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limited number of syntactic categories: S (sentence) NP (noun phrase) and V (verb). (The last category subsumes items that correspond to verbs, adjectives, and prepositions in most Indo-
European [IE] languages.) They both operate quasi-metalinguistically, breaking into sequences of verbs complex meanings which English, for instance, conveys through single (prepositional) verbs. See, for example, Bickerton’s sentences 36 and 37–39, the Jamaican creole *Inaj fluy go a Miami* “George flew to Miami,” and the Saramaccan *go teki kom* “to fetch.” They both share the characteristic of not using a copula in some environments where this is expected in most IE languages. (This follows in part from the fact that V also subsumes adjectives in most creoles.) There are undoubtedly more parallels. For the theoretician of language universals such findings raise questions such as that of the legitimacy of many analyses proposed, for example, in early generative semantics with regard to lexical decomposition and underlying trees. Perhaps creole sentence structures could have presented some (more) real models or evidence for some of the underlying structures proposed in this TG subschool. See, for example, the classic analyses of V+ with-constructions (Lakoff 1968), *kill* (McCawley 1968) and of *persuade* (Lakoff 1970; McCawley 1971).

**LBH and other hypotheses on the origins of creoles.** LBH seems to promise answers to some basic questions that have been overlooked by, for example, the African substrate and the monogenetic hypotheses. In particular, the latter two hypotheses fail to answer the following: (1) Why has the original structure of the putative protocorele not been affected by its contacts with the lexifiers of the new (derivative) creoles? (2) Assuming that “form selection” (Gilman 1981) would have taken place even within the framework of the African substrate hypothesis, why is there a predominance of Akan/Kwa- (rather than Bantu-) related features (e.g. serialization)?

Aside from the (demographic and) contact history evidence adduced by Bickerton, the kinds of structural constraints suggested by his LBH seem to point to an answer. For example, the limitation of innate syntactic categories may favor the development of serialization over embedding or agglutination. Or, as Givón (1979) suggests, Atlantic creoles may have preserved those African linguistic features that are compatible with LBH. Likewise, assuming other supportive evidence, the monogenetic hypothesis can make more sense if it is assumed that the original structure of the protocorele has been preserved because it is the most compatible with LBH under the circumstances of the contacts with the new lexifier languages.

However, such observations do not necessarily suggest that LBH has invalidated the other hypotheses. They only present what has hitherto appeared to be a position conflict as a mere verbal dispute. For instance, Alleyne’s (1980a) second-language-acquisition hypothesis certainly needs to incorporate LBH in order to account particularly for forms and constructions that are typical of neither the substrate nor the superstrate lexifier languages. However, as it filters from parts of Bickerton’s target article, there is little doubt that the substrate languages had a role to play (particularly at the phonological level and sometimes at the morphosyntactic level) in determining the final structures of the creoles. (See, e.g., the case of Tok Pisin in the next section.) Curiously enough, the continuum of creoles suggested by Bickerton’s Figure 1 has a lot in common with that proposed by Alleyne for Caribbean Anglocroes.

Likewise, Hancock’s (1980) more moderate version of the Guinea Coast monogenesis of Atlantic Anglocroes is not invalidated by the fact that it admits LBH in order to account for the selection of the protoforms and structures. The arguments that can invalidate this version of the monogenesis hypothesis are particularly those that can demonstrate multiple, rather than single, origin. Such arguments have little to do with the alleged superiority of LBH misconceived as an alternative to monogenesis.

**Reader, beware!** Bickerton has restricted the range of creoles to those with IE colonial languages as lexifiers. Because of this, he has overlooked another important factor determining the linguistic distance between a particular creole and the model predicted by his LBH, namely the extent of dissimilarity among the languages in contact. Creoles that emerged from the exclusive contact of genetically and typologically related languages (e.g. Lingala and Kikongo-Kituba) show fewer drastic structural simplifications and modifications than those that emerged in other contexts.

Note also that Tok Pisin, for instance, has preserved a basically numeral classifier system, typical of its substrate parents, even though its lexifier (English) has a singulative system (see Schuchardt 1889 and Mühlhäusler 1980). Thus, the degree of homogeneity of the substrate languages alone is another factor that should not be overlooked in assessing the position of a creole on the scale suggested by Bickerton.

The following details must also be noted. (1) Bickerton’s claim that Saramaccan is the only (Anglo-) creole to use *fu “for”* as a main verb is disputed by the Jamaican creole *Yu (ben) fi si mi “you have/had to see me”* (Mufwene, in press). See also Gullah *Tim (bin) fo kam daun “Tim has/had to come down.”* (2) Bickerton’s basis for distinguishing between embedding and serialization with go particularly (see sentences 11–14) is rather obscure. Sentence 13 seems to illustrate mere serialization, in particular if go is to be interpreted as implicatively as he suggests. (3) Bickerton’s analyses of 37 seem to be inadequate. Wouldn’t the structure *[dee o- [3[ei jaka] 3[jioko unu]]* , with a juxtaposition rather than embedding, be more adequate? (4) Finally, what Bickerton explains by means of generic and indeterminate reference (sentences 22–23) may be more adequately accounted for in terms of nonindividuated delimitation of the noun (see Mufwene 1981). In creoles unquantified plural nouns and those delimited with *sean “one/a” are individuated, provided this semantic notion is identified not with “one” (as I first thought) but with “denumerability.” The definite–indefinite reference plays only a secondary role here.

In spite of the above objections, Bickerton deserves independent credit for adding to creole studies an orientation that can only be for the good of both linguistic theory and the understanding of creoles themselves.

**Acknowledgments**

I am grateful to Ben Blount and Charles Gilman for remarks on a draft of this commentary.

**Do creoles give insight into the human language faculty?**

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Bickerton claims that the comparative study of the creole languages, particularly of “radical” creoles such as Sranan and Saramaccan (both spoken in Surinam) provides us with a special window on the innate human language capacity. Ignoring many theoretical issues, as well as purely methodological ones (such as what is a pure or “radical” creole?), I address myself to two issues:¹

1. a. What does Bickerton claim to see through the creole window?
   
   b. What is the relation between Bickerton’s language bioprogram hypothesis (LBH) and the Chomskyan research program, directed at the understanding of “core grammar”?

A large number of linguists accept the idea that there is a nontrivial innate language faculty. The question is, What are its properties? Bickerton (1981) imputed primarily a set of four paradigmatic semantic distinctions to this faculty:

1. a. specific–nonspecific
In the target article some phrase structure rules (without the order of the elements being specified) are added, plus the assumption that only the maximally distinct categories, that is noun and verb (but not preposition and adjective) occur in the radical creoles. Given the lack of prepositions, verbs are used to mark semantic relations such as instrumental (“take”), benefactive (“give”), and directional (“go”) – hence verb serialization. There are problems of fact here, of course. Why are there no prepositions? Saramaccan itself has “perhaps only two true prepositions,” but Sranan and the other creoles have a number of them, in addition to serial verb constructions. A much more systematic survey of serial verbs versus prepositions across a number of creole languages would be needed to warrant Bickerton’s conclusion, but the initial evidence is not particularly promising.

A second, much more problematic claim is that all non-noun phrase complements must be finite clauses. This has as its implication that in (2) the serial “give” complement must be a clause as well:

2. a sutì di hagimbètì [da di wòmi]
   He shoot the jaguar give the man
   “He shot the jaguar for the man

It can be demonstrated that the notion that serial verbs would be part of finite complements is self-contradictory. It is precisely the fact that serial verbs do not head clauses with independent propositional content (which is what is usually associated with finite clauses), that makes it possible for them to function as markers of oblique arguments of other verbs. Serial verbs in this sense are much like prepositions.

If this is the case, the extended discussion of finite purposive clauses (introduced by fu, etc.) in creoles loses much of its sense. It may well be, as Bickerton’s Figure 3 demonstrates, that there is a correlation between the amount of superstrate influence that a creole language has undergone and the possibility of finite purposives (the superstrate languages lack them), but there would be no necessary connection with any bioprogram feature. Without the tensedness assumption, Bickerton’s proposal is rather empty, however, and certainly lacking sufficient “explanatory power to make it worth testing.” I return to this below.

There has been a shift, we should note, in Bickerton’s conception of the universal features of creoles. In his earlier conception (particularly 1974, 1981), the “bioprogram” features of the radical creoles do not necessarily appear in all natural languages. In fact, linguistic change could easily take a language away from the bioprogram for an extended period, until some shock (e.g. sudden language contact) would cause the natural bioprogram features to reemerge. In the target article the bioprogram phrase structure rules are seen as the base for any linguistic system, a base to which individual languages may add on rules of their own.

Although I would not presume to challenge Derek Bickerton’s findings on Hawaiian Creole, I wish to express some doubt about his extrapolation from these findings to other creoles, and about his hypotheses about creolization in general. My remarks are based on evidence from Romance creoles, which may well not be typical of creoles as a whole. As I now understand his present argument, Bickerton suggests that there can be greater or less degrees of creolization in the development of a creole mother tongue, depending on the “impoverishment” of the input to the child’s learning process. Thus in Réunion creolization is slight, because the existence of a substantial number of French native speakers at the relevant period meant there was less linguistic deprivation than in, for instance, Mauritius. Thus Réunionnais is now seen as further away from the “bioprogram,” rather than as “decreolized.”

This hypothesis is more attractive than earlier suggestions, which draw a line between dialects or patois, where there has been continuous transmission of linguistic tradition from generation to generation – and creoles, where tradition has been disrupted. Into the former category would typically fall those patois that are, or were at one time, spoken principally by the

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**NOTES**

1. Saramaccan in fact, shares certain phonological innovations in the Portuguese part of its lexicon with other Portuguese creoles, which suggests a longer, at least pidgin, history, not a sudden emergence of Saramaccan, as Bickerton suggests (Norval Smith, p.c.).

2. Neither the reduplication in Bickerton’s sentence 48 nor the tense marking in sentence 46 are by themselves conclusive evidence for the verbal status of fu. It is not impossible that the fu ... fu sequence is simply the combination of a complementizer and a modal, both being present for reasons of emphasis. In fact, regarding 46, we find many languages in which tense inflection appears on the complementizer, as is the case, for example, in English is to a large extent a contrast in tensedness.

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**Creolization or linguistic change?**

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