One of the major shortcomings of the LBH as put forward by Bickerton is that it makes no clear distinction between form on the one hand and function and meaning on the other in the development of creoles and language generally. There are across creoles similarities in form, function, and meaning. Many of those who prefer substratum arguments are following the path of historical linguistics and are establishing historical connections between languages by directly relating phonological shapes and syntactic structures to meaning; it is the phonological shape that is crucial. The LBH does not address itself directly to this point.

Chomsky's major formulations, which Bickerton claims to be in accordance with, deal with abstract properties. Bickerton's LBH, by dealing with specific structures and phonological shapes, increasingly seems to present a challenge to the arbitrariness of the linguistic sign. For example, in dealing with *fu* (and its phonological variants) across creoles, the LBH gives the impression that the phonological shape itself is a part of the LBH. Serial verb constructions are said to "follow inevitably from the limitations of the hypothesized grammar," but serial verbs must be related to conceptualisation and segmentation of experience which must people syntactic categories. Even if one accepts the LBH at an abstract level, one still has to account for similarity of phonological shape and semantic form (e.g. *give*, *ba*, *da*) across creoles. They must have had a prior existence tied to some specific function. Bickerton's attack on "substratophiles" is therefore clearly overdone.

Another shortcoming is the lack of explanation of the creation of the creole as the language of the community. Whereas in dealing with the normal situation Bickerton proffers an explanation for movement from LBH language to normal target language, he does not explain in the pidgin to creole situation how the LBH language of each individual child coalesced into a dialect. For even if one argues that each individual's language is different, one still has to account for the idea of quick crystallisation as is claimed for Sranan.

In this connection Bickerton makes two statements that lead to greater indeterminacy in the development of the language of the community. First he says, "what is here referred to as the bioprogram grammar would simply constitute the list of preferred settings that the child, in the absence of contrary evidence, would assume to be appropriate." This suggests a relationship between innate factors and external factors in which the external ones determine which innate factors should be employed. Second he says, "Thus ordering in Saramaccan could have been determined by pragmatic factors or by an accidental preponderance of one order in the pidgin." It is not clear how pragmatic factors can influence word order. In addition, if the pidgin is a "language" used by adults, a better explanation of "accidental preponderance" is that order which is most common in the native languages of the people involved in the contact situation.

Bickerton is at pains to show that creole similarities are deep, extensive, and not random. To do this he chooses one major item, *fu*, which he also used to exemplify his theory in Bickerton (1973). Note, however, that the phonological shape *fu* and its equivalents in other creoles can be shown to be lexical items of European derivation and that Bickerton's property 55 is in keeping with the use of the equivalents of these items in European languages. In other words, to Figure 3 could be added a column 5 with the names English, French, Spanish, and so on. It is therefore not clear how this single item whose use is not distinct from that of European languages to which creoles are known to be related establishes a special class, creoles. Bickerton also mentions "four basic binary semantic distinctions that appear to be shared by almost all creoles." Bickerton's word choice here, his sketchy treatment of the distinctions, and the known lack of consistency in these distinctions across creoles do not prove his case.

Finally, in the case of Barbadian speech – which Cassidy

(1980) claims was a creole and Hancock (1980) claims was not, but a dialect of English instead – how does one account for the continued presence of these isolated bioprogramlike instances of *fa* and *ta*:

<i>i fa ta pat it de</i>	"he is (supposed) to put it there"
<i>wa fa zy ta ke</i>	"why (= what for?) should I care?"
<i>ef zy did nuo, zy wan fa tel ya</i>	"If I knew, I wouldn't tell you"

In short, Bickerton does not account for similarities of form across creoles, does not explain how the creole as language of the community develops from LBH language, and does not demonstrate similarities across creoles, which is the *raison d'être* for his theory.

Socioprogrammed linguistics

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On this occasion I am concerned less with whether or not Bickerton can argue successfully for innate "cognitive devices" and a "processing component" as fundamental human endowments than with whether evidence for the hypothesis is found in the so-called creoles. Alternatively, one can assess Bickerton's argument entirely with respect to its ability to explain the ostensible similarities in these languages, since he claims that his "is one, and so far the only, explanation." On the one hand is a single Hawaiian "creole" and on the other a whole group of languages whose speakers have an African ancestry. The sets are presented as having self-evident integrity, but they remain highly problematic. (Bickerton has preempted the word creole, for what he calls "true creoles," misleading talking of "all creoles" when he means only the languages he uses.) The question lies not with the linguistic data, the contemporary forms of which are of easy access, but with their sociohistorical interpretation.

Taking his Hawaiian data, Bickerton assumes a "pidgin" that is degenerate and anarchic as the only antecedent to his Hawaiian Creole. (Others use the pidgin for a language with a stable grammatical system.) While attempting elsewhere to isolate these putative idioms chronologically and evolutionarily, he cannot be said to have made an altogether convincing case. No one – not even Reinecke (1969) – has given proper attention to the linguistic aspects and consequences of the role that the Sandwich (now Hawaiian) Islands played in Pacific trade during the 18th and 19th centuries. In 1842, for example, there were reported to have been 500 Hawaiians in the service of the Hudson's Bay Company west of the Rockies and "Chinooks" (presumably Indians from the Colombia River area) in Hawaii (Society 1956, pp. 105, 133, 137; also Samarin, in preparation). For his own period (1876–1920) Bickerton has not explained (a) how the "extremely rudimentary" idiom was used and (b) whether or not and to what degree its speakers interacted with the speakers of the coexisting creole. Without such sociolinguistic identification his pidgin and creole must remain artifacts of his analysis.

The Atlantic creoles figure more prominently in Bickerton's argument than the Hawaiian although they present greater problems in historiography. He seems to assume that each of the creoles had its own antecedent "pidgin," ignoring the possibility (indeed, probability) of considerable contact between speakers of many different varieties of New World speech. He has not demonstrated that these idioms were formed in the 1650s, nor that they arose very early in their history. Therefore (and for other reasons deducible from Bickerton's argument), the claim that a "fit" has been established between the distribution of linguistic features and "the circumstances under which individual creoles arose" is a specious one. In other words, Bicker-

ton has failed to "provide evidence that the right speakers were in the right places at the right times" for these languages to have developed – a charge he lays against those who would account for the transmission of certain features from African languages. Reversing Figure 3 from right to left suggests a different and quite defensible historical development.

Elsewhere Bickerton's argument is as simplistic as it is in some respects deficient. It is simply not true that "at the beginning of colonization in any colony, dominant-language speakers probably always outnumbered speakers of substratum languages." My detailed documentation of the personnel involved in the colonization of the Congo and Ubangi river basins in the last quarter of the 19th century (work in progress) reveals Europeans to have been vastly outnumbered by their foreign African workers. Even if Bickerton means only "plantation" systems, a distinction which he once invoked (in a personal communication) to dismiss an argument I put forward (Samarin 1980), he would have to characterize the contexts where the new idioms emerged. What he is desperately looking for, of course, is something parallel to what he believes he has discovered in Hawaii. Extreme multilingualism is what he needs, and the absence of viable language targets.

He simply asserts that "hundreds, perhaps thousands of different languages" were involved; and "perhaps as many as a thousand typologically diverse" languages. This could not possibly mean that this many languages were involved in each of the creole's evolution. The assertion is patently hyperbolic. A reasonable number of languages for *all* of Africa – the last I heard – was 2,000.

Baker's demographic analysis (Baker & Corne 1982) – just the kind that is needed for the explanation of the origins of pidgins and creoles – would seem to provide Bickerton with precious data, but it must be remembered that Baker tells the story of only one language on one island, which was integrated into much smaller socioeconomic system. The so-called pidginization index derived by Bickerton is only a metaphor of a measure, since it suggests but does not give information. It is, moreover, at best a gross metaphor. The number of speakers of the European language proportionate to all others at a given time (Figure 2) is only one factor in language contact. One must consider frequency and nature of contact, among others. There is no *a priori* reason why a few persons could not have had an impact disproportionate to their numbers; indeed, that is the more typical situation.

Inexplicably omitted from the index are the children, protagonists – if not *deus ex machina* – of the drama of language creation. How, we ask, did children in 28 years – a figure that Corne deduces from Baker's data (Baker & Corne 1982, pp. 120, 248) – produce Mauritian Creole? The answer must come from analyses that demographers and human geographers might provide with respect to fertility and mortality, with due regard to epidemiology, in a slave-plantation economy. (And Baker's analysis is itself subject to critical interpretation.)

Even where Bickerton might have been able to provide a convincing analysis of the Hawaiian infant speech community, he has not done so. He has only now admitted, under cross-examination, so to speak, that these children *could* have had available to them fully developed languages. Then he makes the astounding assertion that, in any case, when children are acquiring a pidgin as their first language, they cannot make use of normal language material. (Clever debater that Bickerton is, he makes his opponent responsible for proving the contrary!) He has yet to cope with the factor of variability. Not all children heard the same pidgin: Some must certainly have been more developed than other forms.

Bickerton's argument is not strengthened by his *disbelief* in attempts to explain Atlantic creole grammars in terms of African origins. His refusal to consider this possibility is an arbitrary one: requiring the comparison of "whole systems." Why Alleyne's comparisons (1980a) are discounted is not made clear.

(What makes Alleyne's work inadequate is that it is vague about both African linguistic sources for creole features and about the socioeconomic context of creolization.) As an Africanist, I find much – even in the geographically remote Gbeya language (Samarin 1966) – that is too similar to be purely accidental.

Bickerton's assertion that implicit in what one might call the Africanist point of view is the belief that "languages can be made by throwing together a heterogeneous set of items" is indefensible. I do not know any contemporary linguist who would take this position. Nonetheless, it should be admitted that *parts* of a pidgin, possibly important parts, can be absorbed from languages other than those that figure most importantly, as is the case in Sango of the negative (from "stray" speakers of Mongo) and the copula (apparently from Kongo) (Samarin 1982; 1984).

Whatever the merits of Bickerton's hypothesis for advancing our understanding of the neurological bases for language, it certainly has not advanced the explanation of how and why pidgins and creoles emerge. If language is said to be a consequence of adaptation on the part of human beings, that adaptation ineluctably includes the function language has in making possible human interaction. There is that "program" too that makes homo sapiens a social creature.

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Do creoles prove what "ordinary" languages don't?

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The language bioprogram hypothesis was originally put forward by Chomsky as verifiable on the basis of facts about "ordinary," familiar languages. Some scholars were convinced by the argument, but others were not. Later it became fashionable to claim instead that the hypothesis was verifiable by reference to facts about the speech of young children and about the linguistic data available to them, a category of evidence systematic examination of which is less easy for the average linguistics scholar, one of Chomsky's recent interpreters has gone so far as to state that comparison of adult languages plays no part in establishing the truth of the bioprogram hypothesis (Lightfoot 1981:165). It remained true that a number of scholars found no difficulty in reconciling the evidence cited with disbelief in the bioprogram hypothesis. Now Bickerton argues that, irrespective of the facts about adult or child use of the kinds of language familiar to most academics, we must accept the hypothesis in view of observations that can be made only in situations which, for most of his academic audience, are geographically and socially about as remote and difficult to check independently as any on earth. Logically it is quite possible that Chomsky's remarkable linguistic bioprogram hypothesis might be true, and yet that satisfactory evidence was available only on the Hawaiian islands; but Bickerton must be prepared to meet a high level of scepticism on the part of readers who remember when, not long ago, the hypothesis was said to be verifiable from evidence that surrounded them every day of their lives.

One of my difficulties in assessing Bickerton's argument for the hypothesis stems from the fact that (as Bickerton notes in Section 6.0) the hypothesis itself has mutated into a form that