

Associate Editor's report on " Feet in Sri Lanka Malay: no stress please!", submitted to *Phonology*

Summary

This paper claims that Sri Lanka Malay (SLM) has metrical feet, yet no phonetic stress, and hence, that it presents evidenced for stressless feet. Acoustic measurements are presented to support lack of stress, and a rule of penultimate lengthening to support presence of feet. An OT analysis is offered to account for the data presented.

General comments

Perhaps the most accurate way to summarize the judgments of the four reviewers is that this paper has much potential but nevertheless, falls short of supporting its central claim.

On the positive side, the article's stated goals are judged to be clear (review #2), and its central claim (SLM has headless feet) is viewed as sufficiently interesting for *Phonology* (review #3). The paper contains some carefully argued points (reviews #1, 2), is up to date on the literature about SLM (review #1), and the OT analysis is in general of a high quality (review #2). The paper is well-structured (reviews #1, 2) – which needs to be distinguished from well-argued.

On the negative side, the reviewers have identified four major flaws, which prevent publication.

First, the central claim that SLM has no stress is insufficiently motivated: regarding the phonetic evidence that there is no stress, as well as the relationship between phonetics and phonology (reviews #2, 3, 4). On the point of phonetic evidence, the reviewers (#2-3-4) point out that the paper fails to present a truly convincing case that there is no phonetic marker for stress in the language at all. This is because the way the acoustic data were analyzed can be criticized on many grounds, and because a general criterion is lacking for what may constitute an acoustic correlate of stress (see the reviews). Moreover, arguments against penultimate lengthening as a stress marker are considered to be unconvincing by reviewer #2. In relation with these points, reviewer #4 remarks that the experimental part is not written in the customary fashion, and hence, it becomes impossible to evaluate the claims. On the second point, reviews #2 and 3 observe that the author holds rather naïve views on the relation between phonological representation and phonetic interpretation, assuming a direct correspondence. I emphasize the point made by reviewers #2-3 that “stress is first and foremost a psychological issue”. Hence, if it were found that SLM has *feet but no phonetic stress* (assuming that there is no acoustic correlate of stress of stress whatsoever), this still fails to imply that SLM lacks phonological stress. The paper implicitly assumes two situations to exhaust the logical possibilities: a language either (a) has ‘stressed’ feet and hence, it has acoustic correlates of stress, or (b) has ‘stressless’ feet and hence, no acoustic correlates of stress. However, a third logical possibility needs to be considered (c): a language may have ‘stressed’ feet, but still no acoustic correlates of stress, due to a language-specific way in which the phonetic component interprets phonological representations. In order to reject situation (c) for SLM, and distinguish it from situation (b), new evidence is needed that SLM native speakers do not mentally represent stress, such as being stress-deaf (unable to tell on which syllable stress falls, learn stress in second language learning, etc.). Crucially, the psychological evidence for stress-deafness is independent from the acoustic evidence.

Second, the claim that SLM has metrical feet is insufficiently motivated. The argument for feet from penultimate lengthening needs is structured rather weakly (review #2), while no other independent sources of evidence (minimal words, etc.) are used (review #3).

Third, the theoretical consequences of the proposal are insufficiently explored (review #2), in the sense that representations of stressless feet and the constraints enforcing them make predictions about the typology of stress systems, possibly overgenerating possible human languages. These are not discussed.

Fourth, review #3 remarks that “the paper is so poorly organized that I cannot be sure that the argument goes through”. Many points about the quality of argumentation have already been covered by the above remarks; however, further structural weaknesses add to the judgment, stated in reviews #2, 3.

Recommendation

Due to problems stated above, this paper cannot be accepted for publication in *Phonology*. However, the author should be encouraged to revise the paper and resubmit it. The following guidelines may be useful.

Things to be done

1. The claim that SLM lacks stress needs better evidence (reviews #2, 3, 4).
 - Acoustic data on stress-related cues (pitch, amplitude) need a *better analysis* (reviews #2, 3, 4), using “quantified data and statistics showing (a) that average pitch doesn’t differ significantly on different syllables in the word, and (b) that average intensity doesn’t differ in this way either.”
 - *Systematic comparison* is needed of all the new versus given token pairs in the materials in order to see whether or not there is an effect on either pitch, intensity or duration (review #4). Also, a systematic comparison is needed of the same syllable in a position where stress is predicted by both views (Tapovanaye & Smith et al.), i.e. in a two-syllable word with short vowels only (review #4).
 - The experimental data should be *reported* in a standard fashion: the acoustic evidence (pitch and intensity) needs to be presented in a correct way, with quantified data and some statistics (review #3); the description of the experiment should be more complete (measurement procedures to be specified; statistical analysis to be provided; data presentation more structured; etc. review #4).
 - Better evidence is needed against vowel lengthening as a stress marker, or the argument needs to be sharpened using the evidence given. The argument that vowel duration cannot be a stress cue either because it does not occur in all words is very weak, since *other factors* could always come in to obfuscate some of the cues (review #3). If this argument cannot be strengthened, it should be dropped (review #3).
 - The paper needs more nuanced understanding of what a stress correlate can be or, rather, what a phonological correlate of prosodic structure (both constituency and headedness) can be (review #2).
 - The paper should consider (possibly, reject) an alternative analysis: that stress is phonologically indeterminate (or even determinate), but it just is not realized phonetically (review #2).
3. The claim that SLM has feet needs more evidence (review #3)
 - In the current paper, the only evidence for feet in SLM is the lengthening rule. The arguments for feet need to be strengthened, and be based on more data.
 - Another argument: the minimal word requirement. Prosodic words in SLM seem to be minimally bimoraic; this supports the idea of a bimoraic foot in the language.
 - If there are any hypocoristics or the like, that would also be good evidence for the foot.
4. The theoretical consequences of the proposal are insufficiently explored (review #2)
 - This holds both for the stressless foot representations and the OT constraint set.
 - Some exploration of the typological ramifications is needed.
5. The structure needs improvement
 - According to review #3, the current paper is so poorly organized that one cannot be sure that the argument goes through.
 - The discussion of the lengthening rule is fairly convincing, but it is not laid out very clearly; sharpen up the argument against it being weight-to-stress; crucial fact: short vowels lengthen in the open penult of a disyllabic word, but not in the open penult of a trisyllabic word (review #3)

Report #1

Feet in Sri Lanka Malay: no stress please!

Assessment:

This is a well-structured, well-argued and clearly written paper. It is also up to date on the literature about Sri Lanka Malay as well as syllable structure, stress, pitch and length. I recommend it for publication although I also have a few critical observations for the author to consider:

1. on page two, the author should try to reduce the number of near minimal pairs (now 50% of the exx.) and look for more real minimal pairs
2. the author argues against phonemic vowel length being the difference between the first vowels in Sri Lanka Malay luar and ju(w)al. We are told that the long u in jua is the result of an underlying glide in jua. Could be, but historically this is not obvious at all: in the phonological history of Malay, luar and jua both have a sequence *ua (without *w), and the readers need much more evidence to be convinced by the author's analysis.
3. According to the author, a short vowel could just as well be a consequence of the following consonant gemination as consonant gemination is a consequence of the preceding short vowel. Vowel length in itself is not phonemic since it can always be explained away by non-phonemic factors. In the author's analysis, vowel length is not underlyingly present in a case like [kum:is] 'Thursday' vs. [ku:mis] 'moustache'. Nor, apparently, in a case like [pəraŋ]. However, we know that both [kUm:is] and [pIr:aŋ] contain a historical schwa, which is short. Isn't this schwa at least some indication that vowel length is present in the underlying structure of SLM? (So is probably consonant gemination, the author might say, but in any case:) This is something the author should address.

Report #2: Feet in Sri Lanka Malay: no stress please!

Review of “Feet in Sri Lanka Malay”

The main claim of this paper is that Sri Lanka Malay lacks evidence of stress cues, and that this entails structural consequences in the phonology. Specifically, feet in SLM lack heads. Although the stated goals of the article are clear, the paper is well-structured and contains some carefully argued points, and the OT analysis of SLM is in general of a high quality, I don’t think the author(s) sufficiently motivate their core proposal or explore its theoretical consequences. For this reason, I don’t believe that *Phonology* would be a suitable venue for publication.

There are two objections that can be raised to the author’s central proposal. The first has to do with the phonetics, the second with the relationship between phonetics and phonology.

I agree with the author’s argument (p. 6) that penultimate vowel lengthening cannot be taken as a correlate of stress in SLM, and I am sure they are correct in asserting that there is no evidence for the involvement of pitch or intensity either. Since Fry (1958), though, we have gained a more nuanced understanding of what a stress correlate can be or, rather, what a phonological correlate of prosodic structure (both constituency and headedness) can be. In English, for example, there are discernible effects on the magnitude of the glottal abduction gesture in aspirated stops preceding a potentially stress-bearing nucleus, and the degree of glottal constriction varies depending on the stress of a following vowel. Perhaps there are subtle cues in the SLM data bearing on rhythmic type that the author has not considered?

Even if there really are no cues, I still think very short shrift is made of an alternative analysis (p. 15), in which there is phonologically indeterminate stress (or even determinate), but it just isn’t realized phonetically. The author asks ‘how come that SLM speakers do not express stress phonetically although they have it?’ This question seems to betray a mixing of levels, since stress itself represents nothing phonological at all. Stress is one possible (complex) *phonetic* realization of something more abstract, such as the orientation of heads in feet or words, or marks on a metrical grid. Neither heads nor grid marks are standardly conceived nowadays of as having any intrinsically phonetic definition, as is the case with distinctive features.¹ The author’s sole justification for looking for a phonological solution to the problem is that ‘shift[ing] the problem to the phonetic level of

¹This probably wasn’t always the case, as revealed by early accounts invoking line

analysis [...] would not really solve anything'. On the contrary, correctly determining which domain to look for a solution is a major step in finding that solution. Phonologists increasingly agree that many aspects of phonetic variation between languages are under cognitive control. Examples include the details of vowel production, articulatory settings, stress vs. syllable-timing, typical pitch range, and so on. This kind of cognitively controlled microvariation is not necessarily 'phonology' or best handled in terms of ranked violable constraints. For it to be so, we would want to see *phonological* evidence that the variation has consequences for phonological patterning. We can't just impute everything we see in the signal to the phonological system without raising the spectre of overgeneration. The new constraints the author proposes to adopt from Golston (2007), which *militate* against the presence of heads in feet, seems like an unwarranted enrichment of the typology, and the author offers no exploration of the typological ramifications. The obvious question raised by adopting these constraints is whether there is any correlation between the absence of stress and foot-type. The analysis of SLM assumes (headless) bimoraic feet, but can you have languages with headless syllabic trochees or headless iambs? Allowing Golston's constraints into CON would predict the occurrence of these structures, but there is no effort to spell out these predictions or match them with the attested facts. I can only see that the phonological account would gain by assuming that the SLM foot is nothing more than a plain old bimoraic trochee (Hayes 1995), and the absence of stress is a fact about language-particular phonetics. But this, of course, would defeat the stated object of the paper.

Now some miscellaneous comments about style, rhetoric, content and the odd typo.

- pp. 3–4: examples (i)–(vi), between (10) and (11). Perhaps these could be nested in their own numbered example. In the text, it would be better for the reader if the canonical types were referred to in full instead of the Roman number.
- p. 4: it would be helpful to point out that penultimate lengthening only occurs when the syllable is (underlyingly) open.

conflation in cases where there was evidence of directional counting but not surface secondary stress, as in Cairene Arabic.

- p. 5, para 2, line 3: ‘recurring’ should read ‘resorting’; line 6 (same para): ‘underlie’ should read ‘instantiate’, or something similar.
- p. 5, para 3: in introducing Tapovanaye’s ideas, I would point out briefly what the alternative analysis will be. Given the way the canonical word structures are presented in (i)–(vi), I couldn’t see why Tapovanaye would propose a four-mora minimum. This is only explained later on.
- p. 6, para 1, line 3: ‘CC sequences are not distributionally restricted’. This should probably be rephrased to say what is actually intended.
- p. 6, sec. 4.1: section number repeated from previous section (should be 4.2).
- p. 6, sec. 4.1, para 1: other relevant sources here include McGarrity (2003), and Bye and de Lacy (2008), who claim that stress-to-weight phenomena are restricted to *main* stress.
- p. 8, figure 1 caption: ‘boundary effects’ needs to be explained.
- p. 8, sec. 6.1: another relevant reference is Crowhurst and Hewitt (1995).
- p. 9, ex. (19): the prosodic structure for /_ṽtoppi/ is shown as having two feet. I assume this is one too many?
- pp. 12–13, exx. (20)–(24): presumably the extrametrical syllable is also headed by a mora. This should be represented.
- pp. 13–14, text around (26): FTBIM may also be satisfied by gemination here, i.e. [tigga], which is used when you would have to lengthen a schwa. What is it that prevents it from being optimal when the penultimate vowel is a full vowel?
- p. 15, para 1: the assumption that non-ranking of conflicting constraints results in output variation should be tied to Anttila’s work, e.g. Anttila (1997).
- p. 16, para 3, under ex. (35): ‘be satisfied otherwise’ should read ‘be satisfied in a different way’; ‘For readability reasons’ should read ‘For reasons of readability’.

- p. 16, ftnt. 17: the discussion why schwa doesn't raise to a full vowel seems unnecessarily long. Here one could simply assume a faithfulness constraint IDENT[lax], or [RTR]. The note about homonymy avoidance seems gratuitous, since this doesn't necessarily generalize as an explanation why rising to full does not occur.
- p. 17, ex. (39): here I fail to understand why the author uses a faithfulness constraint on preserving vowel length to get gemination. Schwas cannot project a mora because of $*\mathfrak{a}_\mu$. Why not invoke a markedness constraint against long RTR vowels $*\mathfrak{i}$, $*\mathfrak{u}$. This would seem to be motivated for English and other Germanic languages. This would also eliminate the hidden problem in tableau (41), that the candidate $[\text{ti}_\mu \text{g}_\mu \cdot \text{ga}]$, not represented in the tableau, would in fact best the desired output $[\text{ti}_\mu \cdot \text{ga}]$ on IDENT-IO[v-length].

References

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- Hayes, Bruce P. 1995. *Metrical Stress Theory: Principles and Case Studies*. Chicago: Chicago University Press.
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Report #3: Feet in Sri Lanka Malay

Review of
Feet in Sri Lanka Malay
for *Phonology*

This paper has an interesting claim that could use airing in a good journal: that there are headless feet. And I think the data may be there to make the argument. The problem is that the paper is so poorly organized that I cannot be sure that the argument goes through. For this reason I recommend a revise and resubmit. I'll try and summarize the claims and then outline what I think needs to be clarified below.

The argument seems to be this:

- (i) SLM has no stress.
- (ii) SLM has feet.
- (iii) So stressless feet exist.

(i) is complicated by the fact that vowels lengthen in open penults in disyllabic words, which would seem to indicate stress in that position. (ii) is complicated by the fact that it's hard to find evidence for feet without bringing in stress. If either (i) or (ii) go, (iii) goes immediately. I'm not really convinced of (i) yet and I think the arguments for (ii) can and need to be strengthened, so let me say why that is under BIGGER THINGS. Those are the issue that keep me from recommending acceptance. Then I'll turn to SMALLER THINGS that should be considered *in the event that* the article is offered a revise and resubmit by the editors.

3 BIGGER THINGS

1. SLM HAS NO STRESS. A provide some amplitude and pitch tracings that show no real peaks over the penultimate (or other) syllables. This is some evidence that there's no stress in the language, but not enough. If acoustic evidence is to be given it needs to be done right, with quantified data and some statistics showing (a) that average pitch doesn't differ significantly on different syllables in the word, and (b) that average intensity doesn't differ in this way either. Nothing like this is yet in the article at present; though a lot of tokens were apparently collected and analyzed, there's nothing but a few example tracings presented to make the case here. These are suggestive, but they're not compelling.

Without some real numerical analysis to back this all up, it's easy to fall into picking at the claims, so let me pick at the claims... Figure 1 notes that the amplitude drops (shown with little green bars) between certain vowels. But the pairs of vowels are inherently different in terms of their amplitude anyway: thus a > e, i = i, and e > i, which is just what the little green bars show. So it could be that the little green bars show nothing except that open vowels are

louder than mid or close vowels.

A's second argument (p. 9) is this: 'We would like to propose that vowel duration in SLM cannot be a stress cue either because it does not occur in all words.' That's a very weak argument, since other factors could always come in to obfuscate some of the cues. Release is a great cue for a stop, but not all stops are released. If this argument can't be strengthened it should be dropped.

It's important to realize, too, that stress is first and foremost a *psychological* issue: if speaker X says the penult (or whatever) is stressed in their language and I can't find any phonetic correlate, I probably need to keep looking. If speaker X says there's no stress anywhere or doesn't care where she puts the stress or puts it in willy-nilly each time she speaks, then we have some evidence against there being stress. I'd make sure the 18- and 19-year olds knew where stress goes in English or whatever, then ask them whether any syllables in the SLM words are stressed. It would be worth knowing. If native speakers seem to be unaware of stress in SLM, have difficulty learning it in a second language (as Japanese speakers do), or the like, we'd have more reasons for thinking the phonology of SLM doesn't make use of stress.

2. SLM HAS FEET. The only evidence I saw in the article for feet in SLM was the lengthening rule. I actually find the discussion fairly convincing, but it's not laid out very clearly and I always had in my mind the fear that penultimate lengthening really would end up being a good sign of stress. So I think the authors need to sharpen up the argument against it being weight-to-stress. The crucial fact, as far as I can tell, is that short vowels lengthen in the open penult of a $\sigma\sigma$ word, but *not* in the open penult of a $\sigma\sigma\sigma$ word. The authors say the final σ is "extrametrical", which forces the remaining short vowel to lengthen *just to be a foot*; this doesn't happen in longer words. (An aside: the authors should avoid terminology of another day like this if they're going to replace it with something current anyway, as they do, using NonFinality instead of "extrametricality".)

Meanwhile, there's another argument that can be marshalled for feet: the minimal word requirement. Prosodic words in SLM seem to be minimally bimoraic (3.3); following Hayes (1995), this supports the idea of a bimoraic *foot* in the language. That is, the bimoraic minimal word requirement is additional evidence for feet in SLM though the authors don't really come out and say this clearly.

If there are any hypocoristics or the like, that would also be good evidence for the foot. Poser's classic 1990 article on (headless) feet in Japanese spent dozens of pages establishing the arguments for feet in Japanese. The present authors need to marshall more data.

3. ORGANIZATION OF THE MS AND ARGUMENT. I think the authors really need to separate the facts (in one section) from the analysis (in another). It's hard to keep together what the generalizations are and thus hard to evaluate any given analysis.

SMALLER THINGS

THINGS THAT CAN BE CUT

Section 4.1 is about vowel length being phonemic. But does it matter? if it *were* phonemic, SLM could still have a lengthening rule that's driven by stress in the penult. I think the whole issue of it being phonemic is a distraction from the main topic. Section 8 needs to be cut or amplified; right now it's not very compelling.

THINGS THAT SHOULD BE CHANGED

'Vowels can be long, but only in open penultimate syllables.' But see immediately below that statement, (9) [pi:] 'go'. That's the *final* syllable. Oops.

The representation of heads in (18) looks like something from the 1970s. Brackets might be less contentious and show headless feet in a more natural light:

(ka.la)

'Schwa has no moraic weight in SLM.' This isn't a simple fact and shouldn't be reported as one. It may be that schwas are moraless in SLM, but that's an assumption until there's some way to show it directly.

SLOPPY THINGS THAT ARE EASY TO FIX

- Geminates are represented one way in (19) and another in (22).
- Final syllable in (19) should be extrametrical but isn't represented that way.
- Final syllables are shown without moras for some reason; this matters because schwas don't either and have special properties because of it. See (24).
- 'Extrametricality' is an old name for something nobody understood. Since the authors adopt an OT analysis that makes no use of the notion, it seems best to avoid it altogether by using non-finality throughout the paper, not just in the "OT analysis" section. Generally, section 6 can be cut if we have section 7. And I'd leave the old brackets out of the OT account, since they're meaningless in that context.

FTBIM VS. FTBIN

I think that FTBIM(oraicity) can be replaced by simpler and more standard FTBIN(arity). Suppose that feet just need to be 'binary under syllabic or moraic analysis' (Prince &

Smolensky's formulation). Then,

CV < CV > lengthens as before (otherwise it's monomoraic & monosyllabic)

CVCV < CV > needn't change (it's bimoraic)

CVC.CV < CV > needn't change (it's disyllabic)

This is simpler, I think, and makes the winner in (42) not a violator of WBP.

DEP_μ. If I understand it right, Id-IO[V-length] can be replaced by more standard and simpler Dep_μ, simplifying things.

UNDERLYING SYLLABLES GO AWAY? 12-17 have a peculiar property that probably isn't intentional. The URs are syllabified with little dots while the SRs lose the dots and thus, presumably, the syllables? This is at least backwards.

OVERSTATING THE FACTS. At the top of page r we read that 'The structures *CV:C.CV(C), ... do not exist.' But immediately above that we see the exceptional word [ka:r.tu] 'quarter'. Couldn't we be told the structure is (just) rare?

AFM. I think this constraint isn't necessary, which would be good because it requires counting violations (contra McCarthy 2004). If each member of a compound is a prosodic word (Inkelas 1989), the kacamata facts in (43) fall out very nicely.

FORMATTING. It's nice that Phonology doesn't require proper formatting for the initial submission, but it's probably a good idea to do it anyway: don't title the abstract "Abstract", indent the abstract, don't indent the numbers for examples, format the tableaux correctly (shading is all wrong in 26, 28; 40 is half-done and missing a lot of *s), don't italicize headings, etc.

CITATION. There's a conspicuous lack of citation for constraints (inter alia!). So for instance the constraints in (25), (27), (30), (31) aren't attributed to anyone, though they have a real history, and those in (33) are attributed to someone plural ('their constraints') though it looks more like they come from Golston 2007. CULMINATIVITY is mentioned but neither defined nor attributed to anyone. All of this makes the manuscript seem a little amateurish, or rushed, or half-done.

Report #4

Feet in Sri Lanka Malay: no stress, please!

You asked me to look at the experimental-phonetic part of this manuscript. Unfortunately the experimental part of the paper is not written in the customary fashion, so that I find it impossible to evaluate the claims made by the authors. Their basic claim is that Sri Lanka Malay (SLM) has no word stress. I do not really know what this means. Suppose that stress in English would only be marked by a pitch movement (no lengthening, no increased intensity, flatter spectral tilt, vowel expansion) consistently on one specific syllable in the target word whenever the target word is accented (in focus) at the sentence level. Then, surely, we would call this syllable the prosodic head (the stress position) of the target word.

In the present experiment the authors mention that they manipulated the context (preceding sentence) so as to obtain variation in information structure (there are no details in the paper). I assume, therefore, that the authors have tokens of the same target words that were produced as new and as given information. I would then need a systematic comparison of all the new versus given token pairs in the materials in order to see whether or not there is an effect on either pitch, intensity or duration. Such a comparison is not provided.

Apparently there are two competing views on SLM stress

- (1) Stress is on the penultimate or only syllable of the word (Tapovanaye)
- (2) Stress is on the long vowel in the word or else on the initial syllable (Smith et al)

As a first approximation I would like to see a comparison of the same syllable in a position where stress is predicted by both views, i.e. in a two-syllable word with short vowels only. I would then compare the acoustic stress correlates on this syllable with tokens of the same syllable in positions where stress is never predicted (note: syllables with long vowels are excluded from this comparisons, as these are claimed to attract stress).

The authors are not experimental phoneticians. Their description of the experiment is incomplete, no measurement procedures are specified, no statistical analysis is provided, the data presentation is haphazard and the graphs are unpublishable. Also, the authors explain intensity contours in terms of phrase boundary markers. This claim seems spurious to me as the examples given show no effects that cannot be explained away by inherent vowel intensity differences.

Yet, I do not exclude the possibility that the abstract analysis of SLM foot structure and the role of stress in it is basically correct. I leave that to my more linguistically inclined colleagues. Possibly the paper can be salvaged by leaving out the phonetic part altogether.

I attach an annotated PDF with numerous detailed comments and queries on the phonetic part of the paper.