REVIEW ARTICLE

Deconstructing Creole

John McWhorter

Deconstructing Creole. Edited by Umberto Ansaldo, Stephen Matthews and Lisa Lim. Amsterdam: John Benjamins, 2007. Pp. xii, 292. Hardcover EUR 115.00 / USD 155.00. [To order, visit http://www.benjamins.com]

In *Deconstructing Creole*, editors Umberto Ansaldo, Stephen Matthews and Lisa Lim assign themselves the task of combating what they regard as an erroneous strain in creole studies, i.e., the assumption that creoles constitute a distinct type of language that resulted from unusual sociohistorical circumstances.

The editors are, one senses from the scholarly yet quietly aggrieved tone of their introduction, dismayed that certain creolists have attracted considerable attention with an assumption that there was something unusual about how creole languages emerged, which resulted in creoles being less grammatically complex than older ones, languages reflecting more closely than older languages the basic core of the human language capacity. In their Introduction, Ansaldo and Matthews are committed to the alternative view that creole languages are the result of contact between languages, and simply that, and that the result is qualitatively indistinguishable from an older language and certainly no more or less complex than such languages.

To Ansaldo and Matthews, the principal question is this: why would we expect creoles to be a synchronically distinguishable class of languages? The implied answer in their introduction and some of the articles in the volume is that only colonialist biases would lead anyone to suppose that Haitian Creole is identifiably newer — and therefore, less grammatically complex — than Hungarian. More pacifically, they assume that the problem is one of insufficient familiarity with the inner workings of creole grammars, such that a closer look at same will reveal hitherto unknown complexities.

However, one could turn this epistemology upside-down and ask: why would we *not* expect creoles to be identifiable as a synchronic class? Only when technology allowed the relocation of vast numbers of persons from one continent to another did the circumstance ever arise in human history in which vast numbers of adults with a wide assortment of language backgrounds had to (1) quickly acquire

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an unfamiliar language, and (2) use their rendition of the new language as the lingua franca of a new community within a community, in which interaction with native speakers of that language was occasional and socially distant.

Certain creolists have assumed that the result under such circumstances would be a pidgin that subsequently expanded into a full language — a creole. However, this creole presumably was not yet accreted with the bells and whistles that millennia of gradual morphing leave on an old language. Ansaldo and Matthews, in contrast, assume that the result in such circumstances would evidence no "break in transmission," as the in-house jargon has it, and that the natural language created by the slaves would be one just as accreted with arbitrary complexities as Hausa or Albanian. More specifically, Ansaldo and Matthews would appear to suppose that slaves basically learned the new language quite well, merely mixing in some features from their native languages, and leaving out a just a few of the less frequently encountered marginal complexities of the new language.

Okay — but read a grammatical description of, say, the Portuguese-based creole Angolar (Maurer 1995) and then one of Portuguese itself. In doing so, one cannot escape the conclusion that Ansaldo and Matthews' assumption is counterintuitive and peculiar. Why would new languages not be less complex than old ones — given that old languages are replete with complexities that are agreed to have resulted from changes over the passage of time? Why would people creating a new language not start with a structurally more elementary one and build from there?

Ansaldo and Matthews introduce *Deconstructing Creole* as a book intended to prove a tantalizingly counterintuitive argument: that new languages arise, voilà, indistinguishable in any scientifically valid way from older languages, and that those who have argued otherwise are either naive, ambitious, or ethnocentric. I learned much from the book, but in the end, the articles in *Deconstructing Creole* are, together, one more argument in favor of the "exceptionalist" conception of creoles that so alarms the authors of the volume under review.

This unintentional confirmation of the "exceptionalist" viewpoint reveals itself first in the introduction (which is less a curtain-raiser than a piece as substantial as the other articles in the volume). Ansaldo and Matthews designate two "exceptionalist" hypotheses, Derek Bickerton's Language Bioprogram Hypothesis and my own Creole Prototype hypothesis, as conclusively refuted, but this verdict is based on what must be deemed insufficient familiarity with the creolist literature.

For example, they claim that Sarah Roberts' work on the emergence of Hawaiian Creole English has refuted Bickerton's entire Language Bioprogram Hypothesis argument. However, their impression that Roberts' work has shown that "HCE emerged gradually over a couple of generations and from a pidgin that already contained a number of features found in HCE" is a rather eccentric interpretation of Roberts' work. Roberts is a careful scholar who makes clear that Hawaiian

Creole English was not as radically clean a break from Hawaiian Pidgin English as Bickerton once claimed. However, the main thrust of the Roberts' papers is that the creole was indeed created by children, and was in no sense a mere predictable evolutionary step from the pidgin. If a second generation of children added some features to the creole, the fact remains that the first generation created something quite strikingly unlike the pidgin: they created a full language where there had first been none. To wit: on this point, Bickerton is correct, and the same process of children creating a full language has also been identified in the birth of Nicaraguan Sign Language (Kegl, Senghas and Coppola, 1996).

Ansaldo and Matthews are correct that Bickerton's idea that the creole was created by children who heard only pidgin English and had no recourse to the languages spoken natively by their immigrant parents has not held up. Roberts has shown that the children who created the creole were bilinguals who were, in addition, hearing standard English in school five days a week. Nevertheless, the fact remains: they created a creole that had not existed before, a language that no specialist in the history of the English language would view as a typical diachronic development of English grammar like the English of Manchester or Cleveland. Nor is the difference between Hawaiian Creole English and other English varieties traceable to contact with Chinese and other languages plantation workers spoke in Hawaii: the article by Siegel (2000) that Ansaldo and Matthews cite covers some possible Chinese-derived features in the language which, taken together, hardly explain the utterly stark difference between Hawaiian Creole English's grammar and that of Cockney or African-American Vernacular English. Something interesting happened in Hawaii: something "exceptional."

Ansaldo and Matthews' impression that Bickerton's Language Bioprogram Hypothesis has been vanquished entirely, and that the birth of Hawaiian Creole English was a mere matter of a new dialect emerging gradually, is not one that could be founded on a purely empirical engagement with the issue. The authors appear to have approached the issue with certain expectations already set. This is, of course, inevitable in scientific work. However, I suggest that in this case, the result can be explained by a misreading of the evidence.

This tendency to address only a partial range of relevant evidence repeats itself in the volume's various arguments to the effect that creole genesis is simply a matter of languages mixing with no remarkable simplification involved. In their view, any impression to the contrary is simply the result of the fact that the creoles often came in contact with isolating languages.

For example, as noted, in their introduction Ansaldo and Matthews also sound the death knell for my Creole Prototype hypothesis. That hypothesis argues that when a language has little or no inflection, little or no use of contrastive tone to distinguish monosyllables semantically, and little or no noncompositional

derivational morphology, it is a creole — i.e. a new language born as, and then expanded from, a pidgin state. Ansaldo and Matthews's introduction, however, points us to a forthcoming paper which will present a principle that "Creoles with clearly isolating morphology have long existed in intimate contact with isolating superstratal or adstratal languages." This is intended as an argument that creoles only exhibit what I have described as their "prototype" because their creators spoke morphologically isolating languages.

Never mind that this omits that my Creole Prototype also addresses the semantic nature of derivation, rather than just the presence or absence of bound morphemes. More to the point, the idea that creoles have isolating morphology only because their source languages do is, simply, wrong. Guinea-Bissau Creole Portuguese is the result of contact between Portuguese and various West Atlantic languages; all are well-inflected (Kihm 1989). Yet Guinea-Bissau Creole is very low on inflection. As shown by Schwegler (1996, 2000, 2002a, 2002b) and Moñino (1999, 2002, 2007a), the Spanish-based Palenquero creole was created by speakers of predominantly two languages, both of which had ample inflection: Spanish and Kikongo. Palenquero has very few inflections (Schwegler, 2007b). Yet, as Schwegler and Morton (2003: 101–106) point out, its speakers have always had regular contact with Spanish, and they have been bilingual for well over two hundred years (they also use fossilized Kikongo expressions in ritual settings [Schwegler, 1996]). Another one: the pidgin Chinook Jargon was creolized on the Grande Ronde reservation, and yet it was a typically analytic creole (Grant 1996), despite the fearsomely elaborate bound morphology in all of the Native American languages spoken by its creators. In the South Pacific, New Guinea and Australia, sister varieties Tok Pisin, Solomon Islands Pijin, Bislama and Australian "Kriol" have long been used by people speaking inflected languages natively.

Many creoles, then, have arisen from, and co-existed with, heavily inflected languages. And they did so while remaining isolating. I argue that this is because creole genesis entails radical simplification of the grammars of the source languages. It is unclear to me what counterargument Ansaldo and Matthews could present.

In the same vein, in one article in the volume Enoch Aboh and Ansaldo present a case for this conception of mixture. Their prime demonstration is that the configuration of the noun phrase in Surinam creoles can be analyzed as combining features from English and Gbe languages. They follow this with showing that Sri Lankan Malay has case markers derived from those in Sinhala and Tamil, with the implication that Sranan and Saramaccan can be analyzed as products of the same process of mixing that has yielded Sri Lankan Malay.

The problem with this analysis is that if we pull the camera lens back, so to speak, mixture alone cannot account for the difference between contact of the

languages in question. To be sure, Aboh and Ansaldo stipulate that a certain degree of grammatical elision — termed by them as "altered" replication — will occur in "a highly multilingual environment with low normative tendencies." This, for them, explains why Surinam creoles lack inflection: English is rather moderately inflected as Indo-European languages go, and Gbe languages lack it entirely. Hence in a contact situation between these languages, inflection would have been especially unlikely to survive in the new hybrid language.

However, this analysis fails for other creoles. For example, the aforementioned Gulf of Guinea Portuguese creole Angolar arose amidst contact between Portuguese and the West African languages Edo and Kimbundu. Edo is an analytic language, but Portuguese and Kimbundu are well-inflected. Based on the richer lexical contribution of Kimbundu to Angolar than to its sister creoles Sao Tomense, Principense and Annobonese, we assume that the contact situation that yielded Angolar was one in which Kimbundu was a crucial presence.

Yet verb roots in Angolar are invariantly uninflected. Portuguese has grammatical gender marking, while Kimbundu has the ample noun class marking that is typical of Bantu languages. And yet Angolar nouns are bare. Clearly, this is not the result of mere mixture — why would the Edo typology win out so consistently if the issue were a matter of a hybrid "ecology"?

Consider also the French creoles of the Indian Ocean Mascarenes, such as Mauritian. Spoken French is moderately inflected; the Bantu languages such as Makhuwa and Yao that slaves of these islands spoke are highly inflected. The result — an alternation between short and long forms of verbs derived formally from French but not functionally, and reflecting nothing of Bantu grammar at all. Is this is a hybrid of French and Bantu? Obviously not: it is a vast simplification of French structure, within which Bantu played no part at all.

"Inflection may be acquired when it is associated with some semantics," Aboh and Ansaldo mention (p. 48) in reference to these long and short forms of verbs in such French creoles. But why just this? Surely the tense and aspect-marking inflections in Bantu have "some semantics," and yet there are no such inflections in Mauritian and its sister creoles. Among the creators of Angolar, surely Portuguese's plural inflections had some semantics, not to mention the noun class markers derived from Kimbundu, which correlate roughly but robustly with real-world taxonomy.

A case based on Surinam creoles and a case of language contact between older languages such as the one that yielded a Sri Lankan-flavored Malay cannot stand as an argument about creolization in the larger sense. One cannot, in the scientific sense, characterize creolization as a process via address of a single creole or two. Surely, some consider this appropriate out of an assumption that creolization is simply a matter of the same contact processes that resulted in Romanian, Yiddish

or Sri Lankan Malay. This, however, is a mere apriori assumption. Arguments about the typological nature of creole languages can only be usefully addressed with a view towards creoles as a whole.

An additional example: Nubi Creole Arabic is low on inflection. Its creators spoke Arabic, which is very highly inflected, and assorted Nilotic languages which are also well-inflected languages. Aboh and Ansaldo's argument refers to a highly partial sampling of the relevant evidence — ironically, a charge authors in this volume often level at those adhering to the paradigm I have suggested.

Hans Den Besten's contribution to the volume shows that Cape Dutch Pidgin, modeled largely on Khoekhoe, was not a direct relexification of that language, but a "stripped" one entailing a great deal of abbreviation of Khoekhoe structures. In focusing on a pidgin rather than on a creole (which, as Den Besten stipulates, was not the direct precursor of today's creolized Khoekhoe Afrikaans), this article can only relate diagonally to the issue of the typological nature of creoles. However, to the extent that we accept that creoles can be born from pidgins, it is germane that when a pidgin is a creole's source (or one of them), it does not "simply" mirror substrate language grammar, but reduces it (a lesson most comprehensively imparted by Keesing 1988).

My arguments about the Prototype and complexity are often met with "demonstrations" that creoles do indeed exhibit complex features, along with statements about the Uniformitarian Principle, isolating typology, August Schleicher, I-language, etc. However, curiously few analysts seem to have understood that the proper way to refute my thesis is to present an older language that exhibits the same unusually moderate degree of grammatical complexity that creoles do. The metric to follow would be along the lines of the comparisons between Saramaccan and two older languages (McWhorter 2005: 48–57).[1] To put a point on it, to the extent that such a language is not presented, I consider my thesis unrefuted.

Readers of the now defunct CreoLIST may recall that in 1999 Henri Wittmann presented the Niger-Congo Mande language Soninke as a candidate for an older language with the features of the Creole Prototype. However, this is not tenable (cf. McWhorter 2005: 369, 374). The language has some inflections (complete with allomorphy) and some lexical and morphosyntactic tone. Moreover, noncompositional derivation is present so consistently across the world's older languages as to seem a universal of semantic evolution. One might even ask just why a grammar's derivation-root combinations would *not* distort semantically over time, or, rather, just what factor would maintain pristine combinatorial denotations indefinitely while they muddied eternally everywhere else in the world. Third, in this light it is

1. Or, for those who would prefer a more detailed conception of what I consider grammatical complexity to entail, see McWhorter (2007: 21–50).

indicative that Soninke's close Northern Mande relatives in the Mandekan dialect complex (i.e. Mandinka, Bambara, Dyula) have ample noncompositional derivation (viz. McWhorter 2005: 16). Why would Soninke not also exhibit it? Current documentation leaves us unenlightened, since the Soninke lexicon is at present documented merely by a word list rather than a true dictionary. There is, however, a thorough grammar of Soninke (Diagana 1995), which makes it amply clear that it is, according to the framework I have presented in various sources, an older language, replete with elaborated features clearly the result of millennia of accretion.

Other than Wittmann, the only linguists writing on the Prototype hypothesis who appear to understand the actual task involved in constructively addressing what I have written are Anthony Grant and David Gil. Both have contributed articles to the volume under review. Grant's paper is on the Chamic languages of Southeast Asia. The Chamic group consists of Austronesian languages which have lost Austronesian's battery of morphology (well-known from Tagalog and Malay) and become isolating, as the result of long-term contact with isolating Mon-Khmer languages. Grant's argument is that the Chamic languages have drifted into having the same cluster of Prototype features as creoles without any break in transmission, implying that the Prototype is merely a typology that may result from contact with isolating languages.

However, I have already presented above why that idea undergenerates the full range of creole data; and in any case, Grant does not cover the full range of what my Prototype Hypothesis stipulates. Like many analysts, Grant appears to suppose that inflection and tone are more important to cover in addressing the Prototype hypothesis than the compositionality of derivation-root combinations. He mentions very briefly, twice, that these combinations are compositional in Chamic, but does so without substantial discussion, referring only to a marker or two.

Grant's sources for the Chamic languages are mostly word lists and brief grammars. The fact is that typically only more complete grammars of languages actually address noncompositional derivation, rarely identifiable from small dictionaries. Sources of this kind are barely available for Chamic (the fine book-length study, Thurgood 1999, is not concerned with data that would shed light on the topic). As Grant's analysis is overall, he does not (possibly because at the current state of research he *could* not) tell us the extent to which derivation-root combinations are noncompositional in Chamic. This is crucial, because, as I have noted elsewhere (cf. McWhorter 2005: 16–18), there are older languages that have neither inflection nor tone, and they only reveal themselves as older according to the Prototype metric in their derivational noncompositionality. I have called special attention to this phenomenon in the Mon-Khmer language Chrau of Vietnam, for which a decent grammar does exist (Thomas, 1971). That reference work charts derivational morphemes whose meaning is so inconsistent that no single one is

gleanable. Grant has not given us any indication that the Chamic languages are not ones of this kind.

In general, Grant is especially interested in isolating typology. While this is surely a crucial issue in language contact in Southeast Asia, noncompositionality of derivation-root combinations has nothing to do with analyticity. For one, such combinations can be opaque even when derivational morphemes are free rather than bound: in Lahu, *te* is a causativizer, but with *tâʔ* "to carry" the result means "to carry along," not "to make carry" (Matisoff 1991: 432). In any case, a language could be replete with dozens of derivational bound affixes and yet, if all of them made compositional contributions to the words they created, then this would qualify as a Prototype feature.

Another noteworthy aspect of Chamic is its overall grammatical complexity. The main focus of this volume, when addressing my own work, is the Creole Prototype hypothesis rather than my more general subsequent proposal that creole languages are less grammatically complex than older languages, and in much more than degree of inflectional affixation.[2] In the end, I am more interested in the general complexity issue rather than the narrower one of whether or not there is a diagnostic test for creolization. This is the reason why I have written much more on the complexity issue than the Prototype one since 1998, and, most recently, have devoted a monograph on complexity as applied to older languages worldwide (McWhorter 2007).

In this light, Grant usefully acknowledges that the Chamic languages bear complexities of a kind that creoles do not. For example, their contact with Mon-Khmer languages has lent them numeral classifiers, which can analyzed as an incipient form of gender marking (Grinevald and Seifart 2004), and Mon-Khmer's hallmark elaborated phonemic inventories, again unknown in creoles.

Thus while Chamic parallels creoles in lack of inflectional affixation and, in some varieties, lack of contrastive tone, not only are we currently ignorant as to the extent of its derivation's noncompositionality, but we know that overall, Chamic languages display a degree of structural complexity that creoles do not, just as Mon-Khmer languages do even when lacking inflection and tone. Chrau, for example, has 39 numeral classifiers, obligatory in usage (Thomas 1971: 130–4). I propose that creoles lack these things because simplification is central to their birth. It is unclear what Ansaldo and Matthews would propose as the reason, other than serendipity.

Meanwhile, David Gil has argued in various articles that the Riau dialect of Indonesian is radically simplified compared to standard Indonesian simply by chance, such that there is no need to link this degree of simplification to non-native

2. Ansaldo (2007) also explicitly focuses his review of McWhorter (2005) on the Creole Prototype hypothesis.

acquisition and a break in transmission. I have argued (most comprehensively in McWhorter 2007: 222–234) that the history of Riau Indonesian and a comparison of it, Standard Indonesian and relatives of Indonesian in the region make it inescapable that Riau Indonesian is indeed the product of heavy use as a lingua franca by speakers of other languages — i.e. a break in ordinary transmission. In this volume, Gil takes a novel approach and argues that two of these relatives, Sundanese and Minangkabau, are as radically simplified as Riau Indonesian despite there being no evidence that they are the product of a break in transmission.

Specifically, Gil examines the freedom of interpretation of items under association. In a language like English, association is highly constrained by rules such as subjects normally preceding objects, such that if an English-speaking subject is presented with the sentence *The bird is eating* and then shown one picture showing a bird eating and the other showing a cat eating a bird, he or she will point to the former picture. However, if a Sundanese speaker is given the equivalent sentence *Manuk dahar* 'bird eat,' then they will accept both pictures as illustrating the sentence, because it could also mean "He (the cat) eats the bird." The absence of case or inflectional affixation constraining the interpretation makes this possible in Sundanese.

Testing sixteen sentences, Gil shows that of eleven languages, Minangkabau and Sundanese have the freest association according to this metric — and, by implication, parallel Riau Indonesian, whose freedom of associational semantics Gil has examined elsewhere.

However, this associational freedom is but one aspect of what grammatical complexity can consist of, and in many other areas of grammar, Minangkabau and Sundanese surpass Riau Indonesian. For example, they parallel Standard Indonesian's battery of derivational morphology complete with morphophonemic complications in their surface realization. Meanwhile, Minangkabau surpasses even Standard Indonesian in grading distance more finely and in the number of numeral classifiers (Moussay 1981). Sundanese has the same traits, as well as productive infixation, a negative existential marker, a high yogh vowel along with the typical vowel inventory of languages of its Malayo-Javanic group, and other features (Müller-Gotama 2001).

Thus I accept Gil's argument that his study shows that a language can drift into extreme simplicity in a particular grammatical feature — as well as that in this case, Minangkabau and Sundanese clearly surpass even creoles like Saramaccan. However, in that my interest is in overall complexity, it would seem that the study does not, in itself, demonstrate that Minangkabau and Sundanese have by chance drifted into the *overall* degree of simplicity of Riau Indonesian.

And in the meantime, I remain somewhat bemused at the fact that almost ten years after the Prototype article, so few have come forward with older languages

that can be shown to be as "prototypical" as creoles not just in isolating typology or tonelessness, but in compositionality of derivation, demonstrated with a modest but representative sampling of the morphemes and the meanings they create.

I sense that for many grammarians, noncompositional derivation seems rather inconsequential in falling outside of systematic, productive grammar itself. However, it turns out to be exactly the kind of thing that happens to a language's lexicon over time, and thus is every bit as crucial to a diagnosis according to my metric as inflections and tones.

On the other hand, I can not help thinking that if a linguist had found a language of this kind, surely by now it would have been brought to my attention, eagerly presented as refuting the hypothesis many find so disconcerting. Is it possible that no such languages have been encountered? If that is so, it does, of course, constitute support for my hypothesis.

Another theme in the volume is a claim that creoles are the result of unbroken transmission rather than pidginization or any substantial degree of grammatical reduction under any guise. For example, Ansaldo and two co-authors cover the emergence of Baba Malay, a creole Malay with Hokkien Chinese admixture. They note that the Chinese people who created this variety had no experience linguistically or socially that could be associated with a break in linguistic (or cultural) transmission. Their implication is that this bodes ill for the general idea that a break in transmission is a necessary component of creole genesis.

I, for one, would agree, given that I have argued that Riau Indonesian and other similarly reduced Malay varieties qualify as creoles, despite David Gil proposing them as ordinary older languages that have drifted into a uniquely grammatically streamlined state by accident. I have always suspected that a ready riposte to that argument would be that these varieties did not emerge amidst forced population displacement and stark social subordination. As such, Ansaldo and his colleagues' point about Baba Malay is well taken.

They do leave a question unanswered, however. They are striving towards a characterization of creoles as simply the product of mixture between competing grammatical features in a heterogeneous context. However, Baba Malay is not solely a mixture of Malay and Hokkien: it is a highly simplified mixture of both. Hokkien is a variety of Min Chinese, which is a strikingly complex language (despite being analytic; too much discussion of complexity focuses on affixation rather than complexity more generally). If Baba Malay were solely a mixture of Hokkien and Malay, then it would have, if not seven or eight tones like Min, then at least three or four, a healthy array of serial verb constructions, multiple semantically specific negators, multiple complementizers linked to particular shades of meaning, and so on. Meanwhile, we might also expect some reflection of Malay's rich battery of voice- and valence-marking derivational affixes (McWhorter 2007: 199).

This is not a mere guess, as there exist analogous varieties. The Western Cham variety of Chamic, a hybrid of Austronesian and Mon-Khmer languages, has five tones. Acehnese, the "black sheep" of the Chamic family spoken in Sumatra at a great distance from the others, is replete with Mon-Khmer-derived sounds and affixes modeled largely on Austronesian. As often in this book, a broader perspective on the languages in question (here, the Austronesian family and, specifically, the many varieties of vernacular Malay/Indonesian) leads to significantly different conclusions.

Baba Malay, then, not only demonstrates that creoles need not emerge under plantation-type circumstances, but also that creoles are indeed less grammatically complex than their source languages, whether this be on a plantation, in an army (Nubi Creole Arabic), in an orphanage (Unserdeutsch) or among a successful immigrant merchant class (Baba Malay).

Jeff Siegel's article does take a wide-lens perspective, as his work so consistently does. Pointedly, his conclusions are somewhat at odds with the volume's central assumptions. He makes the valuable observation that the debate over whether creoles emerge via a break in transmission of a lexifier language is actually based on a mere issue of semantics. The superstratist argument of linguists such as Robert Chaudenson and Michel DeGraff stipulates that rather than emerging from pidgins, plantation creoles were actually the end result of a gradual dilution of lexifier input as slave populations got larger. The first slaves, existing in roughly equal numbers with whites on what began as small farms, would have acquired a relatively complete rendition of the lexifier, moderated only slightly by the effects of second language acquisition. The next wave of slaves would be exposed to this moderately incomplete rendition, but the effects of second language acquisition would have made their rendition even less faithful to the lexifier itself, and so on, until the result was a creole. Hence there was no point at which there was a significant break in transmission.

Siegel notes that there were, however, documented cases in which a pidgin was the initial phase in a creole's birth, such as Hawaii and the South Seas. It is also germane that there is ample evidence that even at the small farm phase in many colonies, creole languages were already spoken. Siegel mentions the "mermaid text" from Martinique, while McWhorter (2005: 145–149) covers this and many other cases. I am unaware of a sustained engagement with these facts in superstratist work.

More importantly, the superstratists readily acknowledge that second language acquisition was central to the process in places like the French Caribbean. At this writing, it is widely agreed that the features of the Basic Variety level of acquisition described by Klein and Perdue (1997) are directly relevant to the birth of creoles. Thus while the superstratists "may be correct in saying that there was no stable

pidgin in the history of some creoles, there were clearly 'pidginized interlanguages' being used by the slaves" (p. 176) As such, as Siegel puts it, "[o]ne side [i.e., those arguing for a break in transmission] believes that normal transmission entails only the thorough acquisition of a language as a mother tongue through enculturation. The other side [i.e., superstratists] believes that normal transmission can also entail the partial learning of a language as a second language."

I, for one, am unconvinced that plantation creoles emerged as the result of what has been termed "approximations of approximations" (see McWhorter 2005: 145–149); there is, for example, no evidence that the Surinam creoles began as lightly abbreviated renditions of English, and all indication that they did not. There are no French creoles as divergent from their lexifier as Sranan and Saramaccan are from English, and so the idea that Haitian Creole is "a type of French" does make intuitive sense. However, that idea is rather starkly counterintuitive for the Surinam Creoles: few would accept that Sranan is, in any sense, a kind of English. It is a new language entirely.

Nevertheless, the nature of the sociohistorical evidence is such that there will likely never be a smoking gun confirming absolutely my view or that of the superstratists. Their objection, then, that there is no evidence of pidgins preceding plantation creoles is like treating the absence of a fossil of a creature ancestral to both humans and the great apes as confirmation that humans are not descended from primates. Primate fossils are rare and fragmentary in the relevant locations, such that the exact ones we seek may have never been preserved. In the same way, no one on early plantations was concerned with writing down the way slaves spoke — the evidence is simply not there. Our reconstructions, therefore, must be based on extrapolation from other lines of evidence, such as comparison with language contact situations and diachronic development patterns worldwide.

In that light, my conclusion is that a "French" with no grammatical gender, no inflectional affixation elsewhere, no sign of French's deeply entrenched partitive marking, bare verbs, no subjunctive, virtually no irregular verbs, no clitic climbing, and frequent zero copula displays ample hallmarks of being the product of something other than the ordinary stepwise morphing that other French varieties have been undergoing over the roughly two millennia of French's existence. Moreover, it is unclear to me that the idea that non-native acquisition in non-prescriptive circumstances simply accelerates ordinary tendencies of internal grammatical changes explains this difference. No French variety anywhere in the world, or for that matter any Romance variety, shows any signs of even being on the way to a cluster of developments like these.

However, it would appear that many will remain convinced that the "approximations of approximations" idea is valid, possibly in part because of sociological discomfort with presumed implications of the "exceptionalist" idea that the mental

sophistication of African slaves is somehow compromised. If so, the important point to remember is that the end result contrasts with the lexifier to a degree unknown in diachronic developments, even amidst language contact, outside of those entailing rapid acquisition of a language by adults for permanent use as a primary language. Moreover, the creole contrasts not solely in evidencing mix-ins from other languages. The creole contrasts in being streamlined in terms of complexity in comparison to older languages, including the lexifier that seeded it.

Joseph Farquharson's paper is in the vein of several elsewhere in that it seeks to revise the tradition of creolists supposing that creole languages have what is often termed "little or no morphology," either derivational or inflectional. I am not certain that creolists have been as dismissive of the presence of creole morphology as we are often told: after all, there are decades' worth of grammatical descriptions of creoles in which the authors, generally creolists themselves, readily document morphology. In fact, when interpreted from my own work, the conception of creoles having "little or no morphology" of any kind which seems to so arouse analysts is typically based on a reading of some passages in McWhorter (1998: 793) referring to pidgins, which, despite unremarkable traces of morphology in pidgins here and there, are based on a valid generalization few would dispute based on pidgins such as Chinook Jargon, Ndjuka-Trio Pidgin or Russenorsk. The observation has come to be cited divorced of its context, in an article in which I readily acknowledged and discussed the *derivational* morphology which readily develops even early on in *creoles*.

In this light, at the end of Farquharson's discussion, the simple fact remains that creole languages tend strongly to have little or no inflectional affixation, a factor I attributed to recency of emergence. Farquharson contributes assorted observations intended to disassociate low inflectional affixation from age, but it is unclear that they succeed.

I am afraid that evidence that a single speaker of Sri Lanka Malay is documented to have begun using a pronoun as a clitic eons after the language emerged does not constitute evidence that a community-wide grammar can develop an inflection in an instant. Meanwhile, the fact that sign languages like American Sign Language have had what can be analyzed as inflectional affixation since their birth is apparently a function of the manual modality (McWhorter 2005: 321–323). The question is which spoken pidgin language is documented to have emerged with substantional inflectional affixation, other than those spoken by speakers of closely related inflectional languages, in which case their very classification as "pidgins" has always been controversial in any case (McWhorter 2005: 157–159).

Meanwhile, Farquharson cites Alain Kihm's clever argument (2003) that under the Minimalist paradigm, free morphemes can be analyzed as "inflections" just as affixes can, the only difference being that free morphemes are generated via

Merge and the affixes by Move. As I have discussed in McWhorter (2006a), however, Kihm's analysis neglects the fact that boundedness has phonological effects that lead typically to morphophonemic alternations, changes in stress rules, and irregularities — which constitute added complexity. To suppose that there is no difference in the complexity of past marking between Saramaccan's Merge-generated "I saw," *Mi bi sí*, versus Russian's affixal Move-generated *Ja videl* neglects that past-marking "inflection" in Russian entails paradigms of affixes marking number and gender, conditioning assorted stress phenomena, in two conjugational paradigms, and amidst considerable irregularity.

Other articles in the volume propose that valid theories about creole genesis must take sociological circumstances into account more diligently. Nicholas Faraclas and several co-authors note that the different linguistic results in the Caribbean under different plantation powers cannot be reduced solely to linguistic factors, and that sociohistorical ones must also be taken into account.

It is somewhat ticklish for me to comment on this article. There has reigned an idea, based on Cuba and Puerto Rico, that the reason there are so few Spanish-based creoles was because the small-farm stage lasted so long on these islands that once slaves were brought in longer numbers, relations between blacks and whites were, while fraught, less implacably distant than on plantations in Surinam or Haiti, and that amidst this social context a very lightly Africanized Spanish itself was too well-established to be transformed by newcomers.

However, I have written a book (McWhorter 2000) arguing that there were Spanish colonies, little discussed in the creole genesis literature, in which slaves vastly outnumbered whites early on, and interracial relations were distant and brutal. I propose that these colonies — Colombia, Mexico, Venezuela, Peru, and Ecuador — contradict the idea that there was a particular Iberian-type slavery context that discouraged the formation of creoles. More to the point, under the traditional analysis, we would expect that mines and plantations in these mainland Spanish colonies would certainly have yielded creoles as removed from Spanish as Sranan is from English or Angolar is from Portuguese.

Faraclas and his co-authors' article briefly mentions my book, but does not engage with its argument, instead presenting a case which, while an extended and considered one, is essentially a continuation of ones traceable to work of the sixties and seventies by scholars such as Sidney Mintz. I can only say that Colombia, Ecuador, Venezuela, Peru and Mexico stand as challenges to the verdicts presented in Faraclas' article. I would be interested to see what he and his co-authors would conclude upon, perhaps, engaging with those colonies in the future.

Meanwhile, Roxy Harris and Ben Rampton make some suggestions for couching creole analysis within a broader contextual frame including conversational analysis and issues of cultural identity. I understand their sense that the competing

schools of thought on creole genesis, reducing the languages to some scratches on paper and positing historical reconstructions on the basis of fragmentary evidence, neglect that creoles are, in the end, systems of communication used by human beings interacting dynamically with their environment. It is beyond my purview to engage at any length with this thoroughly valid observation. However, I maintain that the systems of communication that creolophones use to express their identities and the layered renditions of consciousness that this requires do so in a code which is less heavily accreted with needless grammatical complexity than people do when expressing their identities in the Caucasus Mountains, and that this is simply because the creolophone's code is newer.

Since I first presented my ideas about the Creole Prototype and creole complexity, it has come to my attention that I needed to spell out my terms more carefully. I have attempted to do so, and at this point, my closest and most comprehensive engagement with precisely what I intend by "complexity" in creoles and in other languages is McWhorter (2006a). I have also presented further thoughts on older languages giving the superficial appearance of being "prototypical" in McWhorter (2006b) as well as a chapter on what grammatical complexity consists of in McWhorter (2007) referring to languages throughout the world.

While articles demonstrating that creole languages have complexity are always interesting, it is unclear to me that it has been demonstrated that the claim that creoles are less grammatically complex than older languages is a mere misimpression from the fact that creoles tend not to have as much inflectional affixation as European languages.

Joseph Farquharson's chapter, for example, is scholarly in tone, and yet he does announce himself as working against what he terms a "prejudice" towards viewing creoles as devoid of morphology. That word is indicative: its pure meaning is simply that of working with a preconception. However, it also has a sociohistorically rooted rhetorical flavor suggesting preconceptions of a particularly unsavory nature.

It must be observed that a statement that, for example, Saramaccan is less grammatically complex than Russian can, in the logical sense, be founded not in prejudice of any stamp, but a simple engagement with the data. It could be asserted, in fact, that to propose that there is no difference in complexity between Saramaccan and Russian grammar is, itself, one which might well involve a degree of "prejudice," here intended solely in its dictionary definition.

For example, Corne (2000: 312), after an examination of the origins of Tayo Creole French in New Caledonia, observed, with no indication of considering it a controversial or even noteworthy statement, that its grammar is "less complex than that of any given ancestral language." Creolist writings have always been replete with statements of this kind by well-respected figures on the basis of sustained

comparative analysis of creoles and their source languages (cf. Keesing 1988 as another example), and are received without complaint. Is Corne's conclusion about Tayo, readily clear from a comparison of it with its Melanesian substrate languages in discussions such as Corne (1995), founded in "prejudice" of any kind? It also bears mentioning that neither Tayo nor its substrate languages are addressed in any of the articles in this volume.

It would appear that a distinct discomfort arises at the prospect of proceeding from statements of this kind about individual creoles to a more general one — i.e. a basic logical induction — that simplification is a central aspect of creole genesis. Books could be written as the result of said discomfort, and *Deconstructing Creole* is one of those that have been. In any case, the volume under review teaches us, despite the intentions of its editors and authors, that creoles are indeed the product of the "deconstructing" of grammar followed by its reconstitution over what has so far been a brief period of time — such that signs of the deconstruction are still very much on view.

References

Ansaldo, Umberto. (2007) Review of McWhorter 2005. *Journal of Pidgin and Creole Languages*, *22*, 170–176.

Corne, Chris. (1995) A contact-induced and vernacularized language: how Melanesian is Tayo? In P. Baker (Ed.), *From contact to creole and beyond*, (pp. 121–148). London: University of Westminster Press.

———. (2000) Na pa kekan, na person: the evolution of Tayo negatives. In Jeff Siegel (Ed.), *Processes of language contact: studies from Australia and the South Pacific*, (pp. 293–317). Montreal: Fides.

Diagana, Ousmane Moussa (1995) *La langue Soninkée*. Paris: L'Harmattan.

Grant, Anthony (1996) The evolution of functional categories in Grande Ronde Chinook Jargon: ethnolinguistic and grammatical considerations. In P. Baker and A. Syea (Eds.), *Changing meanings, changing functions: papers relating to grammaticalization in creole languages*, (pp. 225–242). London: University of Westminster Press.

Grinevald, C and F. Seifart. (2004) Noun classes in African and Amazonian languages: towards a comparison. *Linguistic Typology, 8*, 243–285.

Kegl, J., Senghas, A., and Coppola, M. (1996) Creation through contact: Sign language emergence and sign language change in Nicaragua. In M. DeGraff (Ed.), Comparative grammatical change: The intersection of language acquisition, creole genesis, and diachronic syntax (pp. 179–237). Cambridge: MIT Press.

Keesing, Roger M. (1988) *Melanesian Pidgin and the Oceanic substrate*. Palo Alto, CA: Stanford University Press.

Kihm, Alain (1989) Lexical conflation as a basis for relexification. *Canadian Journal of Linguistics, 34*, 351–376.

———. (2003) Inflectional categories in creole languages. In I. Plag (Ed.), *Phonology and morphology of creole languages*, (pp. 333–363). Tübingen: Max Niemeyer.

Klein, W. and C. Perdue. (1997) The basic variety (or: Couldn't natural languages be much simpler?). *Second Language Research, 13*, 301–347.
Matisoff, James A. (1991) Areal and universal dimensions of grammaticization in Lahu. In E. Traugott and B. Heine (Eds.), *Approaches to grammaticalization*, (pp. 383–453). Vol. II. Amsterdam: John Benjamins.
Maurer, Philippe. (1995) *L'angolar: Un créole afro-portugais parlé à São Tomé*. Hamburg: Helmut Buske.
McWhorter, John H. (1998) Identifying the Creole Prototype: vindicating a typological class. *Language*, *74*, 788–818.
———. (2000) *The missing Spanish creoles*. Berkeley, CA.: University of California Press.
———. (2005) *Defining creole*. New York: Oxford University Press.
——— (2006a) What the creolist learns from Cantonese and Kabardian (Review article of *Phonology and morphology in creole languages*, ed. by I. Plag). *Diachronica, 23*, 143–184.
———. (2006b) Revisiting the Creole Prototype: Signs of antiquity in older languages. J. M. Fuller and L. L. Thornburg (Eds.), Studies in Contact Linguistics: Essays in Honor of Glenn G. Gilbert (pp. 253–268). New York: Peter Lang.
———. (2007) *Language interrupted: signs of non-native acquisition in standard language grammars*. New York: Oxford University Press.
Moñino, Y. (1999): "L'aspect en palenquero: une sémantaxe africaine", *Actances* 10. 177–190.
Moñino, Y. (2002): "Las construcciones de genitivo en palenquero: ¿una semantaxis africana?", Moñino, Y. and A. Schwegler (eds.). 227–248.
Moñino, Y. (2007). Convergencias lingüísticas iberocongolesas en palenquero: ¿integrarse a la sociedad mayoritaria o distinguirse de ella? In Mihatsch, Wiltrud and Sokol, Monika (eds.): *Language contact and language change in the Caribbean and beyond — Lenguas en contacto y cambio lingüístico en el Caribe y más allá*. Frankfurt: Peter Lang,
Moussay, G. (1981) *La langue minangkabau*. Paris: Archipel.
Schwegler, Armin (1996) *"Chi ma nkongo": lengua y rito ancestrales en El Palenque de San Basilio (Colombia)*. 2 vols. Frankfurt/Madrid: Vervuert Verlag.
———. (2000) The African vocabulary of Palenque (Colombia). Part 1: Introduction and corpus of previously undocumented Afro-Palenquerisms. *Journal of Pidgin and Creole Languages, 15*, 241–312.
———. (2002) El vocabulario africano de Palenque (Colombia). Segunda Parte: compendio de palabras (con etimologías). In Y. Moñino and A. Schwegler (Eds.), *Palenque, Cartagena y Afro-Caribe: historia y lingüística* (pp. 171–227). Tübingen: Max Niemeyer Verlag.
———. (2002) "On the (African) origins of Palenquero subject pronouns". *Diachronica, 19*, 273–332.
———. (2007) A fresh consensus in the making: Plural *MA* and bare nouns in Palenquero. In M. Baptista and J. Gueron (Eds.), *Noun Phrases in Creole Languages: A multi-faceted approach.* Amsterdam: John Benjamins.
Schwegler, Armin and Kate Green (2007) Palenquero (Creole Spanish). In J. Holm and P. Patrick (Eds.), *Comparative creole syntax: Parallel outlines of 18 Creole grammars* pp. 273–306. London: Battlebridge Publications.
Schwegler, Armin and T. Morton. (2003) Vernacular Spanish in a microcosm: *Kateyano* in El Palenque de San Basilio (Colombia). *Revista Internacional de Lingüística Iberoamericana, 1,* 97–159.
Siegel, Jeff. (2000) Substrate influence in Hawaii Creole English. *Language in Society, 29*, 197–236.

Thomas, D. D. (1971) *Chrau grammar.* Honolulu: University of Hawaii Press.
Thurgood, Graham (1999) From Ancient Cham to modern dialects: two thousand years of change. Honolulu: University Press of Hawaii.

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