appears far weaker than it used to be. It is fairly clear how the idea of a "modular 'language organ,'" unconnected with non-linguistic 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.O, 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-