THE BEHAVIORAL AND BRAIN SCIENCES (1984) 7, 173–221
Printed in the United States of America

The language bioprogram hypothesis

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Abstract: It is hypothesized that creole languages are largely invented by children and show fundamental similarities, which derive from a biological program for language. The structures of Hawaiian Pidgin and Hawaiian Creole are contrasted, and evidence is provided to show that the latter derived from the former in a single generation. A realistic model of the processes of creole formation shows how several specific historical and demographic factors interacted to restrict, in varying degrees, the access of pidgin speakers to the dominant language, and hence the nature of input to the children of those speakers. It is shown that the resulting similarities of creole languages derive from a single grammar with a restricted list of categories and operations. However, grammars of individual creoles will differ from this grammar to a varying extent: The degree of difference will correlate very closely with the quantity of dominant-language input, which in turn is controlled by extralinguistic factors. Alternative explanations of the above phenomena are surveyed, in particular, substratum theory and monogenesis: Both are found inadequate to account for the facts. Primary acquisition is examined in light of the general hypothesis, and it is suggested that the bioprogram provides a skeletal model of language which the child can then readily convert into the target language. Cases of systematic error and precocious learning provide indirect support for the hypothesis. Some conjectures are made concerning the evolutionary origins of the bioprogram and what study of creoles and related topics might reveal about language origins.

Keywords: child language; creole; evolution; glottogenesis; grammar; language; language acquisition; language origins; learnability; linguistic universals; pidgin; psycholinguistics

1.0 The basic hypothesis

A central issue with respect to human language, and one that is far from resolved, concerns the extent and specificity of the mechanisms that underlie it. At one extreme lie the views of Chomsky [see "Rules and Representations," *BBS* 3(1) 1980] and his associates, who posit a "mental organ" that is as modular and as functionally specialized as the human heart or lungs. At the other extreme lie those of many empiricists who hold that the human mind is a general-purpose problem-solving device, no particular portion of which is specifically devoted to language. For the past two decades at least, debate has ranged over fairly well known territory: the extent to which language is like or unlike other objects of human learning, the role of input in the normal acquisition of language by the child, the ontological status of innate knowledge, and so forth. In the sections that follow, I examine evidence from a relatively little known area of human language which clearly bears on this issue; I argue in favor of a language bioprogram hypothesis (henceforth LBH) that suggests that the infrastructure of language is specified at least as narrowly as Chomsky has claimed.

The languages to be examined are known as creoles, which in turn have derived from pidgin languages. A pidgin is an auxiliary language that arises when speakers of several mutually unintelligible languages are in close contact; by definition, it has no native speakers. A creole comes into existence when children acquire a pidgin as their native language; theoretically this process can occur at any stage in a pidgin's history, but for reasons that will become apparent, we shall be dealing only with creoles that have come into existence very early in the development of their antecedent pidgins. It has long been recognized by creolists that creoles somehow "expand" and render more complex the pidgin grammar that precedes them (Hall 1966), but until recently there was no clear picture of what constituted this expansion, and no indication as to how the expansion was achieved. The LBH claims that the innovative aspects of creole grammar are inventions on the part of the first generation of children who have a pidgin as their linguistic input, rather than features transmitted from preexisting languages. The LBH claims, further, that such inventions show a degree of similarity, across wide variations in linguistic background, that is too great to be attributed to chance. Finally, the LBH claims that the most cogent explanation of this similarity is that it derives from the structure of a species-specific program for language, genetically coded and expressed, in ways still largely mysterious, in the structures and modes of operation of the human brain.

This general argument, at varying levels of specificity and detail, has been developed in previous publications (Bickerton 1974; 1977; 1979; and especially 1981). However, since the appearance of these versions, there have been a number of developments that permit a more sharply focused and explicit presentation of the LBH. These developments include studies by Philip Baker on

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the origins of Mauritius Creole (Baker 1976; 1982; Baker & Corne 1982), studies of Haitian syntax by Claire Lefebvre and her associates (Lefebvre, Magloire-Holly & Piou 1982), extensive fieldwork on Saramaccan syntax by Frank Byrne (Byrne 1982; forthcoming), and an ongoing longitudinal study of the linguistic development of a blind two-year-old by Robert Wilson.

2.0 The arguments for invention

In order to support the LBH it is necessary to show that all, or at least a substantial part, of the grammar of a language can be produced in the absence of the generation-to-generation transmission of particular languages that is a normal characteristic of our species. Note that although evidence for this point would still fall short of demonstrating the LBH as a whole, failure to find support for it would be fatal to the hypothesis. It is not necessary, however, to demonstrate that the LBH specifies the *only* means through which novel linguistic structures can arise, as some contributors to Hill's (1979) volume seem to have assumed.

Creole languages arise where large numbers of people speaking mutually unintelligible languages are forced to associate on a permanent basis but have no preexisting language in common. Such conditions were produced par excellence by European colonialism in the period 1500–1900, and were most recently satisfied in Hawaii in the period 1876–1920. Hawaii is therefore the only place in the world where it is still possible to study the linguistic phenomena produced under these conditions by direct examination of surviving speakers from the relevant period.

In evaluating the Hawaii evidence, one caveat must be borne in mind. This evidence is drawn from a study made during 1973–75, of speakers then in their seventies, eighties, or even (in a couple of cases) nineties. It can be interpreted as evidence for what happened in the period 1900–1920 only on the assumption that the speech of individuals does not change appreciably after adulthood is reached. Since this assumption, although commonplace in linguistics, may not obtain in the unusual conditions dealt with here, we need to see how it might fail and what the consequences of such failure might be.

Take first the speech of pidgin speakers, which will be shown to be extremely rudimentary in structure. In theory there are three possibilities: that their speech in the period 1900–1920 was *more* rudimentary, *less* rudimentary, or *equally* rudimentary. For the present argument, the first possibility constitutes even better evidence than the third – it would mean that the linguistic deficit that had to be made up by first-generation creole speakers was even greater than will be suggested here. The second possibility is highly unlikely. The longer people interact, the more the quality of their linguistic interaction improves. Deterioration of language skills can occur only through extreme isolation or mental deterioration. None of our subjects was a social isolate, however, and all, even the oldest, seemed in full possession of their mental faculties. The second possibility may therefore be disregarded.

In the case of creole speakers, there are two possibilities. The structures that distinguish them from pidgin speakers were acquired either in childhood or in adulthood. If creole speakers acquired the structures as adults, they must have done so by means of acquisitional processes equally available to pidgin speakers, who were also adults. Yet we find structures that are shared by all locally born (i.e. creole) speakers and no immigrant (i.e pidgin) speakers. Such structures can only have been acquired by processes inaccessible to pidgin speakers. The only plausible candidates for such processes are the ones peculiar to children, which, according to the so-called critical period hypothesis (Krashen 1973; Lenneberg 1967; Scovell 1981), are inaccessible to adults. Therefore, forms unique to locally born speakers must have been acquired by them as children.

Given that the present speech of pidgin speakers is as rudimentary as (or less rudimentary than) their speech shortly after arrival in Hawaii, we can illustrate the nature of 1900–1920 pidgin by examples drawn from speakers who arrived in Hawaii during that period (in each case the speaker's native language and date of arrival are given in parentheses). Even the lexicon of this pidgin was highly unstable, as (1)–(3) show:

1. kote, motete, awl frend giv, no? (Japanese, 1918).
   buy, take-back, all friend give, INTERROGATIVE.
   "[They] buy [presents], take [them] back, and give [them] to all their friends, don't they?"
2. insai lepo aen hanapa aen blaenket, pau (Visayan, 1916).
   inside dirt and cover and blanket, finish
   "[They put the body] in the ground and cover [it with a] blanket, that's all."
3. mi onli chachi-chachi go palei, tarin gonon naega bisanis ani (Korean, 1916).
   I only church go pray, other things I business is-not
   "I just went to church to pray; other things were not my business."

(The non-English lexical items are Japanese in [1], Hawaiian in [2] – a form of pidginized Hawaiian was the most widespread means of communication prior to 1900; see Bickerton & Wilson (1984) – and Korean in [3].

Examples such as (1)–(3) have no recognizable syntax. What fragments of syntax can be recognized are heavily influenced by native-language grammar; thus, in (4), objects precede verbs, as in Japanese, whereas in (5), the verb precedes the subject, as in Ilocano:

4. mi kape bai, mi chaek meik (Japanese, 1907)
   me coffee buy, me check make
   "He bought my coffee; he made me out a check."
5. en den meri dis wan (Ilocano, 1918)
   and then marry this one
   "And then he got married."

However, such influence is far from consistent: The speaker who produced (4) also produced (6):

6. baimbai wi bai eka yo, 2,500 bai, foa eka bai, laend (Japanese, 1907).
   Later we buy acre EMPHASIS, 2,500 buy, four acre buy, land
   "Later we bought four acres of land for $2,500."

Indeed the restrictions on expression imposed by the grammatical limitations of the pidgin can only be fully

appreciated by more extended citation of some quite typical utterances:

7. bilding – hai pleis – wal pat – taim – nautaim – aen den – nau tempicha eri taim sho yu (Japanese, 1913)
   "There was an electric sign high up on the wall of the building which showed you what time and temperature it was."
8. gud, dis wan. kaukau enikain dis wan. pilipin ailaen no gud. no mo mani (Ilocano, 1913).
   "It's better here then in the Philippines – here you can get all kinds of food – but over there there isn't any money [to buy food with]."

Pidgin speakers lacked the resources that language normally employs in the expression of complex propositions; as examples (1)–(8) suggest, they had no consistent means of marking tense, aspect, or modality; no consistent system of anaphora (compare the zero subject anaphora of [4] with the stereotypic use of *dis wan* as an all-purpose pronoun for *he, here,* and so on in [5] and [8]); no structure more complex than the single clause (hence no embedding of one sentence within another, whether the embedded sentence be relative clause or sentential complement); and no systematic means for distinguishing case relations. In consequence, parsing of pidgin has to be based almost exclusively on semantics and pragmatics.

Language at this level of degeneracy must have constituted a major part of the input to children learning language around 1900–1920. Bruner and Feldman (1982) are right in pointing out that input from a variety of fully developed human languages was also potentially available. However, as is shown later in this article, there is no evidence that children, in acquiring a pidgin natively, are able to use the latter kind of input, and much evidence that they cannot, or perhaps do not need to.

Input of the type of (1)–(8) presents children with two distinct problems. The first arises from the variability of the data. One might suppose that variable word order would pose no greater problem than is presented by so-called nonconfigurational (free word order) languages (Hale 1978). However, such languages invariably have mechanisms (e.g., pre- or postpositions, case inflections, etc.) for unambiguous marking of case roles; Hawaiian pidgin had none of these.

The second and more severe problem arises from the lack of models for complex structures. The examples that follow represent types of sentences produced by Hawaiian creole speakers who grew up in the period 1900–1920 (in each case the speaker's birth date is given in parentheses). No examples of these sentence types have been found among the immigrant speakers who arrived during that period.

9. dei gon get naif pok you (1896)
   "They will stab you with a knife."
10. dei wawk fit go skul (1902)
    "They went to school on foot."

Here, nouns in the Instrumental and Manner cases are introduced by verbs (*get* and *walk* respectively) rather than by prepositions, as in English. The importance of this type of structure, known as verb serialization, will become apparent as we proceed further.

11. dei kam in da mawning taim go skul (1902)
    "They came to school in the morning."
12. da frs jaepani keim ran awei fram jaepan kam (1896)
    "The first Japanese who arrived ran away from Japan to here."

Note the use of verbs of motion: *go* to mark Locative Case in (11), *kam* ("come") to serve as a directional adverbial ("to here") in (12). These examples illustrate further types of verb serialization.

13. dei wen go ap dea in da mawning go plaen (1896)
    "They went up there in the morning to plant [things]."
14. pipl no laik tekam fo go wok (1902)
    "People don't want to have him go to work [for them]."

Examples (13) and (14) show, first, a systematic means for embedding sentential complements (by means of verb phrases following *go* and *fo*) and, second, a means of distinguishing between accomplished and unaccomplished actions. The "planting" of (13), an action that actually took place, as the context of the sentence makes clear, is marked by *go*, whereas the "going to work" of (14), an action that did not take place, is marked by *fo*. Neither process has any antecedent in the pidgin, and the second has no antecedent in English either.

15. samtaim dei stei kam araun, polis (1900)
    "Sometimes the police used to come around."
16. wan taim wen wi go hom inna nait dis ting stei flai ap (1902)
    "Once when we went home at night this thing was flying about."

Without *stei*, (15) would be self-contradictory – "Sometimes the police came around once" – while (16) comes from a description of ball lightning, an inherently durative phenomenon. Use of *stei* as a marker of nonpunctual (durative or iterative) aspect is another creole innovation; no means of marking aspect of any kind exists in the pidgin.

With one exception, examples (9)–(16) involve the use of full verbs (or forms derived from full verbs) to discharge functions that in English are discharged by prepositions, adverbs, complementizers, or auxiliaries. The exception (*fo* in [14] is closely related to a form that is shown below to be fully verbal in Saramaccan, a creole closer to the bioprogram than Hawaiian Creole. This use of verbs differs strikingly from the common pidgin strategy of using strings of mainly nominal constituents with few or no verbs (cf. [7] and [8]).

17. sam filipino wok ohia dei wen kapl yiaz in filipin ailaen (1896)
    "Some Filipinos who worked over here went back to the Philippines for a couple of years."
18. wan dei haed pleni av dis mauntin fish kam daun (1897)
    "One day there were a lot of these fish from the mountains that came down [the river]."

Here we find fully developed relative clauses – a feature absent from the pidgin of 1900–1920 – but clauses that differ from their English equivalents in that (a) they lack relative pronouns, which are nondeletable in the English equivalents, and (b) where the head noun of the clause is indefinite in reference ("Some Filipinos"), a pronoun

identical in number to the head noun (*dei*) obligatorily follows the relative clause.

19. oni vizit orait ai laik – fo liv ai no laik (1900)
"I'd like it all right *just to visit* – I wouldn't like *to live there.*"
20. enikain laengwij ai no kaen spik gud (1896)
"I can't speak *any kind of language* well."
21. es wan ting baed dakain go futbawl (1902)
"That going [to play] football stuff *is a bad thing.*"

In the pidgin, instability of word order meant that there was no unambiguous way of marking contrast or emphasis by constituent movement. In the creole, with a stable basic subject–verb–object order, there developed a consistent rule of focusing by leftward movement, applicable to complements (19), object nouns (20), or predicates (21).

22. ais krim kon stil faiv sens, ei? (1905)
"Ice cream cones still [cost] five cents, didn't they?"
23. In doz deiz da tichaz get lawng stik (1896)
"In those days the teachers had long sticks."

Although from the beginning the creole had definite articles (*da* in [23]), indefinite articles (*wan* in [21]), and pluralization (*tichaz* in [23]), articles and pluralizers never cooccur with nouns that lack a specific referent, whether they are generic (*ais krim kon* in [22]) or merely of indeterminate reference (*lawng stik* in [23]).

Although restrictions on space preclude a detailed analysis of the data, the contrast between (1)–(8) and (9)–(23) should suffice to show profound differences between the languages of the immigrant and locally born groups. When recorded, members of both groups were distributed across the age range 70–95, with the majority in the lower half of that range. The only variable that distinguished the groups was that of age at first exposure to reduced forms of English. Those exposed in childhood far surpassed those exposed in adulthood in the range and variety of syntactic structures that they controlled.

Thus we can conclude that the deficit between pidgin and creole was filled in a single generation by the first locally born group to be exposed to pidginized English. However, the deficit was not filled only, or even mainly, by the acquisition of English structures. Not one of examples (9)–(23) is grammatical in English, and in most cases the rules required to generate them differ markedly from English rules. The obvious question – whether the necessary rules could have been derived from languages other than English – is discussed in Section 4.0. For the present, we may simply note that the likelihood of rules having been borrowed from substratum languages (the original languages of the immigrants) is sharply reduced by the evidence surveyed in the next section. For, despite the fact that hundreds, perhaps thousands of different languages were involved in the contact situations that produced creoles all over the world, creoles show striking similarities in their syntactic structures.

3.0 Arguments for similarity

In this section I argue that the innovative rules and structures developed by the children of immigrants to Hawaii follow a pattern characteristic of all situations in which the normal generation-to-generation transmission of language is severely disrupted. In a review of such situations, an earlier version of the LBH (Bickerton 1981) took the somewhat simplistic approach of dividing them into two classes on the basis of a single criterion: whether or not the percentage of dominant-language speakers in a given community exceeded 20. In fact, that percentage varied widely over time in any creole community (see Figure 2) and was only one of several factors that determined the degree of severity with which language transmission was disrupted.

These factors (described in detail in Section 3.1) interacted to produce the type of distribution illustrated in Figure 1. Creoles toward the left-hand side of this figure occurred where disruption was most severe, and their grammars may therefore be expected to approximate the bioprogram grammar most closely; those toward the right-hand side evolved under circumstances that were less disruptive and therefore less favorable to the emergence of bioprogram features. In other words, the effectiveness of transmission of preexisting languages will vary inversely with the degree to which bioprogram features are able to emerge. Later I try to show that the distribution of Figure 1 can be supported by purely linguistic evidence. First, however, I wish to demonstrate that it is supported by demographic and historical data. The concurrence of evidence from two such distinct fields should provide firm support for the LBH.

3.1. Demographics of creole origin

Figure 2 (based on P. Baker 1976; 1982; Baker & Corne 1982) shows changes in the demography of Mauritius over the early decades of colonization. In outline, Mauritius seems to have been typical of plantation colonies, although figures would vary from colony to colony (in Berbice, from 1764 to 1833, dominant-language speakers constituted as little as 3% of the general population, according to Schomburgk 1845). At the beginning of colonization in any colony, dominant-language speakers probably always outnumbered speakers of substratum languages; early arrivals among the latter would therefore have had ample opportunity to acquire moderately fluent second-language versions of the dominant language. However, as soon as plantation infrastructure (ports, roads, buildings, cleared land, etc.) was in place, there followed an influx of substratum speakers to provide the colony's labor force. These newcomers had to be trained in their duties, and such training was carried out, not by the masters, but by earlier arrivals among the slaves (and, later, by locally born slaves) (Craton 1978; Patterson 1967). This influx (and, for that matter, the early arrivals themselves) would invariably be drawn from a variety of linguistic groups: Curtin (1969, p. 189) cites a Cayenne plantation that in 1695 had a labor force of 65 divided among speakers of 12 languages. The earlier arrivals would then have to use as a common medium of instruction whatever second-language version of the dominant language they had managed to acquire. This version would then be filtered through succeeding waves of immigration, becoming more and more diluted as it spread further from its point of origin. Several factors would determine the degree of dilution.

Nearest to bioprogram				Furthest from bioprogram
Saramaccan Djuka	Sranan	Providencia/ San Andres C Haitian C	Jamaican C Hawaiian C Papiamentu Mauritian C Guyanese C	Réunion C
Maroonage	early superstrate withdrawal	late	Low PI	High PI

Figure 1. Relative placement of creole languages (based on historical and demographic data). PI = Pidginization index.

First is the length of time between the beginning of immigration and what P. Baker (1982, p. 852) calls "Event 1" – the point at which slave and master populations achieved numerical parity (see Figure 2). The longer this period, the greater the exposure of early arrivals to the dominant language and hence the richer the second-language version that would be transmitted to the first influx. A pre–Event-1 period of sufficient length (e.g. the period 1663–1715 in Réunion; see Chaudenson 1974) would suffice even in the absence of other factors to produce an end product much closer to the dominant language than the majority of creoles (see Corne's contribution to Baker & Corne 1982).

Subsequent to Event 1, the rate of dilution of the dominant language would be determined by the rate of increase in the slave population and the relationship between this rate and the number of pre–Event-1 slave arrivals. Figure 2 shows that a slave arriving in Mauritius in the first few years of colonization would have heard a native version of the dominant language from two out of every three people on the island. A slave arriving a few years after Event 1, however, would have found that eight out of every ten residents were recent immigrants like himself – persons with only a smattering of the dominant language acquired mainly from persons who were themselves nonnative speakers. Subsequent arriv-

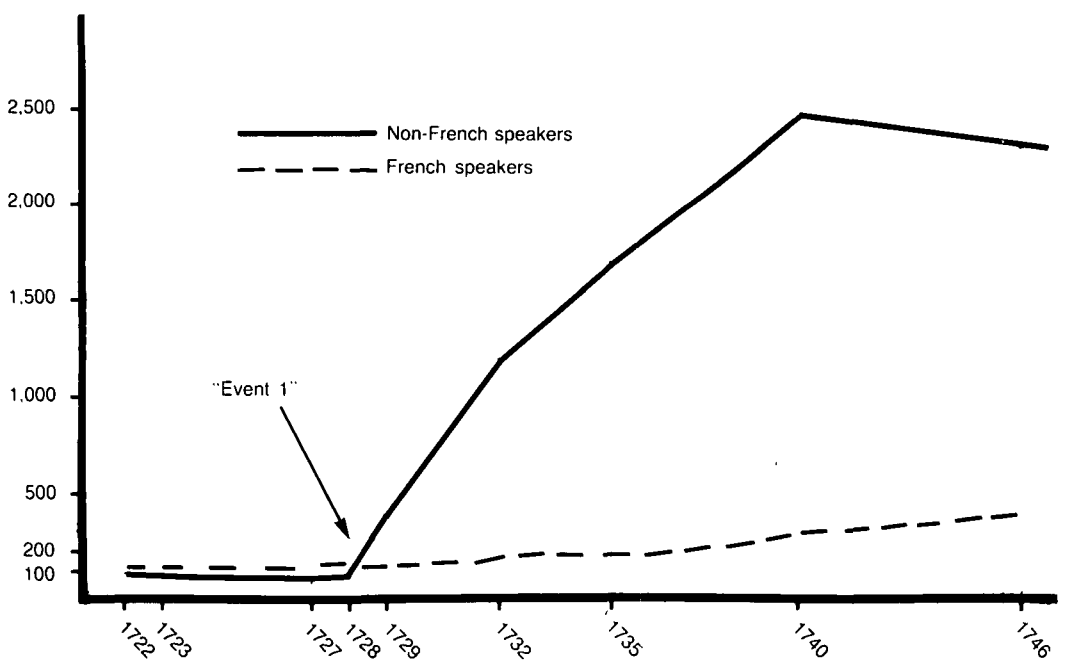

Figure 2. Population of Mauritius (first 25 years of settlement). (Based on Baker 1976, 1982; Baker & Corne 1982.)

als would have found themselves in still more unfavorable positions. Thus dilution of the original model would have been favored by rapid immigration and a low ratio of earlier to later arrivals.

The foregoing suggests that the extent of dilution in particular cases could probably be measured in terms of a pidginization index (PI) to be derived from a formula such as that of (24):

$$24.\ Y \times \frac{P}{R} = PI$$

where Y represents the number of years between colonization and Event 1, P the total substratum-speaking population at Event 1, and R the yearly average of post–Event-1 immigrants. It is important to note that these factors (as well as two others mentioned subsequently) act upon the pidgin antecedent to the creole, rather than directly on the creole itself. A higher PI indicates a "richer" pidgin, one that retains more features of the dominant language, such as inflections or prepositions; a lower PI indicates a "more impoverished" pidgin, one that (in extreme cases) will retain only a small lexical stock of items which have undergone extensive semantic and phonological restructuring. The richer the pidgin, the richer the input to the creole, hence the less the deficit between input and the minimal necessary structure for a natural language; conversely, the more impoverished the pidgin, the greater that deficit, hence the greater the demand on the language-creating capacity of the species. Thus (24) predicts that Réunion Creole, with a high Y value, would be further from the bioprogram than Mauritius Creole, with a much lower Y (although P and R values are roughly similar for both). It also predicts that Haitian Creole, with Y and P values comparable to those of Mauritius Creole, but with a higher R value (807 to 408, according to Baker & Corne 1982, p. 251) would be closer to the bioprogram than the latter.

However, two further factors must be considered. One is withdrawal of the original dominant language because of political change, as with the switch from English to Dutch rule in Surinam, or from French to English rule in St. Lucia. If such withdrawal occurs early enough (as it did in Surinam, although perhaps not in St. Lucia), the result is to cut off any further influence from native speakers of the dominant language such as would otherwise continue to occur, albeit on a reduced scale and in an indirect manner.

The other factor is maroonage, the creation of communities of escaped slaves. Maroon communities lacked native speakers not merely of the original but of *any* dominant language; they also had a low, possibly null percentage of pre–Event-1 arrivals who might have provided a strong second-language model. For example, the Saramaccan community in Surinam (Price 1976, p. 22) was probably originated by slaves who were "all African-born" and "two-thirds of [whom] were . . . less than ten years away from their African homelands." Event 1 probably took place in the late 1650s in Surinam; the Saramaccan community is unlikely to have been founded prior to 1664–65, since Saramaccan contains as many Portuguese as English words, and Portuguese speakers did not arrive until that date; Price gives the most probable date for Saramaccan foundation as around or just before 1680. It is thus most likely that the original Saramaccan community consisted mainly or wholly of ex-slaves who had arrived in Surinam well after Event 1 and who had not remained in slavery long enough to acquire more than a smattering of English and Portuguese words, acquired from others whose knowledge was hardly more extensive. It would follow from this that the input to the first generation of native Saramaccan speakers would have been radically reduced and that Saramaccan, in consequence, should be closer to the bioprogram than perhaps any other creole.

3.2. Creoles and universal grammar

In light of the foregoing account, which suggests a continuum of creolization rather than the typologically homogeneous body suggested in some earlier work (Bickerton 1977; 1981), we can now examine the similarities that exist among superficially unrelated creole languages. Previous accounts of these similarities (including that in Bickerton 1981) may have inadvertently suggested that they involved no more than a collection of heterogeneous features bearing no necessary relation to one another, and hence were an inherently unlikely product of anything as coherent as a genetic program surely must be. Thanks to work cited in Section 1.0, in particular that of Frank Byrne on Saramaccan, it is now possible to remedy this defect, and to show that creole similarities stem from a single substantive grammar consisting of a very restricted set of categories and processes, which will be claimed to constitute part, or all, of the human species-specific capacity for syntax.

The expression "part, or all" relates to an issue on which this paper must touch but which it will not even try to resolve. The most comprehensive model of Universal Grammar (UG) – the tacit knowledge of linguistic structure that human beings must be supposed to have prior to experience – is that of Chomsky (especially Chomsky 1981a; 1982). In this model, UG can be defined in terms of a set of parameters, corresponding to the various subsystems into which the language faculty can be subdivided (binding, government, control, etc.), each parameter having a finite (and small) number of possible settings; various combinations of these settings will then yield all the possible core grammars of human languages. In other words, a human child would have latent in his mind all possible grammars, although differential weighting attached to the various settings would mean that certain types of grammar would have a preferred status. On this view, 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.

However, an alternative view is possible, and perhaps preferable, at least on grounds of parsimony, since it entails less complex innate schemata: that the single core grammar that is actualized to varying extents in the course of creolization constitutes the totality of preexperiential linguistic knowledge, and that this grammar is of a nature that will permit its possessor to construct or compute all those rules, structures, and features of natural languages that are not explicitly specified in the single core grammar, given minimal exposure to such rules,

structures, and features. This alternative view is touched on again in Section 5.0. For the present, I shall try to maintain a neutral position. However, if the alternative view is even to remain open, the bioprogram grammar will have to satisfy at least two conditions: It must neither overgenerate (by specifying categories or processes that are not characteristic of the most radical of creoles, e.g. Saramaccan) nor undergenerate (by specifying categories or processes that are incompatible with one or other of the noncreole natural languages). In other words, it must be possible, at least in principle, to convert the bioprogram grammar into the grammar of any other language by processes of modification or addition, but *not* by processes of substitution or subtraction.

3.3. Creole syntax

Let us assume a grammar in which the only possible constituents are sentences (S), nouns (N), verbs (V), and modifiers of nouns and verbs. Following X-bar theory (Jackendoff 1977), which claims that major constituents must have a similar number of projections (internal levels of hierarchical structure), let us assume three projections for N and V. Since natural languages vary in their constituent ordering, let the rules assign constituency without assigning the order of words or phrases (indicated in the rules by comma notation). The resultant grammar would appear more or less as follows:

25. $S^1 \rightarrow$ COMP, S

("COMP" is here an empty slot into which question words, focused constituents, etc., may be moved.)

26. $S \rightarrow N^3$, INFL, V^3

("INFL" is, roughly, what used to be AUX [iliary] in earlier generative treatments; see Chomsky 1981, pp. 18ff.)

27. $N^3 \rightarrow \left\{ \begin{matrix} S^1 \\ \text{(Determiner)}, N^2 \end{matrix} \right\}$

(In this and subsequent rules, parentheses indicate optionality.)

28. $N^2 \rightarrow$ (Numeral), N^1
29. $N^1 \rightarrow$ (Adjective), N
30. $V^3 \rightarrow V^2$, (S^1)
31. $V^2 \rightarrow V^1$, (N^3)
32. $V^1 \rightarrow$ V, (N^3)

In addition, the grammar would require a single transformational rule, Chomsky's "Move α" (Chomsky 1981); in other words, move anything to any vacant site, with the stipulation that, pending evidence to the contrary, "anything" must be read as N^3 or V; since all unparenthesized constituents in rules (25)–(32) must be present, "any vacant site" is in practice limited to COMP.

3.3.1. Saramaccan syntax. Will the above grammar generate Saramaccan? It almost will – the fit is close enough to suggest at least a possibility that it would have generated the Saramaccan of 300 years ago with only one modification: an ordering rule that, by removing the commas from (25)–(32), made the orders subject–verb–object–complement and determiner–numeral–adjective–noun obligatory. But there is no reason to assign such an ordering rule to the bioprogram. In a language with no formal means of marking case, the only way to distinguish the major cases (nominative and accusative) consistently is to place one before and one after the verb. Thus ordering in Saramaccan could have been determined by pragmatic factors or by an accidental preponderance of one order in the pidgin.

In one respect, the grammar almost overgenerates: In synchronic Saramaccan, the option made available by rules (31)–(32) of having $V–N^3–N^3$ sequences is realized only with a handful of verbs, those with obligatory two-place predicates such as *da* "give." Thus we find (33) grammatical but not (34), and (35) seems weird although (36) is grammatical and natural – typical, indeed, of verbs whose second predicate argument is optional:

33. di womi da di mii di njanjan
"The man gave the child the food."
34. *di womi da di njanjan di mii
"The man gave the food to the child."
35. ?di womi da di njanjan da di mii
"The man gave the food to the child."
36. di womi seni di njanjan da di mii
"The man sent the food to the child" (lit. "give the child").

But this array is well motivated. If Chomsky (1981) is right in assuming that only INFL, verbs, prepositions, or other overt casemarkers can assign (abstract) Case, then the hypothesized grammar, lacking all but INFL and V, can assign only two cases per sentence (Nominative and Accusative), given that case-marking elements must normally be immediately adjacent to the constituents whose case they mark (Chomsky 1982). Only in exceptional instances can case marking "percolate" through an immediately adjacent noun to a second, as in (33). Consequently, oblique cases in Saramaccan must be assigned by a verb, as in (36). For, in failing to provide for prepositions or prepositional phrases, our grammar only marginally undergenerates Saramaccan, which has perhaps only two true prepositions, the all-purpose locative *a* and comitative *ku*.

Thus, serial-verb constructions, which were a feature of Hawaiian Creole discussed in Section 2.0, can be seen to follow inevitably from the limitations of the hypothesized grammar. Consider the following sentences of Saramaccan:

37. dee o-tei faka tjoko unu
They MODAL-take knife stab you (plur.)
"They will stab you with a knife."
38. dee waka go a wosu
They walk go LOCATIVE house
"They walked home."
39. a suti di hagimbeti da di womi
he shoot the jaguar give the man
"He shot the jaguar for the man."

Note the close resemblance between (37) and (9), and the similar use of *go* as a directional case marker in (38) and (11) (*dee waka a wosu*, without *go*, would mean "they walked *in* the house"). Certainly these sentences *could* be generated by the grammar of (25)–(32); however, we have to show that that grammar is the *only* appropriate one.

Our grammar has at least two relevant peculiarities: It cannot generate sentences with nonfinite clauses ("*To see Bill* is impossible") or verb phrases that contain other verb phrases ("They all *persuaded Mary to leave*"). In principle, a sentence such as (37) could be analyzed in either of two ways:

40. [dee o-tei faka $_S$[pro tjoko unu]]
(where pro is a phonologically unrealized pronoun coreferential with the subject of the matrix sentence) or:
41. $_S$[dee o- $_V{}^3$[tei faka $_V{}^3$[pro tjoko unu]]]
(Neither analysis, of course, affects meaning, which remains "They will stab you with a knife" in each case.) Decisive evidence would be the presence of a +Tense node before *tjoko; pro tjoko unu* would then constitute a finite sentence and could no longer be analyzed as a verb phrase, as in (41). The only overt tense marker in Saramaccan is *bi*, which marks anterior tense (similar to, but not identical with, English past perfect). From the fact that (42) is unacceptable, one might assume that (41) was the correct analysis:
42. *dee o-tei faka bi-tjoko unu
"They will take a knife to have stabbed you with."
However – as the gloss of (42), itself unacceptable in English, suggests – (42) may be bad for semantic or pragmatic reasons rather than syntactic ones; the stabbing would have had to precede the taking of the knife. To clarify the situation we must look at a type of sentence similar in structure to (37), but one that might appear, at first glance, to be even less likely to decompose into a string of finite clauses.
43. a go a wosu fu njan
he go LOCATIVE house ? eat
"He went home to eat."
Fu, as Byrne (1982) has conclusively shown, must be derived from English *for* rather than from Yoruba *fun* or *fi*, and one might thus assume *fu* to be a complementizer equivalent to English *to*. However, alongside (43) we find sentences like (44):
44. a go a wosu faa (= fu a) njan
he$_i$ go LOCATIVE house for-he$_i$ eat
"He went home to eat."
Note that *a* is the form for third-person pronouns in the Nominative Case; the Accusative Case form is *en*. This suggests that *faa njan* might be a tensed sentence, and indeed *njan* can be tensed:
45. a go a wosu faa bi-njan
he$_i$ go LOCATIVE house for-he had-eat
"He went home with the intention of eating [but did not]."
More remarkable still, *fu* can be tensed:
46. a go a wosu bi-faa njan
he$_i$ go LOCATIVE house had-for-he eat
"He went home with the intention of eating [but did not]."
This suggests that *fu* is not a complementizer but a finite verb.

Independent evidence that *fu* is verbal can be adduced. *Fu* can appear with the meaning of, and in the position of, a modal (like *o*, the marker of irrealis mode, in [37]):
47. i fu naki di mii
"You [sing.] should hit the child."
Unlike *o* and other true verbal auxiliaries, however, it can be fronted for emphasis, as in (48) (but compare [49] and [50]):
48. fu i fu naki di mii
"You *should* hit the child!"
49. tei de o-tei faka tjoko unu
"They will stab you *with* a knife!"
50. *o dee o-tei faka tjoko unu
"They *will* stab you with a knife!"
Only true verbs can undergo this kind of movement.

The foregoing suggests that a sentence such as (46) should be analyzed as in (51):
51. $_S$[a go a wosu $_S$[pro bi-fu $_S$[a njan]]]
$_S$[He went home $_S$[he had the intention $_S$[that he would eat]]]
However, the grammar of (25)–(32) claims that the nodes would be not merely S, but S^1. If this were the case, there would be an empty COMP node immediately prior to a *fu*-clause as well as at the beginning of the matrix sentence that contained it. Indeed, in emphatic sentences, constituents can be moved into both positions:
52. mi ke faa go a wosu
I want for-he go LOCATIVE house
"I want him to go home."
53. a wosu mi ke faa go
"It's home I want him to go!"
54. mi ke a wosu faa go
"I want it to be home that he goes to!"
Thus, a COMP node must precede *fu* and the *fu*-clause must in turn be dominated by an S^1 node.

Examples (37)–(54) show that although Saramaccan contains clauses that could be analyzed as either finite or nonfinite, it contains many clauses that, though nonfinite in superficial appearance, can only be analyzed as finite. Since it apparently contains no clauses that must be analyzed as nonfinite, the most parsimonious grammar of Saramaccan will generate only finite clauses. Moreover, since Saramaccan lacks complementizers, adverbs, and (with a couple of exceptions) prepositions, the functions of these constituents will be discharged by finite verbs – a process I noted as occurring, albeit to a lesser extent, among the earliest speakers of Hawaiian Creole.

3.3.2. Comparative creole syntax. However, evidence of similarities among creoles, no matter how suggestive, must be systematized and motivated if we are to support the overall picture of creole relationships summarized in Figure 1. Given a sequence of creoles A through D, A being nearest and D furthest from the bioprogram grammar, the bioprogram-related features of B should form a proper subset of those of A, those of C a proper subset of Bs, and so on. At each stage, the "missing" bioprogram features should have been replaced by features (probably drawn from the dominant language) made available by the richer pidgin mix found as one moves rightward across Figure 1. For reasons of space, the present account must limit itself to a cross-linguistic survey of *fu* and its congeners.

Most if not all creoles have a constituent formed in the same way as, and with some of the same properties as, Saramaccan *fu*. In English creoles it may take the phonetic form of *fu, fi, fo,* or *u;* in French creoles, it is usually *pu* (from French *pour* "for"); in Portuguese creoles, *pa* (from Portuguese *para* "for"). Hereafter, *fu* will be used as a cover term for all of these.

In Saramaccan, *fu* has the following properties, among others:
55. It marks complements of uncertain or nonaccomplishment.

56. It functions as a modal verb.
57. It introduces tensed (finite) complement clauses.
58. It functions as a main verb and is itself tensed.

Property (55) is found in almost all creoles. It is most salient in Saramaccan, where there is a three-way distinction among complements, as shown in (59)–(61):

59. a go a wosu go njan
"He went home to eat [and definitely did so]."
60. a go a wosu fu/faa njan (= [43], [44] above)
"He went home to eat [and may or may not have done so]."
61. a go a wosu faa bi-njan/bi faa-njan (= [45], [46] above)
"He went home to eat [but in fact did not do so]."

In most other creoles, apparently, the meanings of (60) and (61) fall together. In part, this is because only in Saramaccan is *fu* a main verb; other creoles, therefore, cannot have sentences like (46). However, any creole with property (57) should be able to distinguish the meaning of (61) from that of (60) by structures along the lines of (45); the Haitian sentences (73)–(74), below, indeed seem to do this, although no work has explicitly analyzed the property.

In Section 2.0 I noted the Hawaiian Creole examples (13) (= meaning of [59]) and (14) (= meaning of [60] and [61]). In Sranan we find the following (from Jansen, Koopman & Muysken 1978, p. 153):

62. a teki a nefi fu koti a brede, ma no koti en.
"He took the knife to cut the bread with, but didn't cut it."
63. *a teki a nefi koti a brede, ma no koti en
"He cut the bread with the knife, but didn't cut it."

The "possibly negative" meaning that attaches to *fu* after the (60)–(61) merger makes (62) logically possible; (63), lacking *fu*, can only be interpreted as self-contradictory.

In Haitian, we find the following (from Hall 1953, pp. 105, 129):

64. Boukinet di li pralé chaché dlo pou-li fè mâjé
Bouquinette say she go look-for water for-she make food
"Bouquinette said she would get water to prepare food with."
65. Malis chaché mwayê disparèt tout pòv nâ-péi-a
Malice look-for way disappear all poor in-country-the
"Malice sought a way to make all the poor people disappear from the country."

In the context of the tales in which these sentences occur, Bouquinette fails to get water (the zombie kills her first), whereas Malice does succeed in driving out all the poor people.

Guyanese creole contains an identical distinction:

66. i gaan a tong [go] sii dakta
"He went to town to see a doctor [and did, understood]."
67. i gaan a tong fu sii dakta
"He went to town to see a doctor [but maybe didn't]."

P. Baker (1972) and Roberts (1975) have given similar examples for Mauritius and Jamaican Creole respectively (for discussion, see Bickerton 1981, pp. 59–61), and, more recently, Fodale (1983) has shown an identical distinction in Papiamentu, using *pa* for "uncertain" complements. Only Réunion (the rightmost creole in Figure 1) fails to satisfy property (55).

Property (56) is found in most, but not all, positions in Figure 1. It is in Saramaccan (cf. [47]) and in closely related Sranan (example from Herskovits & Herskovits 1936, cited in Washabaugh 1975):

68. a fo ta dape
he for stand there
"He was obliged to remain there."

Washabaugh himself (1975, p. 128) cites several examples from San Andres/Providencia Creole (including [69]):

69. im fi komop ya kom sii mieri
"He was supposed to come up here to see Mary."

Similar examples from Jamaican (Bailey 1966, pp. 37, 45) and Guyanese Creole (Bickerton 1975, p. 42) are paralleled by Haitian examples from Koopman and Lefebvre (1982):

70. m te aŝte liv m te pu li a
I TENSE buy book I TENSE MODAL read the
"I had bought the book that I should have read."
71. se nu ki te pu vini
is we who TENSE MODAL come
"It's we who should have come."

In both Mauritius and Seychelles Creole, according to P. Baker (1972), Corne (1977), Valdman (1977), and others, *pu* is consistently used as a modal auxiliary. However, it is at best rare and marginal for Réunion (Chaudenson 1974, p. 839), while equivalent functions for *pa* are unreported for Papiamentu, and *fo* as modal is completely absent from Hawaiian Creole.

Property (57) is still more restricted. Parallels to (44) have so far been reported only from San Andres/ Providencia and Haiti, although one would expect to find them in Sranan and Djuka also. For San Andres/Providencia, we find examples like the following (from Washabaugh 1975):

72. ai wanda ma wuda gi mi piis a kaan fish fi ai go kuk rondong
"I wonder if mother would give me a piece of cornfish for me to cook rundown with."

Similar examples occur in Haitian (Koopman & Lefebvre 1982):

73. yo te vle pu m te ãtre nã trup ŝakmèl
They TENSE want for I TENSE enter in troop Jacmel
"They wanted me to join the Jacmel troop."
74. m te bezwẽ èskõt lè sa-a pu m te repati
I TENSE need money time that-the for I TENSE set-out-again
"At that time I needed money in order to set out again."

The implication, along the lines of (46)–(61), is that the respective speakers did *not* join the Jacmel troop and did *not* set out again, although the authors do not state this explicitly. Thus only those languages toward the extreme left of Figure 1 can use *fu* or equivalent to introduce clauses with auxiliary verbs and phonologically realized subjects.

To date, only Saramaccan, leftmost of the Figure 1 creoles, has been reported as having property (58), illustrated in examples (44)–(54). Thus the distribution of properties (55)–(58) is as shown in Figure 3, a distribution predicted on the basis of purely demographic and histor-

			Papiamentu
			Hawaiian C
		Jamaican C	Jamaican C
		Mauritian C	Mauritian C
		Guyanese C	Guyanese C
	Sranan (?)	Sranan	Sranan
	Providencia/	Providencia/	Providencia/
	San Andres	San Andres	San Andres C
	Haitian C	Haitian C	Haitian C
Saramaccan	Saramaccan	Saramaccan	Saramaccan
1	2	3	4
(*fu* can be tensed)	(*fu* introduces tensed complements)	(*fu* is a modal)	(*fu* marks + certain complements)

Figure 3. Distribution of creoles with respect to four properties of *fu* (data on category 2 for Sranan unavailable).

ical data in Figure 1. Although we still need much information, both demographic and linguistic, to confirm the relationship between these two figures, their closeness of fit is striking enough to suggest a strong and consistent connection between the circumstances under which individual creoles arose and the degree of creolization achieved in each case. The existence of a biological program for language which emerges more strongly where the disruption of language transmission is more radical is one, and so far the only, explanation for such a relationship.

3.4. Semantic similarities of creoles

Considerations of space forbid a full discussion of creole similarities (see Bickerton 1981 for a survey of some, though by no means all of them). It should be pointed out, however, that these similarities are semantic as well as syntactic. In particular, there are at least four basic binary semantic distinctions that appear to be shared by almost all creoles, not excluding the less radical ones. Note that these distinctions form an integral part of the core grammar set out in (25)–(32), since they are invariably grammaticized in the system of determiners and in three of the elements that go to make up INFL (Tense, Modality, and Aspect).

The first distinction affects noun phrases, and marks whether their referents are specific or nonspecific. Creoles have both definite and indefinite articles, but these can only accompany noun phrases that have specific reference (unlike, say, English, in which definite articles can apply to generics – "The dog is a mammal" – and indefinite articles to nouns within the scope of negation – "I don't have a dog"). In creoles, generics, nouns in the scope of negation, and other nonspecifics cannot be preceded by an article of any kind.

The second distinction affects tense, and is marked by an invariant particle preceding the verb and other INFL constituents. The reference point for tense is never the "point present" or "moment of speech" familiar to speakers of European languages, but rather the time of the topic under discussion, which may or may not have present reference. Verbs relating to the topic time are unmarked; thus, a narrative relating a series of past events, in the order in which they occurred, will contain a string of unmarked verbs. However, if the narrator refers back to an earlier event, the verb or verbs concerned must be preceded by the anterior tense marker (a fuller analysis of anterior tense is given in Bickerton 1975, chap. 2).

The third distinction affects modality, and again uses an invariant particle to distinguish irrealis (future, conditional, or imagined events) from realis (any event that has actually occurred or is occurring). Again, realis events, the more common subjects of discourse, are left unmarked. The fourth and final distinction affects aspect, and distinguishes nonpunctual (events, past or present, that have measurable duration or are repeated) from punctual (single, completed events). Nonpunctual events receive a third invariant particle; punctual events are unmarked.

When combinations of markers occur, they do so in the invariant order Tense–Aspect–Modality, and the combinations have meanings that are consistent across all the more radical creoles; for instance, the combination anterior-irrealis indicates unrealized conditions in the past. Table 1 shows markers and combinations for six creoles; note the absolute consistency of the more radical creoles and the slightly lesser consistency in creoles from the right of Figures 1 and 3.

The foregoing account of creole similarities should make it clear that these (a) are both deep and extensive (b) are not random, but stem from a common grammar, and (c) vary in their intensity according to the degree of linguistic deprivation involved in each creole's birth. Such similarities would follow naturally if there existed a biological program for language development as part of the genetic inheritance of the species.

4.0 Some alternative explanations

Scholars who are reluctant to admit biological determination of linguistic properties will naturally look for some way to account for creole similarities in terms of contact with preexisting languages. In this section I try to show that no explanation of this type can be satisfactory.

Two types of explanations have been advanced. The more general, and older, claims that features not directly attributable to the dominant language must be derived from substratum languages (i.e. the original native language of the subject population; see Sylvain 1936; Taylor

Table 1. *Tense, modality, and aspect in six creoles*

± A ± I ±N[a]	Saramaccan	Sranan	Haitian C	Guyanese C	Hawaiian C	*Lesser* Antilles C
− A − I − N	∅	∅	∅	∅	∅	∅
− A − I + N	ta	e	ap	a	stei	ka
− A + I − N	o	sa	av	sa/go	go	ke
− A + I + N	o-ta	sa-e	av-ap	–	go stei[b]	ke ka
+ A − I − N	bi	ben	te	bin	bin/wen	te
+ A − I + N	bi-ta	ben-e	t'ap	bina	bin stei[b]	te ka
+ A + I − N	bi-o	ben-se	t'av	bin sa/go	(wuda)[c]	te ke
+ A + I + N	bi-o-ta	ben-sa-e	t'av-ap	–	–	te ke ka

[a]A = anterior tense; I = irrealis modality; N = nonpunctual aspect. [b]Forms that are extremely rare nowadays. [c]A form borrowed from English, not part of the original creole system.

1960; Turner 1949; and more recently, Alleyne 1971; 1980a; 1980b; Allsopp 1976; 1977). The prevailing strategy of substratophiles has been twofold. If a similarity can be shown between a creole and a substratum (usually African) language, the following syllogism is implicitly constructed:

72. A feature F is found in one (or several) creoles and one (or several) African languages.
73. Speakers of the creole(s) are mainly of African ancestry.
74. Therefore, F was acquired by creole speakers from speakers of African languages.

However, if no similarity exists, it is claimed that "we have to make allowances for plausible processes of change, analogous to what in anthropology is [sic] called reinterpretations and remodelings of such a nature and to such a degree that the relationship between the new form and the input source becomes difficult to decipher" (Alleyne 1980b, p. 8).

Since no one has attempted to specify the nature and extent of such "allowances," the second strategy is invulnerable to disconfirmation and therefore valueless. However, the strategy of (72)–(74) runs into problems almost as severe. A major cause of these problems is the reluctance of substratophiles to provide evidence that the right speakers were in the right places at the right times for features to be transmitted. For example, Bentolila (1971) claims that the creole tense–modality–aspect system reviewed above may derive from a similar system in Fon. However, even in modern times Fon speakers numbered less than half a million (Westermann & Bryan 1970), and figures in Curtin (1969), although far from complete, suggest that they formed an infinitesimal portion of New World immigrants (e.g. less than 0.2% in Haiti between 1760 and 1791). Similarly, most of the forms cited by Alleyne (1980a) as African retentions are supported by evidence from Yoruba. Yet according to Blassingame (1972, Introduction) few if any Yoruba were enslaved prior to 1750 (most Caribbean creoles were formed a century earlier).

However, even if the presence of appropriate languages could be demonstrated, the substratum case would remain incomplete. It would still be necessary to provide plausible mechanisms by which rules could have passed from substratum to creole speakers. There can only be two possibilities. Either rules entered the pidgin and were learned by creole speakers, or early creole speakers were bilinguals with fluent control of substratum languages. These possibilities are not, of course, mutually exclusive. Yet in fact both could apply without rules and features necessarily passing from one language to another.

Bilingualism was extremely common in the early years of Hawaiian Creole. Of the speakers born locally prior to 1905 that we interviewed, all were bilingual, and some were trilingual. In subsequent generations the distribution of bilinguals is skewed ethnically – they are common among Japanese, rare among Filipinos, for instance. Thus there is no doubt that creole speakers *could have* acquired substratum rules and features. However, we find only a handful of trivial cases of substratum influence in Hawaiian Creole.

Why should this be so? Relevant evidence comes from a small group of Japanese extraction, known as *kibei*, born in Hawaii but educated in Japan, who later returned to Hawaii. This odd group of pidgin–creole hybrids – technically creole by birth, but linguistically closer to pidgin speakers – did sometimes relexify (i.e. change vocabulary while leaving syntax intact) complex Japanese structures (e.g. relative and temporal clauses) to yield highly distinctive sentence types such as (75)–(76):

75. luk-laik-pankin-kain get
"There's the kind that look like pumpkins."
76. as-bihoa-stei-taim difrent
"It was different when we stayed here before."

Structures like these, however, were far from common among true pidgin speakers and never produced at all by locally born Japanese who had not undergone the *kibei* experience, even though many of these were Japanese–creole bilinguals. We can only assume that *kibei* and non*kibei* had quite different grammars. The *kibei* left Hawaii before acquiring a full creole grammar (or lost what they had acquired) and subsequently produced sentences like (75) and (76) by constructing some kind of extension to the native Japanese grammar acquired during their schooling. The non*kibei*, on the other hand, could not produce sentences of this kind without totally remodeling the native creole grammar that was primary for them.

The second possibility too (fluent bilingualism) was

realized, but proved quite ineffective, in Hawaii. If substratum rules are to pass into a creole via the antecedent pidgin, they must first be present in that pidgin. Hawaiian Pidgin certainly contained some features, usually of a low level of complexity, that could be attributed to substratum influence. Assume that early creole speakers acquired such features. Either they acquired the whole range of such features or they acquired only those from a particular language (say, their ancestral one). If the first, early creole speakers would have produced phenomena as diverse as that produced by pidgin speakers, and we have shown that this is not the case: Hawaiian Creole speakers have quite uniform grammars, regardless of age. If the second, early creole speakers would differ recognizably from one another – one would have a Japanese-influenced grammar, another an Ilocano-influenced one, and so on. But again there is no evidence that older speakers differ from one another in this way.

It could be argued that such differences may have existed, say around 1910, but have subsequently leveled out. I know of no evidence either for or against such a proposition. However, if such leveling did indeed take place, it must have served to *erase* rather than *preserve* substratum influences, insofar as so few are detectable today!

Substratophiles might suggest that Hawaii was unusual in the typological diversity of its substratum languages. A more uniform substratum (claimed to exist among the West African languages) might lead to a more uniform pidgin, which in turn would obviate many of the problems discussed in the preceding paragraphs. But even if this were true, there would be no way of explaining why a uniform substratum and pidgin in the Caribbean and a heterogeneous substratum and pidgin in Hawaii should give rise to such strikingly similar results.

In fact, the homogeneity of the African substratum has been much exaggerated. The claim by Alleyne (1980b) that creole tense aspect can be accounted for in terms of a "generalised West African system" can hardly be sustained in light of statements such as that of Manessy (1977, p. 148):

> There is practically nothing in common, from a typological point of view, among the verb system of Hausa, which aligns six paradigmatic sets corresponding to five aspects, that of Fula, which combines three voices and three aspects comprising several tenses, and the system of the Bantu languages, whose simplest expression, that of Swahili, comprises four moods, ten tense-aspect modalities, and numerous compound tenses. The same thing could be said of the other African languages cited.

A similar degree of diversity is described by Welmers (1973).

Over the whole range of creole languages, perhaps as many as a thousand typologically diverse substratum languages went into the making of the antecedent pidgins. Moreover, combinations of substratum languages varied widely from place to place, sometimes with little or no overlap. Thus the Sranan substratum was Western Bantu, Kwa, and Guinean, that of Mauritius predominantly Eastern Bantu, Malagasy, and Indian, and that of Hawaii mainly Hawaiian, Chinese, Portuguese, Japanese, and Filipino. If substratum influence was at all significant in creolization, how could such diversity lead to the degree of uniformity described?

Finally, the finding that typical creole structures, including verb serialization and other forms often taken as indicative of African influence, can be derived from a single common grammar further weakens the substratophile position. Substratophiles have never attempted to compare whole systems, but have picked out and compared isolated rules and features from creole and substratum languages. Implicit in this operation is the belief that languages can be made by throwing together a heterogeneous set of items. Yet all we have learned about languages supports the view that they constitute tightly knit wholes in which a few major choices define a wide range of superficially varied phenomena.

This final argument, however, does not work against the second alternative to the LBH, the theory of monogenesis (Stewart 1962; Thompson 1961; Voorhoeve 1973; Whinnom 1956; 1965), which attributes creole similarities to a common ancestor, an Afro-Portuguese pidgin developed in West Africa in the 15th and 16th centuries and subsequently disseminated around the world, relexifying as it spread from Portuguese to English, French, or Dutch colonies. Again, crucial evidence for assessing this alternative comes from Hawaii.

Holm (1982) has suggested that the protocreole could have been spread to Hawaii either by the Portuguese (12,000 of whom had reached Hawaii by the mid-1880s) or by a much smaller contingent (at most 700) from the Cape Verde Islands who were probably native speakers of Caboverdiense, a Portuguese creole. However, most Portuguese immigrants were poor peasants from the Azores and Madeira who were unlikely ever to have been exposed to overseas contact varieties of Portuguese. The Cape Verdeans were too few (less than 0.05% of the population in 1900) to have influenced the community as a whole.

The strongest argument against external influence in Hawaii, however, is linguistic rather than demographic. No immigrant so far recorded, regardless of date of arrival or linguistic background, has ever spoken anything remotely approximating the creole. Now suppose that some no longer traceable group did bring creole to Hawaii. That group must somehow have transmitted it to *all* locally born speakers and *no* immigrant speakers. If locals, but not immigrants, could learn the creole, this could only be because immigrants were adults (thus, past the critical age for acquisition) at time of arrival. Hence, locals must have learned creole as children. Therefore, to maintain the monogenetic claim, we must assume that some group, no longer identifiable, somehow acquired sufficient access to *all* children born in Hawaii after about 1895 to teach them the creole. There is, needless to say, not an iota of justification for any such scenario.

The foregoing discussion suggests that neither substratum influence nor diffusion is adequate to account for the creation of creole languages. In the absence of further alternatives, the LBH or some variant thereof seems inescapable. However, the LBH carries profound implications for the study of language in general, and for the study of language acquisition and language origins in particular. In the remainder of this paper I very briefly survey a few of the implications in these two areas.

5.0 Language acquisition

If a bioprogram for language is available to those children who happen to grow up in a pidgin-speaking community, we can hardly assume that it is only available to, and used by, such children. It must play some vital role in the normal acquisition of language by children in traditional communities. After suggesting what that role might be, I list a few of the many phenomena already noted in the language of children that the LBH may be able to explain.

In Section 3.2, two possible views of UG were contrasted – the view that all possible core grammars are somehow latent in the mind and the view that there exists a single core grammar (perhaps with certain gaps, or not specified beyond a particular point) serving as a base from which more complex grammars could be constructed. Could the grammar sketched in (25)–(32) function in such a way?

That grammar does leave a number of parameters to be set by experience. It specifies immediate constituents but not the order of those constituents. The order of both sentence constituents and constituents within N^3 and V^3 is free. Even creoles use some of this freedom: In the Haitian noun phrase, five out of the six possible modifier sites can be filled, as in *u gwo chwal* "a big horse" or *twa chwal nwa a* "The three black horses" (literally "three horse black the"). What might appear to be unduly limiting lies in the grammar's constraint on the number of categories, and the consequent absence of complementizers, prepositions, and the like, as well as more complex structures such as prepositional phrases. However, natural processes of linguistic change frequently convert verbs into complementizers or prepositions (see Lord 1973), and data from Sranan, a neighbor creole of Saramaccan, surveyed in Jansen et al. (1978) suggest that embedded sentences are currently being downgraded to verb phrases, and verb phrases to prepositional phrases (for discussion see Bickerton 1981, pp. 124–30).

In other words, given structures generated by the grammar of Section 3.3, it is possible to construct from them a range of more complex structures not specifically generated by the bioprogram, even in the absence of appropriate input or a developed target model. Hence the role of the bioprogram for children acquiring a "ready-made" language (rather than creating a creole) is to furnish elementary forms and structures from which (guided by input from the target language) they can develop other and more complex forms and structures.

Some recent work indirectly supports this conclusion. Slobin (1982; 1984), on the basis of a wide range of cross-linguistic studies of acquisition, claims that there exists what he calls "Basic Child Grammar." In line with previous work (e.g. Slobin 1977), he sees this grammar as being generated by a set of "operating principles" (in effect, instructions to the child on how to analyze linguistic data) which the child applies to input. Slobin's approach and that presented here stand in a hen-and-egg relationship to each other. Here it is claimed that the child's operating procedures fall out from the bioprogram grammar: For example, Slobin's "Canonical Clause Form" (Slobin 1984) – "If a clause has to be reduced, rearranged or otherwise deformed when not functioning as a canonical main clause . . . attempt to use or approximate the canonical form of the clause" – would be viewed not as a strategy that somehow results in child grammar but rather a consequence of the bioprogram grammar specified in Section 3, which allows no reduced, deformed, or nonfinite clauses. The issue is an empirical one, but it must be pointed out that operating principles are of little use if, as with pidgin input, there are not enough data to operate on. Indeed, all acquisition models that are *solely* input driven will have serious problems in dealing with the origins of creoles.

For present purposes, however, this disagreement is of minor importance, since C. L. Baker (1979) has shown how a grammar fragment specified by Wexler, Culicover, and Hamburger (1975) and a corresponding set of hypothetical instructions to a child are intertranslatable. The significance of Slobin's findings is that they show, in far more detail than can be summarized here, structures consistently used by young children that violate the grammatical rules of their target languages but are consistent both with the rules hypothesized here for the bioprogram and with surface forms found in creole languages.

5.1. Systematic error

In support of his "Canonical Clause Form," Slobin adduces, inter alia, use of tensed sentences with overt coreferential subjects in child Polish (e.g. *ja chce żeby miałem kotka* "I want that I-have cat" – compare such Saramaccan examples as [44] or [52] above) instead of the correct infinitive form; use of separate clauses for causal constructions in child Turkish, instead of the correct nominalizations (the canonical form of creole causal sentences is biclausal, effect last, as in Guyanese Creole $_S$[*shi mosi de bad*] *mek* $_S$[*shi tek i*] "She must have married [*tek*] him because she was hard up [*de bad*]); use of noninverted clause order in yes or no and wh-questions in child English and German (all creoles use noninverted order in these structures); and retention of SVO order in clauses with pronominal objects in child French (*j'ai vu à elle* "I saw her" rather than the appropriate *je l'ai vue* – no creole has preverbal clitic pronouns). In other words, where the bioprogram conflicts with the grammar of the target language, one finds delayed learning and frequent cases of systematic error. The "errors," however, are often structures that would have been grammatical if the child had been learning a creole language.

Other cases of creolelike systematic error mentioned in the acquisition literature include the use of negative indefinite subject with negated verb (McNeill 1966, *nobody don't likes me* – compare Guyanese Creole *nonbadi na sii am* "Nobody saw him") and the use of intransitive verbs or adjectives as causative verbs (Bowerman 1974, *a gon full Angela bucket* "I'm going to fill Angela's bucket" – compare Guyanese Creole *a gon full Angela bucket*, same meaning!). Robert Wilson's ongoing study of Seth, a blind two-year-old (the first acquistion study to look actively for bioprogram features; personal communication) has shown further phenomena, including the invention of instrumental serialization. At age 27:0, Seth began to produce structures like (77) and (78):

77. Let Daddy hold it hit it (= let Daddy hit the ball with the bat)

Context: ball already present; Seth hands Daddy a bat simultaneously with utterance.

78. Let Daddy get a pen write it (= let Daddy write it with the pen)
Context: Daddy has a pen in his hand with which he has been making diary entries.

The circumstances preclude the possibility that Seth is asking his father to first obtain the bat or the pen. Moreover, Seth already has clausal conjunction with *and*, which he uses to express consecutive actions; so (77) and (78) do not result from ignorance of this structure.

79. Go over there and sit down (24:1)
80. Let's get down and go in the kitchen (25:2)
81. Get a pencil and write it (no pencil present) (25:2)

At the time, Seth had not acquired the preposition *with;* by 27:3 he acquired it, serial instrumentals ceased, and prepositional phrases using instrumental *with* replaced them. But the bioprogram had already fulfilled its function by enabling Seth to know what instrumental structures were and allowing him to construct them from primitive ingredients.

5.2. The preemption principle

But why did Seth not continue to use serial instrumentals, maybe alongside *with* phrases? A model such as the LBH which proposes a single substantive grammar is even more vulnerable than other acquisition models to the problem of how children learn to discard incorrect structures in the apparent absence of overt negative evidence (Brown & Hanlon 1970). (Note, incidentally, that even if one allows for covert negative evidence – failure to understand, parental repetition incorporating correct forms, and so on – the problem is still far from solved; this is partly because the provision of such evidence is highly culture specific [Ochs 1982], but mainly because it occurs randomly and sporadically, whereas the exclusion of incorrect forms is across the board and virtually exceptionless.)

Here, a recent development in learnability theory lends itself to incorporation in the LBH. Pinker (1983) proposes a set of Preemption Principles, related to Wexler's Uniqueness Principle (Roeper 1981), which can in all probability be collapsed into a single principle, (82):

82. If you hear people using a form different from the one you are using, and do not hear anyone using your form, abandon yours and use theirs.

Principle (82) is far from transparent. For it to work, at least two stipulations must be made. First, the child must be able to recognize similarities of meaning and function in such a way that he can identify an adult structure as equivalent to one of his own structures even though the two objects may differ formally to a degree. Somehow, Seth must know that *get the bat* in *get the bat hit it* is equivalent to *with the bat* in *hit it with the bat.* In other words, the child has innately available the semantics of the case roles, in addition to those of numerous other categories such as tense marker, definite article, complement of verb of perception, and so on. Second, the child must initially assume that there is only a single formal mode of expression for each semantic category. If it turns out (as it often does) that there is more than one appropriate form, there is nothing to prevent him from learning additional forms, provided that there is positive evidence from input that such additional forms exist. This second stipulation prevents him from maintaining his original bioprogram form alongside the correct target form.

5.3. Precocious learning

I have dealt so far with cases in which the bioprogram grammar and the target grammar will confict. There are many cases, however, in which features of the target grammar coincide more or less exactly with bioprogram features. Where such similarities occur, the LBH predicts that there will be rapid, precocious, and error-free learning. Again the literature furnishes relevant cases.

Brown (1973) notes that progressive *-ing* is never overgeneralized to stative verbs, even though most other grammatical morphemes (e.g. *-ed,* plural *-s,* etc.) are widely overgeneralized by children learning English. This fact is predictable from the LBH. Progressive events form a proper subset of nonpunctual events, and nonpunctuals form one of the basic categories of creole grammar. Furthermore, nonpunctuals cannot cooccur with statives – or, to be more precise, they always turn statives into processuals (cf. Guyanese Creole *mi no* "I know," *mi a no am* "I am getting to know him"; Sranan *a siki* "he is sick," *a e siki* "he is getting sick").

Maratsos (1974; 1976) designed an ingenious series of experiments aimed at determining whether children could distinguish between the two functions of English indefinite articles: specific reference (*I got bitten by a dog*) versus nonspecific (*I didn't see a dog*). To his surprise, he found that children as young as three years were 90% effective at this discrimination, even though the clues to it, in English, are extremely slight and elusive. This finding comes as no surprise to the LBH, however, since these two functions are quite distinct and are distinctly marked in all creoles (specific indefinite reference by the numeral "one" and nonspecific reference by zero).

The foregoing is merely a preliminary sketch of the kind of theory of language acquisition that might be based on the LBH. Any complete theory would have to incorporate other factors, such as the learning of formulaic chunks (segments longer than a single free morpheme) and their subsequent decomposition into appropriate segments (Peters 1977; 1983). Seth, for example, has *didja, where'd, wheredja,* and the like long in advance of acquiring any trace of support *do; didja,* for example, seems to be a combination of perfective and first-person reference, as in a frequent utterance *didja toot* "I [just] farted." However, support *do* is embedded in these forms in such a way that when he becomes able to analyze them, *do* will automatically emerge in the correct (presubject) position – a position to which the bioprogram grammar would not permit it to be moved. Thus a combination of the LBH with principle (82), and with the strategy of acquiring and later analyzing formulaic chunks, offers at least the possibility of a theory that might go further toward accounting for primary acquisition than existing theories do.

6.0 Language origins

Many writers, even among those who willingly accept the biological nature of the human language faculty (Chom-

sky 1979; Lenneberg 1967; Marantz 1983) regard it as premature or even futile to seek to recover the origins of language (cf. Harnad, Steklis & Lancaster 1976). Yet surely, if this faculty is biologically based, it must have been developed in the normal course of evolution, and therefore must have a real (and perhaps traceable) history. Although any present treatments can only be speculative, I would like to try to show how the LBH suggests some novel approaches to the question.

One of the most striking features of human language is that out of a potentially infinite set of semantic distinctions, only a very small subset is ever grammaticized. Moreover, only a limited subset of that subset is grammaticized in creoles: those distinctions listed at the end of Section 3.0, and a few others, such as number, possession and distal–proximate. Other distinctions, such as gender, agency, or transitivity, which are grammaticized across a wide range of languages, are never overtly marked in creoles. Grammatical gender is entirely absent; agency and transitivity are adequately indicated by the simple presence of a nounphrase following the verb. Tok Pisin has a transitivizing suffix *-im*, but the circumstances that gave rise to Tok Pisin were quite different from those that gave rise to true creoles. The several generations during which it existed as a nonnative language, and the degree of complexity it attained prior to nativization, render it immune to the arguments against substratal influence given in Section 4.0; indeed, precisely those features that differentiate it from true creoles, such as the *ia . . . ia* relative-clause bracketing discussed in Sankoff and Brown (1976), are the ones that reflect the indigenous languages of New Guinea (Bradshaw 1979).

Notice, moreover, that this subset is *always* grammaticized; it is not the case, for example, that creoles are free to choose from it, marking, say, +/−specific but leaving +/−punctual unmarked. These facts suggest that within the set of grammaticizable semantic distinctions there must be some kind of hierarchy, in which the subset marked by creoles takes precedence.

Why should there be some semantic distinctions that *must* be marked, others that *may* be, and yet others that apparently cannot be? Two things should be noted about markable distinctions: They are as fully automatic as any other aspect of syntax (no speaker hesitates over the choice of tenses or articles unless the facts themselves are in doubt) and the binary (or occasionally ternary) terms of each distinction subsume the universe – all referents are specific or nonspecific, singular or plural (or dual), just as all actions are punctual or nonpunctual, realis or irrealis. This suggests that at some stage of information processing all items are tagged appropriately, or differentially stored, or treated in some way such that they cannot be recovered from the memory store without the accompanying information that they are specific, or nonpunctual, or whatever.

Now it could be the case that the complex neurophysiology needed to achieve this differential treatment is merely part of the vast cortical expansion that has taken place in the hominid line over the last few million years. Yet the complexities of memory storage and memory processing in monkeys (Mishkin 1982) and the capacity of pigeons to make categoric distinctions inaccessible to the most powerful computers (Terrace 1983) suggest that some crucial language infrastructure may have been in place for much longer than that. The basic creole distinctions listed in Section 3.4 are all concerned with deixis (fixing location) in time or conceptual space (by "conceptual space" I mean an area in which one can deal with questions such as, Is this x the x I saw last time? or Is action y attention demanding or just part of an ongoing background?). Now deixis in time and conceptual space is obviously adaptive from an evolutionary viewpoint. Creatures at quite a low level of existence can benefit from being able to recall events in the order in which they occurred (rather than a reversed or jumbled order) or being able to distinguish a familiar individual in a given class from an unfamiliar one. But such capacities, linked to the organs directly concerned with language, are all that is required to give anterior tense in the first case, and +specific-new versus +specific+new (the definite – indefinite distinction) in the second. Thus there is the possibility that some features of language may have cognitive antecedents that long predate both homo sapiens and language.

It is essential to be precise at this point. Much of Chomsky's earlier work (e.g. Chomsky 1968) pitted a modular "language organ," which seemed to embrace pretty well everything one might want to call language, against an empiricism that saw language as a mere by-product of some general-purpose problem-solving mechanism. In that climate, any suggestion that a feature of language might ultimately derive from nonlinguistic factors was likely to be treated as antirationalist heresy. However, Chomsky (1980) later made the highly significant distinction between what he calls the "conceptual" and "computational" components – the first embracing semantics and pragmatics and perhaps interconnected with nonlinguistic cognitive capacities, the second involving syntax and phonology and constituting a truly autonomous processing mechanism [see also Chomsky, "Rules and Representations," *BBS* 3(1) 1980].

The position argued here is quite compatible with Chomsky's current stance. No general-purpose mechanisms are invoked, but rather, a series of highly modular task-specific cognitive devices interacting with an equally modular and task-specific processing component which imposes a formal structure on the output of the former. The computational component itself is probably a sapiens innovation, and it is also exactly what distinguishes the anarchy of the pidgin from the rule-governed regularity of the creole.

Presumably within the range of human distinction-making capacities there is a hierarchy such that some distinctions are made more readily and more automatically than others. A reasonable research assumption might be that the strength of any given capacity (as measured by its appearance or nonappearance in the formal structure of creoles) was proportionate to the length of time it had been established in earlier species. Of course, any species can undergo rapid expansion of some feature that had hitherto been relatively insignificant – the giraffe-neck syndrome – but this would seem the exception rather than the rule in speciation.

Similarly (although in this case only our own species would be involved) the nature of creole syntax should indicate what is most basic (and hence perhaps also what is evolutionarily earliest) in the syntax of language in general. One problem confronting contemporary syntac-

ticians is that of distinguishing between "core grammar" (that part of a given language that is somehow neurobiologically specified) and the "marked periphery" (the cultural bric-a-brac of "borrowings, historical residues, inventions and so on"; (Chomsky 1981a, p. 8). The LBH suggests that creole grammar constitutes a kind of "inner core grammar" from which more complex and varied grammars may have evolved and which may bear a relationship to core grammar similar to the one that core grammar bears to the marked periphery; for instance, it has been hypothesized that the latter "relates to the theory of core grammar by such devices as relaxing certain conditions of core grammar, processes of analogy in some sense to be made precise, and so on" (Chomsky 1981a, p. 8), and just such relaxations, processes, and the like could have given rise to the wide variety of contemporary core grammars for all of which Chomskyan universal grammar would require a specific biological endowment.

7.0 Conclusion

The LBH adds a further ripple to the wave that since the middle of the present century has been rising around the bastions of behaviorism, learning theory, and empirical philosophy; in field after field, behavior once seen as acquired has been found to come under the guidance of neurobiological structures. The LBH presents some empirical evidence to reinforce the claim that language, perhaps the most complex of human achievements, certainly the most distinguishing characteristic of our species, falls within this class of behavior. In so doing, it suggests that the biological infrastructure of language may not necessarily be as complex and as extensive as some nativists have suggested – although parts of it at least could be considerably older.

As its name suggests, the LBH has presently the status of a hypothesis rather than a theory; a great deal of evidence from a great many fields will have to be gathered, sifted, and interpreted in order to test it thoroughly. The most one can hope to do in a paper such as this is show that the hypothesis is coherent enough, and of adequate explanatory power, to make it worth the testing. With regard to its explanatory power, there is one final point worth making. Whether it is eventually confirmed or disconfirmed, it has succeeded in bringing together a variety of subdisciplines that have previously had communication problems. Hitherto, creole studies have been carried on largely in a state of blithe indifference to theoretical linguistics, and vice versa; theoretical and empirical students of language acquisition have seldom conversed, and even then have usually found themselves at cross-purposes; the origins-of-language issue, meanwhile, has been abandoned by linguists to the not-so-tender mercies of anthropologists, philosophers, and others. At the very least, the LBH should have shown that the fields of all these subdisciplines are inextricably intertwined, and that each must draw on all of the others if the central mysteries of language are to be resolved.

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Bioprograms and the innateness hypothesis

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I am entirely willing to accept Bickerton's data and the interesting conclusion that creolization is a function of the language acquisition process, creating structural convergence among creoles with very different substrate languages and very different histories. My difficulty with his paper lies with the notion of a bioprogram and the kind of nativist argument that this implies, that is Bickerton's argument "in favor of a language bioprogram hypothesis . . . that suggests that the infrastructure of language is specified at least as narrowly as Chomsky has claimed."

There are in fact four separate kinds of nativist arguments in his paper:

Universality. To the degree that creoles are similar to one another, in ways that are not predictable from common origins ("monogenesis"), the operation of innate biases about the structure of language is implicated.

Invention. If the output of a language processor is not directly predictable from the input, and in particular if the output is more richly structured than the input, innate properties of the language processor must be responsible.

Domain specificity. Insofar as the universal properties shared by creoles are peculiar to language, bearing no resemblance to the properties of other cognitive systems, the innate principles responsible for these properties must be language specific.

Selectivity. In line with Chomsky's distinction between *conceptual* components of language (semantic-pragmatic principles that may well derive from nonlinguistic cognition) and *computational* components of language (phonological and syntactic principles that are unique to language and autonomous relative to the rest of cognition), the bioprogram that underlies creolization seems to contain both conceptual and computational elements. However, even the conceptual elements provide evidence for domain specificity. That is, because the bioprogram refers to only a select subset of the many concepts available to a general cognitive system, there must be language-specific principles for discriminating between codifiable and noncodifiable meanings.

I believe that all four of these nativist claims are debatable, at least in their strong "predeterministic" form. Let me consider them each in turn.

With regard to the universals claim, Bickerton has fallen victim to a very common confusion between biological determinism and universality. In fact, these two dimensions are logically and empirically independent. Many aspects of individual variability are under strict genetic control: eye color, skin pigmentation, height (with nutrition held constant), and so forth. At the same time (and this is the real source of the confusion) many universal or at least high-probability outcomes are so inevitable given a certain "problem space" that extensive genetic underwriting is unnecessary. To be sure, some kind of genetic determinism is necessary to place the organism in the right ballpark for the problem to be encountered and solved. But the genetic contribution often proves to be far smaller and far less direct than one might expect given the reliability of the phenomenon in a given species. I can illustrate this point best

with a set of examples (including some old but still serviceable chestnuts).

Bee hives. A casual observer noting the perfect hexagonal structure of honeycombs is tempted to conclude that the universality and perfection of the hive structure are ensured by "instinct," or, more specifically, by some kind of innate hexagonal principle responsible for the bees' construction behavior. However, it is now well understood that the hexagonal structure is an inevitable outcome of the "packing principle," a mathematical law governing the behavior of spheres packed together at even or random pressure from all angles. The bees' "innate knowledge of hexagons" need consist of nothing more than a tendency to pack wax with their hemispheric heads from a wide variety of directions. By the same line of argument, grammars may be taken to represent a set of possible solutions to a much more complex formal problem, with some solutions falling out more easily than others on purely formal grounds (see Bates, Benigni, Bretherton, Camaioni & Volterra 1979 for a more detailed elaboration of this example).

Hand feeding. Human beings in all known cultures eat with their hands, with or without an intervening tool. And yet we need not invoke an innate hand-feeding principle to account for this universal tendency. Given the nature of the problem space – the position of the human mouth, the types of food to be consumed, and the exquisite all-purpose nature of the hand, placed so conveniently before the eyes – the hand-feeding solution is simply inevitable. Some of the elements that make up the problem space may be innate, but their solution need not be.

Cooking. As Lévi-Strauss (1969) noted some time ago, there are certain deep principles that underlie the cooking and eating of foods across cultures. However, given certain universally discoverable facts about animal and vegetable foodstuffs, the nature of fire, and so forth, we need not rush to the conclusion that these universal principles are innate. Above all, note that the universal facts reside in the structure of the *cooking problem*, and not in the environment per se. Such task structures – like the linguistic problem of coding nonlinear meanings onto a linear speech channel – lie neither in the organism nor in the environment, but at some emergent level between the two.

Children and metaphysics. In a book called *The Child's Discovery of Death*, Sylvia Anthony (1940) interviewed children as young as 2 to 3 years old concerning their beliefs about birth, death, life, and afterlife. Even though most of these children came from ordinary Catholic or Protestant backgrounds, certain metaphysical themes like the concept of reincarnation seemed to be consistently reconstructed by these children in their efforts to explain how things work. Are we to conclude that these selections from the universal stockpile of religious ideas stem from some bioprogram for metaphysics? Perhaps, but there is another possibility: Given certain inputs about the appearance and disappearance of animate beings, only so many logically feasible solutions suggest themselves. The universal conclusions reached by these children could represent inevitability rather than innateness, given the problem space and certain universal properties of mind (e.g. a dialectic principle according to which the child might postulate the opposite of any condition like death or disappearance).

In short, universal or high-probability structures shared by creoles need not necessarily reflect innate tendencies of any direct sort. They may reflect the consistent rediscovery of a set of logically possible solutions to a problem space whose structure is still not well understood (more on this below).

If, as I have proposed, universality and innateness are independent, then the invention argument also loses some of its force. When the output is richer than the input, we must certainly conclude that something has been contributed by the organism. But must we conclude that every invention or radical restructuring of input comes from a preformed stock of ideas? My point is simply this: If universal inventions logically require a nativist explanation, then the same must be said of idiosyncratic but equally creative inventions by the child. In the rich descriptive literature on child language, we find a wide array of peculiar errors and idiosyncratic theories and strategies proposed by small children. For example, in my own research on the comprehension and production of polite forms by Italian children (Bates 1976), I came across one child who had constructed a theory to explain the distribution of formal and informal pronouns in his language: "You say 'tu' in the morning and 'Lei' in the afternoon." In fact, this theory fit the facts about his world rather well, since he spent the morning at home or at preschool but visited shops and other public places with his parents in the afternoon. It is of course the wrong theory for these pronouns of power and solidarity, and it would appear to encode an aspect of the world that is not supposed to receive morphological expression (i.e. objective as opposed to relative time of utterance). Nevertheless, it happened. Are we to explain this rare but creative event in terms of the bioprogram? If, as I have argued, nativist arguments based on universality and invention are logically separable, then idiosyncratic inventions have just as much right to bioprogram status as do inventions that occur far more often. In either case, the "explanation" consists of nothing more than ascribing the event to some catalogue of innate ideas. Obviously such explanations have far less appeal when the invention is rare, because the bioprogram threatens to become infinitely large.

I have already addressed the specificity argument indirectly, by arguing that universal or invented solutions to the language problem may result from more general problem-solving strategies applied to a problem space with unique and task-specific properties. If an invented solution of considerable generality seems to be unique to language, sharing no obvious properties with other cognitive domains, must we conclude that the solution is generated by some autonomous component devoted exclusively to language? I would argue that the specificity of a *cognitive product* does not logically require postulation of an equally specific *cognitive process*. Because Bickerton raises the excellent example of Slobin's (1973) operating principles for language acquisition, let us consider how the product–process distinction applies here. Bickerton takes issue with Slobin only concerning what he regards as the "hen-and-egg" question of whether the operating principles cause universal grammar or vice versa. In the form in which Slobin states his operating principles it is indeed difficult to see how they might apply to or derive from anything other than language itself, as in "Pay attention to the ends of words." However, if these principles were cast into a different format, their relation to other principles of mind might emerge more clearly. An excellent case can be made that many if not all of Slobin's principles are language-specific applications of much more general perceptual and mnemonic principles: figure-ground principles that underlie object perception across modalities (i.e. "Avoid discontinuous elements if you are searching for a unified object"), serial order effects (perhaps responsible for the bias toward ends of words, and if not the ends, beginnings), and so forth. The domain specificity of Slobin's findings may reside more at a descriptive than an explanatory level.

Bickerton does in fact argue that part of the bioprogram may involved nonlinguistic principles of mind, a "cognitive infrastructure" that was in place in phylogeny long before language evolved in our species. However, he seems to accept Chomsky's (1980a) conclusion that this may be true only for the conceptual components of language. Insofar as Bickerton's data reveal computational as well as conceptual candidates for the bioprogram, he concludes that the computational facts must stem from an autonomous and language-specific genetic base. There is another possibility: Just as the conceptual components of language may derive from cognitive *content*, so might the

computational facts about language stem from nonlinguistic *processing*, that is, from the multitude of competing and converging constraints imposed by perception production, and memory for linear forms in real time. Slobin's operating principles are a case in point.

Physicists and other scientists who rely on the manipulation of formalisms in their search for truth recognize that the notational form of an equation often blocks insight into its generality. Progress is made by finding a way to express a particular equation that reveals its similarity to or ability to generate a broader set of formalisms. It remains to be seen whether the unique and language-specific forms described by a Bickerton or a Chomsky can be shown to bear some systematic relation to other properties of mind. However, it is undoubtedly true that this day can be postponed if linguistic findings are described in a format that is maximally unlike the rest of cognition, by scholars who jealously guard their beliefs that language is "special." As Sister Mary Cecilia used to tell us, "All God's children are special." That does not mean that they have nothing of interest in common. I find scientifically reprehensible the kind of scorched earth policy adopted by some of my colleagues with a nativist bent: They describe a peculiarity of human grammar in as arcane a form as possible and then exclaim with glee, "Let's see you cognitive types try and explain that!" A physicist does not extoll with glee the fact that a given equation has bizarre properties that seem to relate to nothing else. Instead, he takes on the challenge of determining how this form might be related to other families of equations, in the belief that an orderly and unified science is preferable. The same approach is possible and desirable in research on language.

This brings me to the final argument, concerning the selectivity of content to be codified in language. The fact that grammars reliably encode only a subset of possible meanings is important and interesting. And yet children show many other selection biases in language acquisition that would be very difficult to explain on strong nativist grounds. Consider some recent findings on first words (e.g. Nelson 1974; Volterra, Bates, Benigni, Bretherton & Camaioni 1979). With a universe full of objects and events to choose from, one year olds are remarkably consistent in the subset of possible referents that they decide to name. If articles of clothing are named, they are likely to be socks, shoes, or jewelry and not the bulkier items of clothing that the child wears every day. Among household objects, utensils and other small items may be named while furniture and large appliances are ignored. If only one parent is named, it is likely to be the parent who spends less time with the child rather than the parent who is always available. And so on. As I understand the strong nativist approach that Bickerton espouses here, these diverse findings must be ascribed to a bioprogram for naming with a multitude of categories specified in advance. And yet there has been too little evolutionary time for the emergence of an innate preference for socks over refrigerators. Nelson (1974) explains the one year old's biases with a much more general principle: a tendency to name those objects that the child can manipulate easily or objects that undergo frequent and interesting changes of state. Similarly, Greenfield and Zukow (1978) have proposed that children prefer to encode the more "informative" or "uncertain" elements in any given situation. Both these accounts explain the selectivity of early lexicons in terms of much more general characteristics of human attention, in other words, the same primitive mechanisms that are responsible for nonverbal orientation to novelty.

To offer another, related example, we have found only one reliable sex difference in our work on early lexical development: a tendency for boys as young as one year of age to talk much more about cars. This tendency holds up even among the sons of feminist colleagues who have struggled against sex typing of toys. It seems unlikely, then, that the sex difference is a product of social reinforcement. What kind of link could there possibly be between testosterone and the internal combustion engine? I cannot answer this question. I think that it is reasonable to conclude, however, that the bioprogram for human sexuality contains no specific informative about cars. Selection biases like this can be innate only by some very indirect route. For all that we know at this point, the same can be said for the selection biases that underlie the grammaticization of meaning.

In all the examples that I have offered, the organism discovers a solution that is not "out there" in the environment. At the same time, these solutions are not "in there" in the genes in any direct way. Perhaps this is all that Bickerton means in his concept of a bioprogram. However, like the "mental organ" metaphor offered by Chomsky (1980a), the bioprogram notion implies far more genetic determinism than we may need in order to explain the interesting data on creolization.

A bioprogram for language: Not whether but how?

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Some sort of a biologically determined program must be operating for the acquisition of language. The discovery of the capacities of the human infant in the last decade leaves little doubt that the accumulation of generations has left its imprint. Consider that human infants can tell the difference between their own mother's voice and a strange female voice in the first few hours of life (DeCasper & Fifer 1980). And infants as young as one month old can hear the difference between categories of speech sounds, such as the difference between *p* and *b* (e.g. Eimas, Siqueland, Juszcyk & Vigorito 1971). We know, then, that certain basic capacities serving communication and language are already in place in the beginning of life. But we do not know how these and other infant capacities relate to language. Are they part of a program that is specifically linguistic? Or are they part of a more general psychophysical program that serves all of perception, including speech perception (see Aslin, Pisoni & Jusczyk 1983)?

The question is not whether there is a biological program for the acquisition of language, but what that program is and how it operates. Bickerton suggests that the program is specifically linguistic. The thrust of his argument rests on the acquisition of pidgin languages as a "first," that is, native, language and the fact that resulting creole languages are similar in certain ways to one another. Acquisition data, then, are crucial to the argument.

With respect to children learning English as their native language, we have accumulated evidence to show that acquisition of verbs is central to their developing knowledge of grammar. In several studies (summarized in Bloom 1981) the verbs that our subjects learned interacted with one or another aspect of linguistic structure to influence how and when that structure was acquired (in particular, the structure of simple sentences, verb inflections, *wh*-questions, and complex sentences). A result of the patterns of acquisition we have observed is a verb typology that includes the molar distinctions of action–state; locative–nonlocative; durative–nondurative; completive– noncompletive. For instance, in their early simple sentences the children learned action verb relations before state verb relations and locative action before locative state relations. The semantic typology of verbs presumably reflected conceptual distinctions in the children's underlying organization of the regularities in events in the world. The critical factor, then, which led them initially to discover the relevant semantic distinctions in the adult language was their cognitive development.

When the children learned new structures (i.e. inflections, early *wh*-questions) they used what we have called "pro-verbs"

("do" and "go" primarily). For example, the pro-verbs were fully inflected virtually from the beginning of the acquisition of verb inflection, whereas more semantically descriptive verbs appeared at first only with the inflection that corresponded to their inherent aspectual meaning (e.g. *-ing* with durative verbs like "play" and past tense with completive verbs like "break"). The children acquired more semantically specific verbs gradually, as they gained increasing control over these early structures, and then in conjunction with learning later-appearing structures. For example, the early *wh*-questions *what, where,* and *who* were used with pro-verbs primarily, whereas descriptive verbs predominated with the later *how, why,* and *when* questions. In these and other ways, the syntactic and semantic properties of verbs were a major factor in the language that contributed to the cognitive requirements for its acquisition.

Initially, form followed function and the first step into language was a cognitive one. I suspect that the same may be true of creoles. The developmental sequence of action relations before state relations in first language acquisition is echoed in the dynamic to static sequence in the evolution of pidgins and creoles reported by Traugott (1977). However, the relation between form and function shifted developmentally for the children we have studied. The shift occurred midway into their third year as the language they were learning, and particularly the verb system, became the dominant factor to influence its acquisition.

Separating the many influences that impinge on language acquisition is difficult. A bioprogram no doubt operates from the beginning. However, the extent to which the influences on acquisition are specifically linguistic, either from within the child or from the language itself in the context of its acquisition, remains to be determined.

With respect to the formation of a creole language, Bickerton proposes that the children learned the pidgin as a first language and creolized it because they did not have access to the dominant language. But what was the input that first-generation pidgin speakers provided their infants at home? If a pidgin language originates in the workplace and the streets as a means of economic survival, why would parents use it to communicate with their children?

If the language used at home was, indeed, the parents' own language, then the creolization of the pidgin took place when the children grew old enough to enter the workplace and the streets themselves, and they learned the pidgin as a *second* language. At this point, social and economic as well as cognitive variables would have interacted with the requirements of the language to determine its acquisition. If so, then the bioprogram for language may be less specific than Bickerton has suggested.

Innate grammars and the evolutionary presumption

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If nothing else, Bickerton's interesting article demonstrates that, as Atherton and Schwartz (1974) originally pointed out, there is no logical connection between the hypothesis of an innate language "bioprogram" and the prevalent nativist insistence that our "language organ" must be species specific. It is hard to see why Chomsky and his followers have tended to assume that "innate" and "species specific" are synonyms, and have argued (for example) that the existence of language universals implies species-specific mechanisms of language acquisition (Fodor, Bever & Garrett 1974) – which is on a par with saying that an underlying universality in color terminology implies that the mechanisms of color vision are uniquely human. The Cartesian tradition embraced by Chomsky (1966) assumes that nonhuman animals can never learn to discourse creatively. This accords with the facts of common experience and with the generally noncreative "utterances" of the various birds and mammals that have been taught suitably stripped-down "languages," but it is hard to square with the evolutionary presumption that at least one mammal (from which we are descended) did in fact acquire fully productive language. As a result, the few nativists that have had anything to say about the origins of the innate bioprogram have tended to dismiss it as an unfathomable mystery or a chance occurrence – for example, by macromutation (Chomsky 1964; 1975; Lenneberg 1964; 1967). The improbability of this sort of explanation remains an embarrassment. Chomsky's (1975, p. 58) riposte that the evolution of physical organs is equally mysterious – that no biologist can "seriously claim to understand the factors that entered into a particular course of evolution and determined or even significantly influenced its outcome" – is false, and would not remove the embarrassment if it were true. (If the creationists ever pick up on Chomsky's Cartesianism, I confidently predict that he will find himself hailed for having proved the existence of a Creator, which is presumably not what he has in mind.)

Bickerton's remarks on the antecedents of language are stimulating, at least to a nonlinguist, and seem to represent an advance in three respects. The first is simply the terminological shift from "language organ" to "language bioprogram," which seems likely to encourage and direct some new thinking about language origins. (The question, Do nonhuman animals have a language organ? tends to answer itself automatically in the negative, for the same reason that Do nonflying animals have a flying organ? does.) The second is the attempt to identify aspects of the "'inner core grammar'" that correspond to adaptively significant aspects of nonhuman cognition, which is a refreshing departure from the Chomskyan distaste for evolutionary explanation. The third is Bickerton's suggestion that aspects of core grammar can be arranged (on the basis of the data creoles) in a hierarchy of universality that ought to reflect the relative antiquities of the underlying cognitive capacities. This seems dubious, but testable. Unfortunately, it isn't clear how any of this could help in attacking the problem of the origins of syntactic structure and productivity, which represent the critical differentiae of language of Chomsky's thought. That stress on the syntactic component of linguistic structure has been forcefully criticized by Atherton and Schwartz (1983). Their criticisms and the results of the ape-language experiments, hint (as does Bickerton) that syntax and productivity may be relatively late and revolutionary developments in the history of language, considerably predated by the appearance of semanticity and arbitrariness. (This would be an example of what Bickerton calls the "giraffe-neck syndrome," which has been more important in evolution than he would like to think.) In the meantime, Chomsky is still talking about our modular language organ, at least to nonprofessional audiences (Chomsky 1983). [See also Chomsky: "Rules and Representations," *BBS* 3(1) 1980.]

On the transmission of substratal features in creolisation

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With Bickerton, I believe that protocreole language genesis can be accounted for in many cases by the LBH, broadly as described in the target article's presentation, "more sharply focused and explicit" than that of Bickerton (1981). That it is more nuanced is largely because Bickerton now takes account of historical data (3.1), although he has not yet gone far enough in

this direction – perforce, since not enough sociolinguistic historiography of creole communities is yet available. I agree with Bickerton that the LBH does not specify the *sole* means through which some characteristically novel features of creole languages may arise. At least some are due to transmission of features from substrate (and sometimes superstrate) languages, a fact undervalued in Bickerton's discussion. I discuss here very briefly four semantactic structures that tend to support this view.

Background. The earliest years of settlement (1721–1735) saw speakers of various West African languages, Malagasy, various Indian languages, and French, all present in significant numbers in Mauritius. Starting in 1736, East Africa became an increasingly important source of slaves, and the last third of the century saw an enormous majority of East African Bantu speakers among all arrivals. This period of massive numerical preponderance of Bantu speakers coincides with a crucial period in the emergence of Mauritian Creole (MC): An earlier period of pidginisation (by adults) and creolisation (by children, with variable input according to their position in ethnosocial space) was ending around 1774, when locally born slaves exceeded for the first time the number of members of the French-speaking "ruling class" (P. Baker's Event 2; 1982:852–55). A homogeneous MC jelled from circa 1774 to circa 1810, when regular introduction of foreign-born slaves ceased. After the abolition of slavery (1835), very many Indian indentured labourers arrived, from various Indian ports; large-scale Indian immigration decreased starting circa 1867.

Four semantactic structures. In MC, the Completive aspect marker *fin* occurs with active predicates:

1. *li fin koze sa fwa la* "he has spoken this time"
   he Com speak this time the

and with nondurative stative ones:

2. *nu mâze fin pare aster* "our meal is ready now"
   our eat Com ready now

In (1), French provides a parallel (*il a fini de parler*), but (2) parallels precisely a Bantu pattern:

3. *ki- -ho- -chiya* (Makuwa)
4. *ni- -me- -choka* (Swahili)
5. *mo fin fatige* (MC)
   I Com become.tired "I am tired"

Cf. also Fortune's description (1955:271) of Shona. Example (2) is also reminiscent of Malagasy:

6. *efa ma- -dio ny trano*
   Com Present clean the house "the house is clean now"

The tense–mode–aspect system of MC reflects overlapping influences. The inclusion of *fin* has modified the bioprogram-generated anterior-based system; *fin* is included because of substratal influences, Bantu and Malagasy agreeing largely with respect to the notion of completive; superstrate *finir de* provides the etymon and a reinforcement of the concept of Completive by occurring with active verbs. (For synchronic and diachronic data and discussion, see Corne 1983.)

Reduplicated numerals occur in two guises. First, as manner adverbials (collectives):

7. *yo vini dis dis* (Haitian; Baudet 1981)
8. *dem come ten ten* [*sic*] (Jamaican; Baudet 1981)
   they came ten ten "they came ten by ten"

This is paralleled in many languages: These include Makuwa, Shona, Swahili, Kikongo (Bantu); Ewe, Twi, Yoruba (West Africa); Malagasy (Austronesian); English (*two by two*); and French (*quatre à quatre*). Second, as distributive numerals used adjectivally:

9. *li don en brok dilo kat kat dimun* (MC)
   he give one jug water four four person
   "he gives one jug of water to/for each group of four people"

This usage, apparently unparalleled in any other creole language, is first attested by Baissac (1880:21, 90). After 1835, Indian languages began to have a perceptible lexical impact on MC (P. Baker 1982: 754–60); this impact extends to syntax as well (cf. P. Baker 1982:856, on MC genitive constructions). Hindi, Indian Bhojpuri, and Mauritian Bhojpuri use reduplicated numerals in the same way as MC. (For data and discussion, see Corne 1983.)

Verb fronting is widespread in Creole languages, including MC, although it does not occur in Hawaiian Creole. An example from Krio (Hancock 1976:16):

10. *no bái yu bái am oh na tíf yu tíf am* ?
    it.is buy you bought it or it.is steal you stole it ?
    "did you buy it or steal it?"

Bickerton (1981:51–56) claims that structures of this type reflect the bioprogram, but I have shown elsewhere (Corne 1984) that a wide range of creole and African data supports neither the LBH nor the hypothesis of transmission from the substratum: Both remain possibilities.

Discussion. Bantu seems to be the immediate source of MC *fin*, especially with nondurative statives, but Malagasy is also important. It is significant that so little of the semantactic complexity of the Bantu tense–mode–aspect systems appears in MC. It is only where Malagasy and Bantu coincide substantially that there is sufficient impetus to modify the "regular" operation of the bioprogram.

Collectives are so widespread, in both super- and substrate languages, that transmission is almost a certainty, again illustrating converging influences.

Distributives in MC are a clear case of substratal influence, occurring *after* MC emerged as a homogeneous creole, and due, one must assume, to widespread bilingualism among Mauritian-born children of Indian immigrants.

Verb fronting occurs in some West African and in many if not all Bantu languages. For MC, it is plausible to suppose that verb fronting, transferred into the jelling creole using French lexical items, would cause no problem of comprehension for most of Mauritius's population (1774–1810). The hypothesis is that large number of Creole speakers were bilinguals who transferred the semantics and syntax of the verb-fronting structures from their languages that had it to the one that did not. This hypothesis is relatively satisfactory for MC verb fronting, less so for other creoles (for a full discussion, see Corne 1984); note, however that a basic form of verb fronting can be derived from Bickerton's rules 25–32.

These examples serve merely to suggest that Bickerton's inferential arguments against the substratophile position, which are derived from the Hawaiian case, may need to be modified in the light of detailed historical sociolinguistic studies, yet to be undertaken for most creole languages. Semantactic convergence in noncreole languages *and* a significant majority of creole speaking and creating bilingual children are sufficient to allow transmission of substratum features within the overall context of creolisation according to the LBH. Of little moment for the LBH, this is important for a fuller understanding of creole languages.

Language acquisition: Genetically encoded instructions or a set of processing mechanisms?

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The hypothesis of a "language bioprogram" is an interesting and provocative one, and Derek Bickerton has presented fascinating evidence in its support from the creation of creole languages in children exposed to pidgin languages. However, some aspects of Bickerton's theory need clarification.

Bickerton places his evidence in the context of determining whether the mind is a general purpose problem-solving device

or whether instead there exists a narrowly specified species-specific program for language. I would suggest that the issue he really raises is whether what can be found to be specifically linguistic is the output of a genetically coded program for language or whether it is the product of ongoing cognitive processes. Bickerton appears to argue for the former position, but much of his speculation is compatible with the latter. Before turning to that issue directly, it is necessary to point out that in the child-language literature there are two separate hypotheses that are often treated together as if they were identical.

The first hypothesis is that children treat language as a structured system, and for this there is a good deal of evidence (e.g. Bellugi 1971; Karmiloff-Smith 1977a; 1977b; 1978; 1979; Newport 1982). This structuring of language by the child into a coherent, interrelated system describable by a grammar would account for the child's inventions and modifications, productions that do not match the adult input, and the interrelated changes that are observed in the syntax. It is possible that such structuring can account for the first two claims of Bickerton's hypothesis, namely (1) that particular creole inventions by first-generation children are innovative rather than being features transmitted from preexisting languages, and, with some additional assumptions, (2) that these inventions show a degree of similarity across linguistic backgrounds too great for chance. It should be noted that evidence for treating language structurally need not necessarily imply any specifically linguistic innate processes, but would be compatible merely with a drive to treat distinctive classes of input (including classes of nonlinguistic input) in a structured fashion.

The second, and quite separate argument, is that the observed structures arise because of language-specific innate programs. This is what is really put forward in the third claim for the language bioprogram hypothesis, (3) that the similarities observed in creole languages derive from the structure of a genetically encoded species-specific program for language. It has never been easy to discover what evidence would be necessary to make this additional claim.

Much of the data presented by Bickerton from creole languages is indeed difficult to account for without considering the possibility of a linguistically specific set of genetically encoded instructions. But he clouds the issue somewhat when he speculates on the reasons for certain linguistic features always being grammaticized in creole languages. The claim that this subset of features "subsumes the universe" is not convincing. Surely a number of other distinctions that are not always grammaticized in creoles are also crucial for categorizing the world for surivival – shapes of objects, causes of events, and threats by predators, to name but a few – some of which are grammatically coded in noncreole languages. More important, Bickerton appears to conclude that this subset of features stems from cognitive antecedents to language, and he hypothesizes "a series of highly modular task-specific cognitive devices interacting with an equally modular and task-specific processing component which imposes a formal structure on the output of the former." This would not appear to be language specific in Chomsky's sense at all. The contrast is not between general purpose and modular mechanisms, as Bickerton characterizes it; rather, it is between a genetically coded program for language (which both Chomsky and Bickerton appear to favour) and language structures arising from ongoing cognitive processes (which Bickerton's speculations on language origins seem to some extent to commit him to).

It is true that Bickerton argues against the notion of ongoing cognitive processes when he criticizes " 'operating principles' " (Slobin 1973) and strategies as being of little use when there are not enough input data to operate on, as is the case with pidgin languages. What, then, is the language bioprogram? It is here that clarification is necessary. Bickerton's evolutionary-adaptive speculations are equally compatible with a view of cognitive mechanisms that work *now* in the child's language acquisition rather than as an innately given core grammar to which they gave rise in the evolutionary past. On the other hand, some of his data – such as the absolute order of tense–aspect–modality in all creole languages – would appear to be very difficult to explain in terms of ongoing nonlinguistic cognitive processes. It is thus unclear what his speculations on the origin of cognitive processes have added to his argument; they appear instead to refute what for him is really the central claim – the existence of a genetically coded program for language – by providing a strong counterargument for at least some part of his data.

Of what precisely does the language bioprogram consist? What are the specific contents of the "core grammar"? It is only when these questions are clarified that we can begin to investigate experimentally whether the human species has an innately given language-specific ability, whether language is the outcome of ongoing cognitive processes, or indeed whether both of these alternatives are true for different parts of the grammar.

Are creole structures innate?

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This commentary on Bickerton's article is a much condensed version of a review (to appear in *International Journal of Linguistics* of his recent book *The roots of language* (1981). It will not be possible here to document all of my arguments as fully as I would like. Those who wish to scrutinize them in greater detail will have to await the appearance of the review. Let me state some of the major ones, however. First, Bickerton has made a number of assumptions about the pidgin stage of a creole all of which are based entirely on the speech of some elderly Japanese, Korean, and Filipino immigrants who arrived in Hawaii between 1907 and 1930. In fact, there is strong evidence (Clark 1979) that by the late 18th or early 19th century a kind of pidgin had developed between English-speaking seamen and native Hawaiians that was linked historically to other forms of Pacific pidgin English and to Chinese pidgin English as well as (although rather tenuously) to the creole English of the New World. These languages share not only certain vocabulary (e.g. *savvy* "know" and *pickaninny* "child, small") but even grammatical features (e.g. *been* a preverbal marker of past time). This pidgin became the lingua franca of Hawaii's polyglot plantation labor force during the last quarter of the 19th century. According to the most detailed account of the history of the language (Reinecke 1969) it took shape among a population consisting primarily of Hawaiians, Chinese, Portuguese, and Anglo-Americans. The Japanese did not begin to immigrate in large numbers until 1888, by which time the pidgin had already become fairly fixed in form, and the Koreans and Filipinos did not arrive until the 20th century. It is even possible that the first Japanese immigrants (before ca. 1900) spoke a very different form of pidgin than did the later ones, because once children of Japanese descent began to grow up in Hawaii a kind of mixed language developed between them and their Japanese-born elders (Reinecke 1969, p. 107, n. 27). It is therefore completely unwarranted to assume that the pidgin as spoken by 20th-century immigrants from Japan, Korea, and the Philippines is in any way characteristic of the incipient stage of Hawaiian Creole English.

There are even more serious weaknesses in Bickerton's theory. Demographic studies of early slave societies in the New World (e.g. Price 1976, esp. table 1, p. 10) reveal an extremely low birthrate among the slaves, attributable to the small percentage of women, reduced fertility among them, and exceptionally high infant mortality. Thus, for several decades the colonial-born slaves formed less than 10% of the total slave

population. Furthermore, even where slaves substantially outnumbered whites the children of the two races were often roughly equinumerous as a consequence of the different birthrates. Finally, contemporary accounts, notably that of Dutertre (1667–71, vol. 2, p. 510), who wrote about 35 years after the colonization of Martinique, stated that nearly all slave children on the island at that time were raised among white families and mixed freely with their children, thus learning only French but no African languages. They had to resort to a pidgin (called a *baragouin* by Dutertre) in speaking to their African-born elders, but were able to speak to one another in the same type of French that the whites used. It seems, therefore, that the creole languages of the New World arose almost entirely as lingua francas among African-born slaves rather than among those who were locally born. In later years, to be sure, when the slaves became the overwhelming majority in a colony and the number of their children increased substantially, the creole often became their only language and in some cases even the only one of the whites. Such was not the case, however, during the formative stage of most (if not all) New World creoles.

In addition, there is evidence that nativization does not necessarily alter the syntactic structure of a pidgin. Kinubi, the Arabic-based creole of an ethnic group in Uganda, is an offshoot of Juba Arabic, a pidgin spoken in southern Sudan. The two separated about a century ago when a group of Sudanese soldiers went to Uganda, and there has been very little contact between the languages since then. The nativization of Kinubi appears to have occurred after the separation, yet the two are syntactically very similar (Nhial 1975).

There are, I believe, explanations of the resemblances among creoles that do not presuppose nativization. First, all of them except Kinubi derive from one of five Western European languages (Dutch, English, French, Portuguese, and Spanish) that share considerable syntactic similarity. Furthermore, Sudanese colloquial Arabic, the source of Juba pidgin Arabic and thus of Kinubi, resembles them in a number of respects. It has predominant subject-verb-object word order and uses preposed particles and auxiliaries to mark tense and aspect. Contact languages tend to select certain features of a target language and to eliminate others. Free morphemes tend to be preserved, and whatever grammatical categories are expressed will use these rather than bound morphemes, which are normally lost. Thus, the preposed tense–aspect markers of creole languages and certain combinations of them represent in part a selection of features from the various target languages. Others, however, appear to be creole innovations, which I discuss further in my forthcoming review.

Substratal explanations of creole features likewise cannot be dismissed (as Bickerton does), even though the substrata varied so much from one instance to another. Certain substratal features are much more easily transferred than others in the context of language contact, specifically those constructions that can be formed by using only basic vocabulary and no morphology, precisely what a beginning language learner acquires first. The most widespread African-like features of the New World creoles are exactly of this type, such as serial verbs – for example, "cook food give [i.e. for] the child" or "take the food go [i.e. away]." Even though not all African slaves (and perhaps not even a majority) spoke languages with this feature, its transferability led to its widespread adoption. Its presence in Hawaiian Creole, on the other hand, can be traced to Chinese and Japanese (e.g. *motte iku* "take go" and *motte kuru* "take come"). It is noteworthy that it is also found in Chinese pidgin English (e.g. "send come, send go"; Hall 1944, pp. 110–11), where Chinese substratal influence is the only possible explanation, since this pidgin was apparently never native to anyone.

Thus, many widespread features of creole languages can be accounted for on the basis of similar structures in either the target or the substratal languages coupled with certain universal processes of selection in the context of language contact.

From pidgins to pigeons

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First, a confession. I am not a native speaker of creole. I am a linguist, but one who has never looked at creoles. To tell the truth, I have never even heard a creole. So I will have to take the risky step of believing Bickerton's data. Risky because when my fellow linguists tell me what I say, what I mean, I am often astounded. However, I will suspend suspicion and believe creole speakers say what he says they say and mean what he says they mean. What follows? From his point of view a very great deal; first of all, the similarities among creoles give us grounds for establishing a single core grammar for all creoles. That sounds reasonable. Considering the way creoles emerge from pidgins in a single generation it follows that this core grammar must be part of the "bioprogram" that leads to acquisition in the first place, a kind of "inner" core grammar from which other core grammars may develop. I'm still with him. There is evidence of child language that sort of supports this guess. But when he gets to his larger claim that the "capacity of pigeons to make categoric distinctions . . . [suggests] that some crucial language infrastructure may have been in place much longer than [the time it took for cortical expansion in the hominid line]" we have to part company. Not because such a possibility is unthinkable to begin with. There may be some fundamental distinctions that are part of the mental equipment of all creatures and they may indeed be things like, Is this x the x I saw last time. (My bet, however, is that recognizing members of a class is more fundamental than recognizing individuals. The real evolutionary advantage comes from being able to represent questions like, Is this the same sort of x as the x I [ate, avoided, mated with last time?].) My peculiar problem is that I share Bickerton's hunch that cognitive capacities have an evolutionary history and further that they may interact with linguistic categories. But a hunch is not a hypothesis. I don't think he has presented us with a hypothesis or research program yet. He says that it is an empirical question, that we need evidence. The problem is that I wouldn't know what sort of evidence to gather, sift, or interpret. I'm not sure what I am supposed to look at, or what I am to compare with what. For example, I suppose that he would want his description of the ability to individuate entities to cover the case in which a bird recognizes its chick as distinct from other chicks. Under his scheme this would mean that the capacity of a bird to tell its own chicks from other chicks is similar to the child making the definite–indefinite distinction in language.

Though that might be a tempting analogy, it's a dangerous one. The child, I would maintain, *understands* the distinction between definite and indefinite. The bird may act on such a distinction, but it is not at all clear that it cognitively operates with this distinction. All that may be happening is that it recognizes a particular smell or a particular place or a particular look, no cognitive processing necessary, only low level sensory matching. What's crucially important is that we believe that the distinctions that humans make are truly cognitive and involve us in intentional predicates like "knows," "believes," "understands," "distinguishes." (Why we believe this is for another time and place.) Now in order to present a continuity hypothesis with real guts you have to tackle head on this problem of intentional predicates. Dennett (1983) did so. He suggests clever experimental strategies for checking on the reasonableness of a *particular* intentional predicate in a *particular* situation. That's what Bickerton doesn't give us – a guide to tell us what counts as an instance of one of his categories and what doesn't, what counts as real evidence and what doesn't. It's not an easy question. Let's move up the ladder from pigeons to apes and consider the ape-language controversy. I have been looking closely at the evidence and arguments about this, and I can tell

you it's no easy problem to sort out. The battles are not so much about what the chimp does, but about what he "intends," "means," "understands," in doing what he does. Premack and Rumbaugh have confronted the problem straight on and have devised a series of experiments that try to tease apart nonintentional interpretations from intentional interpretations. These are for easy cases: Does the chimp really mean apple when he uses the blue plastic triangle or has he just learned a clever game? Bickerton wants us to play a much harder game. His claims are about the higher level concepts like individuation and temporal succession. Remember that what we have to do is to find out about the mind out there, not the one in here. Bickerton may be able to construct a representation of an organism's behavior that uses concepts like "designated individual," and "before," and "after." I'm sure *he* has those concepts. Do *they* is the question?. Now this is not to say the speculation is uninteresting, only premature. To go from pidgins to pigeons in one intellectual bound is too high-stepping for me.

I have a more modest proposal. Let's look at children. We believe already that they are capable of intentional states, that they have minds similar if not identical to ours. Let's first seriously test the hypothesis that children's cognitive development is causally tied to their linguistic development. The careful (and sometimes tedious) work that is needed to show this is underway, at least at the level of the lexicon. It has been shown that the acquisition of particular lexical items is closely linked to the emergence of certain cognitive capacities. For example, the occurrence of a lexical item like "gone," or "all gone," to mark the concept of disappearance occurs precisely when the child has the cognitive capacity to solve problems of a particular level of complexity concerning disappearing objects. Before they can solve the problem they don't have a word for it; as soon as they get the concept at a certain level of complexity they get a word for it. Then they go on developing the concept of disappearance, and it becomes even more complex. Now a chicken and egg problem: Does the fact that the child has attached a word to the concept give him the ability to manipulate it cognitively and make it even more complex, or is the pattern of cognitive development linked to language but independent of it? Pronominal reference comes in at about 18 months in natural languages. Can it be demonstrated that a particular stage in the development of the concept of individuation is concomitant with the occurrence of "you" and "me"? These are the first sorts of questions to be asked. They're not easy to answer, but they are answerable. We have at least some idea of where and how to look. If Bickerton's proposal doesn't hold for us humans, then there is no sense trying to ask about our biological cousins.

What we need from Bickerton is an exact specification of what counts as a cognitive equivalent to the categories and relations in his "'inner core grammar'," a demonstration that these equivalences hold first of all for children, and then some way to tell that animals have these particular cognitive capacities. Then we can seriously test his hypothesis that "the strength of any given capacity (as measured by its appearance or nonappearance in the formal structure of creoles) [is] proportionate to the length of time it [has] been established in earlier species." I'd like to see this hunch become a hypothesis, become a theory. But wishing won't make it so; hard thinking and work will. So I wish Bickerton hard work and good thinking in trying to unravel the central mysteries of language. My guess is that we'll all be at it for a long, long time and that there will be plenty of work left over for our grandchildren.

Grades of nativism

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Bickerton's remarks fall into two parts: first, an empirical proposal concerning the grammatical structure of creoles and the innate bioprogram that makes their acquisition possible, and second, certain metaremarks that suggest, albeit very cautiously, that his language bioprogram hypothesis (LBH) should be seen as a compromise between the antinativism of empiricist theories of language and the rather extreme versions of nativism he associates with the work of Chomsky. In what follows, I would like to suggest that Bickerton's empirical claims should not be interpreted in light of his metaremarks. Thus, even if one concludes that the LBH is convincing and true, a judgment that should be made by those more versed with creoles and child language acquisition than I, it does not lead to Bickerton's view that "less complex innate schemata" are needed or that "the single core grammar that is actualized to varying extents in the course of creolization constitutes the totality of preexperiential linguistic knowledge" or "that the biological infrastructure of language may not necessarily be as complex and extensive as some nativists have suggested."

What is the view of language that Bickerton advances? First, that there is a biological basis for the acquisition of language which is not part of some general all-purpose learning procedures. Second, that this genetically transmitted information is what enables native speakers to develop their linguistic capacities in the face of a rather poor data base underlying the procedure. As Bickerton points out, the acquisition of creoles appears to be a dramatic case of this if one assumes that the linguistic data base for creoles is the pidgins from which they appear to arise.[1] Third, that the specific nonparameterized sort of grammar that Bickerton suggests underlies creoles is all that constitutes the innate endowment of the language faculty. In short, except for the creolelike parts of the grammar of a natural language L, which are innate, all the other parts of the grammar of L are learned.

Let us diagram this part of Bickerton's view and examine it more closely. One can represent the real-time acquisition of grammars by children as the development of a sequence of grammars $G_1, , , , , G_{ss}$ where G_1 is the initial child grammar and G_{ss} is the steady state grammar of the mature native speaker. The process itself can be represented as a two-valued function F (G,d), where the range of the function consists of grammars of various stages m (G_m) and the values of the variables, which are the domain of the function, constitute the grammar of the earlier stage (G_{m-1}) and the data that the child exploits at that stage of the acquisition process (d_{m-1}). Bickerton's LBH claims that G_1 is the grammar of creoles and is innate whereas the features of all the other G_ms are learned and thus not part of the linguistic bioprogram. Note, that if F represents the process of acquisition and we follow Bickerton in assuming that only the properties of G_1 are innate, then for all G_m, $m \neq 1$, the nature of the data d_{m-1} the child exploits will be important in determining the properties of G_m. In particular, differences in the historical sequence of presentation of the linguistic data that the child exploits in the process of acquisition can be expected to have important effects on the shape of G_{ss} if the function F is sensitive to the nature of d_{m-1}. To claim that F is so sensitive, however, is just to claim that all grammars except G_1 are shaped by experience, in other words, are learned.

Given this view of grammatical development, there is a serious problem. Mature native speakers of a given language L, by and large, have robust and, more important, convergent acceptability judgments. However, if *only* G_1 is innately supplied and so the development of G_{ss} is determined in part by the type and order of presentation of the data then we would expect people's acceptability judgments to be more or less random once we got beyond the range of G_1 and speakers had to judge sentence acceptability exploiting their knowledge of languages characterized by G_{ss}. This would be the case if we made the reasonable assumption that the linguistic input that forms the data to the acquisition process differs among people. In short, if people's acceptability judgments converge when dealing with linguistic structures that go beyond the limits of G_1 then this can

be explained in only one of two ways. Either children's histories of exposure to linguistic data are more or less the same (i.e. both the sentence types that children hear and the order in which they hear them are more or less the same) or the emergence of G_{ss} is largely determined by innate factors and so the process of grammar development is largely insensitive to the particular history of exposure to linguistic data that a child experiences.

The first alternative cannot be right. For illustration consider how a child would move beyond the resources of Bickerton's creole grammars. In English, questions and relative clauses are formed by moving a *wh* element to the front of a clause:

1. *a.* [John met who]
   b. [$_{s}$who; [$_{s}$John met ti]]
   c. Who did John meet

So, for example, in (1), *a* is the D-structure of *c*, and *b*, the s-structure, is derived from an application of the rule "move wh." This much is included in Bickerton's account. However, what are not included are the conditions on the rule "move wh." For example, we must explain why "move wh" can apply in (2) but not in (3):

2. Who did John say left the party
3. *Who did John say a picture of amused Sam

We must also explain why *wh* movement in Italian can form relative clauses like (4) but English cannot:

4. * a book which John wonders who bought

It was to answer these sorts of questions that many grammarians felt the need to postulate a richer view of innate endowment than Bickerton appears comfortable with. To explain the difference in acceptability between (2) and (3) it was postulated that movement rules obey the subjacency condition which prohibits the extraction of phrases from certain specifiable complex configurations such as those that underlie (3).[2] To account for the difference between Italian and English as regards the acceptability of (4) it was suggested that subjacency is parameterized. In effect, what counts as a measure of complexity of an extraction site can differ in certain specifiable respects from language to language.[3]

How could these facts be incorporated in a theory like Bickerton's? He could argue either that the subjacency condition is part of G_1 or that it isn't. If it is then the bioprogram must be far more complex than Bickerton indicates. If it isn't then Bickerton must explain how subjacency is learned and why there are convergent agreements among native speakers concerning sentences like (1)–(4). It is hard to believe that anyone has been taught that (3) and (4) are unacceptable in English or that the linguistic environment shaped G_{ss} so that it came to obey subjacency.[4] If this is correct, then a parameterized version of the bioprogram very much like the one that Bickerton counsels against is required and for reasons that Bickerton correctly advances elsewhere; that is, if some aspect of loss is not due to the environment it must be innate.[5]

In sum, it seems that Bickerton's view falls prey to virtually the same shortcomings that he argues undermine hypotheses less nativistic than the LBH. I say "virtually the same" because I think that Bickerton's position in these matters rests on being misled by the case of creoles. Let me explain.

Bickerton exploits a poverty of the stimulus argument in advocating the LBH. He observes that creole *languages* have grammatical structures that are not attested in the *languages* from which they arise. Hence the input cannot be the exclusive cause of the linguistic structures of the creoles. However, this is only one instance of the poverty of the stimulus. What is crucial for determining the innate endowment of children is not what is in the *whole* language they eventually acquire but what is in that *fragment* of the language that they actually exploit – the primary linguistic data. This is substantially less than what exists in the whole language. If one frames the poverty of the stimulus in terms of primary linguistic data and not, like Bickerton, in terms of whole languages, then the strong view advocated by Bickerton is much less persuasive. For Bickerton's immediate purposes his version of the poverty of the stimulus is innocuous and coincides with the version in terms of primary linguistic data. However, once one goes beyond the relatively simple sentences that Bickerton discusses, then the way that one frames the poverty of the stimulus issue becomes crucial. I think that Bickerton may have been led by his main interest in creoles to an incorrect view of what the poverty of the stimulus actually consists in.

Let me add one remark. Bickerton is attracted to the strong thesis that only creole grammars are innate by a methodological principle of parsimony. He appears to believe that less nativistic theories of mind are to be preferred for *methodological* reasons to more nativistic accounts all things being equal. I have suggested above that all things are not in fact equal. But let's say that they were. Why should one accept the view that nativistic theories are less theoretically parsimonious than nonnativistic theories? What does parsimony of the Ockham's razor variety have to do with nativism? Do nativist theories have more theoretical terms just in virtue of being nativistic? Do they invoke more mysterious causal powers? No. They have *different* kinds of theoretical terms and postulate *different* causal processes. They postulate a richer structure to the acquisition device and less richness to the stimulus. However, a more structured mind is not more or less parsimonious than a more structured environment. Methodological parsimony is simply beside the point.

I very much sympathize with Bickerton's view that there is a species-specific innate language bioprogram. But I doubt that his modest nativism is tenable. This should not worry Bickerton, however. For nativism is not something to be modest about.

ACKNOWLEDGMENTS

I would like to thank David Lightfoot and Amy Weinberg for helpful comments on earlier drafts of this paper.

NOTES

1. Bickerton's argument for the LBH is an instance of the poverty of the stimulus argument. For an elaborate discussion of the logic underlying this form of argument see Hornstein (1983), Hornstein and Lightfoot (1981), and Lightfoot (1982).
2. In (3) the *wh* element *who* is moved across at least two bounding nodes, viz. NP and S. Subjacency allows movement across at most one bounding node per application of "move *wh*." For discussion of the details see Chomsky (1981a).
3. In English S is a bounding node whereas in Italian $\bar{S}$ rather than S is a bounding node. This explains the fact of (4)'s unacceptability in Italian but not in English.
4. See Chomsky (1981b) for an elaborate discussion of this point.
5. The subjacency example is just one of many in the literature that lead to the same sort of conclusion. Other cases include the Pro-drop parameter, quantifier types, principles of the binding theory, and preposition stranding, to name just four.

Pidgins, creoles, and universal grammar

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In his thought-provoking discussion, Bickerton argues convincingly that the study of pidgins and creoles provides a unique and fertile testing ground for hypotheses both about the nature of universal grammar and about the biological bases of human languages. Note that he has in effect extended the well-known "argument from poverty of the stimulus" (Chomsky 1980b:3) to the case of creole languages (although he does not use the term). This principle says that properties of languages not deducible from the environmental input are attributable to genetic endow-

ment. The argument holds in the case of creole since, as Bickerton shows, (1) each creole contains many "innovations" or properties not deducible from the input pidgin or substratum languages, and (2) the same properties keep showing up over a wide range of creoles (English, French, etc.) even though the input varies greatly. In what follows I accept these claims and explore the two alternative formulations the author puts forward under his "language bioprogram hypothesis" (LBH).

Bickerton proposes two possible variants of this hypothesis. In the first version, LBH = universal grammar (see, e.g., Chomsky 1980b:9), in which the genetic endowment is a set of principles called universal grammar and the environmental input is a set of parameters (e.g. the head precedes [follows] its complements, X is [is not] an anaphor) which must be set by learning. In the second version LBH = an "inner core grammar," which is essentially universal grammar with many (not all) of its (unmarked) options already set "preexperientially," or in advance of learning. When these settings conflict with properties of the language learned, the new properties are tacked on by "processes of modification or addition."

Bickerton gives a parsimony argument in favor of the second alternative on the grounds that it "entails less complex innate schemata." However, a priori there is no way to decide what counts as more or less complex innate machinery for language. Jacob (1977) has noted that evolution typically patches up or "tinkers" with old genetic mechanisms to build new ones rather than building from scratch a design that might be more efficient from an engineering standpoint. By way of example, Mayr (1982:475–76) has speculated that mammals still produce unneeded gill arches because removing the genetic program for them might upset the entire developmental system. Hence there may be valid evolutionary reasons for more, rather than less, complex genetic schemata.

Second, although very little is known about developmental mechanisms, there is another consideration that might argue against the "inner core grammar" idea and in favor of a universal grammar containing all the core grammars with unrealized options. The fixing of each parameter in either theory ultimately involves the setting of some physically realized option, whether this be in the form of synaptic connections, membrane changes, or even changes in the genetic material itself. The "inner core grammar" version of the LBH seems to imply that during normal language development the nervous system wires itself first with an unmarked (Saramaccan-like) inner core grammar and then rewires itself, if necessary, upon exposure to sentences in the language to be learned. Whatever physical changes are involved in programming the unmarked options of the "inner core grammar," these must all be reprogrammed when these choices conflict with the input language. A cellular analogue would perhaps be if a hemoglobin-producing erythrocyte (red-blood cell), upon finding itself to be in the wrong environment, could reprogram itself to be an antibody-producing B-cell. Such reprogramming is not the usual case in genetics, except in a few instances, as in the pathological reversion of cancer cells to an earlier developmental stage, or by means of experimental manipulation, as in nuclear transplantation (Gurdon 1968). In known cases of gene rearrangement in development as, for example, in the immune system (Leder 1982), no examples have been reported of such rearrangements reverting after the cell has already committed itself in order to allow the cell to follow some other developmental path.

Nevertheless, whichever formulation turns out to be correct, the creole innovations that Bickerton discusses, such as the realized–unrealized complement distinction (cf. Section 3.3.2 of the target article and Bickerton 1982) can in principle provide an independent check for theories of parameters (and markedness) in universal grammar based on noncreole natural languages, as, for example, Stowell's (1982) theory of abstract tense operators based on the realized–unrealized complement distinction in English. Other areas that may shed further light on the alternative formulations of the language bioprogram hypothesis are studies of twins and of the deaf. Twins are occasionally reported to create their own "twin talk," perhaps tapping the resources of universal grammar in ways analogous to creole children. Similarly, it has been reported that deaf children may spontaneously create (ergative) case systems in sign language that do not reflect the case structures of their English language environment (Goldin-Meadow & Mylander 1983:373, n. 6); Bickerton notes similar facts for case relations in creole (Section 2.0).

Of pidgins and pigeons

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Bickerton argues that his extensive studies of creolization provide further support for the view that language is learned by "highly modular task-specific cognitive devices" rather than by a "general-purpose problem-solving mechanism." The issue of modularity is of course central to the study of language and is currently hotly debated. Bickerton also suggests that the general problem-solving view has links to older behaviorist accounts in that it attempts to account for learning in virtually all areas of cognition in terms of the same set of basic content-independent mechanisms. Like behaviorism, this view is also often sympathetic to the notion that the laws of learning are not only the same across all domains but perhaps even across species where the main differences may be only those of capacity; hence pigeons and people may only differ in the power of their cognitive functions, not in their style. Certainly those who claim that apes can acquire language believe that it is acquired via general learning procedures.

Bickerton's version of the modularity notion is the language bioprogram hypothesis (LBH), which states that all children bring to the language learning situation a biological program specifically designed for the acquisition of language. He argues for this conjecture by presenting evidence that:

1. Creoles are created by children, often with virtually no input.
2. All creoles, especially the "purest" ones, share certain fundamental universal properties.
3. Creoles differ fundamentally from pidgins, which are communicative systems that develop without benefit of reference to the language bioprogram.
4. Creoles have many structural properties often not found in any of the possible source languages.

It is clear from Bickerton's discussion that there are those who disagree with each of these four findings. I think Bickerton is right in each case, however, and find his arguments convincing. Moreover, I am very sympathetic with the modularity hypothesis and with his suggestion that advocates of general learning devices share many assumptions with the behaviorists. I do not think, however, that these four findings can on their own be taken as unambiguous evidence for the modularity view. They must necessarily hold for the LBH to be true, but they are not sufficient conditions. In principle, at least, a general problem solver could also account for these four findings.

The findings do show that humans have a built-in tendency to create a natural language with minimal input, which is important since it is not universally believed. Moreover, these findings nicely complement other work, such as that of Goldin-Meadow and Feldman (1977), who demonstrated the creation of sign language systems by deaf children who received little signed input. An innate general purpose knowledge acquisition

device, however, might be able to create language just as it is able to create other sorts of knowledge. Such a view would hold that universals of the created languages merely reflect general properties of this device that pose similar constraints on knowledge representations created in all domains.

Even Bickerton's examples of Slobin's (1973) universal operating principles can be interpreted in this manner; if one looks at how they are stated (e.g. pay attention to the ends of words), one sees they could be consequences of much more domain-general principles (e.g. pay attention to most recent information in serial strings). In fact, in discussing these operating principles, Slobin (1973) explicitly discusses how "general cognitive-perceptual strategies" and "processing limitations imposed by the constraints of operative memory" guide the child's acquisition of grammar (p. 208). Others, such as Bever (1970), have also tried to explain universal properties of language structure in terms of domain-general principles of cognitive psychology.

In short, creole langauges could be created by children, have universal properties, be fundamentally different from pidgins, and yet be learned by means of a general knowledge acquisition device. I don't for a second think this is actually the case; but to show that it is not requires additional arguments and evidence supporting them.

First, and most important, one must make comparisons with knowledge in other domains. This can be done in several ways. One can examine whether the formal structure of the universal constraints on language, especially creoles, is the same as the structure of constraints on other sorts of knowledge systems, such as spatial knowledge, causal knowledge, and the like. One can also examine whether the great creativity observed in creolization is unique to language or whether similar inventiveness is seen in other domains. It is not possible to prove that no other domain could ever have some of the structural features of language, but one can make strong plausibility arguments showing how unlikely it is that some lingusitic constraints could have more general cognitive counterparts and how in some cases it is not even clear how to translate those constraints into another domain.

Second, one can try to show more generally how the acquisition pattern of language seems to follow its own course and is not closely linked to the patterns found in other cognitive domains.

Third, the failure to teach chimps language could be used as evidence against a general problem solver since chimps do seem to be able to learn a great deal else.

One might also try to show the uniqueness of the language acquisition device by focusing on a crucial assumption of Bickerton's work, namely, that there is a critical period in language acquisition during which creoles can be created; otherwise many adults should also progress from pidgins to creoles. Since some have questioned the critical period hypothesis, it is important to address this issue explicitly. In doing so, one might find support for the LBH by showing that other domains do not have critical periods or that they have them at very different ages.

In short, Bickerton's work is strongly compatible with the LBH and constitutes an important piece of evidence, but it cannot rule out a general problem solver alternative until future work is done exploring the particular nature of the universal properties of creoles and explictly comparing them with other knowledge systems.

Finally, I have a question: Why don't all natural languages gradually evolve toward the form of creoles? Bickerton points out how many languages can actually violate universals of the creoles. But since languages do change historically in many dramatic ways, why aren't they gradually sculpted by the children of each new generation into a form that is closer and closer to the canonical creole form? What is it about the socially transmitted aspects of languages that occasionally enables them to override those features of language specified by the bioprogram?

The relative richness of triggers and the bioprogram

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Grammars grow in children as a function of their genetic endowment, Bickerton's "bioprogram," and the linguistic environment to which they happen to be exposed, or the "trigger experience." Scientists make specific claims about the internal structure of the bioprogram using arguments from the *poverty of the stimulus:* They argue that the trigger experience is not rich enough to determine some aspects of the mature system and that therefore the development of the mature system must be guided by internal properties. This form of argument has a long history in biological and ethological studies and over the last 30 years has led to fruitful results in studies of the development of grammars (Lightfoot 1982).

In the linguistic domain hypotheses about the bioprogram and the trigger must meet strict requirements. The bioprogram must provide a basis for a child to acquire any of the grammars that manifest themselves in the world's languages. One therefore cannot say that the grammar of English is genetically prescribed because that would not explain how a child might master Japanese. Similarly, the trigger experience will consist of a fairly haphazard set of utterances that a child encounters in an appropriate context. It will not include information about nonoccurring utterances, paraphrase relationships, ambiguities, or, of course, related forms in other languages; all of this is accessible to a researcher but not to a normal child. Furthermore, the trigger experience cannot be supposed to consist of *everything* that the child may hear, because this would lead us to expect that occasional "performance errors" would trigger some device in the emerging grammar that would perpetuate them; likewise it would leave unexplained that forms and construction types may become obsolete in the historical development of a linguistic community, that is, that certain forms children hear from their parents are not perpetuated through the grammar of the new generation. If data must be fairly robust, frequent, and readily accessible in order to be part of the trigger, one can ask exactly where the limits lie.

There is a trading relationship between claims about the nature of the trigger experience and the bioprogram. If we claim that the bioprogram is extremely rich and specific, our child will not need to derive as much information from the trigger experience as when the bioprogram is less rich.

There have been studies of situations in which the trigger experience is exceptionally poor or distorted. One thinks of studies of children who were isolated from interaction with other people until puberty (Curtiss 1977), or of deaf children growing up in families that do not use sign language and so have virtually no usable trigger as their linguistic (sign) systems develop (Feldman, Goldin-Meadow & Gleitman 1978). Bickerton's work on creoles needs to be viewed in this context. The general properties that emerge in the grammars of children whose trigger consists to a significant degree of "anarchic" pidgin languages need to be explained, particularly when they do not mirror properties that occur in the heterogeneous trigger. Potentially, creole studies done from this perspective could provide a novel and fascinating insight into the nature of the bioprogram.

I write "potentially" for two reasons. For this enterprise to get started, one needs fairly clear and precise claims about the input that creole speakers had from their pidgin and other sources, and about the mature grammars that they eventually attain. The detective work that Bickerton describes in his second section shows how difficult it is to reconstruct with any accuracy the input that the first speakers of Hawaiian Creole had. There is less detective work needed to establish what creole speakers can

say and understand, but, as always, a lot of fine-grained analysis will be needed to make credible hypotheses about the grammars involved and thus about general properties of all creole grammars. Bickerton sketches something of the phrase structure rules of Hawaiian Creole but offers little justification and tells almost nothing about movement rules, properties of anaphors, pronouns and variables, and so on and so on. This simply indicates that the work is at an early stage and will become more interesting as creole corpuses are enriched and worked on by several people from the perspective that Bickerton outlines, just as our knowledge of the grammars of English, Dutch, French, and Italian has increased rapidly through the communal efforts of like-minded analysts.

My second reason for caution concerns the high degree of specificity that Bickerton attributes to the bioprogram. Just as one cannot explain the acquisition of English by saying that the grammar of English is genetically specified, so for creoles. Bickerton claims that the bioprogram provides "a single substantive grammar" which is manifested in creoles; grammars may be further removed from the bioprogram if the trigger experience is more coherent and systematic. This entails two serious problems: (i) why should a language ever develop historically such that its grammar becomes further and further removed from the one provided by the bioprogram, and, most important, (ii) how could a grammar develop properties ontogenetically (i.e. in a growing child) that differ from those given by the bioprogram? Bickerton's answer to the second question is that "the bioprogram grammar [can be converted] into the grammar of any other language by processes of modification or addition" and that a child can develop "more complex forms and structures" which are not given by the bioprogram because they are "guided by input from the target language." The reason for postulating a bioprogram in the first place was that it solves poverty-of-stimulus problems, so Bickerton must now claim that if the bioprogram contains only the core, creole grammar, then where the grammars of noncreole speakers differ from the bioprogram there are no poverty-of-stimulus problems. Until he articulates his bioprogram one cannot argue this point, but he will need to give an account for the poverty-of-stimulus problems identified in the literature for many languages. When he does this, he will almost certainly find that the bioprogram needs to be more open, to be parameterized. When that happens, there will be a convergence: Bickerton will be able to exploit the rich and detailed work on parametrical differences in grammars that one finds in Chomsky (1981a), Rizzi (1982), and the like, and he will illuminate the bioprogram as a result of studying the effects of extremely anarchic trigger experiences.

Bickerton rejects the parameterized view of the bioprogram solely because it entails more complex innate schemata. The complexity of the innate schemata is exactly what we are trying to discover, and there is no reason to say that it cannot be more complex than Bickerton's current hypotheses. Saying that creole grammars are provided genetically leaves us wondering how noncreoles are acquired.

Creolization: Special evidence for innateness?

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It is surprising that in an article supporting the innateness of linguistic knowledge we find the same misguided notions and modes of reasoning used by those who argue against an innate language faculty. Bickerton's work is adding to our understanding of universal grammar, but in arguing that the study of creoles reveals evidence of a new kind for the "bioprogram hypothesis," he accepts certain fallacious assumptions of the critics of innateness. By contrasting the creation of creoles with the acquisition of an established natural language, Bickerton endorses the assumption that children learning a language in the "normal" way are provided with sufficient data to learn certain aspects of the language without innate linguistic knowledge. By arguing that the similarity among creoles provides special support for innateness, Bickerton implies that the similarities among natural languages in constructions that are not exhibited by all creoles can be explained by reference to the history of languages, general cognitive functioning, and nonlanguage-specific learning procedures. But exactly the same reasoning from poverty of the stimulus and nonaccidental shared properties of languages that Bickerton uses to support his arguments can be used to invalidate these assumptions. That is, if Bickerton is right that creolization provides any evidence at all for the bioprogram hpothesis – and I think he is right here – then he must be wrong in supposing that this evidence is special, that is distinct from the usual (Chomskyan) arguments from poverty of the stimulus and universal features.

Bickerton writes, "In order to support the LBH it is necessary to show that all, or at least a substantial part, of the grammar of a language can be produced in the absence of the generation-to-generation transmission of particular languages that is a normal characteristic of our species." But if the bioprogram hypothesis is correct, there is no normal generation-to-generation transmission of languages; each generation, each child, creates the language anew. Bickerton's arguments that the pidgin language could not serve as the inductive base for the creole can be successfully repeated for any natural language replacing "pidgin" with "language of the parents" and "creole" with "language of the child." Without linguistic-specific prewiring, the child cannot "learn" a language by exposure to a well-formed full-blown language.

Similarly, the fact that first-generation creole speakers create creoles that share essential properties does not differ in kind from the fact that children the world over create languages (English, French, Chinese, Chichewa, etc.) that share essential properties. To argue that the similarity among creoles provides special evidence, one must show that creoles are essentially restricted or different in some way with respect to other languages. Bickerton does not show this, and he would be unconvincing if he attempted to do so within his theoretical framework, for his universal grammar categorizes constructions at much too specific a level. Where Bickerton sees differences among constructions cross-linguistically, my comparative linguist colleagues and I see the same universal principles and categories in operation.

Bickerton's general claims about his bioprogram hypothesis do not distinguish his proposal from Chomsky's "principles and parameters" approach to universal grammar. He proposes that

> the single core grammar that is actualized to varying extents in the course of creolization [read, universal grammar] constitutes the totality of preexperiential linguistic knowledge, and that this grammar is of a nature that will permit its possessor to construct or compute all those rules, structures, and features of natural languages that are not explicitly specified in the single core grammar, given minimal exposure to such rules, structures, and features.

But this is just the usual definition of universal grammar. Bickerton tries to draw a distinction between the bioprogram and Chomsky's universals by claiming that his bioprogram provides a grammar, whereas Chomsky's universal grammar provides a schema which must be actualized as parameters are set and rules are determined. But this is a distinction without a difference, if we interpret Bickerton generously.

First, Bickerton's bioprogram does not provide the child with a grammar in any interesting sense. What are the interpretations of the symbols in the bioprogram grammar? What is an S, an NP, and so on? Clearly children need procedures for con-

necting the bioprogram blueprint with the stream of speech they encounter. Bickerton is linguistically naive; he is ignoring much of the work in linguistics of the past decade which has given some content to the categories and relations of grammar, such as noun, verb, subject, and object. When the child who is acquiring a real grammar starts to apply Bickerton's bioprogram to any data (and Bickerton of course has the creole speakers picking up at least a vocabulary from the source pidgin), there will be choices to make, choices Chomsky calls setting parameters.

But perhaps the single most misleading, misguided, and potentially dangerous claim Bickerton makes is that something like his blueprint could be the totality of universal grammar, with other aspects of language being cultural accretions that must be learned by each child with general learning mechanisms. If we look at much of the work in universal grammar today, we find discussion of constructions that fall outside Bickerton's bioprogram. Yet the principles proposed to account for these constructions could not be learned, and languages obey these principles although there is no logical necessity (or reason from general cognitive or learning considerations) that they should. That is, much of syntax, semantics, and morphology that falls outside the bioprogram exhibits the same properties that Bickerton argues for the syntax that falls within it, except that Bickerton himself has not observed the phenomena in the creoles he has studied. Part of the problem here is that Bickerton's grammar is at a much more specific level than the universal principles usually proposed in linguistics. As mentioned above, Bickerton sees differences where comparative linguists see commonalities. In addition to predicting and constraining what Bickerton places in his bioprogram, the broader principles discovered in comparative work shape and determine most constructions in all natural languages.

Also, if Bickerton were right, then we could line up languages in degree of sophistication and cultural complexity. There would be primitive languages – easy to learn but clearly inferior – and advanced languages – more sophisticated and more able to express nuance but at the cost of ease of acquisition. I am not claiming that this is an impossible position, but I would hesitate before I placed such a weapon in the hands of racists who are looking for justification in identifying primitive peoples. I can say without qualification that, except for the obvious fact that languages will differ in the extent and content of their vocabularies, there is absolutely no evidence that languages differ as to complexity in the sense that Bickerton requires. Until Bickerton starts producing more complete accounts of even subparts of the grammars of creoles incorporating the linguistic advances of the last decade, he cannot argue that he has shown creoles to be fundamentally different or impoverished with respect to any other natural language.

How degenerate is the input to creoles and where do its biases come from?

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Bickerton's work is obviously important, and should be looked at seriously in the next years to see how close it is to the actual facts, how valid its interpretations are, and so on. I think Bickerton is right that one of the most serious arguments for some part of language being innately buffered is its robustness, the way it emerges in a wide variety of rearing situations, and the way, apparently, even people with a very low IQ can learn full grammars. Such robustness is characteristic of evolutionarily buffered structures, and it would be surprising if language did not have some of it.

But how much, and of what kind? I find Bickerton's hypothesis at least plausible, and often attractive, too. It is sensible that it is cognitively related, adaptively important structures that would be programmed or buffered, for example. But the data, though supporting this claim, are also ambiguous. They could support a weaker claim that the buffering lies in the cognitive primacy per se of things such as transitivity, event perception, specificity versus nonspecificity, temporal and modal relations, and so on.

The important claim is that children are programmed for both the cognitive relations and their formal realization, as an innate combination. There are two things I'd like to say here concerning the data and their interpretation. The first of these concerns the problem of the quality of the input the children hear, which is obviously the central problem. Future work will have to sort this out. But it is hard to believe even from the examples given that the input the children worked from could have been just the fragmentary pidgin Bickerton presents. At least from the examples, the Hawaiian Creoles have specific lexical items that hardly or never appear in the pidgin. The Hawaiian Creole, for example, has frequent use of *da* as definite determiner, obviously modeled on English *the* (it couldn't come from *this one*, as children don't mispronounce this way). The prominent complementizers *for* and *go* in creoles seem to arise from English uses like *I'm waiting for him to come*, or *you have to go work in the fields*. All these don't seem to be in the pidgin, so the children must be hearing more English somewhere else. Besides this, from the examples, the pidgins have a lot of non-English vocabulary, but the creoles are mostly English. Again this suggests the children must be hearing a lot more English somehwere, or that they are affected by more Englishlike input more strongly. In other words, from what Bickerton presents, I think the pidgins are *too* impoverished even to account for the vocabulary of the creoles. So whoever formed the creoles must have other, presumably more coherent source material. Of course, it is plausible that they still hear a lot of pidgin. But there are many possible hypotheses as to why the dominant language input would be more effective.

Obviously, what comes out in the creoles is not just English. But it could arise from a selectively biased translation of English (or other dominant language) that is better than the pidgins, and much of the selective biasing could come from the cognitive biasing that is natural because of the cognitive centrality Bickerton hypothesizes for the relations per se. Again, if there tends to be change of meaning or use from this original model, such bias could, at least possibly, account for it.

Certain comparisons with children's normal acquisition may also bear on this. Bickerton hypothesizes that things basic in the bioprogram, which are read out in the creoles, should be easier (earlier or more error free or more likely to arise spontaneously) in children's normal acquistion. Sometimes this is true, sometimes not. There are important cases in which it is not true, which seems to indicate expressive or cognitive primacy not associated with particular formal devices. For example, Turkish marks definite grammatical objects (and thus patients) with a postnominal inflection. This contrasts with the prenominal determiners and word-order object marking of the creoles Bickerton studies. But according to Slobin (1982), Turkish-speaking children learn such a system very early, as early as word-order-marked agent–patient relations are learned, and earlier than separate word determiners are learned. One wonders what would have happened if Turkish had been a dominant language for these groups. If this is true, it implies that there is less continuity between what emerges in creoles and what emerges in normal acquisition than Bickerton implies, and that the basic character of transitive relations and definiteness is not associated with a particular formal device. Another example in a recent language evolution is Newport's (1982) analysis that

similar temporal and aspectual relations are marked on verbs in American Sign Language, but they are not marked in a way formally similar to spoken language, again pointing to the expressive function being primary.

Finally, just a comment on the use of the pidginization index, and the use of Samaraccan per se as a basic bioprogram language: As I understand Bickerton's summary, Samaraccan probably had a low pidginization index (PI; cf. target article, Figure 1). But it is also about 300 years old. Language can change a lot in 300 years. It is hard to believe that all of its present structure would come from the initial bioprogram. Apparently, the most recent language Bickerton covers is Hawaiian Creole, which has a higher PI by the other criteria.

These comments argue for the ambiguity of this work. But I think the work is still central, whatever the interpretive outcome, and that outcome could still be close to the one Bickerton supports. As I said already, I find Bickerton's basic ideas about how the very likely innate buffering of language works at least attractive. It is also suggestive that just the systems he concentrates on (specificity, temporal–aspect–reality) are often elaborated in languages to form abstract formal divisions like noun gender, or verb conjugations, or verb–adjective divisions (Maratsos & Chalkley 1980). This all suggests that the basic nature of such distinctions, for whatever reason, makes them easy to elaborate on in the often peculiar way people do. At the same time, what Bickerton finds to be basic is amazingly so compared to the detailed formal apparatuses languages often develop for many things. The idea that the latter could be developments the general faculties and whims the mind imposes on a simple basic grammar (wherever that arises from) is almost as interesting in itself as the hypothesis that the basic framework could be linguistically innate. As Bickerton says, the bioprogram hypothesis, in the form he gives it, or any other, is still just a hypothesis. But the selectivity and bias (which these results already seem to me to demonstrate at least minimally) stand as important facts that normal studies could never show, as Bickerton notes.

Pidgins are everywhere

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In broad outline it's pretty obvious that Bickerton's language bioprogram hypothesis is correct. The only problem is to get it right in specifics. And for this we clearly need all the evidence we can get from a wide variety of sources. I thus found it particularly encouraging to see signs of interaction between the study of creolization and the research program laid out in government-binding theory (Chomsky 1982). I did, however, wonder whether the process of creolization may not be a much more pervasive phenomenon than the usual technical definition implies. My suspicions in this regard can best be conveyed by a minor rewriting of Bickerton's second paragraph. To wit:

The languages to be examined are known as natural languages, which in turn have derived from motherese input. Motherese is an auxiliary language that arises when speakers of different ages are in close contact; by definition it has no prepubertal speakers. A natural language comes into existence when *children* are exposed to motherese; theoretically this process can occur at any stage in the history of mothers, but for reasons that will become apparent, we shall be dealing only with natural languages that come into existence very early in the development of the antecedent motherese. It has long been recognized by developmental psycholinguists that natural languages somehow "expand" and render more complex the motherese that precedes them. . . . The LBH claims that the innovative aspects of natural grammar are inventions on the part of any generation of children that has motherese as its linguistic input.

That is, one of the most surprising discoveries of the late 1960s was that in otherwise normal communities (with a fully formed natural language), some caretakers speak "baby talk" to their infants and young children. Brown (1977) lists over 100 ways in which this "pidgin" differs from a natural language. For example: Mean utterance length is very low, and many of the recursive devices made available by universal grammar are not used; proper nouns are produced where pronouns would be more appropriate, and plural pronouns are used to refer to single individuals; independent pronouns are deleted in non pro-drop languages; fundamental frequency is raised, simple sentences may be assigned more than one primary stress, and falling pitch terminals may be converted into rises.

Language at this level of degeneracy must have constituted a significant part of the input to children learning language around 1967–1977. Yet despite exposure to such deviant utterances, children do manage to acquire the adult language of their community. Indeed, as far as we know, motherese does not even seem to retard the rate of language acquisition (Newport, Gleitman & Gleitman, 1977). Of course, children exposed to English-, Japanese-, or Spanish-speaking mothers are eventually presented with the adult form of the language (or so one hopes). By contrast, Bickerton's evidence that a complex creole can be created from pidgin input in one generation, with very limited substratum influence, must speak to the core of core grammar, that is, to the least marked of all parameter settings. I cannot, however, appreciate the distinction that Bickerton wants to draw between "two possible views of UG." In Bickerton's preferred formulation, the bioprogram, upon minimal exposure to particular features of the linguistic environment, enables the learner to "construct . . . all those rules, structures, and features of natural languages that are not explicitly specified in the single core grammar." This is contrasted with the idea that the child has "latent . . . all possible grammars, although [with] differential weighting attached to the various [parameter] settings." But surely the parameter settings are the mechanisms that mediate between what is innately specified and what is environmentally determined? The two views would only differ if, outside the unmarked setting, any rule at all was possible. I cannot believe that Bickerton would argue that, provided rules 25 to 32 are obeyed, languages may vary in arbitrary ways.

Sign as creole

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Bickerton claims that only in Hawaii can one directly examine the language of first-generation speakers of a creole which was engendered, like most creole languages, by the population movements associated with Western colonialism. However, creolelike languages may well arise in other historical circumstances. For example, in the American deaf community, a variety of demographic, genetic, and educational factors has created a sociolinguistic situation that closely parallels that found in the plantation society in which Hawaiian Creole originated (Fischer 1978). The work of a number of researchers suggests similarities between, on the one hand, gestural language and, on the other hand, pidgins and creoles (Deuchar 1983; Feldman, Goldin-Meadow & Gleitman 1978; Mayberry, Fischer & Hatfield 1983; Newport 1982; Woodward 1973).

The deaf community in the United States numbers some 500,000 prelingually deaf individuals. Of this population, 90% consists of deaf offspring of hearing parents; I refer to them as

"first-generation signers." The remaining 10% consists of deaf children of deaf parents; this small fraction of the deaf community includes all the deaf native signers of American Sign Language (ASL). Most native signers are "second-generation signers"; only a very few have deaf parents and deaf grandparents. Given this demographic context, the vast majority of the users of ASL are not native signers. First-generation signers may gain their initial exposure to ASL as early as age six (if they attend a state residential school for the deaf) or as late as early adulthood. Second-generation signers acquire sign from parents who are not themselves native signers. Only third-generation signers are the children of native-signing parents.

The signing of first- and second-generation signers offers important tests of Bickerton's language bioprogram hypothesis. Like pidgin speakers in Hawaii, adult first-generation signers are linguistically diverse; they have varying commands of ASL, of spoken English, of assorted manual means of encoding English, of so-called Pidgin Signed English, and of "home signs" (signs conventionalized within a single family). Unlike pidgin speakers, first-generation signers typically lack native fluency in any language.

Some first-generation deaf children receive essentially no linguistic input from their parents. These are the children of hearing parents who have elected to educate their children by the oral method and who have been urged by their children's teachers to refrain even from nonlinguistic gesturing. Despite this severely impoverished linguistic environment, these deaf children spontaneously use languagelike gestural systems characterized by gestures for objects, attributes, and actions, by ordering tendencies marking the semantic relations between gestures in multigesture strings, and by recursion (Feldman et al. 1978; Goldin-Meadow 1979; 1982). The "resilient properties" (Goldin-Meadow 1982) of language that emerge in these children's gestural usage are not modeled after the much simpler gesturing of their mothers (Goldin-Meadow & Mylander 1983). Perhaps because the visual modality allows much richer iconic representation than does the auditory modality, these deaf children can invent gestured lexical items and are thus capable of limited linguistic development in the absence of significant linguistic input. Lacking outside pressures, the gesturing of such first-generation signers ought then to be nearer to the bioprogram than is Saramaccan on Bickerton's continuum of creole languages. The properties of these invented sign systems should therefore allow us to specify further the nature of the bioprogram. For example, the presence of ordering rules in the gestural language of these deaf children suggests that the "accidental preponderance of one order in the pidgin" (Bickerton, Section 3.1) will not prove to be a sufficient explanation for the various syntactic orderings found in linguistic systems developed in the face of impoverished input (although, as Bickerton also notes, pragmatic factors may indeed play a role).

Most first-generation signers of these spontaneously developed sign systems subsequently acquire some proficiency in ASL; some also become the parents of second-generation deaf children. This suggests that one substratum to ASL as used by native signers is itself a direct manifestation of the bioprogram. Like the first generation of speakers of a creole, second-generation signers (who are first-generation native signers) receive input from linguistically heterogeneous parents who are not native users of the language employed in the home. Also like those of creole speakers, the mature grammars of second-generation signers are strikingly uniform.

In a discussion of work in progress, Newport (1982) indicates that the vocabulary of first-generation signers consists largely of signs drawn from the "frozen lexicon" of ASL, that is, from the set of signs that lack internal morphology. In contrast, adult second-generation signers use verbs of motion and location characterized by rich, highly componentialized internal morphology. Knowledge of these verbs is, at best, scanty on the part of adult first-generation signers; to the extent that they are known by them, they are treated as unanalyzed amalgams. Newport concludes that the shift from a loose inventory of frozen signs to a tautly organized morphology arises from a distributional analysis by second-generation deaf children of sporadic form–meaning relationships in their parents' signing. She speculates, however, that this type of distributional analysis is not task-specific to language; instead, she adduces evidence that this analytic mode may characterize the way children learn in various cognitive domains. This perspective contrasts with Bickerton's task-specific view of the bioprogram.

In sum, the language development of first- and second-generation signers indicates that, whether task specific or not, the bioprogram (or whatever mechanism leads children to produce linguistic systems that are more elaborate than their input) is not modality specific. Data on language development in the visual-gestural modality will continue to yield important insights into the properties of that bioprogram. The factors that cause 90% of deaf children to be born to hearing parents are not the products of a particular historical era. Fischer (1978) suggests that every generation of deaf children has, for at least 150 years, been forced to recreolize ASL. The demography of the deaf community dictates that this will continue. A surprising prediction follows from this: Although creolization is typically viewed as engendering extraordinarily swift linguistic change, recreolization of ASL by subsequent generations should act as a brake upon linguistic change. This suggests that one substratum of ASL as used by native signers, that substratum being the invented gestural systems of their nonnative parents, is itself a direct manifestation of the bioprogram.

ACKNOWLEDGMENTS

I would like to thank George A. Miller and Elissa Newport for their remarks upon an earlier draft of this commentary. This work was supported by National Institutes of Health grant #HD07205 to the Department of Psychology at the University of Illinois at Urbana-Champaign.

The language bioprogram hypothesis, creole studies, and linguistic theory

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Creole structures and linguistic theory. The best textbooks in TG (transformational generative) grammar suggest that highlighting universal grammar or the set of features that define language must be one of the ultimate goals of the analyses of particular languages. Any analysis that has this orientation is more highly valued than alternatives that do not. In this respect the orientation of Bickerton's language bioprogram hypothesis (LBH) and its predecessor *Roots of Language* (1981) must be acclaimed as a significant contribution to theoretical linguistics. Though interest in creoles is by no means new, Bickerton deserves credit for putting most linguistic theoreticians in a position where they need no longer treat such languages as though they had "marginal" structures, thereby losing the insights that an analysis of them could contribute to linguistic theory.

Aside from some analytic drawbacks, Bickerton has done a good job in pointing out that creoles deserve as much attention from linguistic theoreticians as child language. Some will undoubtedly accuse him of resurrecting the now-discredited baby-talk hypothesis of the origins of pidgins and creoles even though the level at which his LBH applies is that of innovations by children acquiring a first language and turning an erstwhile pidgin into a creole. However, the basic parallels Bickerton highlights between early child language and *prototypic* creoles (i.e. those corresponding to LBH predictions) can hardly be disputed. For instance, both types of languages function with a

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 *Jaaj 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 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 hypothese fail to answer the following: (1) Why has the original structure of the putative protocreole 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 protocreole 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 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) fə kʌm 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 $_S$[*dee o-* [$_V$3[*tei faka*] $_V$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 *wan* "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]

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

b. state–process
c. punctual–nonpunctual
d. causative–noncausative

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 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.[2] 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 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 bioprogram 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 bioprogram 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 bioprogram 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 bioprogram 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 "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

descendants of European immigrants to overseas colonies, where linguistic change may have been rapid, as a consequence of isolation from the metropole, but where it was nevertheless gradual, in that there would be no complete breakdown in communication between generations. Examples would include some varieties of Canadian French, especially when stranded, as in the Mississippi Valley or in cajun Louisiana, as well as the patois of *petits blancs* in Réunion and the islands of St. Barth (département of Guadeloupe) and St. Thomas (American Virgin Islands – among emigrants from St. Barth).

True creoles, on the other hand, would be typically spoken by an ex-slave population, originally working in plantations: Archetypical are the French creoles in the New World and the Indian Ocean, where it is assumed that newly imported slaves communicated in an unstructured jargon, with lexical items plucked from the French of their masters.

Whether there was ever such a polarized distinction is open to doubt; Bickerton's present hypothesis suggests gradation of creolization, which seems inherently more plausible, but he still assumes that creolization is different in kind from more familiar (though not better understood) linguistic change.

We should perhaps exclude from consideration here most Portuguese-based idioms spoken in Asia, and perhaps also in Cape Verde, Guiné-Bissau, and Curaçao, to which the linguistic input was probably a structured pidgin, attested from the 15th century, and which are spoken by people of mixed race whose language loyalties have traditionally been (except in Curaçao) to Portuguese. "Nativization" has been proposed as the term to describe the process by which the pidgin developed into full-blown languages, sharing features inherited or borrowed from Portuguese, but also influenced by other languages, especially in multilingual communities. The version of Portuguese spoken in the Gulf of Guinea islands seems to be more akin sociolinguistically to the French creoles: Bickerton has questioned the accuracy of the available descriptions of these idioms, and so it would be otiose to discuss them here.

More to the point is the fact that French patois share some of the salient features of creoles – which suggests that changes are due not so much to impoverishment of linguistic input to the learning process, as to isolation from the restraining influences of socially prestigious linguistic norms.

Nonstandard French is, in some quarters, known as *français avancé* (advanced French), implying faster progress along a set course of evolution than the conservative, or even reactionary, standard. French creoles can be viewed as having made even more rapid progress along a similar course: Abandonment of gender and substitution of verb inflections by preposed periphrastic particles are the most radical features of creoles, which are adumbrated in the patois, but not fully carried through.

That at least some of the features shared by patois and creole have their origin in uncorrected acquisition errors is quite likely. Bickerton cites the use of stressed disjunctive pronouns rather than preverbal clitics: Modern nonstandard French tends to use the disjunctive alongside the clitic, with the redundant clitic then being dropped frequently in patois, regularly in creole. Elimination of redundancy is a frequent feature of language change, as well as of child language.

Bickerton also mentions the use of negated verb with a negative indefinite subject. This has been a traditional feature of all the Romance languages, except possibly Central and South Italian dialects where Latin usage was more consistently retained (the verb negation was proscribed, as redundant, in standard Spanish and Portuguese in the 16th century). A universal linguistic tendency toward verb negation marking might well be invoked here.

On two points of detail I am doubtful about Bickerton's interpretation of the data. He seems to assume that all creoles have a "definite article" that marks specific noun phrases. The French creoles, unlike the patois, lack a reflex of the French definite article (which had come by the 17th century to act mainly as a gender and number marker). Some creoles and patois use a demonstrative to fulfil anaphoric functions; others make great play of the locative adverb *là* ("there") to pick out nouns, noun phrases, and even clauses. In Haitian the *là* particle seems to have become an obligatory marker of restrictive relative clauses, while remaining optional in other contexts. The assumption that it is a noun phrase marker of "specificity" is, I think, unjustified.

I am also doubtful about Bickerton's analysis of French creole *pu*, which he believes echoes, to some degree, Saramaccan *fu* (Section 3.3.2). Haitian complementizer *pu* and "modal" *pu* both stem from French *pour*, which tends in some nonstandard dialects to become a universal complementizer, especially where some degree of intention is implied. In Haitian its use in this function appears to be fairly recent. "Modal" *pu* is found in Haitian, but not in other New World French creoles: It is not dissimilar to Indian Ocean future marker *pu*, which in Mauritian, but apparently not in Seychellois, has some modal nuance. It seems to originate in the now-obsolete 17th-century French periphrastic construction *être pour* + infinitive "to be about to + infinitive" (again with some degree of intention).

There appears to be little evidence that the *pu* form "marks complements of uncertain or nonaccomplishment" except insofar as it reflects the "intentional" nuance found in French; its modal meaning also appears to derive from French rather than to be a creole creation. Therefore the appearance in Figure 3 of Haitian *pu* in columns 2, 3, and 4, and of Mauritian *pu* in columns 3 and 4, is misleading. As the properties of these particles might be predicted from their French etymon, Bickerton's claim that they can be predicted on the basis of "demographic and historical data" is thereby weakened.

However, sociohistorical conditions are almost certainly related to the extent of change and grammatical restructuring that each idiom – patois or creole – has undergone. Whether "creolization" differs from other linguistic change in nature, or merely in degree, remains still in doubt, as does the role of the bioprogram in language change in general.

Problems with similarities across creoles and the development of creole

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The idea that the individual contributes significantly in the language learning process has been accepted as a part of language acquisition theory. The degree to which this is innate or experiential is a matter for dispute. Whereas some, for example Dulay and Burt (1978), call this contribution "creativity" and identify it with experience, others adopt the Chomskyan argument that if there is nothing apparent in the environment from which an abstract property could be taken then it must be innate.

With specific reference to the development of creoles Alleyne (1979; and 1980a) argues that there is something apparent in the environment to explain similarities in Caribbean creoles. Bickerton disagrees with this, not only by questioning Alleyne's historical interpretation but also by dealing with pidgins and creoles generally. Note, however, that Alleyne (1971, p. 178) had pointed to the following argument which is basically at the origin of Bickerton's LBH: "Some anthropologists are even now considering that some phenomena which had been previously thought to be African derived have absolutely nothing to do with Africa, but are caused by particular social and economic conditions that can exist with similar results." A "universal" argument of this type seems to be of greater theoretical interest than a restricted substratum argument.

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 *fʌ* and *tʌ*:

i fʌ tʌ pʌt it de	"he is (supposed) to put it there"
wʌ fʌ ɔy tʌ ke	"why (= what for?) should I care?"
ɛf ɔy did nuo, ɔy wʌn fʌ tel yʌ	"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.

ACKNOWLEDGMENTS

Financial support for the research that led to the writing of this paper came in whole or in part from the University of Toronto on more than one occasion (from both the International Studies Programme and the Humanities and Social Research Programme), the American Philosophical Society, the Social Sciences Research Council (USA), and the Social Sciences and Humanities Research Council of Canada. Jack K. Chambers made remarks on this commentary that improved it considerably.

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 linguistic 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

appears far weaker than it used to be. It is fairly clear how the idea of a "modular 'language organ,'" unconnected with nonlinguistic aspects of cognition, might yield testable claims; it is much less clear that there is any substance at all in the hypothesis that language depends on some "highly modular task-specific cognitive devices" which are inborn but which are not tied specifically to language. Some of the individual points cited by Bickerton as examples appear to dissolve on examination. "Slobin's [1984] 'Canonical Clause Form'" reads like a linguistic implication of the axiom that, given a job to be done and a set of resources available, if one of the resources will do the job one does not waste effort in acquiring more. I cannot see that the fact that children constructing grammars (or adults in a precreole situation) begin by making the structures of simple sentences serve for subordinate clauses before they develop special subordinate-clause grammar needs a bioprogram hypothesis any more than does the fact that, faced with a tin of paint that needs opening and a screwdriver that can get it open, I do not go to the ironmonger to buy a paint-tin opening device. Likewise, "Wexler's Uniqueness Principle" [Roeper 1981], as stated by Bickerton, does not appear to be "a recent development in learnability theory" but just a statement in linguistic terms of the well-known Popperian axiom that one chooses the strongest theory compatible with one's data – for a child acquiring a language, a strong theory of the language is one that treats forms as ungrammatical whenever experience has not shown them to be grammatical. A disposition to conform to general axioms of theory development such as this is the minimum apparatus that must be ascribed to the child by a sceptic who does not believe in Chomsky's and Bickerton's "modular task-specific cognitive devices."

A point that sounds prima facie more substantial is the finding, quoted by Bickerton from Roger Brown (1976:371ff.), that children acquiring English never overgeneralize the progressive *-ing* suffix although they do overgeneralize most other grammatical suffixes, saying, for example, **comed* for *went* and the like. But what does Brown say about this? He first offers what strikes me as a wholly adequate explanation for this finding, namely, that the applicability of *-ing* is governed solely by a regular principle involving the semantic category "involuntary state" whereas, for example, that of *-ed* is irregular and has to be learned case by case. He then says, "But is it reasonable to suppose that our three children were all able to learn a concept like involuntary state before they were three years old. . . ? Maybe not." He explains that some scholars have embraced an alternative hypothesis according to which "the underlying subcategories are part of the innate knowledge that the human being brings to the language acquisition task." He then argues that there are "fatal difficulties" with this alternative. Brown offers no argument (other than the two words "Maybe not") against the idea that his children did learn the concept between birth and age three, and I read the passage as implying that he believes they did; so it is odd to see Bickerton invoking Brown's findings as support for the language bioprogram hypothesis.

I am not sure that anything could follow from the fact that young children were discovered all to possess concepts such as "involuntary state" versus "process," or the semantic distinction called by Bickerton "specific versus nonspecific" reference," since it seems to me quite arguable that mastery of these very general ontological and epistemological categories might be a necessary precondition for acquiring the more concrete, specific kinds of knowledge we learn when we are old enough to be conscious of learning. (How could one learn that it is wrong to do certain things if one did not already understand the difference between doing and involuntarily being, for instance?) Should that be correct, then the fact that these general categories are possessed by everyone at an early age would do nothing to show that they are innate: Since we know that we do acquire knowledge of the ordinary, concrete, familiar kind, we know that we must previously possess these abstract categories, and if consideration of ordinary knowledge does not force us to adopt a bioprogram hypothesis then no research that reveals that young children do indeed possess the prerequisite abstract categories can make the hypothesis more cogent.

I have not grappled with the heart of Bickerton's target article in Section 3. It is difficult for me to do so, since I am familiar neither with creoles nor with X-bar theory. But when it is so easy to find alternatives to the bioprogram hypothesis with respect to the aspects of Bickerton's argument that I am competent to assess, I must remain sceptical about how much the rest of the argument demonstrates. One point in Bickerton's discussion of creoles that puzzles me greatly is his statement that "no immigrant . . . regardless of date of arrival or linguistic background, has ever spoken anything remotely approximating the creole." A comparable statement occurs in Section 2.0, where it is explained by reference to the "critical period hypothesis." Yet, if this hypothesis predicted that an adult immigrant can never acquire nativelike mastery of a natural language, it would be obviously false: Adult immigrants to Britain sometimes become linguistically indistinguishable from natives except for accent. (Eric Lenneberg, cited by Bickerton as responsible for the critical period hypothesis, did not deny this; 1967:176; he said only that adult second-language learning involves far more conscious effort than children's acquisition of their first language.) If creoles manifest the alleged "bioprogram" in a relatively pure form, one might expect them to be, if anything, *easier* for an adult to learn than mature languages, not harder. So why should they in fact be so uniquely inaccessible as Bickerton suggests? The creole phenomenon, as Bickerton presents it, is so mysterious that I would judge it exceedingly rash to derive from it any far-reaching conclusions about how "ordinary" languages work for people in more familiar societies.

The bioprogram hypothesis: Facts and fancy

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Bickerton's argument for "biological determination of linguistic properties" (Section 4.0) suffers from factual incorrectness as well as from tendentious and often fanciful analyses. Bickerton grossly exaggerates the scope and the character of pancreolisms (i.e. phenomena typical for creole languages irrespective of area or origin). An uncluttered view of the facts reveals a much more sober picture than Bickerton paints. His picture is obscured by an unfortunate tendency to make up for lack of facts by the invention of "analyses." In this commentary I limit myself to purely linguistic aspects and leave out of account Bickerton's daring excursions into language acquisition and even the origins of language.

Bickerton rests his case on three allegedly pancreole phenomena: verb serialization, *fu*-complementation, and tense–modality–aspect (TMA) marking. As regards verb serialization, it is painfully obvious that it is restricted to creoles of West African origin (including São Tomense). To deny the African roots of these languages is as absurd as it is to deny such roots to Caribbean religious beliefs, rituals, and folklore stories (De Groot 1974; Herskovits & Herskovits 1936). Verb serialization is not a feature of any other regional group of creoles, in particular not of Hawaiian Creole (Bickerton 1981:131). *Fu* (and its lexical counterparts) is a very widespread complementizer not only in creoles but also in all Germanic and Romance languages to indicate "purpose" one way or another. Its prominent character made it an obvious candidate for category extension in creole languages. There is evidence (for present-day Saramaccan only) that it has been reanalysed as a verb in Saramaccan (though *not* in Schuchardt 1914, strangely neglected by Bickerton). Bickerton's construal of *fu* as a modal verb in Sranan is a total

fabrication, based on a single locus in Herskovits and Herskovits (1936:166; *So a fu tan dape te den ben kon feni en* [my spelling] "So he had to remain there until they came and found him" – Bickerton's example 68). This sentence is best construed as resulting from a deletion of something like *ben musu* "had to"): *a < ben musu > fu tan*, to enhance narrative vividness. Note that had *fu* in this sentence been a verb, it would have been preceded by the past tense particle *ben*. Finally, the TMA system described by Bickerton is, again, limited to the Caribbean creoles. It does not occur in Hawaiian Creole, despite specious reconstructions. Whether it is a regional innovation or a borrowing is a moot point, but it is not a pancreolism. Also, there are many variations within the Caribbean area itself (see, e.g., Gibson 1982 for Guyanese Creole; Seuren 1981; 1983 for Sranan). If the modals are analyzed as verbs (as Bickerton himself proposes for *fu* in Saramaccan), the principle that modals are not marked for present-durative automatically accounts for the fact that they cannot be preceded by present-durative but can be followed by it (for an analysis of the rule system, see Seuren 1981; 1983). Bickerton's Table 1, moreover, is observationally incorrect, as appears, for example, from his example 45, which is −A (though +Past), +I, and −N. Yet we find *bi* and not *o*. Likewise, Sranan *Fa wi ben o du en now?* ("How would we do it now?") is −A, +I, and +N; yet we have *ben o* and not *sa e*. In short, the factual basis of Bickerton's pancreolist claims is frail, to say the least, and the link with biology is nonexistent.

Bickerton is also off the mark with the history of Saramaccan. Its first speakers did indeed escape from the plantations after roughly 1680, but they already had a native language, Sranan, which came into being between 1650 (the first arrival of slaves) and roughly 1675 (when the last of the English left to make way for the Dutch). There is no doubt that Sranan was born there and then, and the runaway slaves cannot fail to have been in possession of that language, even if there may have been a few newcomers among them. The higher proportion of Portuguese words in Sramaccan compared to English words is a problem precisely because of these historical facts, – a problem for which a variety of solutions is available. There is thus no reason at all to uphold the myth that Saramaccan is closer to biological nature than the other creole languages.

The overwhelming evidence is that creole languages display their typical features regionally, not universally. Those features sometimes result from borrowing, both from substrate (with frequent relexification) and from superstrate, and with modifications due to the collapse of carefully cultivated grammatical systems. And sometimes they result from spontaneous innovation. Whatever is universal to creoles is also characteristic of contact languages of any kind that turn into native languages as a result of circumstances. If established grammar is no longer available, speakers must improvise, and the only means of doing so is to try to make the utterances as *semantically transparent* as possible. It would be a good thing if this statement could be corroborated by fact and theory. Unfortunately, however, linguistic theory is still without a proper psychological theory of semantic processing. Statements about semantic transparency are therefore doomed to remain impressionistic until a good theory is presented. We may surmise that the typical breakdown of morphology seen in contact languages, and the typical use of particles, reduplication, and compounding to make up for lost morphology, represent a "return" to semantic transparency, as does the heavy use made of verbs (as opposed to other word classes), predicates being just about the most basic category in grammar and lexicon. In this respect, creoles do not differ from the Romance languages, the modern Arabic dialects, or, to some extent, English.

A typical example of what is probably an innovation, yet strictly limited to the Caribbean creoles, is the formation of specific question words (wh-words). The typical pattern is that a general specific question marker, taken from a European language, is combined with a marker of person, thing, manner, place, time, kind. In Sranan, for example, the specific question marker was *o* (from English *who*); in the very similar Guyanese Creole, it is *wi* (from *which*); in Haitian it is *ki* (from French *quel*). Thus Sranan has *suma* ("who," from *who somebody*), *san* ("what," from *who something*), *fa* ("how," from *who fashion*), *pe* ("where," from *who place*), *oten* ("when," from *who time*), *sortu* ("which," from *who sort*). (The etymologies are well attested.) Most other Caribbean creoles have similar formations. So far, I have discovered no African or European language that could have been the model for this pattern. It seems reasonable, therefore, to assume innovation. Yet it is strictly regional, though São Tomense is also included. (Bickerton 1981:71 also mentions Indian Ocean creoles, but these derive from French, where *à quelle heure* coexists with *quand*, "when", *de quelle manière* with *comment*, "how", etc.; typically, these creoles have borrowed both formations from French. The absence of this pattern in Hawaiian Creole is "explained" by saying that, since the speakers of its preceding pidgin "acquired the full set of English question words, . . . HCE [i.e. Hawaiian Creole] was never required to develop a bimorphemic set.") If we do indeed have an innovation here, its most likely source is the Afro-Portuguese pidgin in use on the West African coast in the 15th and 16th centuries, and postulated by some as the pidgin source of the Caribbean creoles and São Tomense. There is thus no substance to Bickerton's claim in his Figure 1, that some Caribbean creoles have a lower "pidginization index" than others.

The conclusion must be that sober and modest scholarship still stands in the way of irresponsible flights of fancy.

Child language and the bioprogram

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As Bickerton points out in Section 5, his formulations of the LBH find considerable support in the cross-linguistic literature on child language acquisition. The bioprogram should provide the child with (1) a set of semantic categories for grammaticization, and (2) a set of formal devices for the expression of such categories. The evidence for (1) is quite clear and is receiving increasing support in studies of the acquisition of a number of different languages (Slobin 1984). Such studies also show that some formal devices are more accessible to early learners than others. However, the evidence for explicit innate connections between (1) and (2) does not seem to be as firm as Bickerton suggests. The research strategy is a good one – clearly definable and clearly testable: "where the bioprogram conflicts with the grammar of the target language, one finds delayed learning and frequent cases of systematic error. The 'errors,' however, are often structures that would have been grammatical if the child had been learning a creole language." And in cases "in which features of the target grammar coincide more or less exactly with bioprogram features . . . there will be rapid, precocious, and error-free learning." Careful and detailed analyses of instances of both precocious and delayed acquisition, however, suggest that additional factors may also be at work. Eventually, studies of characteristics common to creoles and cross-linguistic patterns of acquisition will provide a more precise definition of the bioprogram (or what I have called, more broadly, the "language-making capacity").

One set of problems is posed by instances of precocious and error-free learning that do not match the bioprogram. For example, children acquiring agglutinative inflectional languages like Turkish have no difficulty in mastering the use of inflectional case marking, along with pragmatic variations in word order – never going through the stage of noninflectional, word-

order marking of case relations that would be predicted by the bioprogram. Although children do not invent case inflections when they are not present in the input, they can easily be acquired if they are acoustically salient, easily segmentable, and regular. A related issue is raised by the blind child's use of serial verbs in the place of prepositions. Although cases such as this show that children may have access to bioprogram features, such solutions are not inevitable, since most English-speaking children acquire prepositions without passing through a preliminary phase of serial expressions.

Several important points are raised by facts such as these. Bickerton is right in suggesting that something like the semantics of case roles must be innate: The same basic notions underlie Turkish children's use of accusative inflections and the use of ergative inflections or word-order patterns in other types of languages; the same instrumental notions underlie Seth's serial constructions and the prepositional or postpositional instrumentals acquired early on by children learning English or other languages, and so on. Cases such as these have another important common feature: The child expresses a basic notion by a uniform device that has clearly definable substance in acoustic-phonetic terms, indicating an important role for processing constraints in acquisition. The languages of the world provide an array of forms for the grammatical expression of particular notions. It seems overly severe for the bioprogram to narrowly restrict the appropriate means of expression – especially since more than one means is easily acquired. It is also puzzling that languages should have evolved forms inconsistent with the bioprogram.

The LBH must be elaborated to allow for the evolution of linguistic diversity and the ability of children to acquire a range of linguistic forms for particular functions. An important addition would be a sense of the grammatical loci to which particular semantic notions gravitate. This is already implicit in Bickerton's data on the placement of tense, aspect, and modality markers with respect to the verb. Joan Bybee (1983; in preparation) has shown that the order and position of grammatical morphemes correlate with their semantic "relevance" to the lexical unit with which they cooccur, with more relevant morphemes placed closer to the related lexical unit and showing greater tendency toward phonological interaction or binding with that unit. Bickerton is right in suggesting that something like the semantics of case roles must be innate: For example, the same basic notions underlie Russian children's use of accusative inflections (Slobin 1983). Considerations such as these suggest that the bioprogram may lay out "schemas" for possible grammars rather than specifying a single "core" grammar for human language.

To return to the issue of clearly defined, uniform expressions noted above, Bickerton cites my operating principle in regard to canonical clause form: "'If a clause has to be reduced, rearranged or otherwise deformed when not functioning as a canonical main clause . . . attempt to use or approximate the canonical form of the clause.'" He prefers a bioprogram grammar "which allows no reduced, deformed, or nonfinite clauses," though allowing that formulations of end products (grammars) and procedures for building grammars may be "intertranslatable." In my system this operating principle is one of a set of principles to use a basic form or schema across environments – a general principle applying to the forms of words, grammatical morphemes, and clauses. The resulting grammars will be maximally transparent in their form–meaning mappings, allowing the input language(s) to have some role in determining the particular canonical forms in relation to built-in preferences for transparency of mapping. The model is thus not "*solely* input driven," as Bickerton would have it, but an interaction of input with innate structuring tendencies. Surely the first-generation creole children will have had some exposure to a language that could trigger innate formal categories of content words, grammatical morphemes, phrases, and clauses. (Bickerton notes that these children must have been, to some extent, bilingual.) Given such input, children go on to forge a basic grammar consistent with the bioprogram (or, in my terms, following the operating principles of the language-making capacity). We need to know much more before we can specify the contents of that "program." Bickerton's great contribution has been to show us how much we might be able to learn from careful comparison of present-day creoles. If supported by continuing data, the commonality of creole grammars provides striking evidence for "the human species-specific capacity for syntax." The picture will be further enriched by considering common patterns in the range of "developed" languages and their acquisition, along with little-understood constraints of on-line processing and communicative interaction. The basic problem of "modularity" is not solved, but Bickerton gives us another set of important tools with which to deal with it.

Organum ex machina?

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No one would take issue with the claim that our language capacity must depend on various pieces of biological equipment. Language is learned according to a remarkably uniform time schedule across our species, and portions of it can be selectively impaired by lesions in different regions of the brain. Furthermore, several decades of research on language universals confirm the original speculation that all languages are "cut from the same pattern" (Greenberg 1963, p. 255).

The observations Bickerton makes are valuable for the light they shed on the "minimum" pattern of language. Creoles are expectedly embryonic compared with the more mature languages. The latter have elaborated through the millennia by enriching contacts with other languages, by the cumulative development of an oral tradition, and, in some cases, by the aid of a written language that facilitates syntactic complexity. (Witness artificial constructions such as "A, B, C, . . . are D, E, F, . . . respectively.")

However, the fact that language involves biological equipment does not necessarily lead to the "highly modular task-specific cognitive devices" and "equally modular and task-specific processing component" that Bickerton advocates. A piece of equipment may be involved in a certain task, but it may be used for other tasks as well, without being specific to any particular one of them. The devices used in language are surely involved in the more global (and evolutionarily prior) tasks of cognition, memory, and perception. Our goal is to elucidate how these general purpose devices interact with the specific requirements of learning and using language. To posit anything specific to language, in the sense that it serves no other non-linguistic function, seems to me not called for at this point.

Linguists have noted for some time now that all languages share to varying extents a basic vocabulary. No one would seriously propose a *lexical* bioprogram hypothesis as a consequence, since it is obvious that these common words result from the communicative need of similar organisms in similar environments.

The situation is not unlike recent developments in speech production and speech perception (see Wang 1982). Although it is clear that a lowered larynx enhances our phonetic repertoire, there is no compelling reason to believe it is a language-specific adaptation. Rather, the lowering could have resulted simply from anatomical adjustments to upright stance and the restructuring of head and face. Similarly, although infants can be shown to discriminate speech sounds in an adultlike manner, recent

experiments with analogous nonspeech sounds show that this behavior can be "accounted for in terms of general auditory processing capabilities" (Jusczyk, Pisoni, Reed, Fernald & Myers 1983, p. 177).

When the communicative needs go beyond the lexical level, the words get stacked into various hierarchical structures (which will ultimately be linearized for output). Hierarchical structures appear to be a natural product of our cognition whenever we divide and sort the real world, whether we are dealing with objects, events, or whatever. Such structures are everywhere, from scientific taxonomies to the planning of a day's activities (see Simon 1962).

Creole speakers are obviously driven by a communicative need, and it is a fascinating question as to how a common cognitive base all humans share meets this need in highly similar ways in distant parts of the world. Much more data of diverse sorts must be brought to bear on the question. For instance, detailed observations on the schedule of language acquisition by creole children are obviously of critical importance to the question. Should it reveal, for instance, that these children acquire linguistic distinctions that cannot be explained either by the available language models or by their yet limited pragmatic requirements, the LBH would clearly be strengthened. However, in the absence of such findings, I think questions of language evolution can be studied more effectively at this point without invoking any organum ex machina.

Why creoles won't reveal the properties of universal grammar

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To defend the thesis that a substantial portion of the grammatical structure of existing creole languages was created by children drawing on their innate language capacity, Bickerton attempts to show that there was no other possible source for these languages, such as a preexisting pidgin or contact with other languages. This is a difficult task since there is so little historical documentation on the formation of creoles, and numerous objections will undoubtedly be raised by my fellow commentators.

Suppose, however, that it is true that the grammars of creole languages do follow largely from the properties of universal grammar. What consequences does this have for the direction that research in theoretical linguistics on universal grammar should take? Arguments are presented here that the answer to this question is – none at all.

The idea has been popular in the field of pidgins and creoles for a number of years that if pidgins or creoles have simple structures that fairly directly reflect the properties of universal grammar, then, obviously, the best way to find out about universal grammar is to study creoles. Bickerton has supported this view by characterizing ordinary languages as so burdened with historical residue that the properties of universal grammar are obscured, and by criticizing theoretical linguistics for ignoring creoles.

This view is based on the assumption that there are sufficient data in creole languages to determine what those universal grammatical properties are. The problem is that there are dozens of different kinds of grammars that can adequately describe the data of a creole language, and there is no way of choosing between these many alternative analyses without a prior knowledge of exactly what we are trying to discover – the properties of universal grammar that determine the form that grammatical rules can take.

Let us consider a very simple example, the question of determining the phrase structure or constituent structure of a language. It is no easy task to look at a sentence in some language and decide which of the many plausible alternative structures is the correct one. What few tests we have for constituency involve transformations such as movement, deletion, substitution, and the like or clues from other complex phenomena such as case marking, agreement, and so on. The more transformations and grammatical markers that a language has, the more possible constituent structures we can eliminate, and the closer we can come to narrowing the possibilities down to the correct one. Although languages such as English have a fair number of transformations and grammatical markers, the question of the proper constituent structure remains unsettled because there are still many possible analyses that are consistent with the data. The ultimate solution will have to wait until enough information has been gathered from a comparative study of other grammatically complex languages to rule out some of these possibilities. Creoles have few if any movement rules, overt case markers, or the like. These languages would not therefore be of any help in determining the universal constraints on the form of constituent structure. If it is difficult to narrow down the possible choices of constituent structures based on the data from complex languages, of what possible use would it be to limit our study to creole languages with even less evidence on the subject?

Thus, although it might seem obvious to those outside the field that theoretical linguists should focus their attention on creoles if they are serious about investigating the properties of universal grammar, it turns out that just the opposite is true: The languages with the most complex grammatical constructions are most likely to supply information about the properties of universal grammar. We have seen that one reason is that the data of complex languages are consistent with fewer possible grammatical analyses. A second reason is that we can gain a great deal of information from an examination of the interactions of nonuniversal rules with universal rules and principles. Such rules can be acquired on the basis of the kind of data that children commonly hear, but, once learned, these rules can interact with universal rules to produce some very complex phenomena such as subjacency and parasitic gap constructions which the child could not have learned on the basis of the data available to him (cf. Chomsky 1981a; 1982). By studying the interaction of different nonuniversal rules in different languages with the same universal properties, we can eventually build up an accurate picture of what these universal properties must consist of. It is thus the comparative study of such complex phenomena across a range of languages that provides the best evidence about the properties of universal grammar.

Bickerton's view that ordinary languages are burdened with a tremendous amount of irregular structure left over from historical change and that this has overridden the original innate properties has no basis in fact. This view is based on the mistaken assumption that the biological component of language only plays a role in the initial creation of a language and that subsequently languages can develop in entirely orthogonal directions. In fact, the universal component of language plays an important role in language acquisition, which enables a young child to figure out the grammar of a language in a few years when that information has eluded an army of adult linguists for decades. The grammar of each language is constructed afresh in the mind of each child who learns it, and this process prevents the grammar of a language from changing into one that cannot be generated by combining the universal rules and principles of grammar with some set of nonuniversal rules.

A selection of hypothesized properties of universal grammar is mentioned in Bickerton's article to demonstrate that the data of Saramaccan follow fairly directly from current views of universal grammar. These include one version of X-bar theory, the COMP node as a sister to S, and general principles of case

assignment. Although the data from creole languages may well be analyzable in terms of these principles, such hypotheses would never have been developed if theoretical linguists had limited their studies to these languages since these properties are far from obvious in the creole data. Saramaccan has no visible case marking at all, and one would be hard pressed to come to any firm conclusions about the constituent structure of these examples without help from notions of universal grammar developed by a comparative study of more complex languages. Bickerton does propose some universals based on his work on creoles, but these are limited to a set of semantic distinctions which he claims must be grammaticalized in some fashion in all languages. Still, since even Bickerton does not claim that creoles never have any nonuniversal features, we cannot take this as conclusive evidence of universals until we survey other languages.

The conclusion is that although it is quite possible that when we do discover the properties of universal grammar we will find that creoles are perfectly consistent with it, it does not follow that the study of creoles will reveal those properties to us. Nor does even the weaker claim follow, that although creoles may not be the place to start in our search for the universal principles of languages, they are the best testing ground, since they include a few nonuniversal rules. Our problem is not one of figuring out what rules are not universal; we can easily see the absence of a rule in any language, creole or not. Our problem is to determine what the universal rules and properties look like. Here we are operating under a tremendous handicap: If particular rules and properties are innate, there is no reason that they should be deducible from the data of any particular language. We can only see their effects when we study the interaction of these universals with a range of nonuniversal rules in complex languages. Thus if Bickerton's thesis that the grammatical structures of creoles are determined almost entirely by universal grammar and contain few nonuniversal rules is correct, then, ironically, creoles are probably the worst source of data on universal grammar.

Author's Response

Creole is still king

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I was overly optimistic (or, I hope, just premature) in claiming at the end of my target article that the LBH (language biogram hypothesis) would serve as a bridge between disciplines and subdisciplines. Disappointingly, many of the commentators stuck tightly to their own turf, and old arguments reared their familiar heads. If there was a common theme, it was the desire, shared by several (but by no means all) commentators to pull down creoles from the eminence to which I had sought to raise them – a desire that, strangely enough, seemed strongest among my fellow creolists.

First, a few general points: There were some complaints about things I had not said that I should have said. Since I never claimed that my account of the bioprogram was meant to be exhaustive, it was unreasonable (and indeed incorrect; see below) of **Muysken** to state that the LBH does not contain such things as binding theory, or that, if you couldn't prove that Saramaccan *fu* was tensed, you might just as well give the whole thing up. **Roberts,** even more than Muysken, seemed to believe that the *fu* data constituted the bulk of the evidence for the bioprogram. In fact I chose *fu* not because it was even a particularly crucial piece of evidence in itself, but because I had a good spread of data across several creoles, and because it represented a limited domain which I felt could be fairly adequately handled in the space available. I explicitly stated that there was far more evidence than could be surveyed in the target article, and gave indications where to find such evidence. But Roberts seemed prone to some misreading problems, as we shall see.

One omission I regret was dealt with at length by **Meier** and mentioned by others. I am familiar with the fascinating work of Goldin-Meadow and her colleagues (Feldman, Goldin-Meadow & Gleitman 1978; Goldin-Meadow & Mylander 1983) and fully agree that the language-creating capacities of deaf children constitute another window on what must be innate. But it is a steamier window than that of creoles. We as yet know far less about signed than about spoken languages, and, perhaps even more critical, we do not know exactly what the modality differences entail. However universal grammar is constituted, it would be unrealistic to suppose that its manifestations should prove identical in both modalities, since each modality must have peculiar properties that will interact with those of universal grammar and affect its expression. Until we understand these differences, any study that seeks to compare sign languages and creoles will labor under problems of interpretation that may prove insuperable. For this reason I reluctantly avoided the area, even though one recent paper (Edwards & Ladd 1983) found in British sign language many of the features described in Bickerton (1981).

Only one misunderstanding was both genuine and serious. **Cromer** misread my remark that features like punctual–nonpunctual "subsume the universe" as a claim that the short list of semantic distinctions grammaticized in creoles were the only ones conducive to human survival. Absolutely not. By "subsuming the universe," as I thought I had explained in the next sentence, I merely mean that language analyzes all actions and events as either punctual or nonpunctual – there is no intermediate class to which the distinction doesn't apply. Indeed, the fact that the semantic distinctions grammaticized in creoles are a small subset of those grammaticized in language as a whole, and that these in turn are a small subset of the distinctions that human beings are potentially capable of making, is a fact that cries out for explanation. Section 6.0 of my target article was a first attempt in this direction – flawed, doubtless, but one must start somewhere.

Creoles: How do you fancy your facts? I have to apologize for some of my creolist colleagues, because I must devote a disproportionate amount of this response to exposing the baseless claims and plain errors of fact with which their commentaries are riddled. Since the LBH rests primarily on my analysis of creoles, I cannot simply turn a blind eye to these lapses, as I would have preferred to do. To handle things as expeditiously as possible I simply list each claim or error numerically, stating it briefly, naming its perpetrator, then making the correction.

1. A pidgin that arose between English-speaking sailors and Hawaiians became the Hawaiian plantation pidgin (**Goodman**). The sole evidence for this is unsupported conjecture on the part of Reinecke (1969), who in later years freely admitted the limitations of his study (see his letter to me in an appendix to Bickerton & Wilson 1984). In fact, the predominant plantation language prior to 1900 wasn't *any* kind of pidgin English, but pidginized Hawaiian, as the last-mentioned source makes clear.

2. Hawaiian pidgin was fixed in form by 1888 (**Goodman**). There does not exist in the literature so much as a single sentence citation from pre-1888 plantation pidgin English; thus there is no direct evidence that such a thing even existed, still less that it was uniform in structure.

3. Juba Arabic is a pidgin (**Goodman**). Juba Arabic has a substantial native-speaker population and is therefore a creole (Mahmoud 1979; see especially pp. 138–40). Similarities with Ki-Nubi are thus unsurprising, although a paper as brief and superficial as Nhial (1975) is a poor basis for any kind of claim.

4. *Fi* is a main verb in Jamaican Creole (**Mufwene**). Jamaican *fi* was analyzed (correctly) as a modal by Bailey (1966), and nothing since then, certainly not Mufwene's Jamaican and Gullah examples, gives any reason for changing this analysis.

5. I chose the wrong analysis for my example 37 (**Mufwene**). Coordinate VP analyses are ruled out under government and binding theory (and, I suspect, most modern syntactic theories) because they violate the Θ-criterion (Chomsky 1981a, p. 43) by assigning two Θ-roles to a single argument. Moreover (although Mufwene couldn't have known this) Byrne (1983) contains numerous examples of tensed second serials, which entail that the node immediately dominating them must be (at least) an S-node.

6. Serial complement clauses lack independent propositional content (**Muysken**). I'm amazed at this grossly Eurocentric claim from a scholar who has done original work on Quechua. How real-world events are segmented is an arbitrary property of language and not an inevitable property of logic. It would be equally logical to claim that an act of "bringing something for someone" *really* consists of three propositional acts, one of "carrying," one of "coming," and one of "giving" (just as creoles and some other languages analyze things). Moreover, treating verbs as prepositions gets you in real syntactic trouble, because the "prepositional phrase" may be postverbal (as in Muysken's example) but can also be preverbal, as in *a tei di faka koti di bee* (equivalent, on Muysken's analysis, to "he with the knife cut the bread"). Muysken would wind up with bizarre phrase-structure rules if he actually tried to implement his analysis.

7. The *fu . . . fu* sequence in my example 48 could be a sequence of complementizer and modal (**Muysken**). Among single-clause sentences in languages generally, complementizers can only appear in exclamations (e.g. *que seja hawai aqui!*, literally "that be [subjunctive] Hawaii here," a disappointed Brazilian surfer's graffito). Since (48) is not an exclamation, Muysken's claim is an impossibility.

8. I exemplified my theory by writing about *fu* in Bickerton (1973) (**Roberts**). Totally false. Bickerton (1973) dealt exclusively with copulas and pronouns. Bickerton (1971) dealt with *fu*, but neither paper "exemplified [my] theory," or even could have done so, since until 1974 I was a card-carrying substratophile (as Roberts still is). Both the 1971 and 1973 papers deal with linguistic variation and nothing else.

9. My property 55 is shared by English (**Roberts**). English has no systematic means of distinguishing realized from unrealized complements, and *for* is wholly dependent on context for its meaning – compare *I'd have preferred for John to leave* (necessarily unrealized) with *I arranged for John to leave* (realized, by implication).

10. There is no serialization in Hawaiian Creole (**Seuren**). See my examples (9) through (14). If Seuren thinks these aren't cases of serialization, what *does* he think they are?

11. The TMA (tense–modality–aspect) system I describe is a "specious reconstruction" for Hawaiian Creole (**Seuren**). No evidence is offered, nor is any attempt made to dispute specific details of Table 1, where the TMA system of Hawaiian Creole is shown to differ from other creoles only to the extent that conditions discussed in Section 3.0 of my article would predict.

12. Table 1 is invalidated by my example 45 (**Seuren**). Not true. *Bi* is necessary because the subject's *intention* to do something is logically anterior to the *fact* of his having failed to do it. Substitution of *o* would imply that the intention could still be realized and thus contradict the intended meaning of the sentence. As for Seuren's Sranan example, *o* is just a low-frequency alternant of *sa* (Voorhoeve 1962), whereas use of *ben* in a noncounterfactual condition follows from a *change* in the creole system currently taking place in Sranan, easily verifiable from examination of the texts in Voorhoeve and Lichtveld (1975), a widely accessible collection which Seuren surely has, or surely should have, read.

13. Sranan was a native language by 1675 (**Seuren**). No evidence offered, none available. It is simply not known whether a pidgin had creolized in Suriname by 1675, nor even whether what was spoken then can be regarded as "the same as" contemporary Sranan. However, it *is* known that there could have been few, if any, native speakers of any creole in Suriname in 1680, since all slaves from the early (English) period had left by 1678, and even as late as 1690 there may have been fewer than 100 native-born slaves in the entire colony (Price 1976).

14. Guyanese Creole forms all its question words with *wi* (**Seuren**). Completely incorrect. Only one, *wisaid* "where," is so formed. The rest are based on *what* (e.g. *wa plees* "where," *wa mek* "why") or are straight from English cognates (*hau* "how," *(a) hu* "who").

15. Haitian question words differ from those in Indian Ocean Creoles (IOC) in that the latter are straight from French and the former aren't (**Seuren**). Completely incorrect, as the forms themselves demonstrate (sources: Haitian, Koopman 1982; IOC, Papen 1978, cited in Bickerton 1981, p. 71):

i. Haitian *ki bò*, IOC *ki bor* "where" (French *ou*).
ii. Haitian *kumã*, IOC *koma* "how" (French *comment*).
iii. Haitian *ki žã*, IOC *ki maner* "how."
iv. Haitian *ki lè*, IOC *ki ler* "when" (French *quand*).
v. Haitian *pu ki*, IOC *ki fer* "why" (French *pourquoi*).

What was so striking about the above-cited commentaries, especially **Seuren**'s, was the frequency with which they were self-evaluated as exemplars of sober, cautious, and respectable scholarship, in contrast with the "spec-

ious," "fanciful," and "irresponsible" work described in the target article. Indeed, levels of vituperativeness and factual inaccuracy formed an almost perfect correlation – an unsurprising result, I suppose.

Creoles: Some nonarguments. Most creole arguments were little better than the "facts" on which they were based. Some commentators seemed not to realize that arguments they used against the LBH could be used with equal or greater effectiveness against their own claims, or even *in favor* of the LBH. Thus **Seuren** seemed unaware that even if he could prove *fo* was never a modal in Sranan (and "verb deletion" is an unpromising gambit; see Bickerton 1980 on a similar argument in Washabaugh 1975), his areal hypothesis would be in more trouble than the LBH, since in most other Caribbean creoles, *fu* or equivalent certainly is a modal. **Samarin** seemed to think that if the pidgin was variable, with some varieties more developed than others, the learning task of the creole generation would be lightened. But the greater the variability, the greater the likelihood that the child's developing grammar – whether steered by data or by bioprogram is immaterial here – would be continually frustrated by counterevidence, whatever kind of hypothesis it assumed. Moreover, since the input mix would then differ from one child to the next, only some agency such as the bioprogram could explain why, in spite of this, the resulting grammar should be so uniform. **Roberts** asked how I account for some Barbadian sentences with *fu*-like phenomena. He made no attempt to analyze them, so what *he* thinks they represent remains totally obscure. But they look like just the kind of bioprogram phenomena you would expect from a language which, regardless of disputes over its status, originated under just the circumstances we are discussing here.

Even if Barbados had never seen creolization, such examples would prove nothing. One would expect that, in languages generally, but particularly in nonstandard varieties with less pressure toward standardization, bioprogram features would emerge via child language from time to time. Thus **Posner**'s claim that French creoles stem from *français avancé* could just as easily be stood on its head: *français avancé* takes after creoles, since it is just a weaker reflex of the same internal program. Posner had a second two-edged argument: Contrary to what she claims, the fact that French creoles lack a reflex of the French definite article, yet still have definite articles (Posner is splitting hairs if she claims Haitian *-la* isn't one) constitutes evidence *for* the LBH, since it shows that creole children will reconstruct an article system even if, as is usually the case, the original dominant-language system has been filtered out by the pidgin.

Posner's general argument for French influence is just the flip side of the currently more voguish substratophile case. Treatment of the substratum issue was by far the most depressing thing in the commentaries. The target article presented some 1,500 words of detailed and closely reasoned arguments against the substratophile position. Not one commentator even tried to answer those arguments. I might just as well have omitted them and filled the space with more creole data, pleasing **Roberts**, at least. As always – and just as I wrote in Section 4.0! – substratophiles assumed it was sufficient to point to a few surface similarities between some African language(s) and some creole(s) and then expatiate on the foolishness of those who would look further. **Corne** went so far as to hypothesize large numbers of creole-substratum bilinguals, on no evidence save the facts he was trying to account for, totally ignoring my explanation of why even creole-substratal bilingualism doesn't entail substratum influence on the creole. The most moderate stance was **Mufwene**'s; he believed that substratophilia, monogenesis, and the LBH could all happily coexist. What he, and still less the others, failed to realize is that if a feature appears in only one creole whose substratum lacks that feature, it does not matter if there are nine or 99 creoles whose substratum has it – for it has then been demonstrated that substratum presence is no longer a necessary condition for presence of that feature, and substratal and monogenetic explanations become superfluous.

The only novel argument on creoles was **Goodman**'s: locally born slaves were a minority in the early decades of slave colonies and learned the dominant language perfectly; therefore African adults must have invented the creole. But the conclusion does not follow from the premise. The Dutertre (1667–71) citation, which is his only new evidence (population imbalances have never been in dispute), simply confirms what I claimed in Section 3.0 of the target article: Master–slave relations were closer in earlier than in later years, so earlier arrivals spoke something much closer to the dominant language than later ones did. Goodman admits that a pidgin was the local lingua franca, and a pidgin it surely remained until there were enough children who *didn't* have access to the dominant language. The Hawaiian evidence leaves not a shadow of doubt about the relative language-building capacities of adults and children.

At least **Goodman** had some kind of argument. Others, of whom **Samarin** was typical, just threw out unsupported assertions. Figure 3 would be better if reversed. On what rationale? We are not told. I should have explained how pidgin was used in Hawaii and how pidgin and creole speakers interacted. Why? No reason is given. African-creole similarities are too great to be accidental. What might this mean – that African languages and creoles reflect the same bioprogram, maybe? But Samarin is silent.

There is a quality of desperation about these commentaries, a frantic searching for any scrap of evidence, however flimsy or suspect, that might serve to discredit the LBH. This must puzzle the noncreolist reader. Why were they so fervent when they had so little that was new to say and so much trouble getting even that right? Well, threats to the status quo generally elicit reactions of this kind. Creole studies was long a peaceful backwater where people went to the same conferences, met the same colleagues, and – dare I say it? – presented the same papers, year after year. Now those days may be over.

Child language: What, no fallacies? After reading the creolist contributions, I naturally approached the commentaries from child language specialists with some trepidation, since I have no claim to be an expert in that field. However, as I was gratified to find, no one seriously disputed the general outline of my analysis of child language, and what was at issue involved matters of emphasis and interpretation. Some of these, which relate to the modularity question and the relationship between

language and cognition, are best left to the next section.

Bates was bothered by the distinction between systematic and unsystematic error. By her account, since universality and innateness are separable, nonce-form inventions and common ones should have similar status. But here it is the premise that breaks down: The cases she cites, hexagonal honey cells, use of hands in eating, and so on, are universals with transparent explanations (which she gives), and the case of the boy who thought you said *tu* in the morning and *lei* in the afternoon is similarly transparent, given his experience. But systematic error suggests innate forces just because it is *not* transparent – there simply are no good commonsense explanations for why children systematically deform negatives and questions, turn adjectives into transitive verbs, and so on.

Another issue involved **Slobin**'s "operating principles." Slobin (in common with others) did not seem fully to grasp the point that the creole evidence clinches: Any acquisition device *must* be able to operate in the virtual absence of data above the the simple-clause level. In other words, the issue is not one of "*solely* input-driven" versus "input-plus-innate-schema-driven" models, as Slobin supposes, but of a model that can operate in the absence of significant input (its "pidgin" mode) as well as on solely positive evidence (its "normal" mode) versus all others. Here Slobin is extremely vague in his claim that other languages in the contact situation could somehow "trigger" formal categories in the child. Does he mean something like, knowing how to construct a complement clause of unrealized intention in language X helps you to invent a formally quite different clause of similar function in the creole? Put like this, it looks dodgy, but what else could Slobin have meant? I can only repeat: When the input is pidgin, what can "operating principles" operate on?

Both **Slobin** and **Maratsos** were surprised that children can learn Turkish so easily, given that it seems so different from creoles. But is Turkish *really* different? Recall that the bioprogram doesn't supply word order, but does supply case relations (agent of, goal of, etc.) and grammatical functions (subject of, direct object of, etc.). Given no explicit case or function marking in the input (or given irregular marking) the child will impose a configurational solution, as in creoles or the learning of, say, Serbo-Croat (Slobin 1976); where marking is regular and explicit, the child will pick it up quickly. It is not the case, nor have I ever claimed, that the bioprogram predicts a developmental stage of noninflectional fixed-word-order syntax for all languages. As for the "related issue" of serialization by children, how can Slobin claim that most English children don't go through such a stage? Maybe they don't, but how many students of acquisition have looked for serial forms? Indeed, given the leisurely and intermittent schedules of most acquisition studies, it would be easy for them to miss such a development, which must be fleeting even where it does occur.

Next, three misconceptions about acquisition in a pidgin environment have to be cleared up. First, **Maratsos** wondered how a creole could acquire forms rare in or absent from the pidgin. But does this really happen? Maratsos seems not to have grasped the procedure by which forms were filtered down, so to speak, through several layers of deformation, until little but distorted morphophonemic shapes were left – the process is a complex one and was sketched, all too briefly, at the beginning of Section 3.0 of the target article. Meanings and functions were then reconstituted by the creole generation, and apparent similarities to dominant-language features can often be seen to be quite illusory, on closer examination. For instance, Maratsos's *da* is not, contrary to what he supposes, a reflex of *the*. It is derived from demonstrative *that;* conversion of demonstratives into articles is widespread among creoles, witness Mauritian *se* (from French *ce* "this"), Cape Verdean *kel* (Portuguese *aquele, -a* "that"), and this process was necessary precisely because original articles were filtered out in pidginization. There is no good reason to suppose – and many good sociolinguistic reasons *not* to suppose! – that well-formed sentences such as Maratsos cites formed any part of the input to the first creole speakers.

Second, **Bloom** seemed under the impression that creole children came equipped with homes and with parents who shared a language apart from the pidgin. But in the worst slave colonies, with a life expectancy of seven years on arrival, many parents were dead before their children reached language-learning age; parents often had no common language other than the pidgin; and home life was virtually nonexistent, the commonest pattern being one in which both parents worked a 12-hour day (or an 18- or 24-hour day in crop time) while all plantation children under five or so were cared for by one or two women too old or sick to work (children over five being put to work in the weeding gang). Under such conditions, current assumptions about the social matrix of primary acquisition simply do not hold.

Third, **Wang** thought that we should study how creole children acquire language. If he meant how language is created by the first creole generation, he is about 70 years too late; so far as is known, creolization under the relevant conditions is not now taking place anywhere in the world. If he meant how children acquire existing creoles, then this will tell us nothing about how children acquire distinctions in the absence of relevant input. The LBH does predict that children will learn creole languages significantly faster than they learn other languages, and with significantly fewer errors, but I don't think that was what Wang had in mind.

I was sorry no one from learnability theory contributed a commentary. The only reference to learnability, by **Sampson,** unfortunately showed ignorance of basic assumptions in the field. Experience, contrary to Sampson's glib assumption, doesn't tell the child whether forms are grammatical or ungrammatical. It simply presents forms, some of which (the child can't know which) are ungrammatical, while some forms never presented are perfectly grammatical. Moreover, as I pointed out, no preemption principle can work unless the child knows what it means for two structures to be linguistic equivalents, and Sampson didn't suggest any way in which a child might learn this from experience. Incidentally, before taking me to task for ignoring Brown on the -ing suffix, Sampson should have read Bickerton (1981, pp. 156–61), where all Brown's arguments are answered.

Empiricism: Language module versus general cognition. The nearest thing to a full-blown empiricist that the target article flushed from cover was **Sampson,** but he hardly qualified as best of breed, since he kicked the ball through

his own goal by admitting that knowledge of abstract categories is a prerequisite for the kind of concrete knowledge that experience might provide (a point Kant made a couple of centuries ago). But there are only two possible ways of obtaining knowledge, via experience or via innate equipment. What kind of experience could two year olds have that would confer knowledge of abstract categories? If they're not ready for *the cat sat on the mat*, how can they be ready for *all predicates are either active or stative?* – unless, of course, the latter kind of knowledge is innately given.

More cautious commentators worried about the relationship between language and cognition. **Cromer,** for instance, asked whether the phenomena I discussed came from a genetically coded program for language or from "ongoing cognitive processes." Is he trying to suggest that cognition *isn't* genetically coded? Indeed, the empiricists betrayed no realization of the possibility that cognition itself might turn out to consist of a series of narrow, task-specific modules akin to that, or those, devoted exclusively to language. Note, however, that the bioprogram is, as **Cartmill** pointed out, a program, not an organ. But it is a program that takes the output of several "organs," if you like that term – I don't, but I see what Chomsky (1980b) is trying to say with it – and integrates them. One organ may deal specifically with syntactic processing, another with meaning and logical relationships, but the language–cognition issue isn't apples and oranges, it's apples and apples (no pun intended). Therefore it doesn't follow, *pace* Cartmill, that it is unreasonable to speak of a species-specific modular language organ – whatever enables four year olds to master syntax surely merits that description.

However, by "cognition," **Bates, Maratsos, Cromer,** and **Wang** meant . . . well, what exactly *did* they mean? You couldn't tell. Their alternatives to the bioprogram were so vague as to be almost contentless. Cromer talked about structure, and Wang about hierarchy, in exactly the same way: Language was structured and hierarchical, but so were many aspects of cognition. But to claim of any cognitive process that it is structured and hierarchical is hardly more meaningful than to claim it exists – how many formless and egalitarian cognitive processes has one heard of? The real question is, Do language and cognition share the same *kinds* of structures and hierarchies? On all the available evidence, they don't, and until someone comes up with a general cognition analogue of the A-over-A principle or constituent command, there's nothing to discuss.

Some commentators, like **Keil,** were more sympathetic to my position, but reluctant to adopt it unreservedly because a general problem-solving device could not be ruled out. Well, very few of the ideas people no longer believe in were ever "finally ruled out." What happens is that some ideas grow less and less productive in research, yield fewer and fewer insights, while others generate a plethora of new and exciting findings. It's no trick to tell the difference. The motto of winning ideas is "Excelsior!", not "Something may turn up."

But "Something may turn up" was precisely the motto of those who defended general cognitive positions against the LBH. Sometimes this was quite explicit, as in **Bates**'s conjecture that creole similarities might turn out to derive from a set of logically possible solutions that we don't yet know about to a problem that we don't yet understand. Sometimes it was implicit, as in the arguments by Bates, **Wang** and others that because some quite simple and obvious features of language could be accounted for without recourse to the bioprogram, all the more complex features will eventually be explicable in the same way. Well, who could disprove that? But a theory that has to offer promissory notes in place of the hard cash of concrete and specific proposals is well on the road to intellectual bankruptcy, and "proof" and "disproof" are no longer the issues.

Universal grammar: A nativist summit in '85? As befitted fellow nativists, the generativist commentators had on the whole a better grasp of what the LBH was about than any of the other groups. Arguments focused on two main issues: to what extent creoles constituted a special case, and to what extent universal grammar had to be parametrized. Before dealing with these issues, which were by far the most interesting ones raised in the commentaries, I will dispose of a couple of minor points.

I was reading **Jenkin**'s commentary with lively interest – it's nice to hear someone talking about neural infrastructure – until he compared the reprogramming of circuits wired for a creole grammar to the conversion of hemoglobin-producing erythrocytes into antibody-producing B-cells. The analogy is a bizarre one, since there's no reason to suppose that language acquisition of any kind involves change at the cellular level. At most it may involve changes in neural circuitry, and it is well known that circuits are fluid (old ones vanishing, new ones forming) in the early years of life. But even if acquisition were cellular, could Jenkins really believe that creoles differ from noncreoles as much as the cell types he mentions differ?

Then there was **Hornstein**'s ingenious argument that if things were as I described them, speakers of the same language would wind up with different grammars because of variations in the order of presentation of data. But this argument fails to go through on the quite reasonable assumption that, because of developmental factors, a child at any given stage can only attend to a given subset of the data. Order of presentation would then be irrelevant, since children at the same developmental stage would be processing the same subset.

I feel I owe **Hornstein** an apology, though, for this whole argument was only a buildup for a second argument which he would not have needed if I had retained from an earlier version of the article the statement that subjacency held in Saramaccan and that bounding nodes were NP and S. I was trying to cut down on technicalities, but in any case, as already noted, the bioprogram must necessarily have more complexities than could be described in an article primarily designed to justify the central concepts of the LBH. The issue isn't how complex the bioprogram must be, but whether it needs to stipulate more than a single core grammar – and we're coming to that.

First, let's look at the question of whether creoles are special evidence for universal grammar. **Marantz** and **Woolford** were the only generativists to reject this view. **Jenkins, Lightfoot, Marshall,** and **Muysken** all accepted it in some fashion, and **Hornstein** took it under advisement. Woolford's arguments contra were weakened by

unsupported claims – that many different grammars could equally well describe creoles, that universal principles of grammar couldn't have been discovered from the study of creoles – the first of which is probably incorrect and the second of which is certainly undemonstrable. In pursuing these issues, she missed the crucial point: Could creoles give us the unmarked options of universal grammar?

Both **Marshall** and **Marantz,** the former more wittily than the latter, advanced the argument that the creole case was not, in principle, different from that of the normal child. I would agree entirely that what makes creoles special is not a difference in kind from the "normal" child's situation, but rather a difference in degree. Yet quantitative differences in input can add up to qualitative differences in outcome, as all good catastrophe theorists know. Marantz's treatment oscillated between claims that my assumptions were quite different from Chomsky's and claims that my statements were indistinguishable from Chomsky's; somewhere in the middle he too missed the connection between creoles and unmarked options. To put it in his own terms, the normal child receives limited input, true, but an input that is sufficient to determine parameter settings, whereas the input to the child in a pidgin-speaking community is insufficient for this. In consequence, the first child recreates an already existing language, whereas the second child creates a language that didn't exist before.

Marshall saw this, and explicitly accepted my point that creoles must represent (at least) the set of the unmarked settings of all parameters, as did **Muysken** and **Jenkins.** However, neither they nor anyone else would accept that a single core grammar could constitute the totality of our innate linguistic equipment. **Lightfoot,** in a deep and thoughtful commentary, suggested that further development of the bioprogram might lead to a convergence between the LBH and Chomskyan universal grammar. This would seem a priori desirable. Two brands of nativism and one species means there's one brand of nativism too many. Is convergence possible?

Well, convergence is a two-way street, but until these commentaries appeared, hardly a generativist had been willing to put foot to pavement. The ideas of the target article were first expressed, albeit in a much less developed form, nearly 10 years ago. Despite a rather obvious connection between markedness theory and the circumstances of creole genesis, generativists systematically ignored numerous oral and written presentations on these themes. Chomsky, with whom I corresponded in the late seventies, responded much as **Marantz** and **Woolford** did, although with incomparably greater debating skill.

But that's water under the bridge. The question is, Can I show how a child equipped with a single core grammar could acquire the varied core grammars of noncreole languages? Let me begin by conceding that parsimony is not the most appropriate measure when we are considering the (entirely empirical) issue of how much is wired into the brain. I failed to express clearly enough what really worries me about the parameter-setting model of universal grammar – not the quantity of instructions it must contain so much as the fact that some instructions must contradict others: "PP is a bounding node" (for the benefit of future Dutch speakers), "PP is not a bounding node" (for the benefit of future English speakers); "you needn't use subject pronouns" (for the benefit of future Italian and Spanish speakers), "you must *always* use subject pronouns" (for the benefit of future English and French speakers); and so on. Maybe one could interpret the parameter-setting model in other ways, but if so, I wish someone would hurry up and spell them out. Commentators were quick to spot points at which the LBH was ill defined, but they conveniently failed to observe that the parameter-setting model is at least equally vague. As I interpret it, that model seems inherently implausible as an end product of evolutionary process.

But something **Marshall** said hints at a way out of the impasse. He suggested that parameter settings may not themselves be part of the innate component, but mediate between it and the environment. I'm not sure I know what he means by this, but I know what I mean, and it goes as follows:

Let us suppose that on the basis of Saramaccan and other radical creoles, one fully spelled out the bioprogram and found it to consist of:

i. a set of immutable universal principles, that is, principles shared by all human languages, let us say, for the sake of argument, subjacency, the three principles of binding theory (Chomsky 1981a), and similar principles or their equivalents.

ii. a set of mutable constraints on the application of (i).

iii. knowledge that members of (ii) (but not [i]) could be relaxed to varying degrees on the basis of positive evidence.

This model would only positively specify a single core grammar, one very like that of Saramaccan. A child equipped with it, and faced with radically degenerate input, would retain all the restrictions of (ii). A child faced with input that conflicted with one or more of the constraints in (ii) would relax such constraint(s) to the degree indicated by the nature of the input.

A practical illustration may be useful here. Suppose that two of the constraints were an alpha-switching constraint on categories (allowing for [+N−V] and [+V−N], but not [+N+V] or [−N−V]) and a constraint specifying all and only maximal projections as bounding nodes. This would give a grammar with only nouns and verbs (one closely approximated by Saramaccan) and bounding nodes S and NP if one assumes, contra the target article, that S is the maximal projection of V (exactly the case of Saramaccan). Relaxing the alpha-switching constraint and retaining the maximal-projection constraint would add additional bounding nodes (PP, AP) to the grammar. Retaining the alpha-switching constraint and relaxing the maximal-projection constraint would enable bounding nodes to become, say, NP and S^1, as in Haitian Creole. Relaxing both constraints to varying degrees would yield other bounding-node possibilities.

Obviously the above is only a preliminary sketch, and a vast amount remains to be worked out. Such a model has, however, several positive advantages over possible competitors. First, it would provide a principled foundation for parameter settings, arising naturally from the theory. At present, parameter settings have no such foundation, but have to be induced on a hit-or-miss basis from the structural diversity of natural languages; in fact, one could here stand **Woolford**'s argument on its head and ask how on earth one could ever derive the set of unmarked

settings from the tangled complexities of English, French, and similar languages! Next, the model would work in both the modes that an acquisition model must be able to work in: the pidgin mode (null or hopelessly conflicting evidence) and the normal mode (positive evidence only). No one has claimed that any existing model can do this; indeed Chomsky (1981a, p. 9) doubts whether his model would work without indirect negative evidence. Finally, since the model predicts that children will obey (ii) until parts of (ii) are disconfirmed, it also predicts that the bulk of systematic error in children's speech will consist of the production of creolelike forms. None of the acquisition experts among the commentators even tried to deny the accuracy of this prediction. But the parameter-setting model makes no predictions at all about systematic error (another point the generativists conveniently forgot to mention).

The success of any such model depends, of course, on its being able to specify the content of (ii) in such a way that (i) and unrelaxed (ii) yield (substantially) the grammar of Saramaccan, while (i) and relaxations of (ii) (i.e. the relaxation of varying combinations of constraints, probably to varying degrees) yield the full range of core grammars of natural languages. It will be a long haul, but preliminary results look encouraging. **Marantz,** I confidently predict, will say that my position is now completely indistinguishable from Chomsky's; I'll leave readers of the last paragraph to decide that for themselves.

The rest of the story. There were a few other good points that don't easily fit into general categories. **Muysken** noted that different versions of the LBH differed with regard to the extent to which the bioprogram emerged in natural languages. I hope the preceding paragraphs have clarified this somewhat: Some parts of the bioprogram are immutable and some are mutable. But there may also be differences connected with the different modes of organization of syntax and semantics. I feel that somehow the bioprogram is more plastic in the latter than the former; it is a point that will bear more thought and more investigation.

Keil, with several other commentators, asked why all natural languages don't approximate the condition of creoles. I'm often asked this question, which seems to reflect a misunderstanding of how the biological endowment of species works. For instance, Marler (1977) has shown that the innately wired version of some bird species' song emerges only in birds that have been deafened or isolated at birth; all birds reared under normal conditions learn and use the song dialect that happens to be current in their particular area. All biological programs seem to allow for this space for variability and would probably be maladaptive if they didn't. Language does not grow in a vacuum, but interacts with many factors, inborn as well as environmental – the phonological erosions of rapid speech, diverse and diverging cultural needs, the desire to play and to create idiosyncratically, the conservativeness that embalms chance inventions, strategies for compressing information (as in a list like this, which would require horrendous circumlocutions in Saramaccan), and so forth.

Gopnik was the only commentator who looked right down the pike toward the concerns of Section 6.0 of the target article. I found myself nodding sagely at her caveats on the dangers of attributing intentionality to nonhumans, or even babies, until a sudden thought struck me: Hey! wait a minute! Would I really want or even need to establish that a lower creature meant the same as we mean, before claiming that the distinction it made was ancestral to the similar one that we make? Gopnik invoked Dennett (1983) to buttress her case, but I don't think that was quite the point Dennett was making. I even think Dennett might agree with me that first you must have the brute capacity to make a distinction, then the capacity to make it on purpose, then the capacity to know that you are making it, then the capacity to know that it is you who is making it, and so on, and that these capacities are not a random scatter but a series of evolutionarily linked and sequential stages derived from growing sophistication in information processing and increasing linkages between different modules of mind. I suspect too that advances in neurobiology will eventually shed more light on these issues than the kind of neo-Piagetian behavioral investigations Gopnik seemed to envisage. For when I talked about the strength of semantic distinctions being proportionate to the length of time since the start of the capacity to make those distinctions, it was the brute capacity, not the intentional capacity, that I had in mind – and, despite Gopnik, I'll stick with that.

One last word of sympathy for **Sampson,** who regretted that creoles and their crucial evidence should be located so depressingly far from the preferred stamping grounds of white middle-class academics. Yes, and it was just as unsporting of Mother Nature to fix things so that only people with particle accelerators can do advanced physics. But that's how the world is, folks, and creoles and quarks are here to stay.

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