

Against Creole exceptionalism (redux)*

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1. RECONSIDERING CREOLE EXCEPTIONALISM? The primary goal of my Discussion Note ‘Against Creole exceptionalism’ (*Language* 79.2:391–410, hereafter ACE) was to demystify a variety of past and present beliefs—widespread in and outside academia—according to which Creole languages constitute an exceptional class on phylogenetic and/or typological grounds.

Derek Bickerton’s reply (‘Reconsidering Creole exceptionalism’, *Language* 80.4:828–33) does not address the main empirical, theoretical, and historiographical arguments in ACE. Instead, he mistakenly reduces its scope to an ‘attack [of] two “dogmas”’ (p. 828). In essence, his response is an attempt to defend both his LANGUAGE BIOPROGRAM HYPOTHESIS (LBH) and its extrapolation to language-evolution scenarios, both of which are countered in ACE. In effect, Bickerton’s reply provides a handy example of the dogmatic nature of Creole exceptionalism as he repeats some of the very articles of faith that ACE argues against. Thus I use his reply as yet another case study of the facile intellectual practices that have made Creole exceptionalism so resilient among many, though fortunately not all, creolists.

Moreover, Bickerton criticizes what he perceives as unacknowledged shifts in my recent thinking on Creole formation; I address that criticism as well.

2. ON THE PIDGIN-TO-CREOLE LIFE CYCLE. In the LBH-based scenario, Plantation Creoles such as Haitian Creole (HC) are ‘new languages . . . formed in . . . a single generation from input that can be characterised as a jargon or early-stage pidgin with little if any grammatical structure. . . . [T]he work of new language creation can be attributed largely if not exclusively to children’ (Bickerton 1999:49–50).

Furthermore, under the LBH, since bound morphemes generally are ‘lost completely in the process of pidginization that immediately precedes creolization or (less often) assimilated by lexical items’ (Bickerton 1999:69, n. 16), the creolizing child has to create a Creole’s morphological apparatus from the Pidgin’s structureless pieces. It is thus predicted that affixes from a lexifier language such as French are not transmitted to a corresponding Creole such as HC. This claim, though, was falsified in DeGraff 2001a (see also Fattier 1998), which documents robust, stable, and fully integrated morphological resources, etymologically related to French and native to HC.¹ Moreover, work on Creole morphology in recent years (witness the various papers in Plag 2003b) reveals complex and fascinating facts that invalidate predictions that Creoles should have little or no morphology. In other words, the LBH with its morphological bottleneck is empirically untenable.

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¹ Though one might argue that HC ‘borrowed’ its affixes late, via post-creolization contact with the lexifier (i.e. via so-called ‘decreolization’), the earliest records document Creole affixes with French etymology, and the social history of colonial Haiti suggests that French speakers qua ‘affix lenders’ were most accessible to Creole speakers in the earliest stages of HC’s history. See DeGraff 2001a:84–85, 2001b:229–32, 291–94, 2002:374–83.

3. ON THE DEVELOPMENT OF HAWAIIAN CREOLE. According to Bickerton, ‘Roberts’s [archival] research . . . show[s] that a virtually structureless pidgin preceded and accompanied the emergence of [Hawai’i Creole English; hereafter HCE] . . . Significantly, ACE contains no reference to this research’ (p. 830). What Bickerton fails to note is that Roberts has shown that the LBH is categorically refuted by sociohistorical and linguistic evidence from the development of HCE—an allegedly Caribbean-like Plantation Creole that, for Bickerton, was abruptly ‘originated by children born in Hawaii in the 1890s’ (1999:68, n. 5).

Roberts (1998:35–36, 2000:288) argues against any catastrophic ‘single generation’ scenario for HCE formation; her archival data document a somewhat more ‘gradual’ development of HCE—with some ‘hitherto-unsuspected stages in [its] development’. Moreover, Roberts finds that ‘lexically and syntactically, HCE is . . . closer to English than Mauritian [Creole] is to French [its lexifier]. . . . HCE’s lexicon contains no systematic deviations from English . . . Its syntax also incorporates English reflexives, relative pronouns and other features which diverge structurally from lexifier forms in Mauritian and other Creoles’ (2000:294). Finally, Roberts, unlike Bickerton, excludes neither the role of adults nor substrate influence in the emergence of HCE (Roberts 1999, 2000, 2002). Pace Bickerton’s characterization of Roberts’s results (p. 830), she herself concludes that ‘the classic Bickertonian conception of nativization, in which children of immigrants abruptly acquire their parents’ pidgin as their mother tongue, is inconsistent with observed facts’ (2000:257).

Mufwene (2004:470–71) overviews key differences between the socio-historical matrices of the formation of Caribbean Plantation Creoles versus that of HCE, including the possibility that HCE developed not on plantations but in cities (also see Roberts 1998:34, 2000:293). Taken altogether, the above observations invalidate both the LBH-based scenario for HCE’s formation and its wholesale extrapolation to Caribbean Creoles like HC.

4. ON THE (IR)RELEVANCE OF CREOLE FORMATION FOR HUMAN-LANGUAGE EVOLUTION. According to Bickerton, ‘DeGraff’s references to evolution are irrelevant to the status of Creoles but serve to reveal his ignorance of the field. Things do not “evolve” by “cognitive processes” (ACE 398); they evolve by natural selection acting on random variation’ (p. 830, n. 2). Here I stand corrected, but so must Bickerton. His claim of irrelevance contradicts his own recent and recurrent assertions (Bickerton 1990:169–71, 181–85, 1998:354–55, Bickerton & Calvin 2000:149) that his postulated single-generation Pidgin-to-Creole cycle recapitulates the hypothetical single-step emergence of full-fledged human language from some structureless (Pidgin-like) pre-human protolanguage—‘What happened in Hawaii was a jump from protolanguage to language in a single generation’ (Bickerton 1990:171).

Nonetheless, the two phenomena—Pidgin and Creole formation among modern humans speaking modern human languages versus the emergence of human language among our (prelinguistic) hominid ancestors—are not commensurate: they share no fundamental property that warrants taking the former as a recapitulation of the latter (ACE 398–99). And if I am right that Creole formation essentially involves the same internal mechanisms that underlie language change in non-Creoles (DeGraff 1999b: 477, 484–85, 521, 528, with references), then there is no reason why Creoles, and only Creoles, should resemble the archetypal human language, pace Bickerton 1990:171, 1995:37, Bickerton & Calvin 2000:149 (also see n. 2 below).

As for my own use of the phrase ‘cognitive processes’ (ACE 398–99), I was starting from Bickerton’s own extrapolation of Creole-formation mechanisms to language evolution in the species: ‘[T]he earliest true language did THE SAME as modern creoles have done: took content words from the prior protolanguage and bleached and downgraded them, first into free grammatical morphemes, then into mere inflections’ (Bickerton & Calvin 2000:149, emphasis added). This describes ‘grammaticalization’, instantiated in diachronic developments in and outside creolization (as noted by Bickerton and Calvin, *ibid.*) and often seen as ultimately parasitic on COGNITIVE processes in modern minds/brains. Most crucially, grammaticalization is not a process of ‘natural selection acting on [genetically based] random variation’ in some population of hominids some 200,000 years ago.

5. ON THE EVOLUTION OF CREOLE EXCEPTIONALISM BEYOND ACADEMIA. According to Bickerton, ‘the quotation chosen by DeGraff to represent [Bickerton’s] views is drawn from an article by a popular journalist based on a phone interview of more than twenty years ago’ (p. 830).² However, alongside three popular-science articles, I did mention eight scholarly articles detailing the views vulgarized by the popular journalists (see e.g. ACE 398, n. 8). The quotes from magazines and newspapers document the reach of Creole exceptionalism and its popularity beyond specialized linguistic circles. In effect, these quotes illustrate modern propaganda whereby Creoles are still being caricatured, in exceptionalist fashion, as somewhat freakish languages, living replicas of the primitive languages spoken by earliest humans. It is through publications like *Newsweek*, *Discover Magazine*, and *The New York Times* that Creole exceptionalism myths become propaganda and, entering our popular consciousness, become difficult to eradicate and thus all the more dangerous for Creole speakers at large.

As one such myth goes, if Creole languages are primitive, structurally resembling the archetypal human language (Bickerton 1990, 1998, Bickerton & Calvin 2000), then they may well constitute a cognitive handicap for their speakers, who would be better off adopting a ‘superior’ language to meet the expressive requirements of the modern world. Mühleisen (2002) studies related ambivalence toward Creole languages in Creole-speaking communities and elsewhere, an ambivalence with parallels in scholarly circles: In ACE (395, 403–4) I sample supposedly scholarly views from well-known linguists who explicitly draw links between the postulated typological distinctness of Creoles and their alleged expressive inadequacy and nonviability, contra Bickerton’s statement that I deliberately confuse ‘the belief that Creoles constitute a distinct class of languages with the belief that Creoles are degenerate and inferior to other languages’ (p. 829).

6. ON ‘INSINUATING ACCUSATIONS OF RACISM’. Bickerton accuses me of ‘insinuating accusations of racism’ (p. 830). I find this bizarre: the dogmas critiqued in ACE have been held by linguists and nonlinguists, Europeans and non-Europeans, even fellow Caribbean intellectuals and Haitian compatriots, some quite prominent (see ACE 392,

² I may well have wrongly attributed the phrase ‘fossils of language’ to Bickerton rather than Sharon Begley in my quote from Begley 1982 (ACE 398). If so, I duly apologize. However, Bickerton does use the phrase (‘stealing’ it from Begley as he acknowledges; p. 830) in 1990:ch. 5 and *passim* and again in 1998:354, each time without reference to Begley. Since Bickerton has repeatedly enlisted the notion ‘fossils of language’ in his scholarly work in ways that Begley did not (e.g. referring to Pidgins, to early L1/L2 acquisitional stages, and to the utterances of trained apes; see e.g. Bickerton 1995:ch. 5), he must be held accountable for its erroneous scientific implications vis-à-vis the evolutionary status of Creoles qua alleged direct descendants of protolanguage-like Pidgins (indeed, Bickerton 1995:37 claims that ‘Creole languages stand closer to the archetypal pattern of human language than older and more established languages do’; also see Bickerton 1990:171, Bickerton & Calvin 2000:149).

n. 1; 403, n. 14). Should all these scholars, including the black scholars from the Caribbean, feel accused of racism on a par with Bickerton?

Still, one of my goals was to illustrate the popularity and continuity of certain theoretical and empirical mistakes in Creole studies. Many of these mistakes originated in early creolists' explicit, not insinuating, racial prejudices. As noted by Meijer and Muysken (1977:21), early creolists' racism put their imprint on the foundations of Creole studies as their scholarly writings often doubled as apologiae for colonialism and slavery. As stressed in ACE (397) some of these early mistakes about Creole languages' phylogeny and typology occur under new theoretical guises in contemporary creolistics even though most contemporary linguists would certainly reject early creolists' racism (as explicitly noted in ACE 397). My study of recurrent similarities across early and contemporary creolistics was meant to contribute to the intellectual history of our field. Such scholarship does NOT constitute 'insinuating accusations of racism' against individual creolists—not even against those who refuse to acknowledge racism when it is obvious. It constitutes an explicit Foucauldian analysis of textual and ideological linkages between colonial racist discourse in early creolistics and various exceptionalist hypotheses in modern creolistics.

7. A BIT OF 'REFLEXIVE SCHOLARSHIP'. Bickerton (pp. 828–29) faults ACE for not acknowledging 'dramatic changes' and 'volte-face' from 'positions DeGraff himself held until quite recently' to my 'new stance' in ACE. Alongside my 1999c edited volume, Bickerton considers my 1992 dissertation 'recent', citing it often. This unpublished dissertation was written some twelve years ago. For Bickerton, 'DeGraff had it right the first time' (pg. 829), that is, when I knew much less than the little I know now and, crucially, when I SOMEWHAT agreed with him. I stress 'somewhat' because, even in my bold and naive dissertating youth, I did express some guarded reservations regarding the LBH:

[T]he LBH cannot hold in its most radical formulation [given that] the 'Creole-creating' children were not completely insulated from non-pidgin input . . . They were exposed in varying degrees: (1) to their parents' native languages, both directly and through the pidgin spoken by the parents . . . ; (2) to forms of the target language. Thus, it should not be surprising that Creoles display a number of properties similar to those of their substrates . . . and to those of their superstrates . . . (DeGraff 1992:12)

Moreover, attempts I did make to explore the LBH were restricted to two domains of HC grammar: subject- and predication-related properties.

To clarify my post-1992 intellectual development: Bickerton's wish that ACE had explicated the changes in my theoretical outlook since my dissertation is well-taken, and some of these changes are 'reflexively' discussed in DeGraff & Walicek 2004. In ACE though, 'reflexive scholarship' was perforce macroscopic: explicitly taking aim at the course of Creole studies over centuries and across continents and not focusing on individual authors and the evolution of their writings; a microscopic analysis of the latter would surely reveal changes in views by many of the scholars discussed in ACE, including Bickerton (see e.g. Roberts 2000:290, 292).

In any case, I could not have 'had it right the first time', pace Bickerton. Since 1992, I have considered new data and better theories, for instance concerning Creole morphology (DeGraff 2001a,b, 2002) and regarding correlations between morphological and word-order changes whereby Creole-formation patterns parallel those found outside creolization (DeGraff 2004). Such patterns and their analyses have in turn motivated my shift from earlier guarded optimism about the LBH to my current anti-exceptionalist heresies.

Bickerton writes of no 'significant empirical discoveries in Creole studies, or any new linguistic evidence . . . that might have led [DeGraff] to make such changes' (p. 829). What counts as significant may be open to debate, but in the field at large, one can point, for example, to ground-breaking work on the phonology and morphology of Creoles, as in Plag 2003b. Novel insights from this research do have bearing, as Plag notes, on 'the question of how these languages and their grammars come about' (2003a:ix) and pose a challenge to exceptionalist theories of Creole formation (additional references in ACE 396, n. 6).

What of 'the moderate and open-minded approach of DeGraff 1999[c]' which Bickerton contrasts with ACE's 'single-minded crusade' (p. 828)? As with my dissertation (and Roberts's archival research), Bickerton overstates the compatibility of my position in DeGraff 1999a,b with his own. Consider for example the following caveat in DeGraff 1999a:6: 'The nativization-based view of creolization [as in the LBH] has been criticized by many. . . . Creolists' disagreement over the significance of nativization in creolization is reflected in [C]reole-genesis theories'. Hopefully the years between my editing of DeGraff 1999c and my writing of ACE have seen further progress in my understanding of Creole-formation hypotheses and their limitations, yet DeGraff 1999a,b does not stand in opposition to ACE as Bickerton described. In 1999 as in 1992, I explicitly noted contrasts between my position and Bickerton's. For example, only in the latter is a 'virtually structureless pidgin' a necessary ingredient in Creole formation. In fact, my perspective in 1999 was, at least in spirit, already Cartesian-uniformitarian (see pp. 484–85, 521, 528). This perspective is thus summarized:

[T]he notion of 'creolization' as a unitary and distinct linguistic phenomenon evaporates. . . . [C]reoles are no more and no less than the result of extraordinary external factors coupled with ordinary internal factors; that is, creoles, alongside language change, are the result of particular types of language contact whose effects on attained grammars are mediated by the contact situation's unstabilizing influence on the triggering experience. That [C]reoles might just be the (seemingly special) result of contact-induced phenomena of the type found in ordinary language change is not a new idea. (1999b:477)

8. AGAINST CREOLE EXCEPTIONALISM (REDUX). The last statement in Bickerton's reply is an apt conclusion to my own reply: 'While it is surely legitimate to question whether theories, linguistic or otherwise, have a genuinely objective basis, I hope we will continue to decide linguistic issues on the basis of linguistic evidence' (p. 832). I could not agree more, and this ending statement also reinforces my belief about the 'right' antidote against Creole exceptionalism: given the history of our field, we creolists just cannot take it for granted that we 'had it right the first time' (or even the last time); we must continually confront old dogmas with new, and old, data in order to come up with empirically and theoretically better accounts.

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