

Access, prestige and losses in contact languages

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I am generally positive about Muysken's (M) approach, and the potential use of unifying various seemingly related phenomena is obvious. The approach could also serve as a tool in determining to what extent these phenomena actually are sides of the same coin (I am somewhat less convinced of this than most contact linguists).

A less benign, and perhaps even irreverent, description would be that M claims that features in contact varieties derive from either the L1, the L2, both or neither, something that could be claimed *a priori*, without any actual knowledge of the varieties considered. While this does not necessarily imply that his quadchotomy is of no value, there are certain aspects that I am critical of.

My own research interests lie primarily in the area which M subsumes under the heading "language systems in contact" (Section 1.1), and in particular pidgins and creoles (a field with regard to which I would, incidentally, disagree with M's statement that important progress has been made), and I shall restrict my comments to the subfield where I can claim some expertise.

First, I think M's categorisation of pidgins and creoles is somewhat dubious, possibly because I do not share his assessment that his main source on pidgins "provides an excellent overview of" these varieties (Section 3.1).

For instance, M repeatedly characterises Hawaiian English Creole as a case where the lexifier ("L2" in M's terminology) was unavailable. The normal depiction of creole genesis in general is indeed that it involved "limited access" to a language which socioeconomically subordinate groups are eager to learn. I disagree with this, but even if it were true in most cases, Hawaiian English Creole would be an enormously unsuitable showcase. This is because the generation that created it in the early 20th century was schooled in English, and the evidence available through the works of Sarah Roberts clearly shows that the children in question did acquire English both in its spoken and written form, but that they "valued [the emerging creole] as a resource for constructing group identity" (Roberts, 2004, p. 225).

Related to the concept of "limited access" are the concepts of power and prestige. Again, creators of pidgins and creoles are assumed to have striven to acquire the lexifier, and this assumption is so entrenched in creole studies that it is normally not questioned – it simply goes without saying (Baker, 1990, and a number of subsequent publications are among the few exceptions). The reason for the identification of an acquisitional target is typically

associated with the higher status of the speakers of the lexifier. In this context, Russenorsk is almost always mentioned as a special case, and as expected, M does so as well. Russenorsk is indeed odd (though not unique) in being a lexically mixed pidgin, but the explanation given by M and others, namely that this language emerged in a context with an unusually equal power relationship, is questionable. The main evidence for the claim is the lexical mixedness, which obviously makes the reasoning circular. Russenorsk is mixed because, the reasoning seems to go, Russians and Norwegians were on more or less equal footing, and this we know through the pidgin's mixed character.¹ Furthermore, this implies that other, more "normal" or "well-behaved" pidgins typically emerged in contexts characterised by unequal power relations. I suspect that this perception is based on the assumption that Europeans always had the upper hand in their dealings with overseas peoples, which is far from true, at least in the initial contacts. Regardless of the ownership of firearms, it is not immediately obvious that the craving among native populations for beads and liquor was greater than the need for freshwater, provisions and female company on the part of small groups of scurvy-ridden European sailors.

In addition, there are numerous pidgins lexified by non-European languages. In many such cases, we simply know very little about the relevant sociolinguistic context, and where these languages were also used by European colonisers, we would need some independent (not based on language use) evidence that would explain why the seemingly omnipotent European colonisers in these, but not other cases, had the underdog role.

M's description of Pidgin Delaware as a language where the lexifier speakers deliberately simplified their

¹ A similar point could be made for M's comment (in Section 2.3) on Berbice Dutch Creole – that there was a "relative balance over a longer period between speakers of the L2 and a single important L1" (Section 2.3) is an inference based on the language itself (since the early history of the Berbice colony is virtually completely undocumented), and this inference is then used (not only by M, but also by many others) to explain why the language looks the way it does. The circularity should be obvious. According to M, the same circumstances obtained for Senegalese Portuguese Creole (ultimately an offshoot of Cape Verdean), where I cannot even guess what this single language would be. While the lexical material (which is easier to etymologise than the structural features) is mostly from one single language (Eastern Ijo) in Berbice Dutch, this is not the case for Senegalese Portuguese Creole.

language in dealing with outsiders is also problematic in that it implies that such would not be the case for (most?) other pidgins. But one wonders why lexifier speakers in other contact situations would insist on using past subjunctives, politeness distinctions and intricate morphophonemic alternation rules when trying to make themselves understood by outsiders obviously not grasping those details. Not unexpectedly, when there is historical evidence, it generally shows that this is precisely what they did not do, but that the speakers of the lexifier too were active creators of the new language rather than just passive emitters.

There is one other aspect of M's treatment of pidgin and creole languages that I find problematic. He is by no means alone here, but the approach is indeed typical of virtually the entire field. What I have in mind is the focus on what these varieties CONTAIN rather than what they have LOST vis-à-vis the input languages. The standard view deals with the PRESENCE of features from the lexifier and the substrates despite the fact that pidgins and creoles are marked more than anything else by the ABSENCE of such features. At least to some extent, this presumably falls under M's heading "universals", but it is not obvious to the uninitiated observer that these universals most often have a reductive rather than an additive influence. Elsewhere, M used Pidgin Delaware as an example, and this pidgin could also serve to illustrate my point here: EVERY SINGLE language involved in its genesis had a category of pronominal number (as do most languages on earth), and yet, this is lacking in the pidgin (Goddard, 2000, p. 70). By focussing on the origins of features that do exist in pidgins and creoles rather than those that might have been present (were offered by the languages in contact) but are not, we are missing the most important generalisation there is about them, namely that the most important process involved is one of reduction. This is especially so if we think of universals as "something that most languages have", which is a not uncommon use of the term. Clearly, Pidgin Delaware in this regard is not closer to the most common state of affairs in human languages than the languages in contact, nor is this reduction typical of allegedly universal processes of second language acquisition. And the loss of pronominal number in this variety is not an isolated exception – on the contrary, plenty of contact situations leading to the

elimination of features offered by most or all the input languages could be mentioned.²

If this deletion is, as I suspect, included under "universals", it is at least encouraging that in M's quadrangle of factors influencing the four options (see his Figure 4), the quadrant that is proposed to favour universals ("political distance", "typological distance", "lexical distance", "short contact period") in my view matches the settings which most often have given birth to pidgins and creoles far better than any of the other.

A more minor point is that I would, despite my attempts to move the focus away from the "mixedness" of pidgins and creoles, reclassify Papiamentu's use of third-person plural *nan* in M's example (12) as a nominal pluraliser from a universal to a substrate-induced feature. To the best of my knowledge, it is not particularly common among the languages of the world, and if pidgins are included, it is not among contact languages either, and it typically occurs in creoles where this was an option featured in at least some of the relevant substrates.

I leave it to the reader to judge which (if indeed any) implications the above has for M's proposal. I myself think that one of them is that pidginisation and creolisation are less clearly connected to second language acquisition than mainstream creolistics (and M) would have it.

References

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² For instance, the two rather than three degrees of deixis in Philippine Spanish Creole, the preponderance of zero attributive copulas in Atlantic creoles, the lack of pronominal clusivity in Bazaar Malay, the absence of case marking in Icelandic Pidgin Basque, and not least, the generally analytic character of pidgins and creoles even in cases where the input languages were heavily inflected. In all these cases, the pidgin or creole lacks a feature shared by the input languages.