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Why Haitian is a creole, Michif an intertwiner, and Irish English neither: a reply to Mufwene

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1 Introduction

In our article (this volume), we outlined a new hypothesis for the formation of Berbice Dutch, proposing a two-stage genesis: a first phase between 1627 and 1712 in which a Dutch-lexified creole emerged in the Berbice colony, followed by a second phase in which the creole mixed with Eastern Ijo. This second phase started in 1713 following the arrival of ca. 300 speakers of Ijo, and would largely have been completed one or two decades later when greater numbers of slaves began to arrive in the colony. The mixing affected mainly content vocabulary, but some grammatical material (including affixes) was also introduced into the creole. This second phase is, in our view, the actual starting point of Berbice Dutch as we know it. The aim of Mufwene's rejoinder is not to specifically rebut these two stages (though he does criticize aspects of it, see Section 5); he even acknowledges that "the presence of a creolized Dutch spoken in Berbice prior to the 1713 arrival of the Ijos [...] is plausible" (this issue, p. 205). Rather, he takes issue with a number of assumptions that underlie our genesis scenario (but which are for the most part not directly related to it), more precisely: our assumptions regarding the ontological status of creoles and mixed languages.

2 On the definition of 'contact languages'

In line with earlier publications, Mufwene maintains that there are no structural correlates of the concepts of 'creoles' and 'intertwined languages', two categories that we, for the sake of convenience and in keeping with linguistic tradition, have subsumed under the label 'contact languages'. There is not and cannot be anything

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typologically peculiar about varieties dubbed ‘creoles’ or ‘contact languages’, he argues, since contact has played a role in the genesis of every other language: “virtually every modern language [owes] some of their current structures to contact [and] the term *creole* would thus become synonymous with *language*” (Mufwene, this issue, p. 207).

Mufwene’s suggestion that we think ‘contact’, in the general sense of the word, is what defines creoles is, however, a strawman, as is his tacit suggestion that we deny the role of language contact in the formation of traditional languages. It is, of course, a matter of degree: there is both a quantitative and a qualitative difference between the impact contact has had on traditional languages on the one hand, and on what we refer to as ‘contact languages’ (pidgins, creoles and intertwined languages) on the other. In the case of Indo-European, for instance, contact led to some tweaking which (in combination with other factors) led to the daughters finally being recognized as separate from their mothers. In the case of pidgins, creoles and intertwined languages, there was a severe and rather instantaneous disruption of the original input languages. This disruption – in the literature on pidgins and creoles often referred to as ‘broken transmission’ – has in some cases been documented and can in other cases plausibly be reconstructed on the basis of linguistic data.

And so indeed, we agree with Mufwene that, if thus applied in an inflationary manner, the terms ‘creole’ or ‘contact language’ would become synonymous with ‘language’, which is precisely why we would plead for a less liberal use of these terms and encourage linguists to apply them only to languages that show evidence of a non-trivial amount of (contact-induced) restructuring, such as pidgins, creoles and intertwined languages.

3 On prior pidginhood and what really defines creoles

There are a few points on which we agree wholeheartedly with Mufwene. For example, while one may get the impression from his commentary that we support “the universality and unmarked status of SVO” (Mufwene, this issue, p. 211) or that we believe there are “features of both creoles and intertwined languages to the exclusion of other languages”, or for that matter that there are specific “structural features shared universally” within each of these groups, we concur with him that there is no convincing evidence for such positions. To the extent that creoles in different parts of the world do share features, we would argue that this is a mere artefact of (a) their descentance from pidgins and in many cases of (b) the similarities between the input languages. We and Mufwene agree on (b), but not (a).

There are in any case no specific linguistic features (other than those found in all human languages) common to creoles (much less to intertwined languages), but creoles do tend to share a general typological profile atypical of traditional languages.

This typological profile, we think, is a consequence of their prior pidginhood, which in turn is identifiable in the relative lack of grammatical complexity of creoles *vis-à-vis* traditional languages: being new and young languages, descended from pidgins, creoles simply have not had the time to accrete such complexity. Crucially though, the relative economy of creole grammars has no bearing on their expressiveness. And the fact that the (re)building of grammar can be observed diachronically in several creoles (and is reconstructable for others) is of course one of the things that make them such fascinating objects of study.

Countering our view, Mufwene (this issue) emphasizes that there is grammatical simplification not only in creoles but also, for instance, in Romance and English. This is certainly true, but it misses the point by quite some margin. Firstly, simplifying tendencies have been nowhere near as strong in Romance and English as they have been in basilectal creoles lexified by them – again, it is a matter of degree.¹ Secondly, our knowledge of the histories of European languages suggests that much of the restructuring was a protracted affair, taking place over centuries, while the evidence for creoles suggests that they, by comparison, came about rather rapidly.

Mufwene also points out that “the actual lexifiers of creoles were nonstandard dialects of European languages” (this issue, p. 208). This appeal to nonstandard varieties is of course intended to support the idea of continuity (as opposed to broken transmission) between lexifier and creole, and that “creoles have simply extended the morphological impoverishment that was already evident in [...] the actual lexifiers of the creoles” (Mufwene, this issue, p. 207). However, there is nothing inherent in a variety being nonstandard (or colonial, or transplanted) that entails ‘morphological impoverishment’. In fact, even restricting ourselves to European languages, it is not difficult to come up with vernacular lects that are morphologically at least equal in complexity to their respective standards. Consider for instance Acadian: this nonstandard variety of French shows no traces of the kind of simplification seen in French-lexicon creoles. Acadian has even retained the *passé simple* (alongside the *passé composé*), unlike spoken European French.

1 The difference in nature and degree of simplification is most readily observable in the domain of morphology, but it is easily detected in the lexicon and syntax as well. The relative grammatical economy of creoles is also all the more obvious if languages other than Romance or English (such as those of the Americas and Australia) are used as a comparandum – something that almost never happens in the debate on creole complexity.

Nearby Newfoundland English is another colonial nonstandard variety where the kind of systemic reduction seen in creoles such as Sranan or Tok Pisin is noticeably absent. Like with creoles, its history furthermore involved large-scale language shift as many of the original settlers were speakers of Gaelic, and yet all personal pronouns are gender- and case-sensitive and the colonial dialect has preserved distinct accusative and dative object clitics – long lost from standard English (cf. Clarke 2004: 249, 313; Story et al. 1982).

Although the above examples only concern specific features/subdomains of the grammar, they show that there is no *a priori* reason to assume nonstandard varieties to be more grammatically reduced or ‘impoverished’ than the respective standards. Many other colonial lects could be mentioned here, but the bottom line is of course that the nonstandard nature of creole lexifiers cannot account for the profile of creole grammars.

A more straightforward account is that creoles developed from pidgins and that they have visibly young, recently expanded grammars as a result of it. While this pidgin-creole cycle is often labelled ‘hypothetical’, there are several settings where it has been observed rather than inferred (Juba Arabic/Kinubi, Grand Ronde Chinook Jargon, Sango, and the various English-lexicon varieties of the Pacific and Australia being the most obvious examples²). Creolists skeptical of the pidgin-creole cycle hardly ever mention these cases, but instead focus on those creoles (mostly of the Atlantic) for which the historical records are much more patchy and the diachronic processes less well-documented. To us, this is indicative of a fallacy in the work of Mufwene and likeminded scholars – why extrapolate from the unknown to the known rather than vice versa?

4 On Haitian Creole

As noted, for those creoles whose prior pidginhood has not been documented, this phase can be plausibly reconstructed based on the nature of their grammar. This is true even for a case like Haitian, a creole which at first glance may appear to have inherited more productive morphemes than what creole exceptionalism can account for. Mufwene refers to DeGraff’s (2001a) claim that Haitian “has retained numerous derivational morphemes from French” (this issue, p. 207), which, again, is intended to illustrate continuity between the lexifier and the creole. Let us first note that all 13 affixes claimed by DeGraff (2001b: 59–60, 2013) to have been inherited from French are indeed derivational; there is no trace of the lexifier’s

2 See for instance Heine (1982) on Kinubi, Samarin (1980 and elsewhere) on Sango, Zenk (1984 and elsewhere) on Chinook Jargon, Tryon and Charpentier (2004) on the pidgins and creoles of the Pacific and Australia, amongst many other works.

inflections. In addition, 13 is anything but an impressive amount from a crosslinguistic perspective. But more importantly, Mufwene and DeGraff abstain from considering the rather obvious possibility that some of these bound morphemes may simply have been borrowed from French *after* creolization. Haitian Creole has after all co-existed with French up to present.³

The reason given by DeGraff as to why Haitian could not have borrowed material from French is that the majority of Haitians have never been proficient in it. But if proficiency in the donor language were a *sine qua non* for borrowing, one must wonder why one of Haitian's 15 affixes is of Spanish origin, or indeed how more than half of the English vocabulary (including affixes) has come to derive from French and Latin.

In short, Haitian lacks all French inflection as well as the majority of its derivations (and those that it does have may very well have been borrowed post-creolization), none of which is a surprise if we assume the language developed out of a pidgin.

5 On prioritizing linguistic over historical data

One of the points we have repeatedly made (our article, this issue, and elsewhere) is that hypotheses about, and reconstructions of, older stages of languages should be informed first and foremost by linguistic data (to the extent that these are available, of course). While historiography can play a very important auxiliary role, it should not be allowed to override linguistic data. If the map (the socio-historical background, or 'ecology' as Mufwene would call it) and the terrain (the linguistic facts) fail to match, it is the map that warrants revision. This is a major methodological difference between us and Mufwene, who bases most of his views and conclusions on sociohistory, while linguistic data typically take a back seat. As it happens Berbice Dutch offers the perfect illustration of why, in historical linguistics, linguistic data should – other things being equal – be treated with priority over historical ones. Prior to our discovery of the *Sint Antony Galeij* (detailed in Section 3.2 of our article, this issue), no migration of Ijos to Berbice had been documented, but the linguistic facts were so strong that scholars agreed that it *had to* have occurred.

Although Mufwene largely avoids commenting on our genesis scenario for Berbice Dutch, the few instances where he does so, his critique seems to stem

³ The borrowing of derivational affixes is a well-attested phenomenon in traditional languages. In fact, it is not rare for languages to have more borrowed derivational morphemes than native ones, and there are many varieties whose *borrowed* affixes outnumber the *entire* affix inventory of Haitian (Seifart 2013). For creoles where the prestige language is different from the lexifier (making the borrowings easier to identify), there are ample cases of affix borrowing, too.

precisely from his refusal to prioritize linguistic over historical evidence. For instance, at the heart of our historical framework is the above-mentioned vessel *Sint Antony Galeij*, which we have reason to believe brought around 300 Ijo slaves to Berbice in 1713, in addition to ca. 100 slaves collected at the island of Príncipe. Mufwene (this issue, p. 208) casts doubt on the importance of the *Galeij* to the history of Berbice Dutch because it requires one to assume that “the putative minority of non-Ijos that came on the *Galeij* had no role [in the creation of Berbice Dutch], or just a negligible one”. Clearly though, and whatever the precise reasons, the assumption that the slaves from Príncipe (if there were any) had no impact on the formation of Berbice Dutch is simply a conclusion based on linguistic data – the language obviously has a huge Ijo component but a rather moderate amount of influences from other African languages and none, as far as we know, from Príncipe.

Furthermore, in our article (this issue, p. 196), we stated that our hypothesis about the two-stage genesis of Berbice Dutch and the post-1712 admixture of Ijo material “is falsifiable in that if a creole sample from Berbice dating from before 1713 should ever turn up, we predict it will be devoid of Ijo material”. Mufwene (this issue, p. 208) thinks our claim to falsifiability does not hold “because there are no [such] data”. That criticism is odd, because falsifiability is of course all about taking data into account that aren’t there yet but might surface in the future. On the basis of Mufwene’s own admission that the existence of a Dutch-lexified creole in Berbice before 1713 is “plausible” (this issue), our genesis scenario clearly possesses the merit of being falsifiable in case new, pre-1713 data pertaining to the hypothetical Dutch-lexified creole precursor ever emerge. As the very discovery of the *Galeij* should make amply clear, where there is a will, there is sometimes a way: new data (linguistic and historical) can come to light every once in a while even for extinct languages,⁴ but Mufwene’s defeatist criticism (“there are no data”) hardly encourages any search for such data.

6 On mixed languages as a class and what does not define creoles

Mufwene also questions the class of intertwined languages since they lack structural commonalities, which is true. “They are identified as such because only two ethno-linguistic groups were involved in the contacts, which were intimate”, he concludes (Mufwene, this issue, p. 208), which is not true. Rather, the precise nature of the contacts that gave rise to intertwined languages is often uncertain and is usually

⁴ See also Jacobs and Parkvall (2020 and forthcoming) on Skepi.

inferred from the make-up of the languages in question.⁵ And while intertwined languages – as opposed to creoles – indeed cannot be synchronically singled out as a typological class, they can nevertheless be identified as such by the fact that *entire linguistic subsystems* were taken over more or less *in extenso* from two clearly identifiable contributing languages, something that is not true of creoles or of traditional varieties (see further in Section 2.2 of our article, this issue).

Mufwene says that if we insist on grouping intertwined languages together, we might as well include Irish English in the category since “it is the outcome of the contact of two languages” (this issue, p. 209). Plenty of varieties are, but this is completely uninteresting to us. What is interesting is that what most Irish people speak is clearly a dialect of English, albeit with a sprinkling of Irish influences. Such contact-induced developments are exceedingly common around the world, but the emergence of intertwined languages is not. The combination of entire subsystems from one language with those of another (which often translates into a lexicon-grammar split, or, in the case of Michif, a VP vs. NP split) is very rare indeed, and justifies a class of intertwined languages. The members of this group are also typically such that the question of whether input language A influenced input language B or vice versa is moot – no obvious diachronic layering suggests itself. This is in stark contrast with a language such as English, where there would be no doubt (even if we knew nothing about the external history) that we are dealing with a Germanic variety that has been subject to a later Romance (and Scandinavian) overlay, rather than vice versa. In fact, this is even a potential definition of the class – intertwined languages are languages whose linguistic subsystems are derived from two different identifiable contributors, but where one did not clearly precede the other.

Bizarrely, Mufwene (this issue, p. 209) points out that “Michif is no more identical with Cree than French is”, which of course is precisely why we do not call the language “Cree” or “French”, and why we do not treat it as Romance or Algonquian. The VPs of Michif, however, are almost identical with those of Cree, and its NPs with those of French. This is the whole point of classifying Michif as an ‘intertwined’ (or ‘mixed’) language.

The Celtic contributions to Irish English, on the other hand, cannot by any stretch of the imagination be categorized as ‘entire linguistic subsystems’. They are in this case more similar (albeit less conspicuous) to the substrate component of creoles, the chief difference between the two being that the degree of reduction characteristic of creoles is not found in the English of Ireland. Therefore, it is fully reasonable to distinguish between pidgins (reduced), creoles (reduced and then expanded),

5 Gurindji Kriol is a rare exception: here the mixing and subsequent nativization has actually been observed by linguists (Meakins 2012 and elsewhere).

intertwined languages (mixed) and traditional languages (exhibiting none of these properties to a non-trivial extent), of which Irish English is an example.

7 On substratal influence in creoles

In his rejoinder, Mufwene portrays us as if we were denying the existence, or trivializing the importance, of substrate influences in creoles. Rest assured that we do not. Perhaps unbeknownst to Mufwene, one of us has even published an entire monograph cataloguing, tracing and discussing substrate influences in Atlantic creoles (Parkvall 2000). But regardless, the point we make in our article (this issue) about Ijo being *ad-* rather than *substratal* is of course merely a matter of establishing the time-depth of the Ijo contribution: as is customary, we define ‘substrate influence’ as the (non-lexifier) influence exerted on a creole *during creolization*, whereas all posterior influence is dubbed ‘adstratal’ (though the dividing line between them is of course transitional, not absolute).

Although we fully acknowledge the role and relevance of substrate influence in creoles, substrate influence, or ‘mixedness’ for that matter, is not what defines them – it is merely an epiphenomenon of contact and there are after all plenty of non-creoles that are considerably more mixed than creoles. However, there are no other languages which show such clear signs of being derived from pidgins. This prior pidginhood and the ensuing grammatical expansion – not mixing – is what creolization is all about. Incidentally, this also answers Mufwene’s question as to “why intertwined languages are claimed to fit in the category of mixed languages while creoles are not” (this issue, p. 209).

8 On *ad hominem* deflections

There is no denying that contact linguistics arose primarily out of sociolinguistics, but one of the major problems of current creolistics is that all too many creolists appear to be complacent about the tools provided by that discipline. Meanwhile, the fields of language typology and historical linguistics have advanced significantly in the last decades. If one is interested in inherently typological questions such as ‘What are creoles like?’ and ‘How did they become whatever they are?’, a grasp of those two subfields seems to us indispensable. Yet, instead of embracing them (or encouraging others to do so), Mufwene (2003) is on the record questioning the validity of

the Comparative Method.⁶ And he is not alone in his disregard for data-driven approaches; his work has become representative of a wider trend in modern-day creole studies, one in which a mixture of ideology, identity issues, and an obsession with power relations have come to override the discussion of actual linguistic data.

Indicative of that trend are the ever increasing accusations of racism. According to Mufwene (this issue, p. 210), we are “just trying to save distinctions made in the 19th century when European philologists [...] assumed that non-Europeans were evolutionarily too inferior and spoke languages too simple to be able to learn the sophistications of European languages”. Although these *ad hominem* accusations are often tacit, it should be obvious to linguists taking their discipline seriously that they are intellectually dishonest deflections that have no bearing on the actual arguments.

The invocation of a pidgin stage in the birth of creoles, not so long ago still seen as their principle defining feature, is now often (deliberately?) misinterpreted as meaning that creoles are somehow impoverished and unfit for the tasks that languages are intended for. However, the crux of the matter is that ‘grammatical complexity’ and ‘expressiveness’ are largely independent parameters. While we insist that traces of prior simplification can still be seen in creoles, and indeed that they are synchronically less grammatically complex than traditional languages, we see no reason whatsoever to question the axiom (unprovable though it may be) that all natively spoken languages possess the same – and obviously sufficient – expressive power.

Taking that one step further, one could even argue that the combination of grammatical economy and full expressiveness is a testimony to the intellectual achievement of those who created pidgins and creoles. Along those lines, Rudolf Lenz (1928: 986) once pointed out that despite having a “minimal grammar”, the Papiamentu language serves perfectly for daily use by people of all social standings, and he added:

if one accepts Jespersen’s criterion that ‘that language ranks highest which goes farthest in the art of accomplishing much with little means, or, in other words, which is able to express the greatest amount of meaning with the simplest mechanism’, then Papiamentu is one of the most perfect languages in the world (Lenz 1928: 989, our translation from Spanish).

Linguists should avoid making value judgements, but if we were forced to do so, we would gladly subscribe to the Jespersen/Lenz maxim.

⁶ This prompted a flaming response from Thomason (2003), which remains one of the few critical assessments in recorded history of Mufwene’s oeuvre.

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