

The differing status of reconstruction in Trans-Himalayan and Indo-European

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Abstract

The replies to Fellner and Hill (this volume) present the practice of historical linguistics in the study of the Trans-Himalayan family as on the trail our Indo-European forbears blazed. The replies further present “word families” and “allofams” as beacons that light this path; we disagree. Our respondents overlook the different status of reconstructions in the two families. Research at the subgroup level that they point to as Neogrammarian implements a formalist approach to reconstruction, which, fine as far as it goes, lacks the sophistication of reconstructions in more mature disciplines. Not appreciating the different status of reconstruction in the two families, our respondents exaggerate the extent to which Indo-European evinces “word family”-like phenomena and present allofams as more synchronically plausible than they are.

Keywords

reconstruction – Trans-Himalayan – Indo-European – methodology – word families

Résumé

Les réponses à Fellner et Hill (ce volume) présentent la méthodologie de la linguistique historique en sino-tibétain telle qu'elle est pratiquée comme suivant en droite ligne le chemin basilé par les études indo-européennes.

Elles traitent les notions de "familles de mots" et "d'alofams" comme étant dans la lignée de ces études, un point de vue que nous ne partageons pas. Les auteurs des réponses négligent le fait que les reconstructions ont un statut différent dans les deux familles. Les recherches au niveau des sous-groupes qu'ils estiment comme étant néogrammairiennes implémentent une approche formaliste de la reconstruction qui, si elle est acceptable, est loin du niveau de sophistication que l'on trouve dans des disciplines plus matures. N'étant pas conscient du statut distinct des reconstructions entre les deux familles, les auteurs exagèrent l'étendue des phénomènes de type "familles de mots" en indo-européen et présentent les "alofams" comme ayant davantage de plausibilité synchronique qu'il n'en ont réellement.

Keywords

reconstruction - Transhimalayenne - Indo-Européen - méthodologie – familles de mots

1 Introduction

The explanations that one gives to beginning students are always an over-simplification. In a mature field, such as Indo-European, the beginner moves step by step under the guidance of masters to greater sophistication. The challenge for less well studied families is that historical linguists learn directly from textbooks. There is work in Trans-Himalayan linguistics that implements the comparative method in the formalist manner taught in textbooks (§2). However, because of an accumulation of changes other than regular sound change, the formalist approach becomes unsuitable when comparing more distantly related languages. The Proto-Trans-Himalayan reconstructions of James Matisoff stumble on this hurdle; they neither index correspondence patterns among attested languages nor predict attested forms through the application of ordered changes (§3). Neither the formalist approach nor Matisoff's practice fully implement the traditional comparative method. In particular, the latter includes analogy as an explanatory mechanism in language change (§4). The misapprehension of the nature of reconstruction in Trans-Himalayan and Indo-European accounts for our respondents' misperception that Indo-European is replete with alofams and that alofams are typologically plausible (§5).

2 Reconstructions as indices of correspondence patterns

The handbooks and the great theorists of the comparative method often present a reconstructed segment in a proto-language as merely an index to a correspondence pattern among attested languages.

Les «restitutions» ne sont rien autre chose que les signes par lesquels on exprime en abrégé les correspondances. (Meillet1903)

Cela est si bien l'essentiel qu'on pourrait désigner les éléments phoniques d'un idiome a reconstituer par des chiffres ou des signes quelconques. (Saussure1916)

Trans-Himalayanists working on reconstruction at the subgroup level have taken such teachings at face value, and, as Handel and Thurgood emphasize, have done excellent work. Nonetheless, this formalist approach quickly reaches its limit.

In the textbook understanding Indo-European **p-* is definable as the set {Gk. π -, Lat. *p-*, Skt. *p-*, OIr. $\emptyset$, Go. *f-*}, as seen in cognates such as Gk. $\pi\alpha\tau\acute{\eta}\rho$, Lat. *pater*, Skt. *pitr*, OIr. *athair*, and Go. *fadar*, all meaning 'father' (Clackson2007). However, not every **p-* reflects this pattern. The **p-* in **pénk^we* 'five' exhibits the alternative pattern {Gk. π -, Lat. *qu-*, Skt. *p-*, OIr. *c-*, Go. *f-*} (Gk. $\pi\acute{\epsilon}\nu\tau\epsilon$, Lat. *quīnque*, Skt. *pāñca*, OIr. *cóic*, Go. *fimf*); Latin has *qu-* instead of *p-* and Old Irish has *c-* instead of $\emptyset$. If a reconstruction is an index to a correspondence pattern, we must provide the two patterns with two separate indices, for example reconstructing the first as **p* and the second as some other kind of bilabial, but which?

In case of and **ph₂tér* 'father' and **pénk^we* 'five' the two competing correspondences patterns that underpin Indo-European **p-* are reconciled by the differing phonological context in which the two segments occur. In a given language, the attested form can be reached by the application of a series of perfectly regular sound changes in a specific order. In the case of 'father' one sound change in Gothic (**p-* > *f-*, Grimm's law) and one in Old Irish (**p-* > $\emptyset$) together suffice to explain the attested forms. In the case of 'five' we need the same change in Gothic, but instead require the changes **p-* ... *k^w* > **k^w* ... *k^w-* in both Irish and Latin; note that **p-* ... *k^w* > **k^w* ... *k^w-* must have occurred in Old Irish before the general loss of **p-*. The example of **ph₂tér* 'father' and **pénk^we* 'five' shows

that the explanatory value of a reconstruction comes not from its ability to index a correspondence pattern, but from pairing it with a language specific set of innovations that, when applied to the reconstruction in chronological order yield the attested form.¹

We do not intend to say that the formalist approach in Trans-Himalayan meso-level reconstruction has been phonologically naïve. The reconstructions that investigators associate with correspondence patterns are well motivated. Instead our complaint is that these works (e.g. **Bradley1979** and **jacques2014**) reconstruct a protolanguage by associating correspondence patterns with proto-segments, but never do the second half of the job, viz. tracing their reconstructions sound change by sound change back to the attested forms.

3 Matisoff's Trans-Himalayan reconstructions

If reconstructions in Trans-Himalayan follow the Neogrammarian tradition, then either a reconstruction indexes correspondences among attested languages (in the formalist approach) or it is possible to derive the attested forms of at least some Trans-Himalayan languages by applying to the published reconstructions an ordered series of sound changes (in the more advanced approach). Matisoff's Trans-Himalayan reconstructions meet neither of these conditions.

Consider Matisoff's reconstruction **l/b-ŋa* 'five', which Handel defends at some length. It is neither clear how this reconstruction indexes correspondences—nowhere has Matisoff said what correspondence pattern **l-* points to, nor is it possible to derive attested forms from this reconstruction through the application of explicitly identified and chronologically ordered changes. As far as we are able to discern, Matisoff gives the following reflexes of **l/b-ŋa* in his *Handbook*: Jinghpaw *məŋā*, Written Tibetan ཨྲ *lña*, Mizo *panga*, Maru *ŋó*, Abor Miri *a-ŋo*, Chang *ŋau*,² Mikir *pho*, Lepcha *fəngo*, Empeo *miŋeu*, Karbi *pəŋu*, Maram *miŋu*, Kwoirang *məŋyu*, Old Chinese 𠂔 *ngo*,³ Pumi Dayang *wǎN*, Kanauri *ŋa*, Written Burmese ငါး

1 For a step by step presentation of the journey from Indo-European **h₁oktōu* 'eight' to Old English *eahta*, illustrating this methodological point, see **Hill2019**.

2 Note that according to Matisoff whereas **l/b-ŋa* 'five' yield Chang *ŋau*, **ŋa* 'I, me' yields *ŋo* (**Matisoff2003**). Is the divergent vowel reflex an effect of the erstwhile complex onset? Matisoff offers no comment.

3 Reconstructed **C.ŋ^aa?* in Baxter & Sagart's system.

nā, and Lahu *ŋâ* (Matisoff2003). Handel groups these forms into five categories, the fourth of which, relevant only to Old Chinese, requires no discussion here.

- 1) Those that reflect, by regular sound change, a bare root **ŋa*
- 2) Those with a preceding labial element such as *b-*, *p^h-*, or *m-* that reflect **b-ŋa*
- 3) Those that reflect **l-ŋa*
- ...
- 5) Those with additional elements, such as **-r-* and **-ʔ* (Handel, this volume)

Regarding his types 1. and 3, forms can only be said to regularly descend from **ŋa* or **l-ŋa* if the regular phonology is known; does Pumi Dayang *wǎN* descend regularly from **ŋa*? Regarding his type 2, a Neogrammarian cannot countenance a reconstructed **b-* reflecting *b-*, *p^h-*, or *m-* willy-nilly in daughter languages, but must tell a story about where, when, and how this **b-* became here *p^h-* and there *m-*. Finally, regarding type 5, how does Handel know that *-r-* and *-ʔ* in those languages that contain them are ‘additional elements’? Perhaps the *Urform* is correctly reconstructed **ŋraʔ*; why not?

Handel continues:

F&H’s objections seem to be more esthetic than scientific; ... one could presumably satisfy them by rewriting a reconstructed form like **l/b-ŋa* as “**l-ŋa* and **b-ŋa* and **ŋa...*” But is that really more satisfactory? (Handel, this volume)

Yes, simply to explicitly write out Matisoff’s slash and hyphen filled reconstructions is more satisfactory, because reconstructions such as **l-ŋa*, **b-ŋa*, or **ŋa* hold out the hope of predicting attested forms.⁴ Better still is to specify which attested forms are predicted by which variant and specify the sound changes in each language (in order) that mechanically yield the attested forms when applied to the reconstructions. In other words, if Matisoff reconstructs **l/b-ŋa*

⁴ It was not obvious to us, but appears obvious to Handel, that **ŋa* is intended to be covered as an option under Matisoff’s **l/b-ŋa*.

‘five’ in Proto-Trans-Himalayan, he should tell us, *at least attempt to tell us*, how this reconstruction relates to a few attested forms.

4 Analogy

To improve upon Matisoff’s reconstructions we must move beyond the textbook portrayal of the comparative method and add an appreciation for analogy into the practice of reconstruction. The 15 etyma in Allen Nussbaum’s (Nussbaum1986) account of words for ‘head’ and ‘horn’ in the older Indo-European languages all descend ultimately from $*\acute{k}ér-h_2/*\acute{k}r-éh_2$, but none is entirely *lautgesetzlich*. The simplest case is Mycenaean Greek *kerā* ‘horn (material)’ (< $*\acute{k}ér-eh_2$), either a reflex of the rectus stem with an analogical full-grade of the suffix or a reflex of the obliquus stem with an analogical full-grade of the root. In contrast, the pathway from the same proto-form to Latin *cerebrum* (< $*\acute{k}érh_2sro-$) requires six steps, which include a variety of morphological affixations, analogical derivational developments, and semantic changes.⁵ In contrast to the importance of analogy in Indo-European, the word ‘analogy’ occurs only three times in Matisoff’s *Meisterwerk* on methodology (Matisoff1978) and

5 First, $*\acute{k}ér-h_2$, obl. $*\acute{k}r-éh_2 \rightarrow *kṛ-éh_2$, obl. $*kṛ-h_2-$ as a regular productive, so called ‘internal’, derivation (see discussion in Fellner and Hill, this volume, n. 41 and cf., e.g., $*\acute{s}iúH-mén$, obl. $*\acute{s}iúH-mén-$ in Skt. *syúma* ‘band’ $\rightarrow *sṛiúH-mén$, obl. $*sṛiúH-mn-$ in Gk. $\acute{\upsilon}\mu\acute{\iota}\nu$ ‘membrane’) (Nussbaum1986), accompanied by a change of meaning to ‘the head bone’. Second, the meaning shifted further to ‘skull, head’. Third, the analogy $*h_2eys$: obl. $*h_2us-es-$ ‘ear’ :: $*\acute{k}r-éh_2$: obl. X = $*kṛh_2-es-$, led to the obliquus stem becoming $*kṛh_2-es-$ (Nussbaum1986). Fourth, in Proto-Indo-European, in addition to the originally endingleless locative stem (with its own ablaut grade different from the rectus and obliquus), there existed several affixal markers to characterize the locative, the most prominent being $*-i$ and $*-er$ (cf. Ved. *uśás-i* ‘at dawn’ (paradigmatic locative of *uśas-*) < $*h_2us-és$ + $*-i$ next to (a substantive that arose by paradigmatic split of a locative) *uśar-* ‘thing at dawn’ < $*h_2us-s$ + $*-er$) the latter of which suffixed to our form gave $*kṛh_2-s-er$ ‘on the head’ (Nussbaum1986). Fifth, this form was itself turned into an adjective with the adjective forming suffix $*-ó-$ to yield $*kṛh_2-s-r-ó-$ ‘adj. in/at/on the head’ (cf. Ved. *usrá-* ‘early’ < $*h_2us-s-r-ó-$ (Nussbaum1986)). In the final step, this adjective is nominalized with a change of accent to $*\acute{k}érh_2sro-$ ‘thing on the head’ (Nussbaum1986); cf. Gk. $\lambda\epsilon\upsilon\kappa\acute{o}\varsigma$ ‘white’ : Gk. $\lambda\epsilon\upsilon\kappa\acute{o}\varsigma$ ‘white thing > whitefish’; Skt. *kṛśnás* ‘black’ : Skt. *kṛśnás* ‘black thing > black antelope’. Latin *cerebrum* is the direct *lautgesetzliche* outcome of $*\acute{k}érh_2sro-$. The somewhat surprising change $*-sr-$ > $-br-$ is regular in Latin (see Weiss2009).

always in its nontechnical sense. Without the benefit of analogy as an explanatory tool, it is no surprise that he is compelled to reconstruct competing variants of the same etymon; without analogy Nussbaum too would have been left with 15 allofams for ‘head’ and ‘horn’.

In writing that if “F&H have a preferred reconstructed form for ‘five’ ... it would be a welcome contribution to the field if they would present and justify their own proposals” (Handel, this volume), Handel appears to have overlooked our account of ‘five’, together with an analogical justification.

Matisoff reconstructs Tibeto-Burman $*l/b-ŋa$ ‘five’, with proto-variation between the initials $*l-$ and $*b-$ (2003: 149); it is more likely that only the lateral initial is correct (Tib. ལྷ་ *lña*, Dakpa *le³¹ŋe⁵³*) and that forms such as Mizo *pà-ngá* ‘five’ are contaminated by the labial initial inherited in the word for ‘four’ (Mizo *pà-lí*, Tib. འཇི་ *bži* < $*blʷi$, Kuröp *ble*). (Fellner and Hill, this volume)

A wider embracing of analogical explanations in Trans-Himalayan studies is likely to prove fruitful.

5 The implausibility of allofams

Schuessler and Handel emphasize that unexplained variation is characteristic of Indo-European, just as much as Trans-Himalayan.

IE handbooks and F&H convey the illusion that everything in IE is explained, or explainable, with perfect regularity by the classical comparative method. But soon a student realizes that introductory books cite the same few perfect examples ... yet when one leaves their comfort zone, perfect regularity is just out of reach. (Schuessler, this volume)

It is not the case that we (or Indo-European handbooks) convey the illusion that everything in Indo-European is explained. In our paper we are trying to show with examples of Indo-European how certain issues in Trans-Himalayan historical linguistics could be

framed differently and potentially be solved by adhering to universal principles of the comparative method that have proven time and again to be correct, reliable and efficient.

Our criticism of the “allofam” is not an attack on the field of Trans-Himalayan historical linguistics while at the same time elevating Indo-European historical linguistics in praise. We criticize the concept of “allofam” as an excuse for avoiding the systematic application of the comparative method and bring Indo-European—as a one of several methodologically mature branches of historical linguistics—as an example of what can be achieved once the comparative method and Neogrammarian rigor are brought to their full potential. Certainly not everything in Indo-European (or any other field of inquiry) is explained. However, that everything is potentially explainable is not only a conviction of the Neogrammarian approach or the comparative method, but of any scientific endeavor. It is the condition *sine qua non* for scientific progress.

Schuessler finds that “IE etymological dictionaries are full of misfits” (this volume). Once again, we did not claim that historical linguistics does not have problems in need of solving. Nonetheless, likely Schuessler’s choice of reference works exacerbated his impression of chaos in Indo-European. Kluge’s dictionary is not a specialist work and Pokorny’s, of which Schuessler rightly says that it “includes many items ... that cannot be related with precision to each other”, represents a stage or strain of Indo-European historical linguistics that did not live up to the standards of Neogrammarian methodology or the comparative method.

Of the specific Indo-European word families that Schuessler points to, some are chimeric (**skab^h-* / **skap-*) whereas others (Latin *gravis*) are too well understood to merit being called an allofam. He quotes Kluge¹⁹⁸⁹ concerning **skab^h-* / **skap-* constituting a “not firmly determined family”. The phrase “not firmly determined” means exactly that the alternation of **-b^h* ~ **-p* is not regular in the proto-language. Lumping forms together that vaguely mean and/or look the same under the umbrella term of “word family” or “allofam” without stating correspondence sets in an orderly fashion nor the potential ways these forms might relate is the very practice that we are criticizing. Indo-European linguistics was (and sometimes still is), of course, not immune from such lapses. Either **skab^h-* ‘shave, scratch’ and **skap-* ‘form, create’ (NHG *schaffen*) are simply just not related (the comparative evidence for **skap-* is meager), in

which case they should not be presented as a “word family” or, if they are related, there should be an attempt to explain how.⁶ In other words, **skab^h-* / **skap-* is a case like **keh₂p* (Lat. *capio*, Eng. ‘have’) and **g^{he}Hb^h* (Lat. *habeo*, Eng. ‘give’), treated in our article.

Schuessler notes that the Indo-European “stem **g^wṛ-ú* ‘heavy’ yields Skt *gur-úḥ* = Gk *βάρύς bar-ýs*”, but says about Latin *gravis* ‘heavy’ that it

is somehow connected, but how exactly is debatable (we should expect something like *†(g)vervis*). Yet Lat. *gravis* obviously belongs to the same root, same word family, and I for one would like to be made aware of that. (this volume).

In fact, Latin *gravis* relates straightforwardly to Skt. *gurú-* and Gk. *βάρύς barús* etc.; the starting point is the Proto-Indo-European proterokinetic u-stem **g^wréh₂-u-* / **g^wṛh₂-éu-*. The generalized zero-grade of the root **g^wṛh₂-u-* is continued in Sanskrit *gurú-* and Greek *βάρύς barús*, whereas Latin *gravis* < Proto-Italic **grau-* (< **g^wreh₂-u-*) continues the inherited rectus full-grade, plus an **i-* suffix. The extension of inherited u-stems by **i-* is a regular process in Proto-Latino-Faliscan (cf. Lat. *levis* ‘light’ vis-à-vis Skt. *raghú-*, Lat. *suāvis* ‘sweet’ vis-à-vis Skt. *svādú-* and Gk. *ἡδύς hedús*, Lat. *brevis* ‘short’ vis-à-vis Gk. *βραχύς brakhús* and Av. *mərəzu-* etc.).⁷

Making the same point as Schuessler about variation in Proto-Indo-European, Handel quotes the following passage from our paper:

The form **uek^w-es-* belongs to an originally proterokinetic s-stem **uek^w-os-* / **uek^w-és-* attested in Skt. *vácas-* and Gk. *ἔπος épos* ‘speech’. Therefore, the only “variants” of the root **uek^w* in the PIE nominal system are found in

6 If one wants to relate them there are linguistically motivated ways to do so that take no recourse to word families or allofams (see Kroonen2013).

7 For the sake of thoroughness we also clarify that (pace Kluge and Schuessler) NHG *Mistel* ‘mistletoe’ is not related to Lat. *viscum* ‘mistletoe; bird-lime’ (that seems to be related to Gk. *ἰξός ixós* ‘mistletoe, bird-lime’, OHG *wihсила* ‘morello’, and Russian *вишня víšnja* ‘sour cherry’, even though the exact relationship among these is debated). NHG *Mistel* is from Proto-Indo-European **me-sd-ilo-* lit. ‘thing that sits (**sed-*) in the middle (**me*)’ with the (diminutive) suffix **-ilo-* (cf. PGmc. **kurn-ila-* (: **kurna-* ‘grain’) > OE *cyrnel* > English ‘kernel’) common in plant names (Balles1999).

	acrostatic	proterokinetic	hysterokinetic	amphikinetic
rectus stem	R(ó)-S(∅)-E(∅)	R(é)-S(∅)-E(∅)	R(∅)-S(é)-E(∅)	R(é)-S(o)-E(∅)
obliquus stem	R(é)-S(∅)-E(∅)	R(∅)-S(é)-E(∅)	R(∅)-S(∅)-E(é)	R(∅)-S(∅)-E(é)

TABLE 1: Schematic representation of ablaut in Indo-European athematic nouns

... well-defined morphological contexts. (Fellner and Hill, this volume)

In reply Handel offers the following remarks:

The slash in “ $*\underline{\text{u}}\acute{\text{e}}\text{k}^w\text{-os-}/*\underline{\text{u}}\text{ek}^w\text{-és-}$ ” does a lot of work here. The $*e/o$ vowel alternation is not explained—it is treated as part of a single “stem”—but F&H nevertheless consider it non-problematic and undeserving of the term “variant”... Nevertheless, at least as far as I am aware, there is not a clear morphological function that can be associated with the alternation. (Handel, this volume)

The slash does do a lot of work here, but not the sort of work that Handel envisions. In discussion of an Indo-European noun ‘*A/*B’ means that *A is the rectus stem of the paradigm and *B is the obliquus stem of the paradigm.⁸ Thus, the ablaut seen in $*\underline{\text{u}}\acute{\text{e}}\text{k}^w\text{-os-} / *\underline{\text{u}}\text{ek}^w\text{-és-}$ is not an instance of variation, but reflects separate stems that exist in one and the same paradigm. This ablaut pattern is typical of proterokinetic s-stems in Proto-Indo-European (see Schindler1975). The term ‘proterokinetic’, again, is defined in our article as follows:

Proterokinetic: $*\text{péh}_2\text{-}\underline{\text{ur}}/\text{*ph}_2\text{-}\underline{\text{u}}\acute{\text{e}}\text{n-}$ ‘fire’, the nom.sg. $*\text{péh}_2\text{-}\underline{\text{ur}}$ is directly attested in Hitt. pah_2hur and the gen.sg. $*\text{ph}_2\text{-}\underline{\text{u}}\acute{\text{e}}\text{n-s}$ in Hitt. $\text{pah}_2\text{huenas}$. Another example is $*\text{g}^w\acute{\text{e}}\text{n-h}_2/\text{*g}^w\text{n-}\acute{\text{e}}\text{h}_2\text{-}$ ‘woman’ with the rectus stem nom.sg. $*\text{g}^w\acute{\text{e}}\text{n-h}_2$ in Skt. jáni- , Toch. B śana , OIr. ben and the obliquus stem $*\text{g}^w\text{n-}\acute{\text{e}}\text{h}_2\text{-}$ in Skt. gnā- , Toch. B śnoy , OIr. mná . (Fellner and Hill, this volume)

⁸ By contrast in Trans-Himalyan *A/*B appears to mean either *A or *B, with no effort to address the question of how the two forms may have differed semantically, morphologically, or temporally.

Table 1, also copied from our paper, presents an overview of ablaut in Indo-European noun classes, including the proterokinetic type.

The mistaken belief that Indo-European is replete with allofams is perhaps one ground that our respondents regard them as typologically plausible in general. For example, Handel takes issue with our position that a proto-language should not be reconstructed with multiple apparently etymologically related forms indicating the same meaning.

F&H say of ‘feather’: “[A]lthough it is correct to reconstruct both **pét-r-* and **pt-er-* in the proto-language *senso lato*, it is not the case that any one *état de langue* is hypothesized to have contained both. To have left matters at saying **pt-er-* and **pét-r-* are allofams would be unsatisfactory.” My response is: (a) Why could not one state of the language be hypothesized to contain both? (b) Why is it unsatisfactory? (Handel, this volume)

To leave both **pét-r* and **pt-ér* side by side in the proto-language meaning ‘wing’ bypasses a semantic problem and a morphological problem. Addressing the semantic problem first, “it is almost a truism that total synonymy is an extremely rare occurrence, a luxury which language can ill afford” (Ullmann1951).⁹ In French *deuxième* and *seconde* do not mean the same thing, neither do *Grapefruit* and *Pampelmuse* in German. Turning from synonyms to allofams, ‘holy’ and ‘hallowed’ do not mean the same thing; neither do ‘housewife’ and ‘hussy’. Natural languages simply do not have multiple words with identical meaning; reconstructed languages also should not. Turning to the morphological problem, saying that **pt-er-* and **pét-r* are ‘allofams’ is unsatisfactory because doing so leaves their derivational relationship unexplained.

Handel equates an abstinence from allofams with a refusal to examine potentially relevant data:

What we must not do is throw out the data and deny the very existence of the cognate sets before we have carefully examined all the evidence. (Handel, this volume)

Free ranging exploration and unconstrained experimentation are essential to all creative processes. Thus, the desultory collection of

⁹ See also Goodman1952 and Quine1951.

potentially cognates is imperative, but it is a duty to execute in your closet and not in the corners of the streets. When publishing, it is better to stick with solid results and leave behind exploratory speculation.

Handel appears to think that in order to have an etymology a word must be part of a word family. Regarding our acceptance of an anonymous referee's etymology for Old Chinese 𠂔 **mu* 'pupil of the 'eye', he objects

How could one make a hypothesis concerning the derivation of the individual lexical items within a word family unless the word family is first assembled and acknowledged? (Handel, this volume)

We do not understand the problem. One can propose that 'cranberry' is a compound of 'crane' and 'berry' without having first said "crane is an allofam of berry". One does not need a word family in order to propose an etymology.

In the following statement Handel correctly paraphrases one of our views, but misunderstands another. For the sake of clarity we number the two claims.

On the other hand, the approach that they advocate would entail claiming that [1] even obviously related Chinese and Tibetan words cannot be reconstructed to the proto-language because they fail to conform to the strict sound correspondences so far adduced between the two languages, and [2] any further investigation into a possible relationship—including possible morphological derivations—lacks rigor and so must be avoided. (Handel, this volume)

Regarding the first claim, yes, obviously related words cannot be reconstructed until the phonology is worked out; to propose a reconstruction you need to know which proto-segments yield which attested segments in which environments. Regarding the second claim, further investigation into morphological relationships is exactly what is needed. Just as the relationship between **pét-r* and **pt-ér* merits explanation, so does that between the protoforms that respectively give rise to Tibetan 𠂔 *lia* and Mizo *panga*.

Handel himself agrees that “our goal as historical linguists is to solve these problems; that is, to be able to describe in each case a more precise historical relationship linking the allofams in a word family” (this volume), but he still objects that “why F&H think that the use of the term ‘word family’ instead of the use of the term ‘problem’ somehow prevents us from explaining the historical-linguistic relationships represented by the word family is never made clear” (this volume). We do not know *why* the term *allofam* has prevented researchers from explaining the historical-linguistic relationships among cognate words; we simply observe that the term has impeded progress. It happens to be the case that not a single paper is devoted to looking at whether **k-yim* or **k-yum* is the more ancient form of the word for ‘house’,¹⁰ nor is there a single paper exploring the morphological meaning of the **l-* or **b-* in the word for five. Our explanation for the paucity of research attempting to account for ‘proto-variation’ is that presenting these as “allofams” has blinded researchers to the need for these problems to be solved.

6 Conclusion

As Handel perfectly paraphrases, we

...think that word families are nothing more than a convenient excuse for sloppiness and for the avoidance of rigorous application of the comparative method; that they enable and encourage scholars ... to carelessly throw together a bunch of look-alike words, casually assign a polyvalent proto-form to account for them all, and declare to the gullible that they have completed the reconstruction of the ancestral language; (Handel, this volume)

¹⁰ Handel points to ‘house’ as a case where in his view Matisoff’s (Matisoff2003) reconstructed variation between **k-yim* and **k-yum* is warranted, “because some languages reflect the **-im* variant (e.g. WT *khyim*) and some the **-um* variant (e.g. Meithei *yum*), while still others show language-internal variation” (this volume).