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, for instance, conveys through single (prepositional) verbs. See, for example, Bickerton's sentences 36 and 37–39, the Jamaican creole Jnaj flay 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 about those African linguistic features that are compatible with LBH. 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 example, 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 Anglocreoles.

Likewise, Hancock's (1980) more moderate version of the Guinea Coast monogenesis of Atlantic Anglocreoles 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[tei faka] 3[tjoko 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 tean "one/a" are individuated, provided this semantic notion is identified 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.

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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:

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

2. b. 

3. c. 

4. d. 

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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 suti di hagimbeti [da di womi]
   “He shot the jaguar give 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 “biogram” 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 biogram for an extended period, until some shock (e.g. sudden language contact) would cause the natural biogram features to reemerge. In the target article the biogram phrase structure rules are seen as the base for any linguistic system, a base to which individual languages may add on rules of various kinds. If the research program that Bickerton proposes is to have some success, this point needs to be cleared up. This brings us to the relationship between the LBH and the Chomskyan research program.

In recent years three concepts have been discussed within generative grammar that are relevant to the LBH: the core–periphery distinction, parameter theory, and the theory of markedness. Although these concepts are closely linked, I would like to discuss them separately. The core–periphery distinction is, in fact, akin to Bickerton’s earlier conception of markedness: Particular languages may develop rules that are not defined by core grammar, and that can exist inasmuch as they are immediately learnable. The notion of peripheral rules corresponds then fairly closely to the idea that languages can depart from the biogram design.

The theory of parameters constitutes an attempt to think of the process of language acquisition as a series of choices that a child goes through. The choices are innately specified; the particular options have to be fixed by the input data. One of the primary parameters in X-bar theory, which allows the child to construct trees defining the structures of the language and captures the parallels between the individual constituents. The biogram phrase structure grammar proposed by Bickerton is nothing more than a fairly standard version of X-bar theory. The theory of markedness relates to the different options of parameter theory. In a number of cases, one of the two choices is assumed to be the unmarked one; a child learning a language without adequate input would then pick all the unmarked choices, leading to a minimally marked biogram language. Markedness within the categorial system is an example. A linguistic system with two categories needs only one feature. If we take ±N as this feature, we can set up nouns as + N, and verbs as − N. In addition, prepositions and verbs will be nondistinct, since prepositions are nonnominal as well. This version of markedness would lead to the prediction that a language with only the feature ±N can use prepositions and verbs in the same way, rather than not having any prepositions.

Bickerton’s fairly concrete biogram program grammar does not contain “complex innate schemata” such as the binding theory (Chomsky 1981a), but that makes it hard to explain how language is acquired. It is precisely these more abstract features for which it is impossible to construct a learning theory without a complex innate component. Unlike the position he took in 1981 (e.g. p. 298) Bickerton here wants to deny the need for such a theory.

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 one would expect if the complementizer is a tense operator in logical form (see Stowell 1981). In fact, the contrast between that and for in English is to a large extent a contrast in tensedness.

Creolization or linguistic change?

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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 “impoveryment” 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 “biogram,” 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