Book Reviews / Рецензии

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William H. Baxter, Laurent Sagart.
Old Chinese. A New Reconstruction.
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The past decade has seen several publications on the history of the Chinese language that have already become, or are bound to become not merely "milestones" in research history, but something more important: scholarly "companions" — reference tools that are not just destined to be read, formally honoured, and put away, but will have serious scholars returning to them over and over again in the course of daily work activities. The first such publication was Axel Schuessler's etymological dictionary of Old Chinese (Schuessler 2007), which was then rapidly succeeded by an updated and "modernized" edition of Bernhard Karlgren's *Grammata Serica Recensa* (Schuessler 2009). The first of these was already discussed by the author of this review for a previous issue of this Journal (G. Starostin 2009), where the dictionary was judged to be an extremely valuable tool for etymologists and philologists alike, but it was also pointed out that its usefulness was somewhat limited inasmuch as "strong" etymologies (supported by reliable OC reconstructions and systematic external parallels) were not always differentiated from more "speculative" etymologies.

Now along comes *Old Chinese: A New Reconstruction*, a monograph written jointly by William H. Baxter and Laurent Sagart, two of the most authoritative, innovative, and simply hard-working specialists on the history of Chinese phonology. Both of the authors had previously offered their own individual models of the phonology of Old Chinese (OC) — Baxter 1992 is a classic, comprehensive, and elaborate reconstruction that has, for more than 20 years, arguably served in the Western academic world as the most common reference point on the subject since the much earlier (and clearly obsolete in many respects) works of Karlgren; and Sagart 1999 presented a radical rethinking of the structure of the OC syllable and even the OC language in general, which specifically emphasized the importance of recognizing and reconstructing productive derivational morphology, allegedly obscured by its inadequate reflection in the Chinese script.

Despite the obvious importance of both these works, neither of them claimed to be "closing the book" on the reconstruction of the sound system of OC, for two simple reasons. First, the employed methodology, based on the analysis of such highly specific data as rhyme classes of OC poetry and the phonetic principle of the OC script, had to be significantly different from the usual comparative-historical method, traditionally employed to reconstruct the sound systems of unattested protolanguages, and this implied that future changes and additions to the method would be inevitable. Second, as the authors explain themselves in the first two chapters of the book, in the two decades since the publication of Baxter's model, a variety of new linguistic, philological, and archaeological data on OC has become available, much of which has a direct bearing on the reconstructed sound system and sometimes necessitates radical revisions of certain aspects.

It is, therefore, quite an auspicious development that neither of the authors of the book under review, having already produced comprehensive and consistent models of their own, chose to rigidly persist in all of their historical judgements, but agreed to develop and elaborate them further, rethinking and revising their previous reconstructions where new data have prompted such a necessity — moreover, agreed to do this in close collaboration with each other, arriving at a common consensus model, which is indeed a rare development in modern historical-linguistic studies. The very fact of something like this happening implies that *Old Chinese: A New Reconstruction* is not to be taken lightly, and, for better or worse, has a serious chance of becoming the base reference model for OC phonology in the upcoming years. For the serious Sinologist, or, indeed, for anybody with a serious interest in the linguistic prehistory of Southeast Asia in

general, the book is a stimulating and challenging read, and much of the upcoming work on the history of the Chinese language will probably have to revolve around discussions of proposals made therein.

A lengthy critical discussion of a book so richly and densely packed with philological and linguistic arguments even within the scope of an extended review article would be out of the question. Instead, I will choose a different approach. Although the book does not state this explicitly, its structure seems to confirm that it tries to be simultaneously targeted at a general audience of professional and amateur Sinologists without any serious background in historical Chinese linguistics, as well as at a much smaller, specialized audience of scholars who are first and foremost interested in the "new" aspects of the reconstruction. Therefore, I will first briefly outline the challenges that the book poses for the general audience, and then try to outline some of the major advantages and flaws of the authors' "new" approach to the reconstruction of OC, keeping the illustrative data examples to a minimum, with the hopes that this will by no means undermine the validity of many of Baxter and Sagart's (henceforth — B&S) individual discoveries.

First of all, the jacket blurb correctly states that the book is "critical reading for anyone seeking an advanced understanding of Old Chinese", with implied emphasis on *advanced*. Even though, technically, the first two chapters ("Introduction" and "The evidence for Old Chinese") give a general overview of the main sources of data and the main challenges, this overview, densely stuffed into forty pages, is much too short for somebody with no prior knowledge of the basics of OC phonology to get a good grasp on the situation. It is clear that the authors' primary goal is to concentrate on the revisions of their previous research rather than on writing an accessible tutorial — which is perfectly justified. However, one wishes that this were more precisely expressed in the introduction, with the general reader being more explicitly being warned about the book's "advanced level" status in comparison to such predecessors as Sagart 1999 and *especially* Baxter 1992, which is still, in this reviewer's opinion, the single best possible introduction to the complex science of reconstructing OC phonology for the uninitiated student of OC, still accustomed to strictly pronouncing ancient words in their 20th century Beijing pronunciation.

For instance, the detailed and informative overview of the important research of Qīng-era scholars on the rhyme categories of OC, which occupied about 30 pages worth of space in Baxter 1992, is here condensed to a single brief paragraph on p. 2 and a couple more

paragraphs on pp. 22–24; the important preliminary stage of explaining the phonology of Middle Chinese (MC) is reduced from 40 pages in Baxter 1992 to 10 in the new book, and so on.

Another confusing detail in the structure of the book is the inclusion of a special section (chapter 3, "An overview of the reconstruction") that is clearly targeted only at those readers who are already well familiar with previous research, since it consists of a brief description of the innovations that differentiate this volume from Baxter 1992. Not only is it largely superfluous in the light of a more detailed explanation of all these hypotheses in subsequent chapters (perhaps it might have worked better as a special "appendix for specialists" rather than a chapter in its own rights?), but its placement right next to the briefer-than-brief introductory chapters for "neophytes" is downright puzzling. On the whole, structural comparison of this work to Baxter 1992, a work as close to perfection in terms of structuring and sequencing of its complicated material, is rather underwhelming, and makes one wonder whether the whole work was not rushed to the publishers too quickly, before the authors had a proper chance of making it more comfortable for either the specialists, or the novices, or both.

Nevertheless, these are merely subjective impressions; much more important is the issue of how much actual scientific progress has been achieved by the authors, and whether this "new and improved" version of OC reconstruction truly deserves to replace all previous ones as the new base reference model for linguists, philologists, historians, and other specialists whose research is in one way or another connected with the linguistic realities of Ancient China.

The authors' own evaluation of their research states that they take a "broader approach" to reconstruction than most, if not all, previous studies, including their own (p. 3). Most importantly, this involves analysis of as many sources of data as possible — in addition to such major "pillars" of traditional OC reconstruction as rhymes in early poetry and the Chinese script itself, the authors systematically tackle such additional sources as (a) data from modern Chinese dialects, most notably the Mǐn subgroup, whose separation from the main bulk of Chinese dialects is commonly understood to predate the Middle Chinese period, making it of particular importance as a possible retainer of some archaic features of OC, lost elsewhere; (b) old loanwords from Chinese into Kra-Dai, Hmong-Mien, Mon-Khmer (Vietic), and Tibeto-Burman languages; (c) data from various epigraphic sources, such as bamboo strips and silk scrolls from relatively recent

excavations that bring to light numerous formerly unknown particularities of the Chinese script in the Warring States era — particularities that may force significant corrections both for individual reconstructions of specific words and to the phonological system of OC as a whole.

In theory, this broadening of the perspective is a wonderful thing: given the complexity of the task (reconstructing the phonology of a 2,500-year old language without the added benefits of the standard historical-comparative method), any additional source of data is a blessing, and a system that puts together and harmonizes the data of all available sources is, by default, more convincing than a system that is restricted to only a few of these sources. However, it also poses a problem: the "sources" in question vary significantly in terms of their reliability. When we employ the data of Japanese, Korean, Vietnamese, or even Zhuang-Tai words of Chinese origin that penetrated these languages *en masse* starting from the Late Old Chinese and culminating in the Early Middle Chinese era, we have before us a highly systematized picture, well illustrated by numerous examples, where it is very easy to find the respective Japanese, Korean, etc. equivalents for almost any initial or coda of any Chinese syllable. In the case of something like a proposed set of significantly earlier borrowings from Chinese into Proto-Hmong-Mien, we find ourselves in a completely different position: such borrowings, though seemingly numerous per se, are relatively more scarce and do not translate into an easily comprehensible system as smoothly as they do with "younger" Sino-Xenic systems. In other words, any potential borrowing from Chinese into Hmong-Mien has to undergo a more rigorous procedure of justification, and using such potential borrowings as evidence for OC phonology may invoke circular logic — we modify our OC reconstructions because of their correlates in Hmong-Mien, without any additional means to make sure that these Hmong-Mien forms *are* correlates.

As an example, let us take the OC reconstruction *məə.lat* 'tongue' for modern 舌 *shé* (MC *zyet*) on p. 180. The MC initial *zy-* is a regular development from OC *l-*; the only reason why the reconstruction here necessitates a "loose preinitial" *mə-* is comparison with Proto-Hmong-Mien *mblet* 'tongue', which is assumed to be an early borrowing from Chinese, following the hypothesis in Ratliff 2010: 48. Precisely *why* this is assumed, however, remains unexplained. For sure, there is a significant amount of phonetic similarity between *mblet* (which seems to be a fairly reliable reconstruction) and *lat*, or even *C-lat* (if we agree that the MC reflexation *zy-* rather than *y-* necessarily reflects the

deletion of an earlier preinitial), but first, words for 'tongue' very frequently contain *-l-* in the world's languages, and second, it is hardly a good idea to consider Proto-Hmong-Mien *mblet* outside of its lookalikes in Mon-Khmer languages, such as Sre, Biat *mpiat*, Chrau *ləpiat*, Bahnar *rəpiat*, etc. (← *lmpiat* 'tongue', further perhaps to *li(ə)t* 'to lick'; cf. Shorto 2006: 305). No matter what these connections reflect (an old genetic connection between Hmong-Mien and Austro-Asiatic, or areal links between the two), they certainly suggest that the Hmong-Mien word is older than a presumed borrowing from OC circa the middle of the 1st millennium BC. In fact, why not vice versa — a borrowing *into* OC from some early ancestor of Hmong-Mien, with cluster simplification? Such a hypothesis would be at the very least equiprobable to the one expressed in the monograph.

Even worse is the amendment of the OC reconstruction *do-s* 'tree' (modern 樹 *shù*, MC *dzyuH*) to *m-toʔ-s* (p. 124) based on the comparison with Proto-Hmong-Mien *ntjюənH* (sic!) 'tree'. The authors are perfectly correct when they derive the OC word from the verb 樹 *shù* (MC *dzyuX* < OC *m-toʔ*, which I would rather revert to original *doʔ*) 'to plant, place upright'; but how exactly does one arrive from *m-toʔ-s* to *ntjюənH*? (Let alone the fact that during the early contacts between OC and Proto-Hmong-Mien the main equivalent for 'tree' in OC must have unequivocally been the old term 木 *muk*, and that the sociolinguistic basis for the borrowing of a very recent derived innovation 'growing/planted tree' in the general meaning 'tree, wood' is virtually non-existent).

It is for reasons like these (such examples could, of course, be readily multiplied) that I would exert extreme caution when using potential loans into Hmong-Mien as evidence for OC reconstructions — rather than the opposite situation, when already available OC reconstructions may be used to identify any such potential loans; otherwise, we risk landing into a circularity trap, particularly considering the relatively small corpus of evidence where it is hard to confirm recurrent patterns of correspondences.

"External" similarities, however, may be no more treacherous than "internal" similarities — namely, the strong belief in the power of the so-called "word families", a concept that goes all the way back to Karlgren (or, if we want to get very pedantic about it, all the way back to the phonetic glosses of Hàn-era philologists) and has, in this author's opinion, done far more harm to the field of Chinese etymology than good. Roughly speaking, a "word-family" in OC is defined as a set of words that share a noticeable amount of phonetic and semantic similarity (usually differing by

no more than one phoneme) and may, therefore, be suspected of sharing a common original root (like the textbook example of 黑 *hēi* 'black' < MC *xok* vs. 墨 *mò* 'ink' < MC *mok*, where the first word reflects an old contraction with prefixal **s-*). Now it would seem logical that within many of such "word-families" defined by a researcher, some of the noted similarities would be historically conditioned and some would be accidental — and that the more pattern-like the connections are and the more transparent the rules are that explain the grammatical formation of one word from another, the more confidence we can have in their common origin. For B&S, however, it seems that the very fact that two or more words *can* be arranged within a "word-family" is already proof, or at least strong evidence, that they *are* related — a decision that I find highly alarming.

To take but one example, is the phonetic and semantic similarity between MC 膝 *sit* 'knee' and MC 節 *tset* 'joint of bamboo' really sufficient to consider the two words related? B&S answer in the positive, and reconstruct both forms as OC **s-tsik* and **tsˤik*, respectively. But if so, what were the actual derivational mechanisms, involving loss/acquisition of "pharyngealization" and prefixation of **s-*, that originally transformed 'joint' into 'knee' or vice versa (or derived both of them from a third party)? The question is not even *asked* as the new reconstruction of 'knee' is being proposed, let alone answered. And how systematic is this connection? Next to the above-mentioned link between 'black' and 'ink', where the *x-* / *m-* connection is strongly supported by additional evidence, this example is unique. Clearly, this reconstruction is not to be trusted, as are quite a few others, based on no stricter methodological basis than an intuitive feel for "word-family" connections.

Another area in which the authors occasionally seem to be overreaching is the complicated nature of the Chinese script and its multiple variants in attested texts as well as epigraphic, including recently excavated, monuments. It is perfectly true that both the phonoideographic principle *and* the presence of numerous graphic variants, where one phonetic component could sometimes be replaced by another (or could *not*, which also constitutes serious evidence), have been vital in our understanding of OC phonology, especially where syllable-initial consonants are concerned (since this is the only segment of the syllable on which the rhyming system of OC sheds no light whatsoever). However, it is *also* true that we have no 100% guarantee that the phonoideographic principle was always consistently enforced according to *precisely* the same standards whenever a new character

was created; and as we formulate a basic set of rules for the generation of new characters, we should also allow for a reasonable number of exceptions from these rules, without necessarily striving to explain them as minor regularities that override major regularities. After all, this is not regular historical-comparative linguistics that operates on "live" pronounced forms, but a study of the evolution of an artificially created graphical system, something much more prone to human error and various "transmission accidents".

To use an artificial example, if a particular phonetic component is used to transcribe the syllable **pa*, we would expect it to be able to *regularly* serve as a phonetic component for such phonetically similar syllables as **pha, *ba, *pa-s, *pha-s, *pra, *pra-s*, and others that do not deviate from a general "similarity formula" like **P(r)a(-s)*, where *P* = any labial stop and *-s* is a suffix. We would also expect it to *not* regularly serve as a phonetic component for a syllable like **ma*, since the bulk of available evidence shows that *P-* and *m-* were perceived as phonetic entities belonging to different, non-intersecting sound classes, and minimal pairs with *P-* and *m-* are very rarely encountered in the graphical system. However, rare as they are, such intersections do exist (e. g. 武 **maʔ* vs. 賦 **paʔ*), and the question with them is: do we want "total accountability", i. e. a system that allows for no exceptions and offers a (phonetically based) explanation for *everything*, or a system where statistically insignificant deviations are counted as "accidents", and only *truly* systematic evidence is accepted as valid reason to modify our reconstruction of the phonological system? In this particular case, for instance — would we necessarily have to invent an *ad hoc* explanation, e. g. re-reconstruct 武 **maʔ* as **p-maʔ* or 賦 **paʔ* as **m-paʔ*, to account for this strange graphic "accident", or could we just define this as an accident and move on?

It seems that the preferred model for the authors is "total accountability": any encountered oddities in the Chinese script have to be explained, or are at least preferably explained as regular (if, in many cases, unique) reflexations of certain peculiarities of the phonetic system. Many of the rules described in the monograph are illustrated on no more than a couple of cases — sometimes less than a couple, as long as they seem to fit into some general pattern, the historical reality of which remains questionable.

As an example, let us take the reconstruction of OC 午 *wǔ* 'seventh earthly branch', which used to be **ngaʔ* (Baxter 1992: 795) and has now been amended to the far more intricate **[m].qʰˤaʔ*. The main reason for this amendment is that it is otherwise impossible to un-

derstand why 午 *ngaʔ (MC nguX) was used as a phonetic component in 杵 chǔ (MC tsyhoX) 'pestle'. Consequently, 'pestle' is being reconstructed with a different "prefix", but with the same initial as *t.qʰaʔ, and the issue is considered resolved.

The principal problem with this solution is that this situation is *unique*. The usual way to reconstruct OC initial consonants or consonant clusters that differed from their MC reflexations was through identifying *recurrent patterns* in the Chinese script that grouped together syllables with radically different MC initials — and the more recurrent they were, the better they helped weed out historical accident as an alternate explanation. However, alternation between MC ng- (in 午) and tsyh- (in 杵) is anything *but* a recurrent pattern. It occurs only once, and, furthermore, there is a perfectly valid alternate explanation of this historical accident.

The authors propose (with good reason) that 午 was originally the graph for 'pestle'; that it was later borrowed to denote the phonetically similar word for 'seventh earthly branch'; and that still later, the original word for 'pestle' acquired the additional radical 木 so that it could be graphically re-separated from 'earthly branch'. On the surface, this is a plausible scenario (analogous situations are quite plentiful), but there is a serious additional problem with it: namely, the so-called '12 earthly branches', with the notable exception of the very first one (子 zǐ 'child'), *do not represent words of Chinese origin*. Although the characters themselves clearly depict objects, some of which may be identified ('arrow', 'tree', 'pestle', 'k. of animal') and some of which remain obscure, the readings associated with these characters do not represent their regular OC names — which, naturally, leads to suggest that either the characters or at least the words associated with the characters were originally not of Chinese origin. Consequently, it is quite possible that the character 午 ('pestle') originally had two readings — a "non-Chinese" one (OC *ngaʔ) and a Chinese one (*tʰaʔ); later on, a new expanded character was created for the second word. Such an explanation is no more fantastic than the one proposed by the authors — and is more in line with seeing the whole situation as a historical accident rather than a pattern, which would require additional evidence that has so far not been provided.

This very example also logically leads us to yet another problematic area of the new reconstruction: typological plausibility, especially from the diachronic point of view. As we have seen, OC *m.qʰ- (in the word for 'seventh earthly branch') is supposed to yield MC ng-, a development explicitly indicated in table 4.37 on p. 130. At the same time, its unaspirated correlate, OC *m.q-, as seen from table 4.36 on p. 127, is expected to yield MC y- (through an intermediate voiced stage *ɢ-). The latter development is at least consistent with the general system, according to which all initial clusters of the type *m.C- (where -C- is a voiceless stop) develop into voiced stops in MC; the former, however, is unique and completely unsupported by other similar developments (e. g. one should also expect that OC *m.pʰ-, *m.tʰ-, etc., should yield MC *m-, *n-, etc., which they do not). Considering the scarceness and occasional dubious character of the examples, one should definitely treat these reconstructions with a grain of salt.

These and many other rules are established to illustrate what is perhaps the single most significant innovation since Baxter 1992: a large, comprehensive set of OC "preinitials", which come in two varieties ("tightly attached" and "loosely attached", so that a syllable like OC *k.toŋ is deemed phonologically different from *kə.toŋ, and is expected to yield different reflexes in MC, as well as Mǐn dialects and, possibly, old loanwords in Hmong-Mien and Vietic). This theory has been carried over from Sagart 1999 (where the two varieties of syllables were respectively called "iambic" and "fused"), but it has been vastly expanded in the book under review, particularly because the authors claim to have discovered additional evidence: for instance, "loosely attached" preinitials are now associated with the so-called "softened" stops in Jerry Norman's reconstruction of Proto-Mǐn, whereas the "tightly attached" preinitials are said to give rise to Proto-Mǐn "voiced aspirated" stops.

The amount of presented evidence seems so overwhelming, at least in terms of the sheer number of initial consonantal clusters reconstructed for OC, that at least *some* of these innovations in the reconstruction must be right, even if their phonetic interpretation may be reconsidered (for instance, in S. Starostin 1989 the voiced aspirated consonants of Proto-Mǐn were believed to reflect the original OC situation, rather than a combination with a "fused" presyllable). Unfortunately, at this point it is hard to state for sure which of the innumerable combinations of initial consonants reconstructed by the authors hold water and which ones do not — a proper evaluation would probably require almost as much time as it took to produce the work itself. Evidence from Mǐn dialects, Chinese script varieties, "word-families", and Hmong-Mien / Vietic loans is juggled around so freely and so quickly that one never properly understands just how much evidence there is for any single correspondence, or (which is even more important) whether there is any

counter-evidence for that correspondence. Ultimately, utmost caution must be exercised when operating with such reconstructions as OC *m.lru[t]-s 'to fall down' or *t.qʰ(r)A 'chariot', and, most importantly, one should always try to distinguish between reconstructions based on recurrent, systematic, "robust" evidence from those based on a questionable parallel (e. g. do we really have to take for a *fact* that Proto-Hmong-Mien *mbruiH 'nose' is borrowed from OC *m-bi[t]-s?) or a subjective decision on which OC words are related to which others through poorly understood "word family" connections.

One very interesting idea that was previewed neither in Baxter 1992 nor in Sagart 1999 but is rather a relatively recent common proposal by the authors (actually, following up on an earlier proposal by Pān Wùyún) is the reconstruction of a distinct uvular series of consonants for OC (*q-, *qʰ-, etc.), based on the identification of two different types of phonetic series (those that allow for syllables with MC velar initials only and those that allow for syllables with either velar initials or an initial glottal stop in MC: the latter group is identified as having contained uvular initials in OC). This solution looks typologically plausible, but would require serious corroboration from at least one additional source of evidence, if we want to completely rule out chance as an alternate explanation. Incidentally, it could be noted that, in their Proto-Sino-Tibetan reconstruction, I. Peiros and S. Starostin (1996) did set up a separate category of uvular consonants for PST, even though they did not propose the preservation of uvulars in OC; it would be instructive to see how the alleged OC uvulars add up to the Tibeto-Burman evidence and whether there is any (at least partial) correlation between Baxter and Sagart's OC reconstructions in *q-, *G-, etc., and Starostin and Peiros's ST reconstructions beginning with the same consonants.

Another major innovation is the phonetic interpretation of the difference between the so-called "Type A" and "Type B" syllables as an opposition between *pharyngealized* and *unmarked initials* (so that, according to the authors, OC could phonologically distinguish between *ŋa and *ŋˤa, etc.). The discussion over the nature of this opposition has involved numerous and diverse solutions (such as Karlgren's old and by now completely discarded idea of an OC palatal glide in word-medial position, or attempts to reconstruct vowel length), but the one offered here by the authors seems fairly dubious, since it is grounded in purely typological evidence (and rather selective at that). Not only does it seem structurally wrong to regard this "pharyngealization" as a characteristics of the initial

consonant (which not only doubles the phonological inventory of OC, but also allows for such unique combinations as a pharyngealized glottal stop *ʔˤ and its labialized counterpart *ʔʷˤ!), but it should also be remembered that pharyngealization is not at all a typical feature of the area in question: the authors themselves are only able to quote two isolated examples in typological support of the hypothesis (Northern Qiāng of Hóngyán and a couple of Austronesian languages on Taiwan), where pharyngealization is neither as all-pervasive as in the authors' reconstruction of OC, nor is it in any way a relatively old and/or stable feature.

In light of this situation, I would argue that the older hypothesis of *vowel length* as the primary reason for the Type A / Type B opposition, independently put forward by Sergei Starostin and Zhengzhang Shangfang, remains far more plausible from both the typological point of view *and* the historical perspective (since it seems to correlate somewhat well with Tibeto-Burman evidence). Ironically, the authors themselves seem to have discovered a potential additional argument for this earlier interpretation, as they mention some of the early commentarial literature that refers to type-A and type-B syllables as 緩氣 *huǎnqì* 'slow breath' and 急氣 *jíqì* 'fast breath', respectively. Naturally, they try to fit these descriptions into their own argument ("'spoken with slow breath' would also be an appropriate description of a syllable beginning with a pharyngealized onset..."), but why should the issue be made so overcomplicated?

Summarizing all the observations and criticisms presented above (unfortunately, in a very compact form), my current conclusions about the monograph in general are as follows:

1. It presents an immense amount of both major amendments to the OC phonological system as reconstructed previously and minor amendments to specific reconstructions of particular words — all of which have to be taken into (critical) consideration in any subsequent research on OC.

2. It represents the first attempt to *systematically* bring into play comparative data from such previously untapped sources as early contact lexicon between OC and its non-Sino-Tibetan neighbours, and recently uncovered paleographic materials. A major accent should be placed on the word *systematically*, since many scholars had already paid occasional attention to all these matters; B&S are the first ones to place them square in the center of their study.

3. However, a most important methodological flaw on the authors' part is their quest for what I would call "*total explainability*", where just about any observ-

ed inconsistency or (possibly accidental) similarity may be granted the status of evidence, if it is needed to plug a hole in some part of the authors' highly convoluted system of OC. The result is a very shaky and vulnerable system of "correspondences" between phonetic series, MC readings, Proto-Mǐn reconstructions, early Hmong-Mien and Vietic borrowings, "word-family" connections, etc., whose multiple constituents do not always agree with each other and sometimes even give the impression of a "house of cards" that can easily be demolished with a single counter-example or a single piece of newly discovered evidence.

Scholars of OC historical phonetics will, no doubt, agree with some parts of the reconstruction and make use of its shakier parts as a stimulus for further research, so that even some of the shortcomings of B&S may eventually lead to new insights and discoveries. However, utmost caution must be exercised if one wants to make systematic use of *A New Reconstruction* for general reference purposes; at the very least, I would recommend *always* comparing the revised reconstructions with older, more conservative sources, such as Baxter 1992 or S. Starostin 1989.

For the moment, I would rather advocate viewing the book essentially as the current state of a work-in-progress (which is in itself, more or less, acknowledged by the authors themselves) — although, in my opinion, if further progress is indeed to be made, certain methodological principles should probably be amended. Possible recommendations would include a far more rigorous approach to issues of comparative semantics and grammatical derivation in the case of "word-families"; a less permissive stance on what should or should not be admitted as a "borrowing" between Chinese and its non-Sinitic neighbours, particularly in the OC epoch; a tighter statistical control over what should count as significant or accidental connection between or within different phonetic series; and, maybe most important of all, consistent corroboration of the results of the reconstruction by means of comparison with Tibeto-Burmese data. Of course, given the current generally unsatisfactory state of Tibeto-Burman reconstruction, the latter demand may sound too unrealistic; however, ultimately it is only the comparison of OC data on deeper levels of genetic relationship that can verify or disprove the bold claims made by the authors in their new monograph. Until then, most of these claims are bound to remain challenging, intriguing, and deeply controversial.

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