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Commentary on Smith's "Adstrate Influence in Sri Lanka Malay" and "Comments… "[1]

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Abstract

Keywords

Ian Smith has neatly summarized the current state of research on the genesis of Sri Lanka Malay in his article "Adstrate Influence in Sri Lanka Malay". Smith and I agree that both Tamil and Sinhala influence can be found in SLM, we agree that the respective influences are often difficult to disentangle, and we agree on a methodology how to (eventually and hopefully) disentangle the respective influences, with a possibility of subsequently trying to date the influences.

Smith's article really presents no new or controversial claims. It is a toned-down revision of a 2003 paper, where he first analyzed the nominal grammar of the Lankan languages. In the 2003 paper, number and definiteness featured more prominently than now; in the present paper, the only remaining feature pointing towards Tamil influence is the accusative marker. While the SLM system is not a clear calque of Standard Tamil, Smith points out that colloquial varieties of Tamil spoken in Sri Lanka are much closer to the SLM system, a point which is also made in my own article "Establishing and Dating Sinhala Influence in Sri Lanka Malay".

[1] I would like to thank David Gil for comments on an earlier version of this article. All remaining errors are mine.

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Smith decided to write up his 2003 talk upon hearing about my "Sinhala influence" article, possibly as some kind of rejoinder or rebuttal. But I have never contested that there has been Tamil influence in Sri Lanka Malay, so this new version of the article does not really contribute to the discussion. I am happy to acknowledge that there has been Tamil influence, have done so before, and will do so in the future.

What I have contested is the view that SLM is an exclusive Malay+Tamil mix, and that Sinhala influence can be dispensed with. I am happy that we are now arriving at a consensus that acknowledges influences from both local languages. My qualms about this "Tamil bias" (Ansaldo 2008) find their roots in both linguistic and socio-historical data. The linguistic arguments are detailed in this issue of JLC and discussed in Smith's commentary thereof. More on this below. The socio-historical problems are detailed in Nordhoff (2009:40-48,55-61). Basically, Smith's analysis relies on the assumption of heavy intermarriage between immigrant Malays and local Moor wives. He cites Hussainmiya (1986) as his historical source. It is a bit sad to see that my (2009:41-47) critical discussion of the evidence brought forward by Hussainmiya is not mentioned at all. The claim of intermarriage relies on misreadings and misinterpretation of sources. The most glaring examples of this is a quote where "[t]heir language is Ambonese, but the majority also speak Malay, Sinhala, Portuguese and Dutch" is taken to show that Malays spoke (among other languages) … Tamil (Hussainmiya 1990:47)! I am willing to accept new evidence that sheds light on the marriage patterns of the Malays in the colonial period; but find it difficult to deal with the citation of evidence I have already refuted without discussion of my refutation. As it stands, the 'Moor wives' theory lacks empirical support. It appears that the level of intermarriage between Malays and Moors has never been much higher than 20% (Nordhoff 2009:43). This would not be enough to trigger the dramatic changes we find in Sri Lanka Malay. Furthermore, Sri Lanka Malay would be an unlikely target language for Tamil speakers in the first place. Moors spoke Tamil, a language of art, literature, and religion, while the Malays spoke a soldier's jargon, an L2 for most of them. It is obvious that Tamil was much more prestigious than this particular variety of Malay. Add to this that the Moors outnumber the Malays 200:1, all Tamil speakers together outnumber the Malays 400:1, and the idea that Moors would want to acquire Malay starts sounding very strange indeed. Given that Smith does not address my points regarding the interpretation of historical sources and the dubious prestige relations his theory posits, I take it that he accepts these points.

Let us now turn to his commentary. There, he lays out some principles (originally from Thomason 2001) which can be used to gauge whether a

particular development can be argued to be the result of language contact. I agree with these principles, the first one of which is

(1) "look at the language as a whole, not just at one bit of it;"

Unfortunately, it seems that Smith has violated this very first principle by relying on the accusative marker alone. He has effectively looked at only one feature, the accusative marker (originally three, but two of them had to be discarded, number and definiteness). Supposing that Thomason adhered to the principle that the most important things should be on top of the list, Smith's account would not have to be discussed any further at this particular stage. To be fair, one could add the future marker, which SLM and Tamil have, but Sinhala and Vehicular Malay varieties lack. However, during the whole period I have investigated the structure of SLM, I have come across only those two items which point towards Tamil influence, while there are many more which point towards Sinhala influence (see my article in this issue of JLC).

In his commentary, Smith proceeds to attack all the examples I showed where SLM is closer to Sinhala than to Tamil. There are three recurrent patterns of argumentation: either the feature is probably a product of the twentieth century (2.7), the feature is also found in Tamil (2.1, 2.6, 2.8) or the feature does not need Sinhala influence since it could have arisen independently (most phonological features).

I myself pointed out that the morphosyntactic influence from Sinhala could stem from any point in time between the 17th and the 20th century, and that the morphosyntactic features cannot be shown to predate the 20th century. They might, but we do not have the means to prove this. I therefore focussed on phonological features to prove early influence, where we need both dialectal variation in the immigrants' varieties and Sinhala as a kind of catalyst.

Smith says that the phonemic status of prenasalized stops relies on phonological analysis and states that "the merits of these two mirror-image analyses can be debated, but this is not the appropriate place." One wonders what, if not the Journal of Language Contact, would be an appropriate place to discuss this. Basically, there are two competing analyses for the phonological system: Either vowel length is phonemic, and prenasalized stops are secondary, or prenasalized stops are underlying and vowel length is secondary. A simple look at pairs like *maa.kang* 'eat' and *ma.ka.nan* 'food' shows that vowels change their length according to environment. This points towards vowel length being a result of some phonological process/rule/constraint. Phonemic status of vowel quantity is not the first thing examples like these suggest, since one root, *ma(a)kan* can show up with two different quantity values. Prenasalized

stops on the other hand are invariant and behave exactly like other stops. *Thaa.ⁿdak* 'dance(V)' and *tha.ⁿda.kan* 'dance(N)' are cases in point. /ⁿd/ behaves exactly like /k/ in *maakang*.

Smith argues that, in any case, "[a] more satisfactory analysis would treat vowel length and consonant length as part of the same phenomenon of syllable weight". I could not agree more, and have in fact presented such an analysis of consonant length, vowel length, raising of schwa, and presnasalized stops in the framework of metrical phonology (Nordhoff 2009:96-99,120-129), which Smith apparently did not deem necessary to discuss. One of the consequences of such an analysis is that prenasalized stops have to be phonemic in order for things to work out.

Smith accepts that, if SLM has three series of stops, this would make a strong case for Sinhala influence. But then he argues that, phonetically, sequences of long vowels and short nasals, and sequences of short vowels and long nasals are typologically unremarkable. This may be true, but misses the point of the *phonological* status, which remains typologically unusual.

It is true that the roots of prenasalized stops, consonant gemination, and vowel length are found in varieties of colloquial Malay. If only one of those features showed up in Sri Lanka Malay, this would not be remarkable at all. The interesting thing is that, among all varieties of Malay, it is precisely Sri Lanka Malay which has all of them, and that, bizarrely enough, these three features are also found in one of the contact languages. One can of course invoke chance here, but one might as well acknowledge that there *is* an elephant in the room, and that those features together point more towards Sinhala influence than to chance alone.

In defense of Tamil influence on the dental articulation of initial voiced coronal stops. Smith argues that phonological influence can be partial, and that a language can take over one feature from an adstrate phoneme without copying the others. I agree. Smith cites data from Thomason 2001:93 regarding the development of retroflexes in Indo-Aryan.[2] Indo-Aryan acquired retroflex phonemes through shift-induced change from Dravidian, but uses them in phonotactic positions not found in Dravidian. Indo-Aryan thus has taken over a phoneme without its phonotactic constraints. What happened in Sri Lanka Malay was different. There, a new phoneme was created (/d̪/) which

[2] Incidentally, upon opening my copy of the book, I stumbled upon the following sentence [92]: "A final, and especially common, unjustified assumption is that a solid case for a contact origin can be made on the basis of a single feature." Given that Smith quotes pages 93 and 94 extensively, it may be conjectured that he also read page 92, but apparently failed to see the Tamil accusative it is a prime exampke of this final, common, and unjustified assumption.

was neither phonemic in Malay nor in Tamil, and this phoneme only surfaces in positions where it could never surface in Tamil, even as an allophone (Gair, Suseendirarajah & Karunatillaka 2005:xxvi). It can be found as an allophone in word-medial position in Tamil, a position where it is not found in SLM. I think Smith is stretching Thomason's idea that the "shared features need not be identical in all respects" beyond what this idea was originally intended to cover.

Wholesale or partial adoption of a feature, Smith's account simply makes no sense from a learner's perspective either. When confronted with Malay voiced alveolar stops, in initial position, the null hypothesis would be that Tamil learners do the same thing they do when confronted with English voiced alveolar stops in initial position: they get the point of articulation right but struggle with voicing (cf. Suseendirarajah 1973). This leads to initial [t] or [t̪], but not to initial [d̪].[3]

My final point also relates to phonology. Smith asks:"If the knowledge of Sinhala as a second language had such a [hypothetical] strong influence on the phonology of Sri Lanka Malays' mother tongue, why did it not have an equally strong influence in morphosyntax?" Smith suggests that influence in morphosyntax and phonology should go together. This is an entirely reasonable proposal. So let's take a look at morphosyntax in Sinhala, Tamil and SLM: morphosyntax is 90% the same in all three languages. The fact that we cannot prove that a particular feature is from Sinhala (rather than from Tamil) does not entail that Sinhala could not have served as the model language.[4] Features like SOV word order for instance could be of Sinhala or of Tamil influence (or of both). Under a Sinhala-only account, 90% of the syntax of SLM can be explained. The formulation "[Sinhala] did not have a […] strong influence in morphosyntax" suggests that Sinhala would be a completely unsuitable model; this is not true. Sinhala does provide a very good morphosyntactic model. But let's have a look at phonology now: it is uncontroversial that SLM phonology is closer to Sinhala than to Tamil; this is the very point of departure of Smith's

[3] This should not betray the fact that Tamil L1 speakers are in fact often able to acquire voicing distinction in initial position. This is then even maintained in otherwise phonologically adapted loanwords. A case in point is டாக்கொட்டார் <Taakkottar> [d̪a:kkot̪t̪a:r] "doctor" (Gair, Suseendirarajah & Karunatillaka 2005:xxvii). Note that in this particular example, the ban on initial retroflexes posited by Smith does actually not apply. Gair et al. explicitly state: "At the beginning of a word, …, [ட] is like the ඩ (D) of Sinhala; that is, a voiced retroflex stop."

[4] The reverse holds true as well: The fact that we cannot prove that a particular feature is from Tamil (rather than from Sinhala) does not entail that Tamil could not have served as the model language.

argumentation here. If Tamil has, according to Smith, had such a great influence in morphosyntax, how can it be explained that Tamil has had close to no influence on phonology? Smith suggests that morphosyntactic influence and phonological influence go together. We see that there is close to no phonological influence from Tamil. This yields is a text book case of the *modus tollens*:

(2) Phonological and morphosyntactic influence go together
There is no phonological influence from Tamil

There is no morphosyntactic influence from Tamil

I would not go this far myself, but this is what a rigorous application of Smith's methodology yields. I leave it for the reader to judge the merit of this.

Where does this leave the study of language contact? There are scores of varieties of Malay, and a dozen or so dialects of Tamil in Sri Lanka (Sinhala dialectal diversity is less important), and huge idiolectal variation in SLM. It is likely that for every feature of SLM, it would be possible to imagine an internal cause and an external cause. Just saying "this could be an internal development" and dismiss an external cause, or vice versa, is not satisfying. The dialectal diversity of Malay is a major issue. I cannot count the times any more when I mentioned a particular Sri Lanka feature to Uri Tadmor or David Gil, and they replied: "This is also found in that particular small island dialect there and there". But this dialectal diversity is also a chance: we can see in which directions historical varieties of Malay develop often, and which developments are rare. In Sri Lanka, it turns out that we have a singularity of unique developments. This singularity demands an explanation, and the obvious choice is contact.

Historical linguistics and language classification share problems similar to SLM studies: many typological features are common and could be due to either internal or external forces. What counts in these cases is the 'quirkiness' of a feature. The rarer the feature cross-linguistically, the more weight it has as marker of common history. Accusative marking is widespread and is one of the functional categories most frequently encountered in the languages of the world and can develop independently. As such, it has little value. Prenasalization in opposition to both plain nasals and voiced stops on the other hand is a very rare feature, arises less often spontaneously, and is therefore more indicative of contact.

This is not to say that I think that accusative marking in SLM would be the result of an internal development. But the reasons brought forward against prenasalization would invalidate accusative marking as well and would throw the baby out with the bathwater. This is clearly undesirable.

I thank Ian Smith for valuable clarification on diachrony and dialectal variation in Tamil as far as morphosyntax is concerned, but the phonological facts make me stand to my initial conclusion: Tamil and Sinhala have both influenced Sri Lanka Malay from the early days onwards.

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Author Query

AQ1: the status of this piece is understood; still, given its length, please do consider to submit abstract and keywords]