

Proto-Languages and Proto-Sprachgefühl

James A. Matisoff
University of California
Berkeley

In einem hochgewölbten engen gotischen Zimmer.
Weidert unruhig auf seinem Sessel am Pulte.

Habe nun, ach! Phonologie,
Angami, Khamngan, Gurung,
Und leider auch "Rek-theorie"
Durchaus studiert, mit heissem Bemühn.
Da steh' ich nun, ich armer Tor,
Und bin so klug als wie zuvor!

Note

This is a somewhat cut and edited version of the paper presented at the XIVth International Conference on Sino-Tibetan Languages and Linguistics, University of Florida (Gainesville), October 29-31, 1981. Since several footnotes were excised at the beginning of the paper, it will be noticed that the numbering of the notes jumps from 9 to 18. For this I beg the reader's indulgence - it seemed preferable to re-numbering all the rest of the 157 notes!

I would like to thank Paul K. Benedict, Nicholas C. Bodman, David Bradley, Axel Schüssler, and David Strecker for their valuable comments and criticisms, several of which have been incorporated into the notes and addenda.

JAM

	<u>Page</u>
1.0 Background and Introduction	2
2.0 Formalization vs. Explanation: turtles all the way down	3
2.1 Banishing synchronic exceptions	5
2.2 Weidert's 'tonogenetic laryngeal reconstruction theory': stuffing the proto-syllable final	6
3.0 Etymologies of the Angami Labiodental Words: segment- alism vs. prosodism, rigidity vs. variability	17
3.1 BEE 18	23
3.2 DOG 21	24
3.3 MONKEY 23	25
3.4 TWENTY 23	
3.8 The final solution to the problem of the origin of the Angami labiodentals	26
4.0 Stuffing the Proto-form	31
5.0 STAR and MOON	35
6.0 Matisoff's Inadequate Sources and Weidert's Justified Criticisms	37
7.0 Conclusion: a Plea for Mutual Tolerance and Cooperation	40
FOOTNOTES	42
REFERENCES	59
ADDENDA	63
SYMBOLS AND ABBREVIATIONS	63

1.0 Background and Introduction

In LTBA 6.1 (1-38), there appeared an article by Alfons K. Weidert entitled "Stars, Moon, Spirits, and the Affricates of Angami Naga: a Reply to James A. Matisoff." What Weidert was replying to was the manuscript version of my paper "Stars, moon, and spirits: bright beings of the night in Sino-Tibetan," which I had shown him in Mysore, India, in December 1978.¹ At that time Weidert expressed, in what seemed a friendly and constructive way, several criticisms of particular points in the paper, and challenged the accuracy of certain Naga forms I had gleaned from the inadequate sources at my disposal. Since I respected Weidert's firsthand knowledge of the phonology and lexicon of many Kuki-Naga and Barish languages (based on some five years of fieldwork in NE India), I was pleased to have his comments, and took careful note of them. My paper was not published until late in 1980. In its printed version, Weidert is quoted repeatedly.² Although none of his comments affected the main line of my argument or my conclusions, I felt they were certainly worth including, and in fact strengthened my paper by 'keeping me honest.'³

Late in 1979 or early in 1980, while my article was still awaiting publication in Gengo Kenkyū, Weidert sent his 'Reply' to Graham Thurgood for publication in LTBA. It came as quite a surprise, to say the least. The 47 pages of this MS included the comments he had made verbally in India, but much else besides. Most striking, however, was its tone - bitter, dogmatic, condescending, humorless, and self-righteous.

As is well-known, I am a mild-mannered sort, loath to indulge in scholarly vituperation. Yet clearly Weidert's attack had to be published and responded to. Thurgood undertook to edit it and tone it down somewhat. In my judgment Graham's edited version (published in LTBA 6.1) is an extremely skillful and fair representation of Weidert's original - in fact it is a great improvement on the original, both typographically (with well laid-out charts and diagrams, liberal use of italic type to set off forms, etc.) and stylistically.⁴

The basic point of my original paper was that the labiodentals of the Kohima dialect of Angami Naga /pf pfh bv f mv/ reflected a *velar-plus-labial combination at some earlier stage. Sometimes the labial element can be traced all the way back to PTB or even PST, but sometimes it is of quite recent 'secondary' origin.⁵ After a preliminary study of several TB etyma where Angami has a labiodental reflex, a cognate relationship is proposed between Angami *thèmvě* 'star' and the Chinese word for 'moon' 月, reflecting a PST root *s-ŋwa-t. The semantic interrelationships among ST words for MOON, STAR, and SPIRIT are explored in detail.

I submit that none of Weidert's criticisms seriously affect the main premises or conclusions of my paper.⁶ On the contrary, in his zeal to demolish my views, he commits a variety of fallacies in reasoning and interpretation of the data, demonstrating in fact that he has little feeling for the art of historical reconstruction, or what one might call 'proto-Sprachgefühl.'

Of far more interest than the particular points in dispute between Weidert and myself, are the general issues of the theory and technique of historical reconstruction which emerge from the discussion. In what follows, we shall try to strike a balance between detailed examination of the data and due attention to these theoretical issues.

2.0 Formalization vs. Explanation: turtles all the way down

Not content with the wealth of data he has amassed on the tones and segmental phonology of Kuki-Naga and Barish languages, Weidert feels obliged to theorize about it all with the utmost mathematical rigor. He is concerned with the deepest and most abstruse metatheoretical issues,⁷ and couches his descriptive and comparative statements in a forbiddingly formalistic and discursive way. He uses terminology like 'exclusively reflexive-metalinguistic sememe.'⁸ His rules bristle with Greek letters, brackets, and ligatures:

"First name vocative constructions (obligatory):

$$|_{-a_{\epsilon}} \Sigma_{\pi} |_{\sigma} + /_{\lambda} a/ \text{ (with enlarged } \sigma\text{-rule (4))}$$

$$|_{-\Sigma} \Sigma | \xrightarrow{\sigma} /_{-\Sigma}/$$

in male first name constructions)

$$|_{-i_{\epsilon}} \Sigma_{\pi} |_{\sigma} + /_{\lambda} i/.^{9}$$

At first the casual reader might mistake this obscurantism for profundity. (Maybe there's something wrong with me if I don't understand this?) But it soon becomes apparent that Weidert is hard to understand not because what he's saying is so conceptually difficult or all that new and revolutionary, but rather because it is so confused.

I feel that much of Weidert's work, both synchronic and diachronic, displays a severe confusion between formalization and explanation, a fallacy which we might characterize by the dictum 'I have explained it because I have symbolized it.' He is constantly claiming to have 'explained' something when in reality he has only restated it in a fancy formalistic or formulaic way. Far from clarifying the original problem, this procedure tends only to obfuscate it, pushing the discussion into such abstract realms that a realistic responsibility to the actual linguistic data can finally be lost.

* *

* * *

* *

2.1 Banishing synchronic exceptions

In the synchronic dimension, the key issue here is the treatment of semi-productive or sporadic patterns of morphophonemic alternation in a language. The messy notion of an 'exception' is abhorrent to Weidert, and he expends much effort trying to legislate exceptional cases away by writing 'rules' for them:

"The methodologically unwarranted status of 'exceptions' is ruled out in favour of n-ary phonological extension systems PE_1, \dots, n structuring the phonemic level provided that the 'exceptional' features involved be classed as phonetically/phonemically conditioned."¹⁸

Weidert confuses the phonetic features which are merely affected by the operation of a rule with the active, causal notion of 'conditioning factor'. Thus he describes the conditions for the operation of his 'morphological adjustment (μ) rules' in Lushai verb-alternations as follows:

"

$$\left| \begin{array}{c} +\Sigma \\ +\text{tone} \\ +\text{voc} \\ +\text{cons}_f \end{array} \right| \xrightarrow{\mu} \text{/CHANGE/ (Tone, vowel, and/or}$$

final consonant of a syllable are responsible [italics mine] for morphologically conditioned changes of syllables.)

<u>Conditioning factor</u>	<u>Input</u>	<u>Output</u>
τ	$ \text{sen} _{\mu}$	/-sen/ 'to be red'
τ, C_f	$ \text{'kap} _{\mu}$	/ka?/ 'to shoot'
τ, V	$ \text{-lian} _{\mu}$	/'len/ 'to be big'
τ, V, C_f	$ \text{-zuar} _{\mu}$	/zor?/ 'to sell'
C_f	$ \text{rit} _{\mu}$	/ri?/ 'to be heavy'." ¹⁹

A bit further down the page we are given a rule that describes the last type of alternation in the above chart:

$$\begin{array}{c}
 \left| \begin{array}{c} +\text{plosive}_f \\ -\text{tone} \end{array} \right| \xrightarrow{\mu} | \pm \text{glottal stop} | /
 \end{array}$$

(A final plosive in /CVP/ syllables either remains or changes to a glottal stop.)"

Among the examples cited (p. 60) are the pair:

$|\text{trok}|_{\mu} = / \text{trok} /$ 'to be blebby'

$|\text{bok}|_{\mu} \rightarrow / \text{bo}^? /$ 'to be down or recline (as animals)'.

Weidert 'accounts for' the difference here by writing an equalsign in the first case and an arrow in the second. But clearly this 'rule' has no predictive or explanatory power -- nobody can tell a priori which Lushai verbs will change their final plosives to glottal stop and which will not. The $-k$ is not a 'conditioning factor' but merely a party to two different types of morphophonemic relationships.²⁰ Which then are the 'regular' cases and which are the 'exceptions'? Weidert does not vouchsafe us the information as to which pattern is the more frequent in Lushai, but we may be sure that for him neither type of case is exceptional. Has he not written a rule that 'explains' them all?

2.2 Weidert's 'tonogenetic laryngeal reconstruction theory': stuffing the proto-syllable final

Weidert's most considerable contribution to TB comparative studies to date is his long article "The Sino-Tibetan tonogenetic laryngeal reconstruction theory"²¹ [henceforth 'Weidert 1979']. It contains a wealth of new information on the tone systems of many individual Kuki-Naga and Barish languages. In addition, Weidert has figured out some of the basic tone-correspondences among these languages and has laid a firm basis for further work in this field. For this pioneering work all Tibeto-Burmanists must be grateful.²²

Unfortunately, however, once Weidert ventures into the realm of diachronic 'explanations' for the correspondences he has discovered, he runs into serious trouble. His tendency to confuse formalization with explanation leads him to posit

phonetically and typologically implausible tonogenetic proto-entitles, which he then has to juggle and combine in bizarre sequences. His dislike of 'exceptions' makes him attempt to combine historically distinct morphophonemic processes into single sets of phonologically unmotivated and counterintuitive 'rules'. In trying to account for everything at once he only succeeds in creating cumbersome and overstuffed reconstructed forms that make it easy to overlook his real contributions and dismiss his work out of hand.

Weidert's basic thesis is that the four tone-classes of Proto-Kuki-Naga non-stopped syllables²³ arose through the influence of syllable-final laryngeal consonants and **-g*, each associated with a distinctive 'phonation type', thus:

<u>PKN Tone-class</u>	<u><*C_f</u>	<u>Phonation type</u>
TC-I	-Ø	clear voice
TC-II	-ʔ	creaky voice
TC-III	-h	breathy voice
TC-IV	-s	whispery voice

(Tone-class IV has the smallest membership of all the "TC's", and Weidert does not include it in his chart [p. 80].²⁴)

This sort of tonogenetic explanation is of course not original with Weidert, as he freely admits [pp. 60-62], although he seems unaware of just how widespread an idea it has been even in TB studies. The first linguist to offer coherent explanations of the origins of tones in terms of the influence of syllable-initial and -final consonants was A-G. Haudricourt, who brilliantly accounted for the birth of tonal distinctions in Chinese²⁵ and Vietnamese.²⁶ Haudricourt was one of the first to point out the tone-raising power of final *-ʔ* and the tone-lowering power of final *-h*,²⁷ as well as the fact that syllables with voiced initials universally tend to be pronounced on a lower pitch than those with voiceless initials.^{28, 28-f} Linguists like E.G. Pulleyblank²⁹ and Søren Egerod refined these ideas with respect to Chinese, and Egerod introduced the

phonetician Catford's term 'phonation types' into Sino-Tibetan discourse in an important paper (1971).³⁰ The term tonogenesis was coined by Matisoff in 1969, and first appeared in print in Matisoff 1970.³¹

Attempts have also been made to account for the tonal contrasts in the non-stopped syllables of Lolo-Burmese in terms of final *laryngeals, notably Pulleyblank 1963.³² D. Bradley has recently gone so far as to reinterpret the tones of Modern Burmese entirely in terms of phonation types, maintaining that Burmese is not, strictly speaking, a 'tone language' at all.³³ As far as Lolo-Burmese goes, my own feeling is that there is no comparative evidence to justify the reconstruction of a final consonant in Tone *2 ('heavy-tone') etyma at the PLB level, and very little correlation between PLB Tone *2 and roots in final *-s elsewhere in TB.³⁴ The whole question smacks of the chicken-and-the-egg anyway -- since that which we call 'tone' is an indissoluble mixture of simultaneous pitch and phonational features, it seems to me quite unnecessary always to insist on historical primacy for one or the other aspect of the whole feature-Gestalt.

Be all that as it may, it is clear that the world was not totally unprepared for Weidert's application of these ideas to the Kuki-Naga family.^{34a} To Weidert, however, belongs the dubious distinction of pushing this approach to its very limits -- and beyond.

The direct evidence Weidert adduces for *-? in TC-II and *-h in TC-III is suggestive, though hardly overwhelming. In a few modern languages (e.g. Lotha Naga, Mikir, Nocte, Tangsa, and several Baric languages) there are some modern glottal stops in words belonging to TC-II which Weidert considers to reflect a similar final laryngeal in PTB [pp. 78, 92].³⁵ Such glottal stops are never to be found in TC-I words in these languages.^{35a} As for *-h, no direct trace survives in TC-III words in any Kuki-Naga or Barish language, or in Jinghpaw.³⁶

However, for the sake of argument, let us make a leap of faith and assume that Weidert's tonogenetic explanation for the origins of PKN TC-II and TC-III is entirely correct. What have we gained? Weidert feels that the most striking confirmation of his syllable-final 'laryngeal rec-theory' is its applicability to the verbal alternations that have been described for a number of Kuki-Naga and Baric languages,³⁷ and devotes a long section of his article (pp. 94-113) to this topic. These alternating verbs come in grammatically differentiated pairs: the base form (or citation form) occurs in independent clauses, while the derived (or 'non-finite') form is found in embedded or dependent clauses (nominalizations, relative clauses, etc.). It is convenient to refer to these as Form A and Form B.³⁸ Morphophonemically, the B forms differ from their base-forms in a wide variety of ways -- in tone, vowel, and/or final consonant.³⁹

Weidert's attempt at a general historical explanation of these alternations founders on the rocks of his mechanical approach to reconstruction:

- (a) He tries to reduce several obviously distinct morpho-phonemic processes to one, forcing him to write diachronic 'rules' which are arbitrary and bizarre, lacking in either general phonetic plausibility or specific typological appropriateness. Exceptions to his main rules are 'explained' away by a patchwork of even more implausible ad hoc assumptions.
- (b) The phonological content he assigns to the main morphological proto-entity which triggers these rules is also highly implausible and 'atypological'.

Let us consider this last point first. Weidert starts with several valid observations. (1) Almost all verbs whose form A is in TC-I or TC-II and which end in a 'continuant',⁴⁰ undergo a tone-shift to TC-III in Form B. (2) If Form A ends in an open vowel, Form B ends in -t. (3) If Form A of such a

verb ends in the velar nasal $-ŋ$, this becomes the dental nasal $-n$ in Form B. Weidert correctly infers that these last facts point to a dental proto-suffix in Form B; but since TC-III derives from $*-h$ according to his theory, he feels obliged to reconstruct this suffix in such a way that it incorporates both dental and breathy features. Mechanically combining these features into a single proto-segment, what he comes up with is a voiced aspirated stop, $*-d^h$ [p. 98 ff.]!

It is almost embarrassing to have to point out the following elementary typological facts about TB and SEA'n languages in general:

- (a) Voiced aspirated stops are excessively rare in SEA'n languages, even in syllable-initial position.⁴¹, 41-A
- (b) Voiced aspirated stops never occur in syllable-final position in SEA'n languages, to my knowledge.
- (c) In TB, phonemic contrasts in voicing and aspiration are universally neutralized in syllable-final position, so that even plain voiced stops do not contrast with voiceless ones in that environment.⁴² Phonetically, final stops in TB are neither voiced nor voiceless, but simply lax and unreleased.

It is thus a drastic overspecification of redundant features to hypothesize a doubly marked proto-element like $*-d^h$ in final position, the position of maximum neutralization, where the most unmarked member of an opposition should occur.⁴³

It would be more favorable to Weidert's case to assume that he means his $*-d^h$ to be simply an abstract way of writing a prosodic formula meant to apply to the syllable as a whole, so that the superscript stands for a feature [+breathy] that manifests itself phonetically on the vowel. Yet this would be foreign to Weidert's usual 'segmentalist' way of thinking.⁴⁴ It seems clear that Weidert really does conceive of his $*-d^h$ as a consonantal entity -- perhaps as a consonant cluster,

since he permutes its parts in diachronic metathesis rules.
See Figure I.⁴⁵

These are the simple 'regular' developments! For the less beautifully regular ones, Weidert has more elaborate 'explanations' [see below].

It is hard to know where to begin in trying to make sense of all this. Weidert could have spared us this ordeal by recognizing that several different morphological patterns were employed to create the B forms, and by reconstructing more sensible proto-suffixes to do the job.

(A) For TC-I open syllables, *-t is the obvious choice:

<u>Tone-Class</u>	<u>Form A</u>	<u>Form B</u>
TC-I [open]	*CV	*CV-t > Lu. `CVt

A *-t suffix is well-attested in TB verbal morphology. [See Wolfenden 1929 and STC pp. 98-103. It appears in WT, Lepcha, Kachin, Newari, etc.]

In TC-I closed syllables, we need a suffix that is both dental in articulation (since Form A -ŋ > Form B -n) and that is plausibly associated with a breathy phonation type. For this *-s will do quite well:

<u>Tone-Class</u>	<u>Form A</u>	<u>Form B</u>
TC-I [closed]	*CV(V)M	*CV(V)M-s > ^{III} CV(V)M > Lu./CV(V)M/

The *-s suffix is also very well attested in TB verbal morphology.⁴⁹ This is the only consonant which is pronounceable after final liquids and nasals in languages like WT.⁵⁰ We can even accept a development whereby an *-s in combination with a preceding continuant would become transphonologized as a breathy articulation of the syllable's vowel.

Figure I

Proto Syllable Class	Syllable Type	Form A		Hypothesized Development of Form B
PC-I	Open	*CV		*CV-d ^h >*CV ^h d>Lushai/˘CVt/ ⁴⁶
	Closed live ⁴⁷	*CV(V)M		*CV(V)M-d ^h >*CV(V)M ^h d>*CV(V)M ^h > ^{III} CV(V)M>Lushai/˘CV(V)M/
TC-II	Open	*CV?	velar final class ⁴⁸	*CV?-d ^h > {pre-Lushai *CVV ^h g } > {pre-Lushai *CVVk>Lushai/˘CVk/ } {pre-Tiddim *CVVk>Tiddim/˘CVk/ }
			dental final class	*CV?-s-d ^h > {pre-Lushai *CVV ^h sd } {pre-Lushai *CVVt>Lushai/˘CVt/ } {pre-Tiddim *CVVt>Tiddim/˘CVt/ }
	Closed live	*CV(V)M?		*CV(V)M?-d ^h >*CV(V)M ^h d> *CV(V)M ^h > ^{III} CV(V)M> Lushai/˘CV(V)M/
TC-III	Open	*CVh	velar final class	*CV ^h -d ^h >*CV(V) ^h g>*CV(V)k >Lushai/˘CVk/
			glottal final class	*CV ^h -s-d ^h >*CV ^h sd>*CVs >Lushai, Tiddim/CV?/
	Closed live	*CV(V)Mh	non- alter- nating class	*CV(V)M ^h -d ^h >*CV(V)M ^h d> *CV(V)M ^h > ^{III} CV(V)M
			others	*CV(V)M ^h -s-d ^h >*CV(V)M ^h sd >*CVM ^h sd $\begin{cases} \text{IV}_{CVR?}>\text{Lu.,Ti./CV?/} \\ \text{IV}_{CVN?}>\text{Lu./˘CVN/}, \\ \text{Ti./CVP/} \end{cases}$

- (B) For TC-II closed syllables, the *-s suffix still works fine; even if we accept the hypothetical -? in Form A:

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-II [closed]	*CV(V)M?	*CV(V)M-s > ^{III} CV(V)M

Even Weidert drops his -? before *-d^h, so he can hardly object to our dropping it before *-s! A syllable cannot be simultaneously creaky and breathy. (If the -? really existed, it is certainly preferable to conceive of it as a prosodic (i.e. tonal) feature rather than as a consonant in a final cluster.)

For TC-II open syllables, Weidert has confused two separate morphological processes and tried to amalgamate them by a silly rule [see n. 47]. The more numerous class of these verbs has a final velar in Form B, while the other has a final dental. Weidert 'easily explains' this by positing 'assimilation' of his *-d to *-g because of the combined effect of the creaky-voiced root *CV?- and the breathy-voiced suffix *-d^h. This must surely rank as one of the most astounding assimilatory phenomena of all time.⁵¹ The class with final dental is 'effortlessly' explained by a double suffix, *-s-d^h, where the -s- blocks the operation of the assimilation rule.

How much more sensible simply to recognize two separate suffixes, *-t and *-k.⁵² Thus:

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-II [open]	(1) *CV?	*CV-t
	(2) *CV?	*CV-k

- (C) A similar down-to-earth approach may be applied to the more complex alternational patterns in TC-III verbs, which show five different Form B types, three in closed and two in open syllables.

In closed syllables, Form B is either identical to Form A or else has an added final consonant:

Non-alternating type

Here Weidert posits his suffix $*-\underline{d}^h$ in Form B, which he then deletes; thus: $*CV(V)Mh-\underline{d}^h > *CV(V)M^h d > *CV(V)M^h > III CV(V)M$. While it is great fun to add things and take them away again, I could like to make the counterproposal that in verbs of this class there is no suffix in Form B:⁵³

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-III [closed invariant]	$*CV(V)M^h$	$*CV(V)M^h$

For the remaining closed-syllable verbs of this tone-class, 'the derivation by means of $*-\underline{s}-\underline{d}^h$ is obvious' to Weidert [p. 102]. If the root ends in a semivowel or liquid, the effect of this double suffix is to produce a $-\underline{?}$ in Form B in both Lushai and Tiddim; for nasal-final roots, the double suffix is reflected by a nasal in Lushai Form B and the homorganic stop in Tiddim.⁵⁴ Weidert does not offer us any phonetic explanation of the development from $*-\underline{M}^h-\underline{s}-\underline{d}^h$ to the modern B forms, no doubt because this is so obvious.

However, by applying Occam's razor we may achieve the same result with considerably less symbol-shuffling, by merely positing the $*-\underline{s}$ suffix in Form B:

With liquid or semivowel root-final⁵⁵

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-III [closed alternating non-nasal root-final]	$*CV(V)R^h$	$*CVR-\underline{s}$

Since $*-\underline{s}$ regularly becomes $-\underline{?}$ in languages like Lushai, the B-forms present no problem:

'to be dirty'	<u>Lushai</u>	<u>Tiddim</u>
Form A:	-baal	`baal
Form B:	bal?	bal?

With nasal root-final

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-III [closed alternating nasal root- final]	*CV(V)M ^h	*CVM-s

The *-s simply drops in Lushai (after causing the assimilation of Form A -ŋ to -n). In Tiddim, the combination of nasal + *s is realized as the homorganic stop, undoubtedly via the intermediate stage nasal + ?.⁵⁶

	<u>Lushai</u>	<u>Tiddim</u>
'to appear'		
Form A:	-lan	`lan
Form B:	`lan	lat.

Finally, for TC-III open syllable verbs there are two alternational patterns, one with -k reflexes in Form B and the other with -?. These Weidert 'explains' by positing *-d^h and *-s-d^h, respectively, accounting for the modern class in -k by means of his powerful 'assimilation' rule [see the discussion under TC-II above].

Here again I would like to propose the radical alternative solution of deriving the forms in -k from the *-k suffix, and the forms in -? from the *-s suffix, as follows:

<u>Tone Class</u>	<u>Form A</u>	<u>Form B</u>
TC-III [open]	(1) *CV ^h	*CV-k (> Lushai and Tiddim CVk)
	(2) *CV ^h	*CV-s (> Lu. & Ti. CV?).

To summarize, we have distinguished several morphophonemic types of Form B verbs: those with zero suffix and those with consonantal suffixes that derive from *-s, *-t, or *-k. At this point Weidert might well protest that he has 'explained' everything with only two suffixes, *-d^h and *-s! While we have no wish to continue flogging a dead horsebird, we must reply:

- (a) *-d^h is totally implausible typologically, especially in syllable-final position;
- (b) the double suffix *-s-d^h Weidert is forced to posit is even more ridiculous typologically;
- (c) the 'rules' he needs to get from his overburdened proto-forms to their modern reflexes are utterly ad hoc and devoid of phonetic motivation.

Weidert's doggedly mechanical approach reveals a basic misunderstanding of the nature of morphological processes in natural language. It is extremely common for a particular grammatical relationship to be signalled formally in a variety of ways that are not reducible to a single proto-pattern. Inflectional and derivational processes are situated along a continuum of productivity, ranging from the almost completely 'regular' to the truly 'exceptional', and no amount of rule-writing can ever legislate this away. Far from being 'methodologically unwarranted', the existence of exceptional cases follows logically from the nature of language change itself.

When abundant historical documentation is available, a synchronic multiplicity of morphophonemic processes may sometimes be traced back to earlier phenomena in the language's history. Thus the bewildering variety of modern English 'irregular' past tense formations may be traced back more or less to the seven classes of Old English strong verbs,⁵⁷ and these in turn to the strong verb conjugations of Proto-Germanic and ultimately to PIE ablaut patterns. Yet even in cases like these, one can go only so far. Analogical levellings and restructurings have immensely complicated the developments,⁵⁸ and at any given historical stage of English it would be absurd to try to 'reduce' all the patterns to one or two 'basic' types -- or a fortiori, to derive the strong verb inflections from the regular 'weak' conjugation in "-ed."⁵⁹

When we are dealing with languages whose history is much more imperfectly known, it behooves us to be doubly wary of such reductionism.

The three suffixes **-s*, **-t*, and **-k* that I suggest for the Kuki-Chin B-forms do not seem an excessive number,⁶⁰ and are all attested elsewhere in TB, though it is now quite impossible to tell what their relative antiquity is. They probably never had absolutely clearcut or mutually exclusive semantic functions, and it seems certain that none of them was ever productively applicable to every verb in any TB language or in PTB itself.⁶¹ We can never explain why any given verb was 'conjugated' with one suffix rather than another. Yet it is far better to admit honestly that one can't 'explain' everything than to offer pseudo-explanations, especially when these do violence to what is known about the historical typology of the language family in question.

The historical linguist must avoid reasoning like Molière's successful candidate for the doctorate in medicine, who explained the sleeping power of opium by its 'dormitive virtue'.⁶² Nothing is easier than to offer an 'explanation' that raises as many questions as it answers. One is reminded of the elderly lady who explained the earth's stability by hypothesizing that it was standing on the back of a gigantic turtle. When William James gravely asked her what the turtle itself was standing on, she immediately replied, 'Another turtle, of course.' When the philosopher persisted with the obvious next question, the lady chortled in triumph and shouted, 'This will do you no good, Mr. James! It is turtles all the way down!'

We can only agree wholeheartedly with Weidert when he says, "What ultimately determines the acceptance of a theory such as Rec-T_L [the 'final-laryngeal tonogenetic rec-theory'] is the ease, explanatory power and elegance with which this theory is able to explain the empirical data."⁶³

3.0 Etymologies of the Angami Labiodental Words: Segmentalism vs. Prosodism / Rigidity vs. Variability

It is perhaps time to return to Weidert's attack on my "Stars, moon and spirits" article. It will be recalled that

in order to establish the plausibility of identifying Angami thèmvǝ 'star' as cognate to Chinese 星 (OC *ngiwǝt), I first presented in detail a series of seven etyma where Angami labiodentals correspond to labiovelars in other Sino-Tibetan languages: BEE, DOG, MONKEY, TWENTY, NINE, BITTER, and GOAT.⁶⁴

My purpose was to demonstrate that

'The Angami labiodentals ... are consistent reflexes of distinct entities that must be set up at various time-depths for Proto-Angami, for PTB or for PST itself. These ancestral sounds were not labiodental, but rather labiovelar.'⁶⁵

These seven etymologies are all, I submit, extremely good, and most are simply unimpeachable. Yet Weidert attacks every single one of them indiscriminately with a variety of arguments so weak and confused that it is hard to imagine that he can believe them or make sense of them himself.

Insofar as Weidert's attack represents a coherent intellectual position at all, it would seem to reflect two extreme theoretical views: (1) a kind of rigid 'segmentalism' which holds that a feature like labiality must inhere in one and the same (or one-and-only-one) part of the syllable at all stages of a language's history; and (2) a view that proto-forms are monolithic and invariant entities, so that reconstructions must incorporate in a single form prototypes of all *variant forms in the daughter languages*.⁶⁶

Let us quickly look at each of the seven etyma in question.

3.1 BEE⁶⁷

Angami mèpfɿ 'bee' is shown to derive from the well-attested PTB root *(m-)kwa.y [STC #157], for which there is also a Chinese cognate 蜂 'kind of small wasp' [GSR 351c], reconstructed *kway in Bodman 1980, p. 138 (#294).

Weidert proceeds to the attack in a priceless passage which is worth quoting at length:

'With regard to BEE, Matisoff states: "In this root the labial element must be postulated at the PTB level, since it is attested in Lolo-Burmese, Kuki-Chin-Naga, Nungish, and Himalayish" ... The claim implicit in this statement is that the initial and the coda of the reconstructed etymon for BEE should be separated as follows:

<u>*Initial</u>	<u>*Coda</u>
kw-	-a.y

This is exactly what Benedict has reconstructed (*kwa.y) for PTB. Amazingly, in this kind of etymologization a negative transfer or linguistic interference phenomenon can be discovered which is typical of English speakers and has so far gone unnoticed in the literature. To see this point, two more BEE forms ... must be quoted: Lushai khuai, Tiddim Chin -xuai. In both languages, the -u- element is by no means a post-consonantal bilabial non-vocalic off-glide; rather /-ua-/ is phonetically a separate entity ... and has to be treated accordingly as a *separate phonemic unit* ... Initial and coda of BEE now have to be separated as follows:

<u>*Initial</u>	<u>*Coda</u>
k(h-)	uai ., ⁶⁸

This simplistic twaddle (complete with its sweeping slur on Anglophone linguists) is typical of the quality of Weidert's critique. In my entire discussion of the labiovelar prototypes of Angami labiodentals, there is never any claim that the *labial element was necessarily either 'part of' the initial or 'part of' the rhyme. This is a pseudo-issue! As even beginning students of TB soon come to understand, the semivowels -w- and -y-, because of their dual vocalic/consonantal nature, are capable of intimate phonetic interaction both with the syllable's initial consonant and its nuclear vowel. When I speak of 'labiovelars' I simply mean 'velar-cum-labial,' or 'velar plus a labial element.' In a given daughter language at a certain point in time there may indeed be synchronic structural reasons for preferring to

write the -w- as a superscript (k^w) or on the line (kw), but this is a phonemic question. It is quite irrelevant to our diachronic discussion whether in any daughter language we would prefer to analyze a labiovelar complex as 'one phoneme or two'.⁶⁹ When we are dealing with conditioning factors for diachronic developments what is crucial is simply the presence or absence of a phonetic feature at some earlier stage. We can only tell a posteriori whether a *-w- (or to use a more neutral though less convenient symbol, a *-u-) stood in a closer phonetic relationship to the preceding consonant or the following vowel.⁷⁰ Medial semivowels by their very nature are 'Janus-headed', looking backwards and forwards at the same time. Nothing is more unstable diachronically than the 'peak of sonority' of a syllable. Throughout the history of the tightly-knit monosyllables characteristic of the ST languages, 'certain phonetic features seem to have bounced back and forth from vowel to consonant and back again.'⁷¹ Weidert's dogmatic statement that 'it is phonetically more plausible to explain a change from -ua- to ^wa than vice versa',⁷² is thoroughly preposterous, both for languages in general and for ST in particular.

Weidert even seems to believe that if a [-w-] is considered to be part of a syllable's initial consonant, then the nuclear vowel must develop diachronically as if the syllable contained no labial element at all⁷³ -- i.e. as if the mere fact of the linguist's writing the w above the line were enough to 'protect' the vowel from its influence! Does he then believe that initial consonants in general are powerless to condition diachronic developments in the following vowel?

To sum up, all that was required to trigger the development of the Angami labiodentals was a -w- (or -^w or -^u) that came in contact with a velar initial. Weidert's disingenuous quibbling about whether the labial element should be assigned to the initial consonant or the rhyme is powerless to cast doubt on the phonetic existence of this labial conditioning factor itself, either for BEE or for any of the other etyma in question, as we shall see.

3.2 DOG⁷⁴

The labial element in this complex root has been established beyond any shadow of doubt for the PTB and PST levels. The 'basic allofam' was *kwiy [STC #159], though a number of prefixes are reflected in various TB languages, including *d-, *n-, and *s-. To complicate matters, the velar element was not always preserved: in some languages it was metanalyzed off as a prefix, so that the labial appeared in initial position; in other cases the velar seems to have been pre-empted by the sibilant prefix.⁷⁵

I referred Angami tèfó to an allofam with dental prefix, *d-kwiy (reflected also in Jili təkwi and Nung təgi).

In the face of the overwhelming evidence to the contrary, Weidert refuses to recognize a velar-plus-labial combination at the PTB level, and sets up the etymon as PTB *s-xyui?.⁷⁶ He offers no other example of this consonant combination in TB, but declares the Angami f in tèfó to be the 'regular development' from *s-xy- ['Reply' p. 11], with the following intermediate stages:

*s-xyui? > *t-xy^wə? > *t-x^wə? > *t-f^wə > tèfó.⁷⁷

'The other developments are straightforward: Tibetan *x- > kh- (or *xy- > khy-) ... Kachin *xy- > *ɣ- > g-, etc.'⁷⁸

These 'straightforward developments' are of course mere rearrangements of the elaborate figments of Weidert's imagination. No independent evidence is anywhere adduced to motivate setting up the new PTB phoneme *x- rather than the *k- that has heretofore gone unchallenged by Tibeto-Burmanists. There is nothing to corroborate the proposed series of developments in Kachin, etc. With equal pseudo-precision we might just as well write

Kachin *xy- > *ɣy- > *ɣž- > *gž- > *gr- > *g-

(each individual stage of which is 'straightforward phonetically'), or any other string of symbols which pleases us.

In my paper I suggested that Angami f- (rather than the expected pf-) in this root was the result of the rare and complex consonant combination *d-kw, but freely admitted this was 'an ad hoc explanation'.⁷⁹

It is always possible and sometimes necessary to invent an ad hoc explanation for an anomalous case. It is even true that some such ad hoc 'solutions' are more plausible than others. The only harm is in deluding oneself that an explanation which covers only a single case establishes a 'regularity'. Consider the Lahu reflexes to the following well-known set of forms:⁸⁰

	PTB	PLB Tone	WB	Lahu
'wind'	* <u>q-liy</u> [STC 454]	*1	<u>le</u>	mû-hɔ̌
'boat'	* <u>m-liy</u> [STC 474]	*1	? <u>ahle</u>	hɔ̌-lòʔ-qō
'heavy'	* <u>s-liy</u> [STC 95]	*2	<u>lê</u>	<u>hô</u>
'bow' (n)	* <u>d-liy</u> [STC 463]	*2	<u>lê</u>	<u>hô</u> -ma
'grandchild'	* <u>b-liy</u> [STC 448]	*2	<u>mrê</u> [mliy in Inscriptions]	ɔ̌-hɔ̌ ~ ɔ̌-hwē
'four'	* <u>b-liy</u> [STC 410]	*2	<u>lê</u>	<u>ɔ̌</u>

Why is the initial in the Lahu form for FOUR zero rather than h? A dozen ad hoc 'explanations' are possible -- and I've tried them all! (Unfortunately, however, I have been hampered by the lack of a 'Rek-theorie' to enable me to decide which one is right.) Hopefully Weidert will be able to come to the rescue and solve this problem for us Lolo-Burmanists once and for all, by setting up an initial proto-cluster like *?xmbɩ- which regularly became Lahu zero.⁸¹

3.3 MONKEY⁸²

I derive Kohima Angami tèpfí 'monkey' from the well-established TB root *woy [STC #314], specifically from a doubly-prefixed allofam *d-k-woy which is directly attested by the Khonoma Angami form tekwi.⁸³ This is rejected out of hand by Weidert, exclusively on tonal grounds, even though Weidert admits that the tone correspondences are irregular in this set (reflecting variation between his TC-I and TC-II)!⁸⁴ This is such a weak argument that it is hard to believe it is made in good faith.

3.4 TWENTY⁸⁵

I derive Angami mèpfǎ from PKN *m-kwul, an allofam of the PTB form *m-kul set up in STC #397. For once Weidert offers no objection to positing a -w- in this etymon, but prefers the more complex reconstruction *m-gywul ~ *m-gwyul, since this 'explains everything' ['Reply', p. 18].

3.5 NINE⁸⁶

For this complex etymon, several different prefixes are attested in one or another daughter language (*d-, *s-, and *b-). In addition the root itself shows variation between *-kuw and *-kwa. I refer Angami thèpfǎ to the allofam *s-kwa.⁸⁷

Weidert complains about 'the troublesome variability of the prefixal element in most of the languages, a problem not encountered with the other word-classes (nouns and verbs)'.⁸⁸ This astonishing statement reveals that Weidert does not have much familiarity with comparative TB data. Prefix-variation is found in a large number of nominal and verbal roots, of which ANT (PTB *-rwak) [STC #199] will serve as an example:

- *k-rwak (> WT grog-ma, Gyarung kǒrǒk, Lohorong khörök);
- *p-rwak (> WB pərwak, Lahu pú-ǵǵ?);
- *s-rwak (> Nung səɾɔ);
- *d-rwak (> Miri təruk, Dafla torub).

Of course Weidert would probably not wish to consider these initial elements to be prefixes at all, and would doubtless wish to reconstruct a cluster that could 'explain everything' in a 'regular' manner, perhaps *kpsdrwak.⁸⁹

As it is, he suggests for NINE a number of similar reconstructions to get rid of the prefixal variability, though he finds it hard to choose between *sdua?, *dsqua?, and *tzqua?, since they all have 'the same explanatory result.'⁹⁰

3.6 BITTER⁹¹

The widespread root BITTER is reconstructed simply as *ka [STC #8], with no labial element. I derived the Angami form pfhə from a secondary labiovelar which arose from the reduction of a dissyllabic prototype *ka-ba, where the last syllable is the verbal-noun or 'infinitive' suffix -pa * -ba:

*ka-ba > *kaβa > *kawa > *kəwa (or (kəa) > *kwa > *k^wa > Ang. pfhə.

This suffix crops up all over TB and is directly attested for this root by WT kha-ba 'to be bitter; being bitter'.⁹²

Against this hypothesis Weidert offers a particularly powerful argument:

'The attempt to generate a medial -w- from a suffixed form as found in Classical Tibetan kha-ba ... is doomed to failure because this would be the most improbable explanation of all for the Angami labiodental affricate.'⁹³

This is, 'it can't be so because I say it isn't so.'

As an example of an intermediate stage in the reduction of the bisyllabic prototype I offered Lotha khoa.⁹⁴ Weidert minces no words about this form, saying flatly:

'The Lotha form khoa that Matisoff quotes ... does not exist. The correct Lotha form is /²ko/.⁹⁵

Here again I can't be sure whether Weidert really does not understand what I said, or is just pretending not to. The Lotha form khoa certainly does exist -- and I have since heard it with my own ears! It is, to be sure, bimorphemic, and should be segmented as kho-a.⁹⁶ I recently elicited this verb from Ms. Wonjano Ezung (Sept. 1981), a native speaker of Lotha,⁹⁷ who explained that kho- 'made no sense' all by itself. The suffix -a is usually used when a Lotha verb is cited in isolation,⁹⁸ and examples may be multiplied at will: tso-a 'to eat' (cf. WT (b)za-ba), yu-a 'to drink', yip-a 'to sleep' (cf. WT yib-pa 'hide oneself', WB ?ip 'sleep', Ao yip, 'id.', etc.), phanthi-a 'to stand', etc.

How are we to make Weidert understand the point at issue here, and how are we to convince him of the existence of the Lotha form? Maybe a few Greek letters would help:⁹⁹

$$(1) \pi\tau\beta\epsilon_{-\alpha_{\rho\mu}} \rightarrow \lambda\nu_{-o}$$

$$(2) \pi\tau\beta\epsilon_{-\alpha_{\sigma}} \rightarrow \lambda\nu_{-a}$$

$$(3) \pi\tau\beta\epsilon/ka/_{\rho\mu} + \pi\tau\beta\epsilon/ba/_{\sigma} \rightarrow \lambda\nu/kho/_{\rho\mu} + /a/_{\sigma}$$

'bitter' + 'infinitive suffix'

This having been said, I am now prepared to withdraw my hypothesis about the dissyllabic origin of Angami BITTER in the light of new evidence Weidert presents, specifically the sets for SPAN and CHIN. A more general explanation of the origin of the Angami labiodentals, incorporating Weidert's valid criticisms but preserving the essence of my own thesis, will be presented below [3.8], after a brief comment on GOAT.

3.7 GOAT/CATTLE¹⁰⁰

I derive Angami tèmvó 'goat' from PTB *-ŋwa 'cattle' [STC #215], hypothesizing the same development from *labio-velar to labiodental nasals as was demonstrated for the stops.^{100-A}

Weidert's objection to this etymology is not on semantic grounds,¹⁰¹ but based entirely on 'irregularities' in the rhyme correspondences between Angami and other KN languages. After due reflection he sets up *-eel as the rhyme for this etymon -- but then proceeds to rob his argument of all force by admitting that it is doubtful that Angami m̃ə could derive from 'a rime such as *-eel'!¹⁰²

Since the details of the Lautgesetze for KN rhymes have obviously not been totally worked out, even by Weidert, and since -ə is obviously the most frequent Angami reflex of PTB etyma in *-a, including several after nasal initials,¹⁰³ I see no reason to change my view about this etymology.

Weidert's objection [p. 12] that the Zemei form l¹he⁴meu requires a closed syllable provenience is not decisive, since it is perfectly possible that this form reflects an allofam with a suffix of some kind.

3.8 The final solution to the problem of the origin of the Angami Labiodentals

We can only arrive at an Endlösung of this intricate problem if we are eclectic and open-minded. As we shall see, Weidert has made several important contributions to the solution by his critique of my original paper. Unfortunately, in his zeal to demolish my views entirely, he has thrown out the baby with the bathwater, and rejected every single etymology I proposed. For my explanation in terms of a medial *labial element he has substituted an unrevealing and problematic counter-proposal.

As far as I understand it, Weidert explains the origin of the Angami labiodentals by using as conditioning factors both certain proto-vowels [only *a and *-ua(-)]¹⁰⁴ and the modern Angami vowel /-ə/ [w].¹⁰⁵ Thus the claim is that if a labiodental occurs in an Angami word before a vowel other than -ə, it must have arisen from an etymon in *-(u)a- (e.g. BEE). If this cannot be demonstrated, then the word must be left out of consideration entirely (e.g. MONKEY).

There are severe problems with Weidert's counter-proposal, but his positive contributions to the clarification of this problem are several:

- (1) Weidert provided two more examples, CHIN and SPAN, of words which began with PTB simple velars before **-a*, where there is no evidence at all of a labial element in the proto-rhyme, but where Angami has developed labiodentals:

CHIN PTB **m-ka* [STC #470] > Ang. ⁵_u²_{me}¹*pfhə*,
Chakhesang ²_{me}¹*khu*;

SPAN PTB **ka* [STC #469] 'divaricate, be stretched apart, open wide' > Ang. ¹*pfə* 'span', Chakhesang ¹*ku* 'id.'¹⁰⁶

My ingenious explanation of the Angami reflex of BITTER (PTB **ka* > Ang. ¹*pfhə*, Chak. ¹*khu*) in terms of a disyllabic prototype [above 4.6] is now no longer necessary, since Weidert has demonstrated that syllables of this type regularly underwent dentilabialization even in the absence of a labial element in the proto-rhyme.

Weidert's proposed development [p. 15] **ka* > **kə* > **k^wə* > Ang. **k^wu* > **p^wu* > *pfə* is, however, overelaborate and unrevealing. Comparative evidence suggests that the first sound-change to affect **ka*-syllables was the backing and raising of the vowel to *-u* (as in the Chakhesang forms above). Several good etyma with PTB **-a* have ended up with *-u* in Angami after other initials, e.g. 'five' PTB **b-ŋa* [STC #78] > Ang. ³_{pe}²*ŋu* and 'tooth' PTB **s-wa* [STC #437] > Ang. ⁵_u²*hu*.¹⁰⁷ We may next suppose that a 'parasitic' semivocalic glide developed between the velar and the *u* (**ku* > **k^wu*) as attested in Monshang,¹⁰⁸ Lotha, and Loloish languages like Lisu and Jino.¹⁰⁹ At this point we have clear sailing to the development of labiodentals:

**ka* > Angami **ku* > **k^wu* > **k^wu* > *pfə*.

This analysis has the advantage of a unified explanation of the labiodentalization process in terms of a labial

vowel or semivowel. A similar rationale can be given for the behavior of *-a after labial stop initials [next section]. There is thus no need at all to give up the labial conditioning factor for this process, but merely to emphasize that in some instances this labial was secondary while in others it went all the way back to PTB or PST.

- (2) Weidert has also shown that dentilabialization occurred in words with initial labial stop:

THIN / PTB *ba [STC #25] > Ang. ²rə⁵pfə, Chakhesang ²re⁴pu
 SEARCH / Ang. ⁵pfhə, Chak. ⁴phu
 MALE/FATHER / Ang. ⁵u¹phu, Chak. ²u¹pu
 CARRY (as baby on back) / Ang. ¹pfə, Chak. ¹βɣ. 110

The first three of these sets Weidert reconstructs with PKN *-a, and the last with *-ua. There is no reason to dispute the absence of a labial element at the PTB level after the initial in THIN. For the three other words, however, there is strong evidence of *-a * *-u interchange in TB. Though CARRY presents no problem in any case, since Weidert concedes the labial medial, it should be pointed out that there is another PTB root *buw 'carry on the back' [STC #28] which undoubtedly stands in an allofamic relationship with the root set up as *ba 'carry' [STC #26].

For SEARCH, there is also a general TB root with -u- vocalism with the meaning TURN OVER/SEARCH FOR: 111
 WT h**ub**ub 'turn upside down', PLB *p**u**p (Lh. phū? 'turn over; search for', Akha po^ 'roll over; search for', Luquan p'u 22s 'turn over' [TSR #19]).

For MALE/FATHER, where Angami has the unexplained reflex -u rather than ə, there are also two general TB roots attested, one with *-(w)a and one with *-u. For FATHER there is *pa [STC #24], which Benedict revised to *pwa on the basis of forms with w- initial (e.g. Jinghpaw

wa). But there is also a Lolo-Burmese root *pu¹ 'male' which appears, e.g. in the Lahu names for the male of certain animal species: pɛ̃-phu 'male fowl' (archaic); chi-phu-qā 'male barking deer'; ḡā'-phu-qā 'cock, rooster'; ho-phu-qā 'bull elephant', etc. (Cf. also the well-attested root *puw 'grandfather/elder brother' [STC #23], also probably related.)

In any case, nothing is more natural than for a labial initial or semivowel to condition rounding of the following vowel. We may suppose the following type of development (parallel to that hypothesized for the velars, above):

(a) $\left. \begin{array}{l} *pu \\ *pwa \end{array} \right\} > \text{Angami } pfu \text{ (FATHER)}$

(b) $*pa > \text{Angami } *pu > *p^w u > *p^w u > pfə \text{ (THIN, SEARCH, CARRY).}$

(Presumably the etyma that already had a labial component in the proto-rhyme affricated their initials before those whose rhyme was simply *-a).

- (3) Finally, Weidert has made a positive contribution by documenting the fact that 'the labiodental affricates in Angami are the final phonetic stage in a sequence of changes all synchronically observable in other KN languages.'¹¹² Some languages only go so far as to change *-a to -u, or -u to -wu or *-wa to -vw, or *-wu to -v^wu, etc., without going so far as to change velar initials into labials.

To summarize then, we may distinguish three classes of cases where etyma developed labiodentalization in Angami:

(A) Primary medial -u/-w-

In these cases there is extra-KCN evidence for a labial element between the proto-initial and the nuclear vowel. It is senseless to quibble over whether this should be written "-w-" or "-u-".¹¹³ (Perhaps we can compromise by writing it "-u-")

These etyma include:

BEE	* <u>m-kwa.y</u>	GOAT	* <u>d-ŋwa-</u>
DOG	* <u>d-kwi.y</u>	STAR	* <u>s-ŋwa-</u>
NINE	* <u>s-kwa</u> ¹¹⁴	FATHER	* <u>pwa</u> * <u>pu</u> . ¹¹⁵

In this general category we may also include MONKEY, where the labial element is the root initial, and is preceded by a prefix and followed by the nuclear vowel:

MONKEY *d-k-woy or *d-b-woy.¹¹⁶

(B) Primary vocalic -u-

In these cases, the nuclear vowel of the root was -u- at the PTB level:

TWENTY	* <u>m-k(w)ul</u>
NINE	* <u>d-kuw</u> * <u>s-kuw</u> * <u>b-kuw</u> . ¹¹⁷
MALE/(GRAND)FATHER	* <u>pu</u>

(C) Secondary vocalic -u- (< *-a)

The credit for the identification of this class belongs to Weidert, though he did not apply his insight in a constructive manner.

(1) After velar initials

BITTER	* <u>ka</u>	} > pre-Angami * <u>ku</u> > * <u>k^wu</u> etc.
SPAN	* <u>ka</u>	
CHIN	* <u>m-ka</u>	

(2) After labial initials

THIN	} > pre-Angami * <u>bu</u> > * <u>b^wu</u> etc.
CARRY ON BACK	
SEARCH	
	* <u>pa</u> > pre-Angami * <u>pu</u> > * <u>p^wu</u> etc.

* * *

By now the patient reader will have realized that this whole controversy is something of a tempest in a teapot. For

all his bluster about 'having to create a bilabial medial element from nowhere',¹¹⁸ to explain the origin of the Angami labiodentals, Weidert himself needs to posit *-ua- to handle many of the etyma in question. For the others, our analyses differ only as to the particular point in time at which the labial element appeared.¹¹⁹ Weidert misinterprets my original analysis as a claim that all Angami labiodentals can be traced back to labiovelars at the PTB or PST level - but actually the new data he brought to bear on the problem only reinforce the 'cyclical' approach I had adopted from the beginning:¹²⁰ some of the labial conditioning factors were primary, others developed secondarily.¹²¹

Weidert would have saved us all a lot of trouble if he only had made the effort to understand what I was saying in the first place.

4.0 Stuffing the Proto-form

In a highly interesting passage,¹²² Weidert compares the reconstructional techniques of Benedict, Matisoff, and himself with respect to etyma where there are complex or unusual correspondences in syllable-initial position.

First Weidert gives a kind of parody of Benedict's approach to 'teleoreconstruction':¹²³

'If there occur different reflexes of proto-units in different languages that are hard to combine either to single homogeneous sounds or into consonant clusters, one should resort to the better-known or historically attested languages and "reconstruct" a form from which at least the forms in the better-known languages can be derived without too many intermediary phonetic developments ...'

He then mentions Matisoff's 'composite reconstruction of etyma by means of allofams',¹²⁴ whereby 'all the various forms of one and the same etymon are grouped into sets of sub-forms that share only a restricted number of common elements. An example of this technique is NINE, which is given as

$$\begin{array}{ccc} * & \left. \begin{array}{l} d- \\ s- \\ b- \end{array} \right\} & kuw \quad * \quad \begin{array}{l} s- \\ b- \end{array} \right\} kwa. \end{array}$$

Finally, we learn about Weidert's very own 'reconstruction principle':

'If complex developments exist in several languages ... a solution must be found that tries to explain, through a single reconstructed proto-form,¹²⁵ as many of the different phonetic developments as possible;¹²⁶ this technique usually leads to the postulation of clusters as far as initial consonants are concerned ... There are two observations ... that legitimize such a treatment. First, contrary to expectations, this technique does not lead at all to complex over-burdened proto-units and proto-clusters for every etymon; it in fact leads to clusters just in those cases where we actually expect them on scriptural [sic] evidence. Secondly, the consonant clusters of Classical Tibetan yield a first-class instance of an ancestral TB language that has preserved and developed consonant clusters with as many as four elements.'

He then proceeds to cite WT brgya 'hundred', and shows how his PKN reconstruction *byrya^h is practically identical to it.

This passage merits the most careful study. On the one hand, Weidert is claiming that his constructions are not at all 'complex' or 'overburdened'. On the other hand he reconstructs monstrosities like *smxar for STAR [below 6.0], *mrxu^{dh} for STEAL,¹²⁷ and *mrqsla for SPIRIT [below]. He resolutely refuses to consider such constructs as too complicated, and uses as justification the complex clusters of Written Tibetan.

This argument, I submit, reveals a profound misunderstanding of the phonotactics and morphology of Written Tibetan, of the cyclical and variable nature of the prefixation process in TB, and of language typology in general.

It is worth taking a closer look at the WT consonant clusters. The most cursory study quickly reveals that there

are severe constraints on what clusters may occur, and the longer the cluster the more severe these constraints become. It is true that clusters of three consonants are common enough in WT, and there is every reason to believe that Proto-Tibeto-Burman also freely tolerated combinations of

Prefix + Root-Initial + Glide
Consonant (w,y,r,l).

However, the repertoire of prefixes that can be attributed to the proto-language was relatively small,¹²⁸ and phonotactic relationships of mutual exclusion existed between particular prefixes and particular C_i's at all time-depths.¹²⁹

When we proceed to the realm of 4-consonant combinations, we find that they are excessively rare. Even at his most adventuresome, Benedict almost never sets up strings of four proto-consonants, the only examples in STC being *b-r-gyat * *(b)g-ryat 'eight' [# 163]¹³⁰ and *s-hyway * *s-hwiy 'blood' [#222].¹³¹ As far as WT is concerned, a search through Jäschke's dictionary¹³² would have rapidly demonstrated to Weidert that only six 4-consonant combinations occurred in that language, and that they all had a very similar phonological shape, viz: (1) bsky-; (2) bskr-; (3) bsgy-; (4) bsgr-; (5) brky-; (6) brgy-. The first consonant in the cluster is always b-, the second either -s- or -r-, the third either -k- or -g-, and the fourth either -y- or -r-, with the constraint that not more than one -r- may occur in any cluster (i.e. if -r- is the second consonant, the fourth can only be -y-):

(a) [with -s- as second element]

b - s - $\begin{matrix} k \\ g \end{matrix}$ - $\begin{matrix} y \\ r \end{matrix}$;

(b) [with -r- as second element]

b - r - $\begin{matrix} k \\ g \end{matrix}$ - y .¹³³

Furthermore, in the vast majority of cases (the only two significant exceptions being brgya 'hundred' and brgyad 'eight'), these 4-consonant words are verbal forms wherein the b- is segmentable off as a prefix, adding either the meaning of 'perfective' or 'future' to the verb-root. That is, there is a morpheme-boundary between the first consonant and the rest, so that we never find more than three initial consonants within the same morpheme. Thus,

bskyur < skyur 'throw'; bskrad < skrod 'expel';
bsgyans < sgyon 'fill,stuff'; bsgrigs < sgrig 'put in
 order'; brkyaŋ < rkyon 'stretch'; brgyud < rgyud
 'fasten', etc. ^{133-A}

We are left with the two numerals brgya '100' and brgyad '8', which far from being typical of words of this type, stand out as glaring exceptions. Prefixes in TB numerals are notoriously variable, susceptible to influence from each other.¹³⁴ (This in fact is true of initial consonants of numerals in other language-families, e.g. Indo-European.¹³⁵)

There is a very simple point to all this. No language permits just any old sequences of consonants to be thrown together in word-initial or word-final position. There are always more or less rigid phonotactic constraints at work (though of course these vary from language to language, and a fortiori from language-family to language-family). English permits very complex initial clusters compared to Japanese,¹³⁶ while Russian or Hupa has an even richer variety. Tibeto-Burman, by and large, permits a maximum of three.

Weidert throws his proto-consonants together helter-skelter, with no regard for any system of proto-constraints on possible consonant combinations. Not only does he set up 4-consonant sequences, but he occasionally outdoes himself and posits five, e.g. *mrghla 'spirit'!¹³⁷

Needless to say, this sort of reconstructional licentiousness makes it ridiculously easy to 'explain' any anomalous correspondence. Since there is mathematically a very large

number of possible four- and five-consonant combinations, a new proto-cluster can be posited for practically every etymon in the reconstructed lexicon. To operate at this level, however, is to trivialize the whole enterprise. Proto-forms are not sausages to be stuffed.¹³⁸

5.0 STAR and MOON

A large portion of my original paper¹³⁹ was devoted to exploring the semantic interrelationships among the concepts STAR, MOON, and SPIRIT, in order to provide a background for my equation of Angami thèmvǝ 'star' and Old Chinese 月 *ngiwǎt 'moon'. Weidert, after completely misrepresenting my proposed cognate identifications in this area, says in another remarkable passage:

'If STAR has to be reconstructed as *smxar ~ *msxar on the basis of the Tibetan, Himalayish and Kuki-Naga evidence, what remains of its equation with Chinese *ngiwǎt 'moon'? Since the phonetic facts are so obvious in this case, trying to disprove the proposed semantic crossconnections is a waste of time.'¹⁴⁰

Leaving aside the issue of wasted time, two obvious things must be said:

- (a) There is no reason under the sun (or moon or stars) to reconstruct the widespread TB root for STAR (PTB *s-kar [STC 49]), underlying, e.g. WT skar-ma and Jg. šəgan, with an *-m- in the initial cluster. [See below.]
- (b) Even more to the point, I had never proposed identifying this *skar root with Chinese *ngiwǎt in the first place!¹⁴¹ My thesis was, and is, that three separate proto-roots existed in this area:

JAM I. PTB *s-kar 'star' (for which no Chinese cognate has yet been found)

JAM II. PTB *s-la or *s-gla 'moon' [STC 144]

JAM III. PST *s-ŋwa-t 'bright nocturnal celestial body'.^{141-A}

It is to this third root that I assigned Angami thəmṽ 'star' and Chinese *ngiwāt 'moon', as well as the first syllable of Lahu māʔ-kə 'star'.¹⁴²

Weidert's proposed roots here are two in number:

AW I. PST *sdla^h-d^h ~ *sdla^h-s

This little beauty is Weidert's prototype for both Chinese *ngiwāt and PTB *sgla (which he reinterprets as *sdla).^{142-A} The phonetic developments he posits to account for the Chinese form are an eloquent testimony to the power of his methodological principles: PST *sdla^h-d^h/-s > Sinitic *sgla^h-d^h/-s > *ṅgla-d^h > *ṅqwyat > *ṅywat. This involves such 'well-attested' sound changes as *sq- > *ṅq- and *-l- to *-wy-!

The only trouble is, by this method we could equally well derive the Chinese word for MOON from, let us say, the PST word for EIGHT (*brgyat ~ *bgryat),¹⁴³ thus:

*brgyat > Sinitic *b^wgyat > *b^wnyat > *bnywat > *ṅywat."

Weidert's second root is as follows:

AW II. PST *smxar ~ *msxar.

This is our old friend *skar with a few Weidertian touches added, notably the substitution of *-x- for *-k-, and the addition of an *m. It is this protoform that Weidert uses to account for Angami thəmṽ via the following phonetic development.

*s-mxar > *s-mxan > *th-ma > thmṽ.

Three observations must be made here:

- (1) The proto-rhyme *-ar did not become -ə in Angami but rather -a, as shown by the set *sar 'new',¹⁴⁴ > Ang. ke²sa³ which Weidert himself cites (p. 21).
- (2) The *-k- in the proto-form *skar was a severe embarrassment for Weidert, and he had to get rid of it. This he did in the simplest manner possible -- by fiat:

'Tibetan skar- can be interpreted as *sxar-' (p. 29). Eureka! (Of course it could also 'be interpreted' as *sqar or *spnar or anything else, but never mind.) He then proceeds to declare medial *-x- a 'weak consonant' [ibid.], and drops it without further ado:

*s-mxan > *th-ma.

- (3) Since Weidert needs an m to substitute for the *k in order to transmute *skar into Angami thèmvǎ, he has to come up with it from someplace or other. The only justification he offers for this m is the Gurung (Himalayish) form musā:ra¹⁴⁵ 'star'. When a Gurung dictionary is consulted, however, it becomes rather clear that there is a morpheme boundary after the mu-, which undoubtedly means SKY.¹⁴⁶ This Gurung mu occurs as a free morpheme, and also in such expressions as mu ŋxeba 'to thunder' (lit. "sky roaring") and mu thōŋnyū 'the sky is clear' [ibid.].

This is the only 'evidence' Weidert has to justify the -m- in his proto-form. On such a slender reed hangs Weidert's etymology for Angami thèmvǎ.

6.0 Matisoff's Inadequate Sources and Weidert's Justified Criticisms

At the time I wrote my original article, the only KN language of which I had some firsthand knowledge was Angami. For forms in other Kuki-Naga languages I was dependent on two sources of information. By far the more important of these was the unpublished doctoral dissertation of G.E. Marrison,¹⁴⁷ which contains forms from over 30 languages and dialects. In a few cases I consulted the series of bi- and tri-lingual dictionaries published in the early 1970's by the Nagaland Language Society.¹⁴⁸ In my article I had the temerity to characterize these works as 'two particularly excellent and copious sources of information' (p. 3). In

his 'Reply', Weidert neglects to mention that I used Marrison at all, and instead spends several bitter paragraphs attacking those poor little NBP Dictionaries.

I am the first to admit that my sources were far from ideal, and grossly inadequate for microlinguistic comparative work. Neither Marrison nor the NBP dictionaries indicate tones at all, nor do they syllabify the forms they contain. Many subtleties of the vowels and consonants are missed as well. Nonetheless, for me at the time these were the most 'excellent and copious' sources at my disposal, and I was grateful for them.¹⁴⁹

Throughout my article I made modest disclaimers about the uncertainty and tentativeness I felt when it came to the details of the sound-correspondences among the KN languages:

'Comparative KCN studies are in their infancy, and it will be some years before "micro-linguistic" work of the kind now possible in Lolo-Burmese can be attempted. In particular, we are still hampered by the lack of tonal information for almost all of these languages' (pp. 3-4).

'We are still far from having figured out every detail of Angami phonological developments' (p. 10).

'... it would be premature to attempt to do so on the basis of present knowledge' (p. 18).

'So far we have been relying mostly on forms from languages whose phonological history is still not well worked out' (p. 22).

'It must be emphasized that these reconstructions are still highly tentative' (p. 35).

At the time of writing, I was unaware that Weidert had done so much detailed and accurate fieldwork on the phonologies of many KN languages, and especially on their tonal systems. His 'laryngeal tonogenetic' article that we published in LTBA 5.1 was the first time any of his material on KN languages had appeared in print.¹⁵⁰ Despite the

excessive length of that article and its often bizarre reconstructions, the data it contained -- i.e. the actual forms from the various KN languages, in accurate transcription with the tones indicated -- we felt to be extremely valuable.

I am grateful to Weidert for pointing out several places in my article where I misanalyzed a form I had gleaned from Marrison or an NBP dictionary.¹⁵¹ These include:

- (a) The missyllabification of Pochury awutsi as a-wut-si instead of a-wu-tsi [p. 21, fn. 46];
- (b) The failure to recognize that the Chang morpheme -nu was a noun-formative suffix [p. 35 and fn. 76], and similarly with the Konyak -nha or -ha [p. 20].¹⁵²
- (c) Sangtam chinghi 'star' should be syllabified as ching-hi, not as chi-nghi [p. 21; 'Reply' p. 22].

On the other hand, I must thank Weidert for providing me with another cognate to Angami thèmvǝ that I had not found! As he says in a grudging footnote:

'The only form that can possibly be related to
*ngiwǝt is Miju Mishmi ṇa ci "star".'
['Reply', p. 35 (n. 31)]

Thank you! I'll take it, since it apparently fits perfectly, both phonetically and semantically, with the root *s-ṇwa-t I am trying to establish!

On one other matter I now believe Weidert to be correct. As a minor point in the paper (having nothing to do with my basic concern of justifying the comparison of Ang. thèmvǝ and Chinese 月 *ngiwǝt), I suggested [p. 34] that a new PTB root for MOON, *kriy, be set up on the basis of Naga forms like Angami khǝ 'moon' and Lahu ha-pa-khǝ 'moonlight'. Weidert has shown that the Angami and other Naga forms I cited are rather to be related to PTB *s-gla, and that the complex initial correspondences (e.g. Ang. khǝ-/Lu. tlh-/Anal trh-/

Sema qh-/Liangmai l- ~ h-/Rongmei b-) are paralleled in the etymon for BRAIN ['Reply', p. 23]. I am prepared to admit that my proposed *kriy root represented 'a gross misunderstanding of the regular correspondences,' though I am still puzzled about the Lahu form.^{153, 153-A}

7.0 Conclusion: a Plea for Mutual Tolerance and Cooperation

Weidert does not actually reject every single idea I have ever proposed. He accepts certain concepts of mine tacitly, e.g.:

- 'laryngeal dissimilation' or 'breathiness dissimilation' as a possible phonetic process [W. 1979, p. 103; 'Reply', p. 33 (n. 20)];¹⁵⁴
- 'prefix pre-emption' ['Reply' p. 19];
- 'allofamic' reconstructions [see the exs. quoted above].

Once in a while he even gives me specific credit for something.¹⁵⁵

Despite Weidert's impressive accumulation of data, it is quite clear that his task of reconstructing Proto-KCN is just beginning, and that he cannot perform that vast task alone. Judging from the state-of-the-art in Lolo-Burmese and Himalayish studies, which are much farther advanced, but where all kinds of problems still remain, it will be some time before a definitive picture of PKN emerges. (The more we have learned about Lolo-Burmese, the more we are impressed with its complexity.)

Weidert has done some good preliminary work, especially in figuring out some of the basic KN and Barish tonal correspondences.¹⁵⁶ His work represents a considerable advance over his predecessors in that field. It is, I suppose, only natural that he resents the intrusion of outsiders into 'his' Kuki-Naga domain. He is concerned that his 'reconstruction theory' be the 'top competitor amongst TB reconstruction theories.'¹⁵⁷

I suggest that the reconstruction of PTB is a noble enterprise, where a spirit of competitive territoriality is out of place. We should pool our knowledge and encourage each other to venture outside of our specialized niches, so that we begin to appreciate the full range of TB languages -- a family as vast and diversified as Indo-European.

If Weidert should ever choose to write on a problem in Lolo-Burmese, his ideas will be given the most careful and sympathetic attention. Meanwhile, it is to be hoped that a true feeling for Tibeto-Burman will develop in us all, so that our Sprachgefühl will deepen and widen to encompass the full range of the modern languages as well as the Ursprache that we are building up, piece by painful piece.

FOOTNOTES

1. The occasion was the Tenth International Congress of Anthropological and Ethnological Sciences. (I had previously presented the paper at the Eleventh Sino-Tibetan Conference at the University of Arizona, Oct. 1978.)

One evening in my Mysore hotel room Weidert produced the manuscript of his article "The Sino-Tibetan tonogenetic laryngeal reconstruction theory" and asked if I could arrange to have it published in LTBA. Despite its length, I was glad to oblige, and it appeared uncut in LTBA 5.1 (1979), pp. 49-127).

2. I thank him in fn. 1 (p. 2), mention his work and his comments in fn. 6 (p. 4), fn. 15 (p. 7), fn. 33 (p. 15), fn. 46 (p. 21), and p. 35 (including fn. 76). See Gengo Kenkyū 77, 1-45, 1980.
3. I viewed this as the continuation of a long-standing tradition of mutual citation between Weidert and me. I had been a reader of the MS of his book Componential Analysis of Lushai Phonology (1975), and several of my comments ended up in the printed version. As Weidert explains in a generous paragraph in his Preface (p. x):

"The second person to whom I owe a lot in preparing the final draft of this book is Dr. James A. Matisoff, Professor of Linguistics at the University of California at Berkeley. He related his criticisms on an earlier draft of this book in a letter ... I found his proposals for correction (which in fact came close to a critical review) so stimulating that I could not refrain from citing at least those longer remarks from his letter that make reference to historical perspectives of Lushai phonology ... The reader will find four of these remarks as footnotes 15, 16, 21, and 25 respectively. Again I would like to thank Prof. Matisoff for the great trouble he took in reading the typescript of this study."

My review of this book appeared in JAOS 99.3 (1979), p. 496.

4. In what follows, page references are to this published version. A few times I have occasion to refer to something in the MS that was edited out. These points are clearly indicated.
5. No general claims are made concerning the 'one phoneme or two' status of these velar-cum-labial combinations.

6. Such valid criticisms as he makes all involve side-issues - cognate identifications I had hesitantly offered while freely admitting that I did not have enough data to be sure of myself (esp. in Sections 3.1 and 4.2). See below, 6.0.
7. Few indeed must be the readers who have had the Sitzfleisch to pore over pp. 62-77 of Weidert 1979, the long, arid, and irrelevant section entitled "The preliminaries: what is a reconstruction theory?"
8. Weidert 1975, pp. 2 ff. All he means by this jawbreaker is elements like the cran- in cranberry, or German Him- as in Himbeere 'raspberry'. To these 'orphan morphs' I have given the name 'morphan' (Matisoff 1973a, p. 60; 1978a, pp. 59-63).
9. Op. cit., p. 112. This seems to mean that if a man's first name ends in low-tone a, this vowel undergoes no tone-change in the vocative, but if a name ends in high-tone i this becomes falling-tone in the vocative.
[ε = etymological reconstruction, ɛ = syllable, π = phonological adjustment, σ = syntactical adjustment, λ = liaison].

This mania for Greek letters (which we are tempted to call hellenogrammatophilia) seems to be endemic in Central Europe. The Czech comparatist Kamil Sedláček has given us many examples in his Gemein-Sino-Tibetische (1970).

18. Op. cit., p. 2. It is clear that for Weidert any morphophonemic alternation that can be stated in phonological terms is 'phonologically conditioned', even if the alternation only affects a few particular lexical items which must be listed individually. Thus he would certainly write a 'rule' to 'generate' the plurals of English mouse and louse. More seriously, he might even try to write a rule 'deriving' mice and lice from *mouses and *louses [see below].
19. Op. cit., p. 59.
20. Old-style American structuralist morphophonemics would use two different consonantal symbols here -- e.g. an ordinary [k] for the cases where there is no change and a [K] for the cases where -k ~ -ʔ. This amounts to tagging these classes differently in the lexicon, and is in fact far preferable mnemonically to Weidert's treatment -- though of course it does not 'explain' things any more than Weidert's notation does. Weidert returned to this

problem in a diachronic context in his 'tonogenetic' article [see below 3.2], where he 'explains' the sporadic development to -ʔ as due to the influence of his *-s-infix, which occurred equally sporadically precisely in those verbs that developed -ʔ in their non-finite alternant. What then conditioned the occurrence of this *-s-?

21. LTBA 5.1 (1979), pp. 49-127.
22. Weidert's achievement is thus roughly comparable to what R. Burling accomplished for Lolo-Burmese tonal studies. See Burling 1968 and the Review thereof by Matisoff (1968).
23. I.e., syllables ending in a vowel, semivowel, or nasal (what Thai grammarians refer to as 'live' syllables). Weidert's term for these is 'smooth syllables'.
24. It is mentioned in n. 25 [p. 81], and discussed on pp. 103-105. Weidert seems to fudge on whether it should be accorded equal historical status to the other three. It sometimes appears that he wishes to group proto-syllables in *-s with those which ended in final stops [p. 61], since /s/ is a 'segmental consonantal unit.' For Baric, Weidert does unequivocally set up four *TC's, but confesses that his rules have numerous exceptions, and allows that his Baric tone-chart 'is not of such a conclusive and convincing nature as the diagram of the KN TC's' (p. 86). In the following discussion, we shall confine ourselves to Kuki-Naga, where Weidert feels he is on the safest ground.
25. Haudricourt 1954a, "Comment reconstruire le chinois archaïque".
26. Haudricourt 1954b, "De l'origine des tons en vietnamien".
27. For Haudricourt, final *-s as such does not have a tonogenetic effect until it has changed to *-h. Weidert, however, insists on keeping *-s and *-h apart, and imputing to each a separate tonogenetic effect.
28. See Haudricourt 1961, "Bipartition et tripartition ...". Haudricourt's works are not specifically cited by Weidert.
29. Cf. Pulleyblank 1961.
30. Cited in Weidert 1979, pp. 60-1.
31. The origin of the two-way tonal split in Loloish stopped syllables was explained in Matisoff 1972 in terms of the transphonologization of the *voiced/*voiceless contrast in syllable initial position. A general theoretical overview of tonogenesis in SEA is presented in Matisoff 1973b.

32. On the evidence of the Burmese writing system, where the 'heavy' tone is often indicated by a graphic descendant of Sanskrit visarga (-ḥ), Pulleyblank supposes its origin to have been PLB *-h (< *-s), while Burmese 'creaky' tone is referred to a final *-ʔ. (For a convincing alternative explanation of the origin of Bs. creaky tone in terms of an *s- prefix, see Thurgood 1981.)

A similar explanation of the origin of Burmese heavy tone is presupposed in Haudricourt 1975.

For Karen also, a variety of final laryngeals have been posited by R.B. Jones (1961) to account for some features of Karen tonal developments.

For Jinghpaw, Maran (1971) has tried to reanalyze tonal contrasts in terms of voicing contrasts in syllable-final position. [See the review of this work by Matisoff (1973c.)]

33. Bradley 1980.

34. The only examples I have found are these:

'bone' PTB *rus [STC #6] > PLB *ruw² (WB rûi, Lahu ṣṣṣ);

'two' PTB *g-nis [STC #4] > PLB *ʔnit * *ni² [TSR #160] (WB hnac, Lh. ní);

'seven' PTB *s-nis [STC #5] > PLB *snit > PBurm. *ʔnit, PLolo *N-šit * ši² [TSR #128] (WB khu'-hnac, Lh. ṣṣṣ).

In these last two sets, ultimately prob. the same etymon, WB has developed a final stop, but some Loloish languages show reflexes of Tone *2 (with preemption of the prefix in SEVEN). Good TB etyma in *-s are excessively rare in any case, and this will hardly do as a general tonogenetic explanation for PLB Tone *2. (On the other hand it is perfectly possible to assume that the few PTB words in *-s joined the [pre-existing] LB tone-class we call *2.)

- 34a. Weidert actually reproaches himself (pp. 56-60) for a previous attempt (1977b) to explain the KN tones in terms of classes of initial consonants, and clearly feels there is something wonderfully new about his explanation solely in terms of final consonants. Actually, however, tonogenetic theory from its very inception has recognized both ends of the syllable as having tonogenetic potential.
35. It is curious that W. is so willing to admit this extraordinary preservation of a notoriously unstable laryngeal element, while being so very reluctant to even consider the possibility that some of the medial *-w-s that I adduce in my explanation of the Angami labiodentals might go back to the proto-language! [See above 2.0, below 4.8.]

- 35a. On the other hand, in a much larger number of modern languages (Weidert lists 22 of these on p. 91) no special laryngeal features are associated with any tone, and in several others (e.g. Lakher, Rongmei, and Liangmei) all syllables are pronounced with glottal stop or creaky voice regardless of their proto-tone. It is thus entirely possible that the modern glottal stops in TC-II words are secondary even in Weidert's 'criterial languages'.
36. "As regards a breathy phonation type with final *-h, its trace is no longer observable anywhere, but must be inferred from the development to KNBJ TC-III etyma." [p. 94; italics mine].
37. Weidert refers to these as an 'additional range of observational phenomena ... that should play the decisive role of the ultimate arbiter with regard to the acceptance or rejection of a laryngeal TB reconstruction theory' [*ibid.*, p. 94].

These verbal alternations were noticed long ago for Lushai (Lorrain and Savidge 1898, p. 26; Lorrain 1940, p. xiii), but the first systematic and scientific account of the phonetic shape and grammatical function of such alternations was by Henderson (1965) for Tiddim Chin. They have also been described for Bawm (Löffler 1970, 1974; Schwerlii n.d.), and again in admirably thorough fashion for Lushai (Weidert 1975, pp. 59-68). Weidert has recently discovered comparable phenomena in Nocte and Tangsa (which he assigns to the 'Arunachal subgroup of Barish') [*op. cit.*, pp. 107-113]. This leads him to make the astonishingly premature claim that this kind of verb alternation 'has to be regarded as a common TB feature' [p. 55].

By the way, Weidert nowhere gives his criteria for assigning a language to the Baric rather than the Kuki-Chin-Naga group.

38. With characteristic symbological infelicity, Weidert labels both his tone-classes and these verb-alternants with roman numerals, so that 'II' could mean either 'TC-II' or 'Form II'.
39. Cf. the examples in the chart given above (3.1).
40. Weidert uses this term to comprise semivowels, liquids, and nasals, and symbolizes this class by the awkward digraph "Ct." For clarity's sake we substitute the symbol "M".

41. The only example I can think of in TB is the Loloish language Nasu (Kao 1958), whose initial voiced aspirates are secondary developments from PLB *prenasalized initials (Matisoff 1972, pp. 15-16).

It is of course true that in many SEA'n languages (notably in Mon-Khmer) syllables beginning with voiced stops have breathy voice as a prosodic feature throughout the syllable -- but Weidert does not conceive of his *-d^h as a prosodic formula [see below].

42. It is simply an orthographic convention that the single series of final stops of Written Tibetan is written with the same symbols used for voiced initial stops. A similar convention is adopted in Haas' romanization of Thai, where the single series of final stops is written "-b, -d, -g." See Haas 1956.
43. See Troubetzkoy 1958. Weidert seems not to have absorbed the essence of Prague school phonology, which has proven to be one of Europe's most enduring contributions to world linguistic theory. It is all the more strange that he should be so concerned with 'redundancy free phonemic representations' that he insists on writing /pv/ rather than [bv] for Angami Naga ('Reply', p. 31, n. 11).
44. See the discussion in connection with the medial labial element *-w-, below 4.1 et seq.
45. This chart has been based exactly on Weidert's rules (pp. 98-102). The layout is my own.
46. The low tone of Lushai stopped syllables with short vowel is synchronically predictable and redundant. (The tones of Standard Thai stopped syllables are also partially predictable by vowel length. Short vowels can only be on high or low tone, long vowels can only be on falling or low tone. Synchronically there is thus a contrast on the low tone. Diachronically, however, the tones of stopped syllables were once entirely predictable in terms of the voicing or voicelessness of the initial consonant and the length of the vowel, so that no contrast need be set up for Proto-Tai. Similar stopped proto-syllable types lacking contrastive tone are postulated for Chinese and Lolo-Burmese.
47. I.e., closed with a continuant [n. 40, above]. In syllables of this type there is contrastive vowel length, while open syllables are always redundantly long. (In this respect also the proto-system resembles Thai.)
48. As a bracing example of Weidert's pellucid style, it is worth quoting the original at this point [pp. 100-101]:

'As regards the MORE NUMEROUS open syllable group of verb stems, it is easily explained [italics mine] by P-K I. *CV? + II. *CV[?]-d^h + (assimilatory change of *-d > *-g because of two laryngeal elements without intervening sibilant, plus compensatory vowel lengthening) *CVV^hg + (with falling tone in Lushai = association with /CV(V)Ct/ low tone [TC-III] syllables) VCVk (TC-Vb, = long vowel CVVP syllables), = Lu. [~]CVk, Ti. -CVk (Ti. rising tone = association with /-CV(V)Ct/syllables [=TC-II] probably because the glottal stop element in *CV?-d^h dominates over the breathy element, thus *CV?-d^h + *CVV?g).'.

49. See STC, ibid. Weidert uses *-s too, as we can see from the chart, but "squanders" it by positing it in the wrong places!
50. In English phonotactics as well, s occupies a privileged position. It is the only consonant that appears syllable-initial before such consonantal combinations as -tr, -kr, etc.
51. Velar stops can certainly develop into fricatives or laryngeals (e.g. PIE *k > Gmc. *x > Eng. h; PTB *-k > Jinghpaw -?), but a dental becoming a velar by assimilation to the combined (?) effect of creaky and breathy voice is, to mix metaphors, a horse of another color and a rara avis indeed. Come to think of it, a good term for this kind of phenomenon might be 'horsebird assimilation.'
52. The Kuki-Naga verb suffix *-k is mentioned in STC (n. 289, p. 101). (The Kachin [= Jinghpaw] form yu 'descend' quoted there from earlier sources should actually be ?yũ?, directly comparable to Lushai zük, so in that particular etymon the -k looks like part of the root.)
STC sneaks in a *-k suffix here and there after non-verbal roots as well (e.g. n. 60, p. 16).
53. Lest this be thought too radical a proposal, we may point to non-alternating English strong verbs like cut, spread, hit, shed etc. For more analogies between Lushai/Tiddim and English verb morphology see below.
54. Velar nasals become dental in the two languages.

55. In this context, we are using "R" to stand for 'liquid or semivowel' and "M" to mean 'nasal'. (Above, where it was not essential to distinguish between the classes of continuants, we used "M" to stand for them all.)
56. Stops and nasals have a way of interchanging in the presence of a glottal element, due to changes in the timing of the release of the velar and glottal closures. Cf. the development of Proto-Tai *[?]b- to m- in Shan and Po-ai [Li 1977, p. 68] and of *[?]d- to ɾ- in Po-ai [*ibid.*, p. 107]. See also Benedict 1976.
57. See, e.g. Campbell 1959/1975, pp. 307-320.
58. Chaque mot a son histoire. Interestingly enough, Weidert attacks the 'vague notion' of analogy [p. 59], but raises it as a possibility whenever he finds it convenient [pp. 99, 104, 106].
59. One can imagine Weidert trying to 'explain' bring/brought by deriving it from the sing/sang, ring/rang type, reasoning as follows:
 'There was obviously a protosuffix *-d^h that occurred after the original past-tense form, *bræn, thus:
 *bræn-d^h. It is child's play to explain what happened after that: *bræn-d^h > *bræ-d^h (loss of ɲ before d^h) > *bræ^hd (metathesis) > *bro^hd (backing of vowel before laryngeal) > *brɔt [= brought] (devoicing in final position).'
60. Modern Newari, e.g., has 5 distinct verb-conjugations: 2 with stem-final vowel, one with -n, one with -l, and one with stem-final -p -t -k. Many of these stem-final consonants can be shown to be suffixal on the basis of comparative evidence. See Malla 1981, pp. 29-30.
61. The same is of course true for all the prefixes we set up for PTB. See the discussions of this point in Matisoff 1972a, 1972b, 1978b, 1979a, etc.
62. Cf. the brilliant verses in macaronic Latin and French in the Troisième Intermède, after Act III of Le Malade imaginaire: Mihi a docto doctore / Domandatur causam et rationem quare / Opium facit dormire? / A quoi respondeo / Quia est in eo / Virtus dormitiva / Cujus est natura / Sensus assoupire. ("By a learned doctor I / Am asked the cause and reason why / Opium makes one sleep? / To which I give reply / Because there is in it / A dormitive virtue / Whose nature it is / To make the senses drowsy".)

63. Op. cit., p. 66, n. 7.
64. Matisoff 1980, pp. 8-19.
65. Ibid., p. 8.
66. In Weidert's defense it must be said that these extreme views, which he adopts in this attack pour les besoins de la cause, are belied by other aspects of his work, where he does seem to show some understanding of prosodic features and variational phenomena.
67. Matisoff, ibid., pp. 8-9.
68. Weidert, 'Reply' pp. 9-10; from the sentence beginning 'Amazingly ...' we quote from the MS version.
69. Or as Y.R. Chao (1934) so engagingly phrases it, 'one-piece sound' or 'two-piece sound'.
70. In many cases of course there is nothing to decide this question one way or the other in terms of subsequent phonetic developments.

A few examples from Lolo-Burmese will quickly illustrate how -w- and -y- sometimes behave diachronically like features of the initial, but at other times like features of the rhyme:

- (a) 'bamboo' PLB *wa² > WB wâ, Lahu vâ
 [*w- functions as the initial consonant, and the regular *-a > -a development is unaffected]
- (b) 'pig' PLB *wak > WB wak, Lh. và?
 [*w- is the C_i, and the regular *-ak > WB ak, Lh. a? is unaffected]
- (c) 'hide' [v.t.] PLB *?wak > WB hwak, Lh. fá
 [*?w- as a whole functions as the C_i; the absence of glottalization in Lh. is due to 'glottal dissimilation' (Matisoff 1970; also TSR #178)]
- (d) 'emerge' PLB *?twak > WB thwak, Lh. tá?
 [the *-w- functions as part of the rhyme, and the Lh. vocalic development is affected]

- (e) 'dog' PLB *k^wiy² > WB khwê, Lh. phê
[here the Lh. initial bespeaks an especially close relationship between the labial element and the velar, so that we set up a unitary labio-velar consonantal phoneme at the PLB stage; two parallel examples exist, NEST and COMB (see below 4.2; also Matisoff 1978b and Benedict 1979)]
- (f) 'bee' PLB *bya² > WB pyâ, Lh. pê
[*-y- functions as part of the rhyme, fronting the Lahu vowel]
- (g) 'eye' PLB *s-myak > WB myak, Lh. mê?
[ditto; TSR #145]
- (h) 'boil, cook' [v.t.] PLB *?gyak > WB khyak, Lh. cá
[*-y- functions as part of the initial, affricating the Lahu initial but leaving its rhyme unaffected; note the 'glottal dissimilation' in Lahu (TSR #61)].

71. Matisoff 1973b, pp. 78-79. I have been known to refer to this phenomenon by the inelegant but graphic term 'intersegmental slopover'. Eugénie Henderson calls it 'feature shuffling' (1975).
72. Reply, p. 32 (n. 15). See above, 2.0 (a).
73. His strategy therefore is twofold: (1) to impute to me the mistaken claim that the *labial element always belonged exclusively to the initial; (2) to try to show that the nuclear vowel sometimes developed differently than it would have if no labial element were present in the rhyme ("the problem with this analysis is that the *-a rhymes following Matisoff's *-w- do not pattern like the rest of the KN *-a rhymes" ['Reply', p. 9].) But of course a vowel after a labial or palatal semivowel (whether or not the semivowel is deemed to be part of the initial) may well develop differently from the same vowel in the absence of any preceding semivowel!
74. Matisoff 1980, pp. 9-12.
75. Interestingly enough, Weidert accepts and adopts my 'pre-emption' analysis (Matisoff 1980, p. 11) for forms like Dimasa si and Mao o-si, without giving me credit ('Reply' p. 19). I coined the term 'prefix pre-emption'

(i.e. the driving out of the root-initial by a prefix) in the early 1970's, and it first appeared in print in Matisoff 1972b, p. 275. Like 'tonogenesis' it now seems to have passed into the public domain. See also Matisoff 1979a, p. 24.

76. He is induced to reconstruct -y- on the basis of WT khyi, though WT has no contrast between khyi and khwi. He gets the idea of the *s- prefix here from me, though here again he got what I said wrong. Weidert says ('Reply' p. 19) that I referred the Angami unaspirated dental prefix to PTB *s-. What I actually suggested (Matisoff 1980, p. 17) was that PTB *s- became the aspirated dental prefix thē- in Angami, while PTB *d- became Angami tē- (Matisoff 1980, p. 17 and n. 39).
77. 'Reply', p. 17.
78. Ibid.
79. See above [2.0 (b)] and Matisoff 1980, p. 10. Actually another morpheme in this series, BEE, also shows variation between pf- and f- in Angami [ibid. (p. 9, n. 19)], so that this is not a problem that requires the elaborate solution of setting up a new proto-phoneme.
80. I first mentioned this interesting set in Matisoff 1969. Benedict had noted several of them independently. See my note 195 in STC (p. 61).
81. Please - 'ad hoc mir ništ keyn tšaynik'!
82. See Matisoff 1980, pp. 12-13.
83. In this case the 'labiovelar' is a secondary combination of an old velar prefix and root-initial w-. Both the labial-initialled form (> e.g. Jinghpaw woi) and the velar prefix without dental (> e.g. Kadu kwe) are directly attested, as well as the dental prefix without velar (> e.g. Jili təwe).
84. 'Reply', p. 11. When it suits his purposes, however, Weidert is willing to admit that cognates may display a tonal irregularity. Speaking of the set for TWENTY [4.4 below] he says 'Khamngan has ¹kēI, which looks like a cognate, but the tone does not fit with TC-I' ['Reply' p. 18].
85. Matisoff 1980, pp. 17-18.
86. Ibid., pp. 15-17.

87. I speculate (p. 17) that the -kwa variant, which underlies Lushai pəkua (< *b-kwa) as well as the Angami form, might have arisen from an old suffix -a (*-kuw-a > *kwa). Weidert ('Reply' MS, p. 24) admits that Lotha Naga has an -a suffix in numerals!
88. 'Reply' p. 18. Italics mine.
89. See the discussion of 'proto-form stuffing', below 5.0. The pə-prefix in WB is a reduction of the full morpheme for 'insect' (*buw [STC 27]) that is still clearly reflected by the first syllable of the Lahu form.
90. 'Reply', p. 18.
91. Matisoff 1980, pp. 13-15.
92. The '-wa stage' of this old suffix (which is related to the gender or agentive suffix *-pa) is well attested in various Himalayan languages (e.g. Sherpa).
93. 'Reply' (MS version), pp. 15-16.
94. I had found this form in the Lotha Dictionary of the much-maligned Nagaland Bhasha Parishad [see below], p. 42.
95. 'Reply', p. 11.
96. This is exactly what I said in Matisoff 1980 (p. 15 and n. 33), where I specifically quote Weidert as having pointed out the (obvious) fact that *-a usually becomes Lotha -o. This holds only for root-morphemes, however, and the *-ba suffix evidently retained its -a vocalism. It is a commonplace phenomenon for the vowels in (unstressed) functors to develop differently than they do in (stressed) root-morphemes. Cf. for example the Lotha morpheme tí 'negative imperative' (< PTB *ta & *da [cf. STC p. 97]), where the Lotha -i is different from the usual -o reflex of *-a.
97. She is the wife of Vikuosa Nienu, the Angami speaker who was our original informant at Berkeley in 1974-5. Incidentally we are now carrying on fieldwork in both of these languages during 1981-2.
98. Exactly as one always cites Lahu verbs with ve (Matisoff 1973, 4.711), Akha verbs with -eu (Lewis 1969), Burmese verbs with te, Jinghpaw verbs with ?ai, etc.
99. Explanation of symbols:
- | | |
|------|--|
| πτβε | 'proto-TB etymology' (i.e. reconstructed PTB form) |
| λν | 'Lotha Naga' |
| ρμ | 'root morpheme' |
| σ | 'suffix'. |

100. Matisoff 1980, pp. 18-19.
101. He obviously agrees that the Angami word for 'cattle' mithu is a loan from Indo-Aryan (Matisoff, ibid.).
102. 'Reply', p. 32, n. 18.
103. E.g. NOSE (PTB *s-na [STC 101] > Ang. ú-nhə) and LISTEN (PTB *r-na [STC 453] Ang. rəñŋ). Cf. Matisoff, op. cit., p. 14. For a discussion of Angami ʒpe²ŋu 'five' < *b-ŋa, see below.
104. 'Reply', p. 26. He specifically denies that -u(-) alone could trigger this development (ibid.), though there is strong evidence to the contrary (see TWENTY, NINE). Note that the *-ua- Weidert uses is identical to my 'medial labial' conditioning factor. (See the discussion of BEE, DOG, etc., above.)
105. 'Labiodentalization was caused by the unrounded velar vowel /ə/ [ɯ], but the labiodentals are no longer restricted to that environment synchronically.' ['Reply', p. 14].
106. 'Reply', p. 15. Weidert does not provide the PTB prototypes here. The identification of the words for 'span' with STC #469 is my own. Actually the PTB reconstruction should probably be modified to *ka *ga, since the Ang. and Chak. reflexes are unaspirated, as is the WB reflex kā. Vikuosa Nienu informs me that Ang. lpfə refers to the distance between the thumb and the extended middle finger, not between the thumb and the little finger. The opening is thus 'interrupted' by only one finger rather than three which reinforces the plausibility of the semantic association with 'wide open.' See STC n. 333, p. 121.
107. Weidert cites these (p. 13) without comment. TOOTH was mentioned in Matisoff 1980, p. 14 (n. 32). The backing of *-a to u is attested in several Lolo-Burmese languages (e.g. Ulu, Luquan) and in a number of KN languages (e.g. Kabui [= Nruanghmei], Maram, and Liangmai [= Kwoireng]). See STC, p. 58.
108. Weidert, 'Reply' p. 16.
109. Bradley 1981, p. 6. At this point the -u could afford to become unrounded, since the labial burden was shared by the preceding glide.
110. Cf. also buā? (-ā? is the infinitive suffix [see above 4.6]). Weidert writes this root as ²pVw (1979, p. 122).

111. In many individual languages this etymon has both shades of meaning. Cf. English expressions like 'I turned the whole house upside down looking for it' or 'He left no stone unturned in his search.' Cf. Matisoff 1972, p. 32 (n. 5).
112. 'Reply', p. 19.
113. Weidert of course insists on "-u-" (see above 4.1).
114. This etymon has an allofam with primary vocalic -u-, so it could be included equally well in the next category. Weidert (1979, p. 123) reconstructs the allofams *jqua?(s) ~ *dqua?(s).
115. This etymon may also be considered an example of 'primary vocalic u' [class (B) below].
116. Since Weidert has shown that labiodentalization could occur with labial as well as velar initials, either reconstruction is now possible. In any case both *k- and *b- prefixes are attested for this root (Matisoff 1980, p. 13).
117. See n. 114.
118. 'Reply', p. 32 (n. 15).
119. See his diagram ('Reply' p. 15), especially the arrow that goes from "Kə" to "Kʷə".
120. To repeat the quote from Matisoff 1980, p. 8: 'The Ang. labiodentals ... are consistent reflexes of distinct entities [labial semivowel or nuclear *-u (sometimes < *-a)] that must be set up at various time-depths for Proto-Angami, for PTB or for PST itself.' (Italics and interpolation in brackets are new.)
121. For an analogous and striking example of the various diachronic layers of phenomena (some very ancient and some very recent) that created the syllabic nasals of Mpi (S. Loloish group), see Matisoff 1978b, pp. 13-17.
122. Unfortunately edited out of the printed version of the 'Reply', this passage is on pp. 22-23 of the MS version.
123. See Benedict 1973.
124. See Matisoff 1978a, 'VSTB'. It is interesting to note that Weidert himself frequently slips in 'allofamic reconstructions' when it suits his purposes, without of course calling them by that name or using my symbol 'x'. Among the examples of his 'closet allofams' are his

reconstructions for TWENTY (*m-gywul ~ *m-gwyul) ['a form that explains everything' ('Reply', p. 18)]; STAR (*s-mxar ~ *m-sxar [ibid., p. 21]; BLOOD (*dsywi?-s *d-hywi?-s [Weidert 1979, p. 123]); and, curiously enough, NINE (*jgua?-s ~ *dgua?-s [ibid.]). In 'Reply' (p. 18) he confesses that for this last etymon he cannot decide among *dgua?, *sdua? ~ *dsqua?, or *tzqua?!

125. Italics his.
126. Needless to say, skilled practitioners of the 'allofamic technique' also try to explain as much as possible by a single proto-form before resorting to allofams!
127. Weidert 1979, p. 123. We have already extensively discussed the incredible clusters in syllable-final position that Weidert reconstructs in accordance with his 'laryngeal tonogenetic theory'. In this section we are primarily concerned with his proto-form stuffing in syllable-initial position.
128. See Wolfenden 1979, and STC, pp. 103-123.
129. Cf. for example the constraint in WT that the only prefixes which could occur before aspirated stops were m- and h-.
130. We have seen how Weidert seizes upon HUNDRED, which closely resembles EIGHT in phonological shape. Both these numerals are obviously very special cases [see below].
131. Benedict's reconstruction for LUNGS, *tsywap [STC 239], is reanalyzed by him as *tśwap, i.e. a cluster of only two consonantal phonemes. I have shown at length how this form is to be derived from a dissyllabic compound *tsi-wap, with both elements being independently attested. (Matisoff 1978b, pp. 115, 121).
132. Jäschke 1881/1958.
133. In the usual romanization of Classical Tibetan other four-letter sequences occur, but this is merely an orthographic matter: 'bsny' = /bsñ/, 'brny' = /brñ/, 'brts' = /brtṣ/, 'rtsw' = /rtṣ/.
134. Cf. for example Jinghpaw mäsūm '3', mālī '4', mənā '5', where WT has a different prefix for each (gsum, bži, lṇa). For '8', Jg. has mæ- (mætsát), but for '100' it has lə- (lətsa). For some discussion of EIGHT and HUNDRED, see STC, p. 45 (n. 148).

135. See for example Slavic forms like Russian devjatj '9', where the *n- initial was changed to d- under the influence of desjatj '10'. Another classic example is Latin quinque 'five' < PIE *penkwe, under the influence of the initial in quattuor '4' < PIE *kwetwer.
136. But of course within limits - thus the only English consonant that can occur before kr- is s- (e.g. scratch), etc. For an exhaustive chart of possible English initial consonant combinations, see Sapir 1921.
137. 'Reply', p. 25. See our facetious but faithfully Weidertian reconstruction for ANT, above 4.5.
138. For a book-length treatment of a theoretical framework where 'proto-form stuffing' is eschewed in favor of a recognition of proto-variation, see Matisoff 1978a (VSTB).
139. Matisoff 1980, pp. 25-39.
140. 'Reply' MS, p. 30.
141. Can it be that Weidert did not even understand this?
142. I am faintly surprised that Weidert never tried to combine all three of these roots into a single proto-form, perhaps *s-klɲwar-t!
143. The semantics present no difficulty. There are nine planets, and if we subtract one for the moon we are left with eight.
144. See STC, pp. 147, 172, 189.
145. 'Reply', p. 21. There is a mistake in the edited version of the 'Reply' at this point. The forms 'Tamang sā:r, Thakali sar, Gurung musā:grā' are inadvertently included under the set for NEW, though they all mean 'star'. It must be said that this passage is not very clear in the MS version (p. 30).
146. Glover 1977, p. 68. The PTB root is *r-muw [STC 488].
147. The Classification of the Naga Languages of North-east India, SOAS (1967).
148. This organization is known in Hindi as the Nagaland Bhasha Parishad (NBP), also sometimes translated as the "Linguistic Circle of Nagaland".
149. I do have some idea of what a good dictionary should look like. I have been working on a dictionary of Lahu for 16 years now. The manuscript now runs to some 2200 pages, but publication is still several years off.

150. Except of course for his monograph on Lushai (Weidert 1975). See above.
151. As indicated above, I corrected several of these in the published version of the article, giving Weidert due credit.
152. It hardly seems fair, however, for Weidert to refer to 'Matisoff's comparison technique of equating suffixial and prefixial elements in some languages with actual stems in others' ['Reply' MS p. 31]. It is not that I advocate mistaken morphemic analyses as a laudable 'technique' in comparative linguistics! Though I may make such mistakes out of ignorance, I am chagrined when they are pointed out and glad to be corrected.
153. Lahu khî could well descend from PLB *kriy [cf. *kriy 'foot' (WB khre, Lh. khî)]. The morpheme occurs in the compound ha-pa-khî 'moonlight; midnight' where it is the first syllable ha- that descends from PTB *s-(g)la. It also appears in the compound še-qo-khî 'midnight', and as a verb [ha-pa khî ve 'to shine (of the moon)']. Apparently cognate is the 1st syllable of the form 'tshl-³mo 'night' in a Loloish dialect described by Fu Mao-chi. The Lahu syllable does not seem to be a loan from Tai (cf. Siamese khyyn 'night'), since this morpheme has been borrowed into Lahu under a different tone, khî, as in tâ-khî-tâ-vâ 'all night and all day' (cf. Siamese thán-khyyn-thán-wan).
154. See Matisoff 1970.
155. E.g., the observation that PTB *s- become a dental stop prefix in Angami ['Reply' p. 19]; the suitability of the 'allofam' concept to KCN forms for 'bloom; flower' [p. 28].
156. He thus deserves recognition as a pioneer, much as Burling does for his basic work on the tone correspondences among Lisu, Lahu, and Akha (1968).
157. Weidert 1979, p. 114.

* * *

* *

*

REFERENCES

- Benedict, Paul K. 1972. Sino-Tibetan: a Conspectus.
Contributing editor, James A. Matisoff. Cambridge
University Press. ("STC")
- _____. 1973. "Tibeto-Burman tones, with a note on
teleoreconstruction." *Acta Orientalia* 35, pp. 127-138.
- _____. 1979. "Four forays into Karen linguistic history."
Edited and expurgated by James A. Matisoff. *LTBA* 5.1,
pp. 1-35.
- Bradley, David. 1980. "Register in Burmese." 23 pp. MS.
- _____. 1981. "The linguistic position of Jino." 14 pp. MS.
- Burling, Robbins. 1967. Proto-Lolo-Burmese. *IJAL* 33.2,
Part II. Issued also as Publication 43 of Indiana
Publications in Anthropology and Linguistics, Mouton
and Co.
- Campbell, A. 1959/1974. Old English Grammar. Oxford
University Press.
- Chao, Y.R. 1934. "The non-uniqueness of phonemic solutions
of phonetic systems." *BIHP, Academic Sinica*, Vol. IV, .
Part 4, pp. 363-397.
- Egerod, Søren. 1971. "Phonation types in Chinese and South
East Asian languages." *Acta Linguistica Hafniensia*
13.2, pp. 159-172.
- Glover, Warren. 1977. Gurung-Nepali-English Dictionary.
Pacific Linguistics Series C, No. 51. Australian
National University. Canberra.
- Haas, Mary R. 1956. "The Thai System of Writing." American
Council of Learned Societies. Washington, D.C.
- Haudricourt, André-Georges. 1954a. "Comment reconstruire le
chinois archaïque." *Word* 10.2-3, pp. 351-364.
- _____. 1954b. "De l'origine des tons en vietnamien."
Journal Asiatique 242, pp. 68-82.
- _____. 1961. "Bipartition et tripartition des systèmes
de tons dans quelques langues d'Extrême Orient."
BSLP 56, pp. 163-180.
- _____. 1975. "Le système des tons du karen commun."
BSLP 70.1, pp. 339-343.

- Henderson, E.J.A. 1965. Tiddim Chin: a Descriptive Analysis of Two Texts. Oxford University Press. London.
- _____. 1975. "Feature shuffling in Southeast Asian languages: or "How big is a segment?" Paper presented at Eighth Sino-Tibetan Conference, Berkeley.
- Jäschke, H.A. 1881. A Tibetan-English Dictionary. London. Reprinted by Routledge and Kegan Paul Ltd., London 1958.
- Jones, R.B. 1961. Karen Linguistic Studies: Description, Comparison, and Texts. University of California Publications in Linguistics, No. 25. Berkeley and Los Angeles.
- Kao Hua-nien. 1958. A Study of the Grammar of the Yi [Nasu] Language (in Chinese). Scientific Publishing Co. Peking.
- Lewis, Paul. 1968. Akha-English Dictionary. Data Paper 70, Southeast Asia Program, Cornell University. Ithaca, N.Y.
- Li Fang-kuei. 1977. A Handbook of Comparative Tai. University Press of Hawaii. Honolulu.
- Löffler, Lorenz G. 1970. "The Bawm (Lai-zo) tonal system." Paper presented at Third Sino-Tibetan Conference, Cornell.
- _____. 1974. "Bawm verbal forms and the tonal system of Central Chin." MS.
- Lorrain, J.H. 1940. Dictionary of the Lushai Language. Royal Asiatic Society of Bengal. Calcutta.
- Lorrain, J.H. and F.W. Savidge. 1898. A Grammar and Dictionary of the Lushai Language. Shillong.
- Malla, Kamal. 1981. Contemporary Newari: a Working Outline. 91 pp. MS. Berkeley, California.
- Maran, La Raw. 1971. Burmese and Jingpho: a Study of Tonal Linguistic Processes. OPWSTBL, Vol. IV. Urbana, Illinois.
- Matisoff, James A. 1968. Review of Burling, Proto-Lolo-Burmese. Language 44.4, pp. 879-897.
- _____. 1969. "Lahu and Proto-Lolo-Burmese." Occasional Papers of the Wolfenden Society on Tibeto-Burman Linguistics, Vol. I, pp. 117-221. Ann Arbor, Michigan.

- _____. 1970. "Glottal dissimilation and the Lahu high-rising tone: a tonogenetic case-study." *JAOS* 90.1, pp. 13-44.
- _____. 1972a. The Loloish Tonal Split Revisited. Research Monograph No. 7, Center for South and Southeast Asia Studies, University of California, Berkeley. 88 pp.
- _____. 1972b. "Tangkhuul Naga and comparative Tibeto-Burman." *Tōnan Azia Kenkyū* (Kyoto) 10.2, pp. 1-13.
- _____. 1973a. The Grammar of Lahu. University of California Publications in Linguistics, No. 75. U.C. Press, Berkeley and Los Angeles. To be reprinted, 1982.
- _____. 1973b. "Tonogenesis in Southeast Asia." In Larry M. Hyman, ed. Consonant Types and Tone. Southern California Occasional Papers in Linguistics, No. 1, pp. 71-96. Los Angeles.
- _____. 1973c. Review of Maran, Burmese and Jingpho: a Study of Tonal Linguistic Processes. *JAS* 32.4, pp. 741-743.
- _____. 1978a. Variational Semantics in Tibeto-Burman: the 'Organic' Approach to Linguistic Comparison. ISHI Publications. Philadelphia.
- _____. 1978b. "Mpi and Lolo-Burmese microlinguistics." *Monumenta Serindica* No. 4 (Tokyo). 36 pp.
- _____. 1979a. "Problems and progress in Lolo-Burmese: Quo Vadimus?" *LTBA* 4.2, pp. 11-43.
- _____. 1979b. Review of Weidert, Componential Analysis of Lushai Phonology. *JAOS* 99.3, p. 496.
- _____. 1980. "Stars, moon, and spirits: bright beings of the night in Sino-Tibetan." *Gengo Kenkyū* 77, pp. 1-45.
- Nagaland Bhasha Parishad. 1971. Lotha-Hindi-English Dictionary. Kohima.
- Pulleyblank, Edwin G. 1961. "The consonantal system of Old Chinese." *Asia Major*, n.s. IX: Part I, pp. 58-142/ Part II, pp. 206-265.
- _____. 1963. "An interpretation of the vowel systems of Old Chinese and of written Burmese." *Asia Major* n.s. X, pp. 200-221.
- Sapir, Edward. 1921. Language. New York.

- Schwerli, Verena. n.d. Bawm Grammar. 346 pp. MS.
- Sedláček, Kamil. 1970. Das Gemein-Sino-Tibetische. Deutsche Morgenländische Gesellschaft. Wiesbaden.
- Thurgood, Graham. 1981. Notes on the Origins of Burmese Creaky Tone. Monumenta Serindica, No. 9. 94 pp.
- Troubetzkoy, Nicholas. 1958. Grundzüge der Phonologie. Vandenhoeck and Ruprecht. Göttingen.
- Weidert, Alfons. 1975. Componential Analysis of Lushai Phonology. J. Benjamins B.V. Amsterdam.
- _____. 1977a. "Angami Naga phonology." 37 pp. MS.
- _____. 1977b. "Rekonstruktionsmethoden für schriftlose Sprachen: Entstehung und Entwicklung von Tonsprachen in Südostasien." MS.
- _____. 1979. "The Sino-Tibetan tonogenetic laryngeal reconstruction theory." LTBA 5.1, pp. 49-127.
- _____. 1980/1981. "Stars, moon, spirits, and the affricates of Angami Naga: a reply to James A. Matisoff." MS Version 1980. Published in LTBA 6.1 (1981), pp. 1-38.
- Wolfenden, Stuart N. 1929. Outlines of Tibeto-Burman Linguistic Morphology. London.
- * * *
- Allen, Nicholas J. 1975. Sketch of Thulung Grammar. Cornell East Asia Papers, No. 6. Ithaca, N.Y.
- Benedict, Paul K. 1976. "Sino-Tibetan: another look." JAOS 96.2, 167-197.
- Bodman, Nicholas C. 1980. "Proto-Chinese and Sino-Tibetan: data towards establishing the nature of the relationship." In Franz van Coetsem and Linda Waugh, eds. Contributions in Historical Linguistics, pp. 34-198. E.J. Brill. Leiden.
- Karlgren, Bernhard. 1957. Grammata Serica Recensa. Bulletin of the Museum of Far Eastern Antiquities, No. 29. Stockholm.
- Marrison, Geoffrey E. 1967. The Classification of the Naga Languages of North-east India. Volume I, 292 pp. Volume II, 460 pp. Doctoral dissertation, Univ. of London (SOAS).
- Maspero, Henri. 1912. "Etudes sur la phonétique historique de la langue annamite: les initiales." BEFEO XII, fasc. 1.

ADDENDA

- 28-A Haudricourt's ideas on these matters in turn owe much to the work of Henri Maspero (e.g. Maspero 1912).
- 41-A N. Bodman reminds me (pers. comm., Dec. 1981) that Thulung Rai (Nepal) has voiced aspirated phonemes - but of course in syllable-initial position. See Allen 1975, pp. 11-12.
- 100-A Richard Kunst points out (pers. comm., Oct. 1981) that in the Shang oracle bones, GOATS and CATTLE are signified by the same graph. It is worth mentioning that the English word bovid comprises not only cattle and goats, but also sheep and buffalo.
- 133-A Actually the s- and r- in these verbs are no doubt prefixal as well, so that it can be argued that WT does not even permit more than a two-consonant cluster within a morph.
- 141-A To these we should probably add a fourth root, as Weidert suggests, underlying Sangtam ching-hi and Chinese *sieng/ sieng [GSR 812x] 'star.' See below 6.0 (c) and Matisoff 1980, pp. 36-38.
- 142-A Bodman favors the reconstruction *d-sla to account for WT zla-ba 'moon', and cogently cites Sunwar 'tāslā in support.
- 153-A Benedict (pers. comm. 1981) suggests identifying Lahu khā with WB krañ 'clear, bright.' But not only is the aspiration discrepancy problematic (a better WB match would be khrañ 'a ray') but the rhyme is also an insuperable problem, since WB -añ regularly corresponds not to Lahu -ā, but to Lahu -ē (< PLB *-in or *-in), as in:

THREAD	WB <u>khrañ</u>	/ Lh. <u>khē</u>
NAME	WB <u>mañ</u>	/ Lh. <u>mē</u>
LIVER	WB <u>ʔəsāñ</u>	/ Lh. <u>ḥ-sē</u> , etc.

* * *

SYMBOLS AND ABBREVIATIONS

X × Y	'X is an allofam of Y; X and Y belong to the same word-family'
Ang.	Angami
BEFEO	Bulletin de l'Ecole Française d'Extrême Orient
BIHP	Bulletin of the Institute of History and Philology, Academia Sinica
BSLP	Bulletin de la Société de Linguistique de Paris
Chak.	Chakhesang
GSR	<u>Grammata Serica Recensa</u> [Karlgren 1957]
IJAL	International Journal of American Linguistics
JAOS	Journal of the American Oriental Society

JAS	Journal of Asian Studies
KC	Kuki-Chin
KCN	Kuki-Chin-Naga
KNBJ	'Kuki-Naga-Barish-Jinghpaw'
Lh.	Lahu
LTBA	Linguistics of the Tibeto-Burman Area
Lu.	Lushai
NBP	Nagaland Bhasha Parishad
OC	Old Chinese
OPWSTBL	Occasional Papers of the Wolfenden Society on Tibeto-Burman Linguistics
PIE	Proto-Indo-European
PK	Proto-Kukish
PKN	Proto-Kuki-Naga
PLB	Proto-Lolo-Burmese
PST	Proto-Sino-Tibetan
PTB	Proto-Tibeto-Burman
SEA	Southeast Asia
STC	<u>Sino-Tibetan: a Conspectus</u> [Benedict 1972]
TB	Tibeto-Burman
TC	tone-class
Ti.	Tiddim Chin
TSR	<u>The Loloish Tonal Split Revisited</u> [Matisoff 1972a]
VSTB	<u>Variational Semantics in Tibeto-Burman</u> [Matisoff 1978a]
WB	Written Burmese
WT	Written Tibetan

*

*

*