

The case was never closed

McWhorter misinterprets the ecological approach to the emergence of creoles

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In his first guest column in this journal, McWhorter (2012) alleges that ‘the “feature pool” hypothesis’, as he caricatures my ecological approach to the emergence of creoles, is ‘counterintuitive’ and ‘intriguing’, because, in his misinterpretation, its practitioners claim or suggest that structural ‘simplification play[s] no significant role’ in this process. ‘An opposing idea is that creole genesis involves not only mixture, but also considerable simplification of grammars, to a degree far beyond what grammars undergo naturally, under contact or not’ (p. 171). ‘The “feature pool” hypothesis (...) fares beautifully as an idea, but much less so as a mechanism’ (p. 172).

It may surprise some readers who share McWhorter’s views that I agree with the essence of the ‘opposing idea’, viz., the competition and selection generated by the feature pool will not explain alone everything about the emergence of creoles, which, I maintain, is a protracted complex restructuring process driven by various interactive factors. Nor indeed can simplification alone tell the full story, even when it is combined with ‘mixture’. A host of other mechanisms of change, including grammaticization, generally constrained by Universal Grammar as a filtering mechanism and by several ecological factors, bear on the language speciation process. The ecological factors include the Founder Principle (which, contrary to the misinterpretation Faraclas 2012 makes of it, applies to more than speakers of the lexifier), other aspects of periodization (consistent with gradualism), and population structure (having to do with the heterogeneity of the population, demographic proportions, segregation/ integration, and power relations). The list of ecological factors is by no means closed, subject to future research. The general idea behind this complex scenario is that all evolution is local, responding to the local ecological dynamics/pressures, which is precisely why the different creoles that evolved out of the contacts of more or less the same languages are still different from each other (see below), despite the family resemblance that enables us to lump them in the same category evolutionarily and typologically.

However, I must dispute a number of straw men that McWhorter has constructed in his column to defend a position that none of the people associated with the alleged 'feature pool hypothesis' (see below) would argue against, though, as just suggested above, it is far from being the full story. The straw-man building starts with the passages I just paraphrased and quoted from the first two pages of the column. I have never referred to the feature pool as a 'mechanism' because, as its meaning suggests, it cannot be. It is simply a state of affairs that arises out of the contact of languages in the minds of speakers, who have access to features of the relevant systems.

In Mufwene (2001), the concept was proposed, on the model of gene pool in biology, to highlight, against what McWhorter (2001) would characterize as formation of new languages out of the 'pulverization' of their lexifiers' systems, the fact that there is little in the structures of creoles that does not originate in the systems of their lexifiers or some of the substrate languages that these had come in contact with. I then hastened to explain in my development of the analogy between biological and language evolution that the model to follow was not that of animal biology but that of bacteria (rectified in Mufwene 2008 as that of viruses), because the features are subject to modification, depending on the host/speaker. The general idea behind it, which is not limited to second-language acquisition, is that language acquisition is a misnomer for what is otherwise a reconstruction process, which typically replicates the target language with modifications. Deviations or divergences from the target are a matter of degrees, though they are more extensive under particular ecological conditions, such as those that produced creoles and pidgins. (I return to this below.)

DeGraff (2009, ignored by McWhorter!) is one recent convincing argument along the same lines, showing that Caribbean Creoles such as Haitian should be grouped genetically with their European protolanguages. This position does not entail that Haitian Creole is a 'dialect' of French; it simply underscores the fact that, if one abides by the logical conclusions of the Comparative Method, Haitian Creole is as much a descendent of nonstandard French as French is accepted to be a daughter of Vulgar Latin. Both are consequences of the appropriation of a language by a different population of speakers, who restructured it partly under the influence of their substrate languages, although, with a couple of exceptions, Romanists appear to have generally dodged the role of substrate influence in the emergence of the Romance languages.¹ A different genetic-affiliation conclusion smacks of double standards, as I argue in Mufwene (2001, 2003, 2008).

1. Ironically, Romanists introduced the term *substratum*, coined by Graziadio Isaia Ascoli in 1867, to historical linguistics. Another Romanist, Walther von Wartburg would introduce the term *superstratum* in 1934, or maybe a couple of years earlier.

It is for a very good reason that I did not title Mufwene (2001) *The feature pool hypothesis*, largely because I was developing the idea that started in Mufwene (1986), shared by others such as Hancock (1986) and subsequently characterized as the complementary hypothesis. I propose a number of other principles and mechanisms, as stated above, in order to account for the complex process of the emergence of creoles and pidgins, regardless of whether or not they instantiate structural simplification compared to both their lexifiers (which is not always the case, as I point out below) and the relevant substrate languages.

Note that McWhorter is mistaken in suggesting that while feature recombination is no more than a 'description' (I would *not* argue otherwise!), simplification is an 'explanation' (p. 173). As far as I can tell, it describes a process, period. He should also know that this is part of the tradition in historical and genetic linguistics, as we use terms such as *metathesis*, *lenition*, *weakening*, *contraction*, *grammaticization*, etc. to *describe* the kinds of changes that have happened. Besides, like feature recombination, simplification can happen in any number of ways. In this respect, McWhorter should be objective enough to appreciate the meticulous ways in which Enoch Aboh (in 2009 and in several other essays) shows how 'hybridization' occurs within the word and not just in the grammar. His work enriches our understanding of how language mixing takes place, indeed 'challenging decades of work relating creoles to pidgins, foreigner talk, second language acquisition, "weak" parameter settings, etc.' (p. 172), which indeed has not told us what the units of selection and mixing are, nor provided us with as much information about the extent to which substrate influence itself can be imperfectly transferred into a creole.

Should those of us pursuing the research program I articulated above prove correct, then the paradigm shift would be consistent with the history of science, when such changes upset the received doctrine and competing alternatives. It is not the longevity of the tradition that proves the validity of a position, it is how the position itself explains things relative to everything else we think we know. In the case of the emergence of creoles, much of the debate has had to do with whether traditional or current accounts are consistent with the economic history of colonization, whether the processes and combinations of features associated with creoles, such as those proposed by McWhorter himself, are really unique to these new vernaculars, and whether creole exceptionalism is as justified as claimed by him and, to some extent, Good (2012), in the same *JPCL* issue.

Even in his column, McWhorter has adduced no evidence that suggests why creole exceptionalism must be sustained; and I hope that part of the refutations below is convincing enough to show that there is no need for this. In fact, what we are learning about these new vernaculars suggests that genetic linguistics has not always been practiced in an exemplary way. I showed in Mufwene (2001, Ch. 5; 2008, Ch. 3) that genetic linguistics can learn back from some of the issues we

are wrestling with in genetic creolistics, which question myths about the purity of language at any point since the human dispersal out of Africa 30,000–50,000 years ago and about whether languages can speciate independent of the triggering effect of ecological changes occasioned most often by population movements (including simple splits which offset the balance of power among competing features) and/or language contact.

There are some important reasons why I personally have not dwelled on simplification. The idea of feature recombination on the viral model leaves enough room for this to occur, although not ineluctably or across the board. First, as every creolist should know, loss of morphological marking has not always proceeded hand in hand with loss of semantic distinctions (a fact that Good's 2012 abstract research program does not factor in either). My study of number delimitation in Gullah and Jamaican Creole, for instance, shows that the kind of change that the lexifier underwent was primarily a shift in the typological alignment of the subsystem. Like in their English lexifier, PLURAL continues to be expressed in these creoles, but with a free marker rather than with an inflection, and its expression is governed by different kinds of grammatical constraints, such as when reference is INDIVIDUATED, NON-GENERIC, and is not in the environment of a numeral quantifier. Generic reference is expressed with a NON-INDIVIDUATED bare noun, like in Sinitic languages. Only a shallow analysis will conclude that such a change boils down to simplification of the grammar. (Good 2012 would perhaps argue that this is 'contextual paradigmatic complexity', which need not be simplified.)

Likewise, although one (in McWhorter's mindset) may argue that the Guyanese TMA system is simplified compared to that of English, note that it has two ways of expressing the PROGRESSIVE (call it 'DURATIVE' if you prefer), one of which is not equally grammaticized in English: *im a taak* 'he is talking' means something different from *im de a taak* 'he is busy talking'. Gullah and Guyanese also have a specialized HABITUAL marker, [dɛz] in Gullah but [dɔz] in Guyanese, that has no counterpart in Standard English, which depends on the simple present tense, whose more specific interpretation depends on a host of other factors.²

Besides, one may well argue that Gullah's and Jamaican and Guyanese Creoles' TMA systems also instantiate shift in typological alignment, as the restructuring of lexifier produced a 'relative tense' system (common in some of the relevant substrate languages, especially from the Kwa family) rather than the 'relative-absolute

2. Doesn't this smack of the interpretation of the morphosyntactically undelimited verb in several creoles, those lexified by European languages? An important difference lies in that one must rely on the STATIVE/NONSTATIVE distinction to determine whether reference is made to the past or to the present, and on other pragmatic factors to determine whether the time reference must (not) be interpreted as HABITUAL.

tense' system of English. Only misguided typologists who claim that the parametric options of some languages are universally less marked than those of others, based on apparently world-wide statistical distribution, would also want to claim that one type is systemically simpler than another. They would thus be ignoring the associated interpretive rules that one must apply in the relevant system; and the conclusion would amount to what I term 'colonial, hypermetropic, and wishful linguistics' in Mufwene (1989a). Languages as symbolic systems are predicated on the association of form and meaning and/or function!

In the same vein, the personal pronouns of Melanesian pidgins, with their DUAL/PLURAL and INCLUSIVE/EXCLUSIVE distinctions, remind us again, as did DeGraff (2001a, 2001b, 2009) and elsewhere against McWhorter's sweeping generalizations, that the evolution of creoles' and pidgins' systems has not always been in the direction of simplification. (Good 2012 is correct in noting that such complexity can be generated by the emergent system itself, a point to which I return below.) Aboh & Smith (2009), which McWhorter cites in his column, likewise includes an informative chapter by Christine Jourdan on Solomon Islands Pijin, which shows how, consistent with the positions that creoles and pidgins have evolved (or continue to do so) gradually, verbs are derived from prepositions by adding the suffix *-im/-em/-um* to them, extending apparently the same suffix that marks a verb as transitive.

If I must be so charitable as to interpret McWhorter's claim of simplification as applying only to bound morphology, consistent with his controversial stipulation (1998) that 'prototypical creoles' have no inflectional morphemes (however, cf. DeGraff 2001a, 2001b, 2009), such simplification is not unusual in the history of languages. Contrary to the creole exceptionalism that McWhorter embraces proudly, Modern English is the outcome of a similar evolutionary trajectory (compared to Old English); so are today's Romance languages, which led Chaudenson (1992, 2001) to conclude that creoles had carried to completion processes that were already underway in the lexifiers themselves.

Before any sympathizer of McWhorter's jumps to the incorrect conclusion that this denies the agency of speakers of substrate languages in shaping creoles' structures (what, as a matter of fact, Faraclas 2012 falsely accuses both Chaudenson and Mufwene of!), Chaudenson says clearly that creoles would not be different from their non-creole kin without the agency of the populations that produced them, thus without the often (partly) congruous influence of *some* of the substrate languages. (See also Corne 1999 about this.) However, since the substrate languages were not typologically identical, we are back to competition and selection in the feature pool. Also, since some of all the substrate languages were brought to the contact settings later than others, we are back to periodization, the founder principle, and population structure, among other ecological factors to be explored. So,

the emergence of creoles needs more complex accounts than just ‘mixture’ and simplification, contrary to what McWhorter suggests in his column.

As to why Palenquero has not retained the inflections of its Spanish lexifier or of Kikongo, there are a number of ecological factors that McWhorter does not bring up in his simplistic discussion. We should remember that the name *Kikongo* is a colonial construct for a cluster of languages (e.g., Kimanyanga, Kiyombe, Kiladi, and Kifioti) which are not necessarily mutually intelligible and may display some variation regarding the canonical template of the verb complex. Typologically the agglutinating morphosyntax of Bantu languages is different from the principally inflectional system of Spanish. For instance, as also noted by Good (2012), the grammatical functions of the Spanish article are not the same as those of a noun class marker in Bantu. The latter cannot be used to articulate distinctions regarding definiteness, though it can be used to indicate number distinctions. NOUN CLASS distinctions are not quite the same as GENDER distinctions. Noun class shifts can be used creatively to express denotational changes (Mufwene 1981), whereas gender alternations in pairs such as *(le) serveur* ‘(the) waiter’ vs. *(la) serveuse* ‘(the) waitress’ or *(le) peintre* vs. *(la) peintre* ‘painter’ mark only changes in referents.³ The article can be separated from the head noun by an adjective; the noun class marker cannot. So, it cannot be taken for granted that when two languages relying on bound morphology come in contact with each other we can expect them to retain some morphology. Besides, it is a terrible mistake to assume that the language learner either acquires the target features faithfully or transfers alternatives from their own vernacular to it. Studies of foreign workers’ French by Perdue (1995) reveal that there are cases when the learners, including speakers of other Romance languages, produce nonnative features that cannot be attributed to the learners’ mother tongues.

In any case, whether or not the resulting system is simpler, the contact of Spanish with languages of the Bakongo people did not preclude competition and selection, which favored at least the Spanish lexicon and undoubtedly some aspects of its grammar, leaving it up to us to figure out why the evolution of Spanish into Palenquero took the particular trajectory that leaves little trace of Kikongo elements rather than another. Simplification does not answer this question; nor does it explain (rather than describe) what has happened. There are many whys that none of the competing approaches has not been able to answer. I keep invoking ECOLOGY as a prompt to look for an explanation and to discourage shallow analyses as proposed by McWhorter. On the other hand, we probably understand ‘explanation’ in different ways, as suggested above.

3. The system is even more complex, as one can argue that the gender is specified in the head noun but the article, just like adjectives, agrees in number and gender with the noun. The Bantu noun class prefix expresses the semantic CLASS and NUMBER specifications themselves.

The comparison McWhorter makes between Palenquero and Media Lengua is in fact poorly chosen. From an ecological perspective, the latter is an endogenous variety, comparable in some ways to Melanesian pidgins, whereas the former is an exogenous variety. The implication is that endogenous varieties are more likely to display stronger substrate influence (up to relexification?) than exogenous ones. The difference may have to do with similar nonnative influences reinforcing each other in an endogenous setting, thus exhibiting in this particular case less competition than in an exogenous setting hosting speakers from different ethnolinguistic backgrounds.

McWhorter could have invoked Berbice Dutch for that matter, with the dominant Ijo population being comparable to the Bakongo population. Berbice Dutch provides more incontrovertible evidence of substrate transfer than other Caribbean creoles, but then another question arises of why, if simplification was the ineluctable evolutionary trajectory for creoles, it did not strip Berbice Dutch of all those markers such as the nominalization suffix *je*, the cliticization of *ne* to sentence-final negation and other words, the morphologically marked transitive *-εke* vs. intransitive alternate *-ε* (Kouwenberg 1994). Or should one stipulate in this case that Berbice Dutch is not a creole, assuming (against myself) that there is some structural definition of creoles? Or, consistent with McWhorter (1998), should it be declared a non-prototypical creole?

Similar quandaries about the evolutionary trajectory of creoles' morphology have arisen before, regarding Lingala and Kikongo-Kituba in my case, when I then thought that any contact-induced language variety that had native speakers could count as a creole (Mufwene 1989b). Among several differences, the former is more tonal, retains more verb morphology, and maintains a reduced Subject-Verb concord system, while the latter has a mixed tonal and pitch-accent system, has kept derivational but only one inflectional morpheme (*áka*) in the verb complex, and has no Subject-Verb concord at all. An important observation that should encourage more research is that all evolution is local, subject to various local ecological factors, some of which we may not be aware of yet. It appears that even contacts among Bantu languages alone produced the kind of cross-system variation that is observable among Atlantic creoles. In the case of Kikongo-Kituba, one may want to factor in the fact that some Bantu languages, such as my native and ethnic Kiyansi (Bantu B85), which were involved in the relevant contacts, have no Subject-Verb agreement (Mufwene 2006). Kimanyanga itself, the lexifier, does not appear to have a consistent Subject-Verb agreement (Mufwene 1994, 1997). And we should of course not ignore the chance factor (as also acknowledged by Good 2012), which simply means that in some cases we cannot figure out the relevant explanation. I challenge McWhorter to quote a single passage from my work that corresponds to all the suggestions listed on

p. 175 of his column. Or is this another one of his unfair straw-man building style to promote his positions?⁴

McWhorter is simply disingenuous in his other comments. To wit, his discussion of my account of why the counterpart of the English preposition *for* has grammaticized into a complementizer and an obligation modal in several creoles lexified by English and French. He paraphrases me as follows:

Mufwene (2009[a]: 375) treats the usage of *for* as both a complementizer and modal in many creoles as what he terms a ‘showpiece’ of FP argumentation. That argument, typical in form of Mufwene’s FP work in general (e.g. Mufwene 2001), is enlightening in itself — but in the post-Bickertonian sense I alluded to above (p. 174).

What I actually say about how internal dynamics of the emergent language variety can influence the way in which particular grammatical characteristics evolve (quoted below) is quite different from the paraphrase (Mufwene 2009a: 375):

This explanation is undoubtedly going beyond my own position that what the new morphemes recruited to play particular grammatical functions can do and how they can do it is determined not only by their current meanings but also by their lexical categories. For example, it is more likely for speakers to exapt a preposition, which can be used predicatively, to function as a modal predicate rather than to do the same with a noun.⁵ My showpiece in Mufwene (1989[c], 1996[...], 2008) was the bifurcated evolution of the preposition *for* as a complementizer (in a way not significantly divergent from the *for-to* complementizer in nonstandard English) but also as an obligation modal in the resultant creoles, unlike in the lexifier.

The note (reproduced partially here) appears as such in my publication. The reader was provided the relevant bibliographical information to check my analysis for bifurcated evolution, against earlier unilinear claims that *for* became a complementizer first and then a modal or vice versa.⁶

4. He has constructed similar straw men elsewhere regarding my views, claiming for example that I subscribe to a ‘limited-access’-to-the-lexifier position while chastising me in the same breath of being a superstratist (McWhorter 2000). Any careful reader of McWhorter (1998) will notice that nearly all the paraphrases of my positions are at odds with the quotations that precede them. Is the author incapable of interpreting competing positions?

5. To my knowledge, most creoles, and many languages, which allow copula-less predicate phrases not headed by a verb still insert a copula when the semantic head of the predicate phrase is a noun. This makes it difficult to recruit a noun that denotes obligation or possibility to grammaticize into modal marker.

6. Michel DeGraff (p.c., 5 August 2012) has pointed out to me that Haitian Creole allows constructions such as *Jan doktè* ‘John is a doctor’ and *Martelly prezidan* ‘Martelly is president’, in

Regarding the ‘interactional pressures’ within the extant system that I alluded to, note that the grammaticization of FOR into a modal in the relevant creole is facilitated by the fact that a preposition can be used predicatively without a copula. As head of the predicate phrase it can thus be extended to a modal function, which one cannot do with a noun, at least in English creoles. While I subscribe to emergentism, I would be remiss not to state here that a number of things appear to have evolved in a well constrained way, not necessarily by Universal Grammar but by the specific typological peculiarities that the grammar of the emergent language was developing. There are some constructions that are scaffolded by others, which is typical of grammaticization, a processes that makes them derivatives of those they are extrapolations of. This is one of the cases where one need not invoke substrate influence as the primary reason why things evolved the way they did, though some substrate languages may have provided congruous reinforcement.

McWhorter relies heavily on Plag (2008a, 2008b) in his arguments against the putative “‘feature pool” hypothesis’. However, he fails to note DeGraff’s (2009: 948–958) solid refutation of Plag’s claim that Creoles emerged as ‘conventionalized interlanguages of an early stage’. DeGraff shows that Plag’s hypothesis, like many others that consider Creoles as languages in ‘arrested development’, is empirically and theoretically untenable. I myself have addressed related issues arising from the mistaken interpretation of the emergence of creoles from interlanguages in Mufwene (2010). Basically, the ecological conditions under which foreign workers in Europe produce their interlanguages are not comparable to those that produced creoles and pidgins, which emerged in geographically complementary distribution (Mufwene 2008). Moreover, increasing evidence is mounting now from trade colonization that highlights the role of interpreters in the initial contacts between Europeans and non-Europeans which disputes traditional accounts of how pidgins emerged in the first place. We should be focusing more on how interpreters learned the target language. All this will be dealt with in future work.

McWhorter also remarks (p.178): ‘We must wonder’, Mufwene (2009[a]: 393–4) briefly notes in a footnote followed by a brief passage, why creoles tend to

which bare nominal predicates do not require any copula. Thus, constructions such as the following, in which *bezwen* can be analyzed as a modal, may appear to follow the same grammatical pattern: *Li bezwen manje* ‘He/She needs to eat’ and *Machin sa a bezwen ranje* ‘This car needs fixing/to be fixed’. As pointed out by Michel DeGraff himself, ‘what we see here is yet another case of a verbal periphrasis from French (in this case, the pattern of *avoir besoin de* VERB “need to VERB” giving rise to a grammaticalized modal construction in HC, namely *bezwen* + VERB as in the afore-mentioned examples’. In an alternative and perhaps complementary analysis, the occurrence of nominal predicates without a copula would be a key factor in the reanalysis of the noun *bezwen* as a modal. (For more on nominal predicate constructions in HC, see DeGraff 1997.)

eliminate redundancy, proposing that the loss is due to unspecified ‘cross-systemic or intra-systemic interactional pressures’. He omits reference to the following passage with which the note is associated (Mufwene 2009a: 393):

If McWhorter (2001a) is correct, as it indeed seems, that creoles get rid of redundant features that are not so essential for a language to function,⁷ and since both the economic histories of the territories where these colonial vernaculars evolved and the archival records suggest that they must have evolved gradually (see in fact Baker 1995), then we must wonder how the restructuring producing the reduction of the redundant features occurred in the first place. In other words, what are the particular cross-subsystemic or intra-systemic interactional pressures that led to the reduction?

The reader should not omit the fact that this main-text passage ends with a question mark. Because I doubt that indiscriminate invocation of simplification explains everything, I definitely invite the reader to look for different, complementary explanations, without rejecting McWhorter’s position offhand, despite all the problems I have highlighted above. I certainly do not claim in the passage that I have the answer, which in itself is something that some of us should learn to acknowledge in some cases. Evolution is so complex and subject to sometimes elusive factors that one should consider simplistic accounts such as offered by McWhorter over the past several years no less critically than the more complex alternatives which, as one creolist I will not name once told me, are difficult to pin down or falsify.

McWhorter then claims on p. 179 that one of the ‘unintended consequences’ of the “‘feature pool’ hypothesis’ is the following: ‘[it] suggests a watchcry: *The proper goal of the study of creole languages is to demonstrate that there is nothing especially interesting about them*’. It is not obvious to me why standing against creole exceptionalism, with which this allegation may be associated, implies McWhorter’s conclusion. The preface to Mufwene (2001: xiv) includes the following statement:

7. This observation is not an endorsement of McWhorter’s strong position. As a matter of fact, I have in mind phenomena such as grammatical gender, Noun + Adjective or Subject + Verb agreement, and the copula, which are not discussed in McWhorter (1998). My statement is simply a recognition of the fact that at the morphosyntactic level some markers have been done away with. One must still bear in mind that, as demonstrated by DeGraff (2001a, 2001b), contrary to McWhorter’s claims, many derivational morphemes have been retained and they are used in a variety of non-transparent combinations, as would be expected of any natural language. Also, as argued above, alternative strategies have been adopted in lieu of inflections (to mark especially nominal number and tense-aspect), and, in some other cases, even new distinctions have been introduced.

The essays included in this book reflect an effort to prevent creolistics from simply being a consumer subdiscipline which espouses gratuitously, without questions asked, some still-unjustified working assumptions and theoretical models accepted in other subdisciplines of linguistics. Like any of these, creolistics should contribute to understanding Language in part by highlighting those assumptions about this peculiarity of humans which are not supported by any creole data.

The book includes a chapter titled ‘What research on the development of creoles can contribute to genetic linguistics.’ Mufwene (2008) devotes Chapter 3, titled ‘Population movements and contacts in language evolution’ to explaining how genetic creolistics, with all the tough questions and issues it is facing, can highlight some issues arising from genetic linguistics. It includes a subsection titled ‘The creole-like evolution of English and the Romance languages.’ The book also includes a chapter, 5, titled ‘What do creoles and pidgins tell us about the evolution of language?’ and another, 7, ‘Grammaticization in the development of creoles.’ Chapter 11 in the same book, titled ‘Language birth and death’, is about the contribution that genetic creolistics can make to explaining why and how languages die.

I have since then published other essays showing how a contact-based approach to the emergence of creoles that factors in periodization and population structure can be extended to shed light on how non-creole varieties of American English, which show less substrate or adstrate influence from continental European languages, emerged (see, e.g., Mufwene 2009b). This extension of my ecological approach to the emergence of creoles precludes suggesting unjustifiably, as does most of the literature on the emergence of English creoles and African American English, that Europeans are better language learners, just the opposite of what creole exceptionalism may lead one to conclude.

Now, we can disagree on how I think creolistics can contribute one way or another to linguistics at large, just like the study of any other language (group). I can also be mistaken or inaccurate on the specifics of some of my proposals, as Enoch Aboh (in 2009 and in other publications) has shown very competently regarding the level of feature recombination and language hybridization. However, it is ridiculous to construct a straw man to damn individuals who reject or choose to ignore simplistic accounts.⁸ I do not see why fighting against creole exceptionalism

8. The same is sadly true of Faraclas (2012:215), who, ignoring the pains I have taken to verify my biological approach with well-established biologists, claims that ‘The Founder Principle is embedded in an outdated paradigm of patriarchal science that is being increasingly rejected by scholars’ but cites no single biological reference to back up his allegation. As a matter of fact, his simplistic explanation of the Founder Principle reveals that he hardly ever read or understood the chapter on the subject matter in Mufwene (2001), which promotes the Cafeteria Principle, underscoring, however, the significance of ‘principle’ in this phrase to explain how substrate influence occurs and is undeniable when it is made obvious. He claims on p. 216 that ‘the historical

in the way I have done in my work should be interpreted as seeking to ‘eliminate (...) creoles as a legitimate subject of inquiry’ (McWhorter 2012: 179). Although I also participate in research areas other than creolistics, I have never denied my professional identity as a creolist.

McWhorter is concerned that those practicing the “feature pool” hypothesis ignore the ‘much more overspecification, structural elaboration, and irregularity’ that ‘older languages’ have but creoles lack (p. 179). As he should remember, he proposed the notion of ‘creole prototype’ in part to deal with the fact that creoles are not identical in regard to this statement. DeGraff (2001a) gave an extensive rebuttal of it that I need not repeat or even try to paraphrase here. Surprisingly, he does not even cite it in this column.

McWhorter also disputes in his conclusion (p. 180) the following statement of mine: ‘The extent of morphological complexity (in terms of range of distinctions) retained by a “contact language” largely reflects the morphological structures of the target language and the particular languages that it came in contact’ (Mufwene 2009a: 386).

This is indeed the conclusion I drew from reading carefully the contributions to Aboh & Smith (2009) and from my experience with the emergence of Kikongo-Kituba and Lingala, if we are willing to call all these languages creoles. There are apparently no data that McWhorter will accept to reconsider his position. Or maybe he will consider the programmatic arguments by Good (2012), who accepts creole exceptionalism nonetheless but only finds McWhorter’s position too strong. I hope that the other readers will be more considerate of the relevant evidence, historical and linguistic, complex as it is, than McWhorter is disposed to.

Finally, McWhorter implicitly elevates his contributions to creolistics to a level where our arguments must apparently be measured up to his as the state of the art, invoking a ‘post-Bickertonian sense’ (p. 174), as if Derek Bickerton were out of the picture already. Putting the scholarship in genetic creolistics in perspective, it is not obvious to me that Bickerton’s views do not matter any more, unless of course we would like to throw out the baby with the bathwater. Based on the author index of Kouwenberg & Singler (2009), he is still very much included in many of the current debates about the emergence of creoles. I think a relatively more objective history of (genetic) creolistics over the past few decades, including the suggested need to periodize individual scholars’ influences and to assess their

evidence does not support the conclusions that Mufwene draws from’ the Founder Principle, but he does not articulate the ‘historical evidence’. Then he even accuses me of denying substrate influence. He must have seen a different text from that published in the chapter on the Founder Principle. Mufwene (2010) and Chapter 8 in Mufwene (2008), titled ‘Transfer and the substrate hypothesis in creolistics’ make my position abundantly clear on the subject matter.

respective significances, will be written after most of the current creolists, including McWhorter and myself, will be very much out of the picture and unable to verify how accurate it is. Otherwise, the case of how best to account for the emergence of creoles was never closed, certainly not by the practitioners of the alleged “feature pool” hypothesis; and I doubt that it will soon.

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