was he, not Saussure, who proposed that the elements were consonantal, and cognate to laryngeal consonants of Semitic.

17. The *C. preinitial in 五 *wǔ* < *C.ŋ^a? is reconstructed to account for reflexes in Mǐn, Hakka, and early loans in Kra-Dai (= Tai-Kadai); Norman (1991: 211) reconstructs *ŋh- for “Old Southern Chinese.” See B&S (2014: 171–173) for a more detailed discussion.

18. Standard Mandarin *wěi* is irregular; we would expect *wèi*.

19. In *Yīn Zhōu jīnwén jíchéng* (Zhōngguó shèhuì kēxuéyuàn 1984), they are numbers 2841 and 4342 respectively.

20. For a more detailed discussion, see Mǎ Kūn (2019, 2021).

21. Notice the assumption that if 五 *wǔ* and 午 *wǔ* were homonyms in the *Guǎngyùn*, they must have been homonyms in Old Chinese also. The *Guǎngyùn* gives “powerful internal evidence” about these words in Middle Chinese, but its authors cannot have known whether or not they were homonyms in Old Chinese.

22. See, for example, Lín Yìguāng (1920, juàn 1); Yáng Shùdá (1954); Mǎ Xùlún (1957)—the latter two quoted in *Gǔwénzì gǔlín* (1999–2004, 10: 1141); Hé Línyí (1998: 509); Qiú Xīguī (1988: 233; 2000: 335); Huáng Dékuān (2007: 1426); Jì Xùshēng (2010, 1021); and Zhāng Shìchāo himself (2011: 1).

23. The vessel is the “Sì sì bì qí yǒu 四祀邳其卣,” number 5413 in *Yīn Zhōu jīnwén jíchéng* (Zhōngguó shèhuì kēxuéyuàn 2007).

24. The vessel is the “Bó Chōng hé 伯春盃,” number 9939 in *Zhōngguó shèhuì kēxuéyuàn* (1984–1994), cited in Ji Xùshēng (2010: 601).

25. We further noticed that our translation of “陰气午逆陽” was faulty, because we failed to translate “午”: 午 *wǔ* and 逆 *nì* both appear to be verbs, meaning “oppose” and “go against” respectively. Ho Dah-an also chastises us for not following Duàn Yùcái’s emendation of “午逆” to “悟逆,” a change which, even if correct, does not change the sense or our argument.

26. The date of the Qín stone drums has been debated for centuries; Gāo Míng (2010) gives what we consider persuasive arguments that they come from the late 4th century BCE. Ho puts them in the 6th century BCE, saying that we “would not be happy to see such a result because of the anticipation of six hundred years,” as if we had claimed that the relevant changes had not happened *until* the time of the *Shuōwén*. Here again Ho Dah-an has not read us carefully: we said that they had become interchangeable “*at least* by the time of the *Shuōwén*” (B&S 2014: 130; emphasis added).

27. We here correct a typographical error in our original text, which cites the Bùyāng form as “/ma 0 lok 8/” (so also on p. 179); the correct form is “/ma 0 lok 8/,” i.e., phonetically [ma⁰ lok¹¹] (Li 1999: 199). (Here “8” is the conventional numbering for a lower-register stop-final tone, analogous to the lower entering tone of Chinese.)

28. Just to keep things straight: we believe that /ma 0 lok 8/, not /ma 0/, is an early loan from Chinese.

29. Notice again Ho’s use of the word “proof.”

30. N.B.: not “stinging insect” as we say in our book; here we used Karlgren’s gloss, which was for a different word written 蠅, which occurs in *Zhuāngzǐ* 莊子.

31. We should have pointed out that the Mǐn forms with dental initials for 蠣 *lì* “oyster” and 鹿 *lù* “deer” are not direct evidence for Proto-Mǐn *-d-, which would require evidence from Northern Mǐn. We are grateful to South Coblin for pointing this out.

32. A more complete list can be found in Sagart (2004).

33. According to the usual pattern, we would expect *ŋ^ʳraj to become MC *ngae* [假開二平麻], not *ngea* [蟹開二平佳]. But in fact, the MC finals *-ea* [佳] and *-ae* [麻] are frequently confused in our sources, presumably because of

dialect mixture, and words of the form *C^rraj not infrequently have *-ea* instead of the expected *-ae* (see B&S 2014: 269–271).

34. We know of no pre-Qín examples of 圭 *guī* as a phonetic element to write {厓}. As far as we know, the earliest example of 崖 is in the Mǎwángdūi text “Xiàng mǎ jīng 相馬經,” from Western Hàn; see Chén Sōngcháng (2001: 381). Such a character also appears in the Shuihǔdì 睡虎地 text “Fǎlǜ wèn-dá 法律問答”(strip 28), found in a tomb of the state of Qín from shortly before unification in 221 BCE. But the character’s meaning in the text is disputed, and in any case, from the context, it can’t represent {厓}; see Shuihǔdì Qín mù zhújiǎn zhěnglǐ xiǎozǔ (1990: 51, 100).

35. Compare the Proto-Tibeto-Burman reconstruction (STEDT website) *ɲar TOPKNOT / FOREPART / FRONT SIDE.

36. Here 散 *sàn* < *sanH* [山開一去翰心] may also be a sound gloss, but as of now, we lack the evidence to decide between *-n and *-r in this word; we reconstruct it as *mə-s’a[n]ʔ-s. See B&S (2014: 177).

37. Notice that although Middle Chinese sources give shǎngshēng pronunciations for 𪛗 *yǎn* < *ngjenX* and *ngjonX*, this rhyme sequence suggests a píngshēng pronunciation for Old Chinese.

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摘要

何大安 2016 年對我們的《上古漢語新構擬》(2014 年)一書的評論中幾乎沒有討論該書的主要主題或建議：相反，他把重點放在給我們挑錯上。據他說，這些“錯誤”“反映了作者過時的概念和他們基礎訓練的不足”。本文是我們對何氏評論的回應。我們討論了他所說的那些“錯誤”。有兩點他真的發現了錯誤(對《說文解字》中一個條目的錯誤解釋和一個錯誤的引用)，我們很高興能夠有機會改正。但這兩個錯誤都沒有對我們的結論產生實質性的影響。何氏提到的其他“錯誤”根本不是錯誤：在大多數情況下，它們反映了何氏的基本誤解——對我們的書，對其他資料，以及對歷史語言學的基本原則。鑒於其中的一些誤解事關上古漢語構擬的大局，我們試圖糾正這些誤解，以正視聽。

關鍵詞

上古汉语的构拟 韵部 韵尾 借词