

Derek Bickerton's Bioprogram: A Proposal and Its Critics

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One of the more fascinating and far-going hypotheses put forth within the field of linguistics the last couple of decades is the creolist Derek Bickerton's *Language Bioprogram Hypothesis* (henceforth LBH). The basic idea behind LBH – i.e., the hypothesis – is that by looking at how creole languages are created, and similarities between different creoles in particular, making comparisons with child language acquisition, one can come to conclusions concerning how the human race developed language originally, be it thousands or millions years ago.

These, rather strong, claims have naturally met with strong reactions, positive as well as negative. This paper aims at presenting LBH in a curt form; what Bickerton's basic ideas and motifs are, and how he supports them. Another aim is to briefly outline the reactions LBH has caused from a plethora of different disciplines such as acquisition studies, neurology, linguistics, psychology, anthropology, and even creole studies.

Given the limited scope of this paper, I have refrained from including the numerous language examples provided by Bickerton and his critics; they mainly prove his (or his critics') points, and may for the present purposes be considered as accurate.

The Bioprogram Hypothesis I: *Roots of Language*

The basic idea is first presented by Bickerton in his book *Roots of Language* (1981; henceforth *Roots*). Bickerton presents three questions in the Introduction, to which he will try to provide “at least a partial answer” (*Roots*, p. xi) , viz.:

- 1) How did creole languages originate?
- 2) How do children acquire language?
- 3) How did human language originate?

Naturally, the answers to questions 1 and 2 are the prerequisites for answering the third question.

One of Bickerton's points is that the three fields concerned by the questions above are traditionally treated as unrelated, and that in order to provide answers to each of them, one needs to look into the other fields to obtain sufficient knowledge.

Roots is divided into five chapters that deal with, respectively:

- 1) Creoles and pidgins.
- 2) Features Bickerton claims to be shared by most creoles, thus providing support for LBH.
- 3) Language acquisition in general, mainly child language acquisition
- 4) The foundations for the LBH, taken to be a species-specific structure in the brain.
- 5) An attempt to answer to foreseeable criticism.

Given the clear lines of thought in Bickerton's presentation, we shall stick to it in this paper. What follows immediately is a short summary of *Roots* in the order just presented above.

Roots... Pidgin into Creole

The quest for the origins of language has been seen as impossible not only since linguistics as a science was born, but even long before modern times. Part of the problem is that, if one doesn't believe in language as a (literally) God-given gift to humanity the question "what was the first language?" cannot have an answer, since no language would, or could, definitionally, possibly be the first language – there would, and must, always be a predecessor.

There is, however, an exception to that observation, since there is a set of languages whose origins with a fairly decent degree of accuracy can be traced, and that, consequently, can be dated: those languages are generally referred to as *pidgins* and *creoles*. These languages can be dated, since they (most of them, at least) are the products of the European colonial expansion between 1500 and 1900. The Europeans, most often English, Portuguese, Spanish or French, made contact with a large number of different populations, and in order to trade, enslave or whatever lay in the interests of the Europeans, *lingua francas* were created, almost always a mixture of the local language(s), and/or the language(s) of the immigrant slave population and the language(s) of the colonists.

The traditional definition of *pidgin* refers to any such *lingua franca* that has arrived at a fairly high degree of stability, acknowledged by both (or more) parties, but still is not the native tongue of any person. The traditional definition of *creole* is a pidgin that is learnt as a first language, i.e., a pidgin that has become the native tongue for some individuals, later a group of people, and eventually a whole population.

Many such languages still exist, the most known of which perhaps is the English-based Tok Pisin, spoken in New Guinea.

However, Bickerton modifies the traditional definition of a creole, inasmuch as he requires that a creole must be created out of a pidgin that had not existed for more than a generation – which rules out Tok Pisin – and arose in a population where no more than 20 percent of the population spoke the dominant (colonial) language and the remaining 80 percent spoke several languages. Why, then, does Bickerton put these constraints on the definition of a creole? He explains:

“By limiting our research area in this way, it becomes possible to concentrate on those situations in which the human linguistic capacity is stretched to the uttermost.” (p.4)¹

One of Bickerton's main points here is that children growing up in such a community face a situation very different from that of the normal child. Or, as he puts it:

“[E]very existing theory of acquisition is based on the presupposition that there is always and everywhere an adequate language to be acquired” (p. 5)

Bickerton is of the opinion that in certain slave communities, there existed no fully-fledged language, but only, rather primitive pidgins for the child to be exposed to. Thus, the child was not exposed to a language the way most children are, and therefore had to “create” its language itself. This, in turn, reveals to us what the basis of human language looks like. If we can identify creoles that arose in communities adhering to the definition presented above, and compare structural and other properties of these creoles, then we would gain knowledge about the human language apparatus in general, and in doing this, also about the origins of language. Bickerton claims that there is at least one such creole: Hawaiian Creole English (HCE) that arose out of Hawaiian Pidgin English (HPE) in a

¹The quotation will be from *Roots* until further notice is given. This is done in order to avoid too many “ibids” in the text.

very short time – less than a generation, according to Bickerton – and much of his argumentation is based on observations of these two languages.

In 1973 and 1974, Bickerton and colleagues made recordings of people who were immigrant speakers of HPE and of locally-born speakers of HCE (Bickerton & Odo 1976; Odo and Givón 1976; Bickerton 1977). By comparing the languages, Bickerton has drawn conclusions as to the creativeness of the human language apparatus.¹

According to Bickerton, creolization (the process that turns a pidgin into a first language, i.e., a creole) occurred around 1910, at the very latest 1920.

There were considerable differences among the HPE speakers depending on the time of arrival at Hawaii, and also depending on what first-language they had, e.g. Japanese, Filipino or another language. For example, considering basic word order, Japanese speakers varied between 30 and 60 percent SOV sentences, whereas Filipinos exhibited between 15 and 50 percent of VS sentences (S most often being a full noun). This, and other similar observations, leads Bickerton to the conclusion:

“[A] child in Hawaii who sought to learn basic word order by intuitive processes alone would have ended up in total bewilderment.” (p. 9)

According to Bickerton, HPE presented a learner with problems, partly since there were no clear or consistent rules to learn, but also, from a communicative point of view, since several features of natural languages were largely or completely missing, like:

- Consistent marking of tense, aspect and modality
- Relative clauses
- Movement rules
- Embedded complements
- Articles

Most often, HPE sentences consisted of nouns and verbs paratactically strung together, whereas HCE, like all native languages, have the above features. So, how did they evolve? One answer would of course be that the first language of the HPE speaker influenced the children who learned HCE as his/her first language. This cannot be true, says Bickerton, since

“[T]he erasure of group differences in that generation was complete. Even locally-born persons cannot determine the ethnic background of an HCE speaker by his speech alone, although the same persons can readily identify that of an HPE speaker by listening to him for a few seconds.” (p. 15)

This would imply that while HPE variants are influenced by the speakers’ native languages, the same is not the case with HCE. This, in turn, would – or at least could – imply that the grammatical complexification in HCE would have another source than the respective native languages of the parents to the children who first learned HPE as their first language (thus turning into HCE).

¹It is not obvious that the people he recorded would still speak the very same language as they spoke when they were young in the 1920’s, and Bickerton discusses this question. However, given the limited scope of this paper, we shall waive the issue here.

In the rest of the chapter, Bickerton examines differences between HPE and HCE by looking at the following phenomena:

- Movement rules
- Articles
- Verbal auxiliaries
- *for-to* complementization
- Relativization and pronoun-copying

Bickerton draws the conclusion that none of the differences in these fields can be explained by looking at the substrate languages – i.e., the native languages of the slaves – since HCE forms often do not adhere to any of the existing native languages of HPE speakers. When discussing articles, he makes the observation that:

“HCE speakers, however, follow neither the under-generalization of the Japanese speaker nor the over-generalization of the Filipino speak.” (p. 23)

When looking at specificity, he concludes:

“We must conclude, as with word order, that the zero marking of non-specifics was an HCE ‘invention’ and one firmly rooted enough to override counterevidence from other languages known to its speakers.” (p. 26)

Focusing on sentential complements, he states that:

“... HCE has made two distinct innovations, one semantic, one syntactic. The syntactic innovation consisted of taking *fo* and *go*, a preposition and an imperative marker, respectively, and using them to introduce embedded sentences, which were themselves an innovation. /.../ [T]he semantic innovation—distinguishing realized from unrealized complements without precedent in HPE, in English, or in any of the substrate languages.” (p. 33)

When seeking an explanation for these, and other, innovations, Bickerton argues that there are but two possible alternatives: one is that they are produced by a general problem-solving device, the other being that they are the result of “innate faculties genetically programmed” (p. 41). Bickerton ends the chapter with the words:

“If Hawaii were the only place where people had been faced with the problem of reconstructing human language, it would be impossible to decide between these alternatives. However, Hawaii is far from unique. There are a number of creole languages in other parts of the globe, but produced under very similar circumstances. /.../ If some general problem-solving device were at work, we would not expect that in every different circumstance it would reach the same set of conclusions. /.../ However, if all creoles could be shown to exhibit an identity far beyond the scope of chance, this would constitute strong evidence that some genetic program common to all members of the species was decisively shaping the result.” (pp. 41–42).

In the subsequent chapter, he sets forth to prove that the latter of these alternatives is in fact the correct one.

Roots... Creoles

In this chapter Bickerton looks at different linguistic phenomena, and makes comparisons between, in his view, unrelated creole languages. A problem here, he acknowledges, is that creoles, unlike many non-creoles, lack comprehensive descriptive grammars, and that, even if they are described in grammars, the “grammars stop where the syntax gets interesting” (p 44). He also points out that, considering the two creoles that he is most familiar with (HCE and Guyanese Creole), all previous descriptions are flawed and misleading. All this makes it very hard to vouch for the exactitude with which observations can be made.

Creoles are, as Bickerton points out not “ab ovo creations”. They are the results of their preceding pidgins (of course), but they are also influenced by both substrate languages (i.e., the local language(s) or the native language(s) of the non-dominant population) as well as superstrate language(s) (i.e., the language of the dominant population). One must always consider the possibility that a given phenomenon has its basis in one of these languages, and it might occasionally be hard to decide whether or not this be the case.

Yet another factor that plays a role is that of *internal change*, something that occurs in all languages irrespective of ancestry, and is far from predictable in any strict sense of the word.

Moreover, a *contact-induced change* is the process of *decreolization*, a term referring to what happens when a creole renews (or maintains) contact with its superstrate language, something which is attested for a number of English-based creoles.

All this presents the creolist with obstacles when it comes to deciding the origin or basis of a given phenomenon. Bickerton argues against what he refers to as “substrato-maniacs”, i.e., people who try to find the sources of creole structures in *substrate languages*.

The opposite school is referred as the *monogenesis* school, which claims that all creoles have the same ancestor, an Afro-Portuguese pidgin developed in West Africa in the 15th and 16th centuries.

Bickerton then compares an array of different creoles in order to establish linguistic similarities. I shall briefly summarise Bickerton’s findings (claims), following the original order.

Movement Rules

Bickerton points out that focused constituents are moved to sentence-initial position in HCE. He then establishes that this is true for all (sic!) creoles. Given the fact that natural, non-creole languages, exhibit a wide variety of focusing means, none of which occur in creoles, this is taken to support his hypothesis that there must be some innateness at work here.

Articles

Here, Bickerton states, there is a well-nigh complete correspondence between HCE and all other creoles. All creoles have the same set of articles, a definite article (for presupposed NPs); an indefinite article (for asserted NPs); and zero (for nonspecific NPs). Again, this occurs irrespective of what article system the superstrate or substrate languages had.

Tense-Modality-Aspect (TMA) Systems

Most creoles express tense, modality and aspect with three preverbal free morphemes. If they co-occur they occur in the TMA order. Moreover, the (literal) meaning of the particles is the very same in several creoles: the tense particle indicates anterior; the modality particle expresses irrealis and the aspect marker expresses nonpunctual.¹

Realised and Unrealised Complements

All data available to Bickerton exhibits a structure “identical to that of HCE, i.e., complementizers selected by the semantics of the embedded S” (p. 59).

Relativisation and Subject-Copying

Here some differences between HCE and other creoles exist, inasmuch as most creoles have relative pronouns, while these are lacking in HCE. This, however, is explained, Bickerton argues, by the fact that HCE has not had the time to obtain them, an explanation supported by the fact that among the creoles who possess relative pronouns, there exist conservative dialects in which these are deletable in subject position. (I will not deal with subject-copying here, given the limited space.)

Negation

Creoles normally negate nondefinite subjects, nondefinite VPs and verbs in negative sentences. HCE shows, albeit weakly, some general tendencies in that direction.

Existential and Possessive

Many creoles use the same lexical item to denote existentials (“there is”) and possessives (“have”). This is never the case, Bickerton claims, in the superstrate languages concerned. HCE adheres to this completely.

Copula

More or less all creoles show similarities here. Since adjectives are surface verbs in creoles, they do not require copulas.

Adjectives as Verbs

Here Bickerton notes that in several creoles the adjectives form a subcategory of stative verbs. He comments:

“This resemblance between creoles so widely separated in location and origin is quite striking. Moreover, I know of no creole where an alternative analysis of adjectives would be required.” (p. 69)

¹In HCE, the particles are *bin* (tense), *go* (modality) and *stei* (aspect).

Questions

All creoles have the same syntactic structure in questions as in statements, and if question-particles are used, they are always placed at the end of the sentence, and are optional.

Question Words

If a question corresponds to a WH-question in English, the WH-word is placed before the declarative form of the sentence. The question words are most often directly borrowed from the superstrate language, but the general pattern is so clear that Bickerton predicts that if that were not the case, the creole would develop a similar set of forms.

Summary

Having thus looked at eleven parameters, Bickerton concludes that in three cases, HCE and all other creoles are identical, and in six there are great similarities, despite differences on a detail level. In the remaining two cases there are some similarities. Only in two fields, HCE differs markedly from the general pattern. Bickerton:

“This degree of identity is quite remarkable when we consider that HCE shares none of the substratum languages of the other creoles—except that a superstrate language for some creoles was a substrate language in HCE, i.e., Portuguese! /.../ The only thing HCE seems to have in common with other creoles is that all have European superstrates” (p. 73)

Seemingly, Bickerton presents the reader with heavy evidence as to the alleged similarities between creoles. He also point out (as in the quotation above) that substrate languages cannot account for these similarities. He further notes that some of the ‘typical’ creole features cannot be found in the superstrate language either. In his own words:

“We have seen that even taking into account the, in some cases, several centuries of time that have elapsed since creolization, and the heavy pressures undergone by those creoles (a large majority) that are still in contact with their superstrates, these languages show similarities which go far beyond the possibility of coincidental resemblance, and which are not explicable in terms of conventional transmission processes such as diffusion or substratum influence. /.../ Moreover, we find that the more we strip creoles of their more recent developments, the more we factor out superficial and accidental features, the greater are the similarities that reveal themselves.” (p. 132)

This leads him to propose the LBH.

Roots... Acquisition

In this chapter, Bickerton treats language acquisition by children. His hypotheses here are that features being ‘close’ to the bioprogram (i.e., typical creole features) should be easier to learn for children. He gathers evidence from child acquisition studies, describes what features children over-generalise, under-generalise, seem to learn without problems or have great difficulties with and so forth. With a few exceptions, Bickerton states that most acquisition studies confirm the LBH. He lists four main areas where acquisition and creoles are on a par.

These are:

- The distinction between *specific* and *non-specific*, proven by the universality among creoles of zero versus indefinite article, and the errorless acquisition in English.
- The distinction between *state* and *process*, where creoles exhibit skewed verbal systems as to their distribution of nonpunctuals, juxtaposed to the errorless acquisition of English *-ing* forms, and errorless acquisition of Turkish *-dl* and *-mls* forms.
- The *punctual–nonpunctual* distinction, where the latter exhibit universal marking in creoles, compared to errorless acquisition of past tenses in French and Italian.
- The *causative* versus *noncausative* distinction, where creole N_iV/NVN_i alternation in creoles is compared to errorless acquisition of causative marking in Turkish and Kaluli.

This leads him to conclude:

“We have now received a wide range of evidence, dealing with the acquisition of widely different features in several different languages, which cannot easily, if at all, be accounted for by existing theories of language acquisition, but which follows naturally if we assume the existence of an innate bioprogram for language.”
(p. 207)

Roots... Origins

Bickerton then sets forth to suggest that if the similarities shown to exist between unrelated creoles – supported by phenomena in language acquisition, are the consequence of an innate bioprogram – then these characteristics should be able to show us how language started in the human race.

Bickerton starts by dismissing the notion that language should have evolved from gestures, as is sometimes suggested.¹ Further, he dismisses the idea that human language should have evolved out of any non-human communication system, such as those seen among other mammals today, in particular the higher primates. Language, he argues, probably evolved rather quickly, when some mutation created a qualitatively new step in the human brain to be able to cope with the kind of referential concepts only (most likely) humans seem to grasp. Language cannot be the result of gradually more complex ape-like gestures or groans, since

“Language depends crucially not on complexification but on the power to abstract, *as units*, classes of object, classes of actions, classes of events, and classes of yet more abstract kinds”. (p. 219)

Those classes, he says, cannot be found in other mammals than human beings. He also declares:

“I suspect that /.../ the common notion /.../ that language bootstrapped its way upward, creating the conceptual categories it needed as it grew, producing thought, consciousness, and volition as mere epiphenomena, is simply false.” (p. 261)

We shall here jump Bickerton’s theory of language origin for space reasons; what he basically does is to sketch a theory that can accommodate his feeling that the ‘first languages’ probably looked like bioprogram-close creoles.

¹In a far more recent work, *Grooming, Gossip and Language*, biologist Robin Dunbar, also completely dismisses the idea that language should stem from gestures.

How, then, does he explain that modern languages do not look like the bioprogram? Why do languages differ so much from what the bioprogram provides us with? Whence this complexity and variation? The answer, Bickerton says, “is that, in a sense, the biological language self-destructed.” (p. 290). He explains further:

“Previously, /.../ our ancestors had all lived roughly the same kind of life /.../ Now, some went on hunting and gathering and some became pastoralists and some became cultivators and some founded cities and lived by farming other people. New needs arose. /.../ No biological language could have been designed to suit the needs of all human under all the different circumstances in which humans could live” (p. 290–291)

Roots... Conclusions

After having become more and more speculative, Bickerton states that his aim is to create “the unified theory of language acquisition, creole language origins, and general language origins”. This surely is a great undertaking, and, as we shall see, it has received its fair share of criticism from all the fields concerned.

However, before turning to the critics and commentators, let’s have a look at a couple of later versions and amendments made by Bickerton after the first presentation in *Roots*.

The Bioprogram Hypothesis II: *The language bioprogram hypothesis*

Three years after *Roots*, Bickerton himself provides a short version of his hypothesis in the journal *THE BEHAVIORAL AND BRAIN SCIENCES* (1984:7, pp. 173–221), including comments from several people from different areas. I will just briefly mention a small number of differences between the original version and the 1984 version, as well as some clarifications Bickerton adds in the updated version.

In order to support LBH – a theory which, he point out, is testable – it is vital to show that the grammar of a language can be produced in the absence of normal transmission between generations.

A linguistic feature he mentioned already in *Roots*, but which is given more weight here, is *verb serialisation*, a typologically universally marked feature (in the Jakobsonian sense), common in creoles.

He also discusses the time factor in deeper detail, i.e., how fast creolization must have occurred in order to show signs of the bioprogram. Another thing he discusses further is the relation between LBH and Chomsky’s *Universal Grammar* (UG), which he considers to be fairly close to what he proposes, albeit differing in detail.

He once again dismisses substrate-based explanations,¹ as well as the theory of monogenesis. In his view, no contact-based explanation can explain the creation of HCE, a language which can only be explained in terms of some kind of bioprogram.

¹He now refers to proponents of substrate explanations as ‘substratophiles’, rather than ‘substratomanics’.

The Bioprogram Hypothesis III: *Beyond Roots: The Five-Year Test*

Two years later, in 1986, Bickerton publishes the article *Beyond Roots: The Five-Year Test*, in *JOURNAL OF PIDGIN AND CREOLE LANGUAGES* (1986) 1:2, pp. 225–232. He here takes on a more drastic view on his hypothesis. Thus (influenced by the MIT graduate Hagit Borer), he states that:

“There is one syntax, therefore, the syntax of creole languages is the syntax of all languages. What makes languages seem different from each other is that they have different lexicons and their lexical items have different properties. What makes creoles seem different from other languages, and more like one another, is that they have fewer lexical items and the lexical items they do have possess fewer distinctive properties: pidginization strips items from the donor lexicon and strips properties from the items that do survive.” (*Beyond Roots...*, p.228)

He further claims that creoles can be understood in terms of three processes:

Retention, referring to items inherited by a creole from antecedent languages, in particular functional rather than content items, since content ones are more likely to influence the syntax of the creole.

Loss, referring to the items a creole fails to keep. The dominating factor at play here is contact with the superstrate language: the more contact, the more likely it is that a creole will retain lexical items. With small exposure, all bound grammatical morphology will be lost., but other factors are also at work here, like phonological structure and allomorphy.

Reconstitution, referring to items that were lost during the process of pidginization, but for some reasons had to be replaced, since universal syntax needed them. Tense is mentioned as one ‘obvious case’. Bickerton reminds the reader that he suggested in *Roots* that the distinction between punctual and nonpunctual is the earliest and most fundamental form of predicate modification, something he no longer thinks is correct.

On the whole, this article, as well as the 1984 article, exhibits a clear influence from Chomsky’s *Government and Binding Theory* (GB), which makes sense. A theory that attempts to explain the innate structure of all human languages, i.e., the genetically coded program in the brains of human beings, should have a look at other such theories. I find it a little surprising that Bickerton adopts the terminology of GB without further explanation.

Reactions and Criticism: A Short Survey

Given the fact that Bickerton’s claims are both speculative, far-reaching, and cover several different fields of research, it is to be expected that reactions come from people both within in his own field, creolism, and people from the other fields concerned, psychology, neurology, grammar, anthropology, sociology and so on and so forth.

And reactions there are, of course, galore. As mentioned before, the article in *THE BEHAVIORAL AND BRAIN SCIENCES* includes reaction and comments from a wide range of scholar from different disciplines. I will here briefly sketch what these reactions are. I have chosen to do this “field-by-field”, although the division into “fields” is somewhat arbitrary.

Reactions: Psychology

The psychologist **Elisabeth Bates** of University of Californian San Diego, does not question Bickerton's data, but has difficulties with his nativist claims. She identifies four different nativist arguments, viz.:

- Universality

The claim here is that creoles look like one another, not since they have a common ancestor (the monogenesis explanation), but rather since they are the result of "innate biases". Bates argues that Bickerton confuses biological determinism and universality, two entirely independent phenomena. She points out that many individual variables are under strict genetic control (like eye colour), whilst many universals are so inevitable in a given situation that genetic coding is not called for, i.e., the genetic causality is far smaller than one might expect. She exemplifies with the perfect hexagonal structure of beehives, now known to be the result of mathematical packaging laws, irrespective of what whatever "instinct" bees might have about construction. Says Bates:

"[G]rammars might 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". (*The language bioprogram hypothesis*, p. 189)¹

And later:

"[T]he linguistic problem of coding non-linear meanings onto a linear speech channel – lie neither in the organism nor in the environment, but at some emergent level between the two." (p. 189)

Therefore, she concludes, Bickerton's alleged creole universals need not be the result of innate tendencies, but instead of a "consistent rediscovery of a set of logically possible solutions" (p. 189).

- Invention

Bickerton's view is that whenever a language phenomenon is non-predictable from the input, or more richly structured than the input, then an innate mechanism is responsible. Bates agrees to the extent that whenever the output is richer than the input, then the organism must have contributed. She does not agree with Bickerton, however, that this calls for a nativist explanation. How, she asks, are we to explain children's creative, but faulty ideas and inventions when learning a language? Do these also belong to the bioprogram? If the bioprogram is viewed this way, it becomes nothing more than a catalogue of ideas, threatening to become very large indeed.

- Domain specificity

Bickerton claims that the innate properties in question must be language-specific, i.e., not part of human cognitive mechanisms in general. Bates retorts that "the specificity of a *cognitive product* does not logically require postulation of an equally specific *cognitive process*" (p. 189). She points out that Slobin's 'operating principles', cited by Bickerton, like "pay attention to the ends of words" could well be seen as a subset of more general operating principles in human cognition, like general figure-ground principles that seem to work across several modalities.

¹Once again, 'ibids' are avoided, and all references will refer to this article if nothing else is stated. Each of the critics' comments has its own title, cf. References.

- Selectivity

Selectivity refers to the fact that grammars encode only a very small part of the universe of possible meanings. This, Bates points out, *is* interesting and important, but children show many other biases that could hardly be explained by genetic coding in linguistic terms. For instance, when children decide to name clothing, they are far more likely to name socks or jewellery than the clothes they wear themselves. Likewise, if they name but one parent, it will probably be the parent who spends less time with the child. As Bates points out, there has simply been too little evolutionary time to create an innate preference for jewellery over baby clothing (or refrigerators or other things the child is exposed to but does not name). Rather, Bates argues, is naming following general cognitive principles; children name things that are more informative, or more easily manipulated, or things that undergo interesting changes of state and so forth.¹

Bates finishes by saying:

“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. /.../ [T]he bioprogram notion implies far more genetic determinism than we may need in order to explain the interesting data on creolization.” (p. 189)

Another scholar who takes issue with the question of specificity is **Lois Bloom** of Columbia University, New York, (ibid.). She asks whether children, apparently well-prepared in some ways for life (they can tell the difference between their mother’s voice and other female voices within a few hours after birth), make use of a specific program for language, or more general problem-solving devices. She agrees with the general idea of a bioprogram; says Bloom:

“The question is not whether there is a biological program for the acquisition of language, but what the program is and how it operates.” (p. 190)

Bickerton, as we have seen, is of the opinion that the program is specifically linguistic. Bloom claims that, according to her studies on acquisition, semantic distinctions are being learned by children as a result of their general cognitive development. Bloom is of the opinion that a bioprogram certainly is at work from the very beginning of a child’s life, but the extent to which that program is linguistic, if at all, remains to be proven.

Bloom also questions – as we will see other people do – whether there really is cause to believe that mothers actually did communicate with their newly-born children in a pidgin language they did not fully master, casting doubts on the input Bickerton builds his arguments on.

Richard M. Cromer of MRC Cognitive Development Unit, London, finds Bickerton’s idea “interesting and provocative”. Cromer, however, is of the opinion that language is processed by a general problem-solving device, and that the notion of a species-specific language processor is not necessary to explain data collected in language acquisition studies. Cromer calls for clarification concerning the properties of the bioprogram, especially concerning the claims that language is processed by a species-specific processor, or as he says

“... 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 /.../ in a structured fashion.” (p. 193)

¹Bates here refer to Nelson (1974) and Greenfield & Zukow (1978).

Cromer thinks that the data presented by Bickerton are equally compatible with the view that a general problem-solver is at work, not at all requiring any species-specific language processor. If there is one, then Cromer wants heavier evidence in favour of it.

Frank C. Keil of Cornell University, Ithaca, New York, finds LBH very convincing. However, he thinks that the four basic arguments provided by Bickerton (that creoles are created by children with almost no input; that all creoles share some properties; that creoles differ significantly from pidgins; that creoles often have properties not found in any possible substrate languages), albeit probably true and convincing, are not sufficient conditions to prove LBH. He argues that a general problem -solver could well account for these four phenomena. Keil, however, supports the idea that humans have an innate drive to create languages, with minimal input, and cites studies that show that deaf children spontaneously create sign languages with very little signed input. Keil admits, though, that it is not impossible that a general problem-solver could have the same effect. Says Keil:

“[C]reole languages 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.” (p. 198)

In order to find those arguments, one needs to look into other domains of knowledge. He proposes four areas. The first is to check whether similar creativity is found in extra-linguistic domains, like spatial knowledge. Second, one can study whether language acquisition follows its own course, independent of other cognitive domains.¹ Third, the fact that chimps do not learn language could be taken as evidence against a general problem-solver, since nobody doubts that chimps use such a device when they are taught human languages. Fourth, one could bring the critical period hypothesis – i.e., the hypothesis that there is a critical age after which it is well-nigh impossible to learn any language, had one not learnt one before – into the discussion.

Keil seemingly agrees with Bickerton’s hypothesis on the whole, although he states that one cannot entirely rule out the possibility of a general problem-solver.

Michael Maratsos, Institute of Child Development, University of Minnesota in Minneapolis, throws in some additional support for LBH in stating that one of the arguments in favour of Bickerton’s claims is the robustness of language learning, e.g., the fact that even people with a very low IQ can learn a language. This kind of robustness is typical of evolutionary buffered structures, he explains. The question, then, is “how much?”. A crucial point in Bickerton’s argumentation is what kind of input the first speakers of HCE were exposed to. Maratsos, like Bloom, doubt that the children would have been exposed to just a fragmentary pidgin, as Bickerton claims.

Maratsos also draws attention to Bickerton’s corollary claim that things in the bioprogram should be easier to learn in normal acquisition. This is not always the case, according to Maratsos. For example, Turkish post-nominal inflectional definite marking is learnt very early, as early or even earlier than prenominal determiners and/or word-order marked agent–patient relationships, typical of creoles, are learned.

Richard P. Meier of Princeton University, New Jersey, points out that there are additional sources of information besides creole languages if one wants to find evidence for or against LBH. Says Meier:

¹Note here that several other people cited in this paper claim that this is not the case,

“[I]n 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” (p. 201)

Out of 500,000 prelingually deaf people in the United States, 90 % are the offspring of hearing parents. Meier calls this group “first-generation signers”. The remaining 10 % are “native signers”, most of whom are second-generation signers. Rather few third-generation signers exist, children of native-signing parents.

Meier is of the opinion that the signing of these groups, respectively, might shed important light on Bickerton’s hypothesis, since first-generation signers, like Bickerton’s pidgin speaker, differ to a great extent in their signing. Consequently,

“[S]ome first-generation deaf children receive essentially no linguistic input from their parents. /.../ 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 semantic relations between gestures in multigesture strings, and by recursion” (p. 202).

If there is a bioprogram, argues Meier, then the signing of such first-generation signers should be closer to the bioprogram than some of Bickerton’s proposed examples.

Second-generation learners, who receive input from non-native signers, linguistically heterogeneous, exhibit very uniform, mature grammars, like those of creole speakers.

Meier argues that “language” is not modality specific, and that studies of other modalities might yield further insight into what might constitute inborn capacities of language.

Bickerton agrees with Meier that “the language-creating capacities of deaf children constitute another window on what must be innate” (p. 212), but he thinks that “it is a steamier window than that of creoles” (p. 212).

Reactions: Anthropology

Matt Cartmill of Duke University in Durham, North Carolina, disagrees with the notion of species-specificity, or, as he says:

“It is hard to see why Chomsky and his followers [e.g., Bickerton] have tended to assume that “innate” and “species specific” are synonyms.” (p. 191)

Cartmill says that this “is on a par with saying that an underlying universality in colour terminology implies that the mechanisms of colour vision are uniquely human” (ibid., loc. cit.). On the whole, however, Cartmill finds Bickerton’s proposal “refreshing”, and says that the bioprogram search for evolutionary explanation—very much rejected by Chomsky—is a welcome contribution to the study of language.

Salikoko S. Mufwene, University of Georgia, regards Bickerton’s LBH as a significant contribution to theoretical linguistics. However, Mufwene throws in the following warning:

“Reader, beware! Bickerton has restricted the range of creoles to those with I[ndo-]E[uropean] 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 /.../ show fewer drastic structural simplifications and modifications than those that emerged in other contexts.” (p. 203)

Dan I. Slobin, University of California, Berkeley, agrees with Bickerton that acquisition studies to some extent support LBH. However, whereas Bickerton seems to be of the opinion that the bioprogram provides the child with both a set of semantic categories for grammaticization and a set of formal devices for the expression of the said categories, Slobin argues that the connections between the two are probably not as strong as Bickerton suggests.

One problem Bickerton faces, Slobin points out, is posed by precocious and error-free learning that does not match the bioprogram. Thus, Slobin asks how Bickerton would explain the fact that English children acquire prepositions without passing through a stage of serial expressions, and that Turkish children master case marking without ever using word order marking, typical of creoles.

William S-Y. Wang, University of California, Berkeley, agrees that “our language capacity must depend on various pieces of biological equipment”, as proven by the uniform time schedule of learning across the species and evidence from brain research. However, he does not agree with Bickerton that some task-specific device needs to be invoked, since language acquisition can be fully explained in terms of general problem-solving cognitive devices.

Thus, Wang puts forth, the lowered larynx could have resulted from anatomical adjustments to upright stance, sensitivity for speech sounds could well be accounted for in terms of general auditory processing capabilities, as research with non-speech sounds have shown (Jusczyk et al 1983), and hierarchical structures seem to be a part of general cognition, not specifically syntactic modules.

Reactions: Linguistics

Chris Corne, University of Auckland, New Zealand, agrees with Bickerton that LBH can explain protocreole language genesis, at least in many cases, and that it by no means constitutes the sole causal factor through which novel features appear in creole expansion. Corne is of the opinion, however, that Bickerton undervalues contributions from sub- and superstrate languages in his general approach. Corne provides examples from Mauritian Creole (completive aspect; collectives; verb fronting) and shows that these features could well have their roots in Bantu languages, i.e., constitute examples of substrate influence. He concludes that

“[A] wide range of creole and African data supports neither the LBH nor the hypothesis of transmission from the substratum: Both remain possibilities.” (p. 192)

Thus, Corne calls for some caution that substrate explanations need not be dismissed before more historical data are available.

Morris Goodman of Northwestern University, Illinois, questions Bickerton’s data. Goodman states that there are good reasons to assume that some kind of pidgin had developed on Hawaii already by the late 18th century between English-speaking seamen and native Hawaiians. This pidgin – or lingua franca – was clearly influenced by other

Pacific pidgins and shared both vocabulary and grammatical features with those languages. Bickerton bases his arguments on HCE on interviews with old Japanese, Filipino and Korean immigrants. Goodman points out that when the first Japanese immigrated in 1888, there was already a pidgin there, “fairly fixed in form”. Koreans and Filipinos did not arrive until later. Thus, Bickerton’s claims that there never was a fixed pidgin falls, and consequently much of his conclusions. Goodman also points out that demographic data show that for decades, colonial-born slaves formed less than 10 % of the population. Goodman argues that these, and other data, suggest that

“[T]he creole languages of the New World arose almost entirely as lingua francas among African-born slaves rather than among those who were locally born.”
(p. 194)

Goodman, like Corne, thinks that Bickerton dismisses substrate explanations too easily. Many features, Goodman claims, can be explained in terms of language contact and substrate influence, perhaps coupled with certain universal principles, whatever these principles might be.

In a review article (*Roots of Language; Review*, 1985), Goodman returns to Bickerton’s assumptions that creole languages are independent developments and not outgrowths of prior languages (p. 109). Goodman believes that there is good reason to believe that Bickerton’s account of the language situation on Hawaii during the period concerned was not anything like what Bickerton assumes, and that a fairly fixed form of pidgin was already there. Goodman also questions Bickerton’s demographical data, and argues that Bickerton’s account of the history of Curaçao is “grossly inaccurate”, since the permanent population of native speakers did in fact not grow with the speed suggested by Bickerton.

Goodman agrees with Bickerton when he rejects monogenesis attempts to account for cross-creole similarities. Goodman also thinks that substratal theories fail to explain these similarities. However, Goodman thinks that there are alternatives to the innate bioprogram Bickerton suggests, when trying to explain the similarities. For example, some grammatical features are more easily learned, and thus transferred between languages than others. This (Goodman is not very clear here, I think), assumedly would depend on general cognitive capabilities, rather than language-specific acquisition devices at work at young age. Says Goodman:

”There is no need to assume that children played any role whatsoever in introducing these constructions into the emerging creoles. They were adopted simply because there were no equally strong competitors in either the other substrata or the target language.” (*Roots of Language; Review*, p. 123)

Goodman also points to what he considers a fundamental difficulty with Bickerton’s proposal:

“It can, of course, never be proved with mathematical certainty that a particular syntactic construction was introduced into a given language as a result of contact with another, since any feature so acquired could also have evolved entirely independently. Conversely, it can never be proved that any such feature was not acquired as a result of contact unless it can be demonstrated that no such prior contact took place, and this is what B[ickerton] has not succeeded in doing.” (*Roots of Language; Review*, p. 127)

Myrna Gopnik of McGill University, Montreal, attacks Bickerton on a higher level, as it were. She agrees with him that a bioprogram or “core grammar” might be the explanation to whatever features creole languages exhibit. However, she does not agree with him when he claims that certain behaviours found among animals provide evidence

that some language infrastructure must have been in place for a very long time, in evolutionary terms. She does not think that Bickerton has

“... presented us with a hypothesis or research program yet. He says that it is an empirical questions, that we need evidence. The problem is that I wouldn't know what evidence to gather, sift, or interpret.” (p. 194)

She says that while a child *understands* certain distinctions, like the distinction between specific and nonspecific, a bird (or some other animal) might well *react* to such a distinction, but that it is very hard to draw any conclusions concerning what these reactions are to the bird, and consequently, to draw any conclusions as to what such behaviour tells us when it comes to the origins of language. The hard part here is to differentiate between intentional and non-intentional behaviour, which leads to the question whether general cognition precedes or succeeds language. In order to test this, Gopnik wishes for a more exact definition by Bickerton as to what counts as “cognitive equivalents to the categories and relations in his ‘inner core grammar’” (p. 195). Until then, she finishes, Bickerton's claims are not testable.

David W. Lightfoot, University of Maryland, sees in LBH an attempt at solving the so-called “poverty-of-stimulus problem”, i.e., the label that expresses the opinion that the child is exposed to a insufficient amount of data to explain how it can learn a language with the speed with which it actually learns a language. So far, so good, Lightfoot thinks. However, he cautions when it comes to LBH specifics, and Bickerton's argumentation in connection therewith. First, he questions how much one can actually know about the input the first speakers of HCE had. Second, he thinks that Bickerton ascribes the bioprogram too high a degree of specificity. If that much is encoded genetically, why should any language ever develop away from the bioprogram grammar, and how could a child ever develop rules that are not part of the bioprogram grammar. Asks Lightfoot:

“Saying that creole grammars are provided genetically leaves us wondering how noncreoles are acquired.” (p. 199)

Alec Marantz of Harvard University, Cambridge, Massachusetts, while agreeing with Bickerton in principle, heavily attacks one of Bickerton's fundamental claims: that first-generation speakers of creoles differ from any other language-learner. Says Marantz:

“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. /.../ 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 knowledge. /.../ [I]f Bickerton is right that creolization provides any evidence at all for the bioprogram hyp[ot]heses – and I think he is right here – then he must be wrong in supposing that this evidence is special. /.../ 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.’” (p. 199)

Pieter Myusken, Instituut voor Algemene Taalwetenschap, Amsterdam, is of the opinion that Bickerton's bioprogram phrase structure grammar is nothing more than a fairly standard version of X-bar theory. However, Bickerton's grammar lacks the complex innate schema of Chomsky's government and binding theory, and that without such abstract features, it is hard, or even impossible, to construct a learning theory.

Rebecca Posner, University of Oxford, although not in doubt when it comes to Bickerton's analysis of HCE, is very sceptical concerning his analyses of other creoles. She points out, that it is very doubtful whether there was ever a clear distinction between creolization and language change – leading to *patois* forms – in general. Her own studies on French-based creoles do not seem to support such a view. According to Posner, it remains in doubt whether creolization “differs from other linguistic changes in nature, or merely in degree” (p. 205). Posner is also convinced that sociohistorical conditions are more involved in these processes than Bickerton acknowledges,

On a more detailed level, some of Bickerton's claims as to the universality of certain features are also doubtful. For example, Bickerton claims that all creoles have a definite prenominal article. Posner argues that an analysis of the candidate in French creoles, *là*, as being a definite article would be unjustified. She also questions Bickerton's analysis of the French creole *pu*.

Peter A. Roberts, University of West Indies, Barbados, is with Bickerton in that the individual contributes significantly in the learning process. However, he finds that LBH has a short-coming in that it does not differentiate between form, function and meaning clearly enough. One can find similarities across creoles on each of these levels. Substrate theories work mainly on the levels of phonological shape and syntactic structure, linking these directly to meaning. Bickerton does not pay enough attention to these. Roberts is of the meaning that Bickerton actually fails to account for similarities between creoles. Bickerton also fails to explain how creoles become the language of the community, given the LBH.

William J. Samarin, University of Toronto, is not primarily concerned with whether or not innate processes are at work in language acquisition, but rather whether evidence for these processes can be found in creoles. Samarin says that Bickerton may have a point concerning the input to HCE, but when he is trying to show that other creoles, with mainly African ancestry, “he cannot be said to have made an altogether convincing case” (p. 206). Bickerton, according to Samarin, neglects sociohistorical explanations, and thus his identification of “pidgin and creole must remain artifacts of his analysis” (p. 206). Says Samarin:

“He [Bickerton] seems to assume that each of the creoles had its own antecedent ‘pidgin’, ignoring the possibility /.../ of considerable contact between speakers of many different varieties of New World speech.” (p. 206)

Samarin also says that Bickerton's claim that dominant speakers outnumbered substratum speakers at the beginning of colonisation in any colony is simply untrue. Samarin's own research shows that Europeans were vastly outnumbered by African workers in Congo and Ubangi in the last quarter of the 19th century, for example.

Samarin finishes his criticism by stating:

“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.” (p. 207)

Geoffrey Sampson, University of Lancaster, England, criticises Bickerton's arguments from a language acquisition perspective. Bickerton points out that children learning English never overgeneralise *-ing* forms, while they have more trouble

accommodating *-ed* forms. Sampson points out that Bickerton's source, Roger Brown (1976) offers a different explanation from Bickerton's, namely that:

“[T]he 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. /.../ [I]t is odd to see Bickerton invoking Brown's findings in support for the language bioprogram hypothesis.” (p. 208)

Pieter A. M. Seuren, Filosofisch Instituut, Nijmegen, is very sceptical, and argues that Bickerton's LBH “suffers from factual incorrectness as well as from tendentious and often fanciful analyses.” (p. 208). Concerning verb serialisation – one of Bickerton's main points – Seuren states that

“[I]t is painfully obvious that it is restricted to creoles of West African origin /.../ Verb serialization is not a feature of any other regional group of creoles” (p. 208)

He concludes:

“The overwhelming evidence is that creoles display their typical features regionally, not universally. Those features sometimes result from borrowing, both from substrate /.../ and from superstrate, and with modifications due to the collapse of carefully cultivated grammatical systems. And sometimes they result from spontaneous innovations.” (p. 208)

Ellen Woolford, Pennsylvania State University and University of Texas, Austin, raises the question what creole studies can do to increase knowledge about universal grammar. Her answer is “none at all” (p. 211). The problem, she reasons is that creoles, instead of revealing to us the properties of the inner core grammar, are the *worst* possible languages for that purpose. The reason is that, when we are presented with the output of a certain language, there are always a large number of possible analyses at hand, and the problem is to decide which analysis is the correct one. In order to do so, the more transformations, movement rules et cetera the language possesses, and the more one can compare the language with other languages of high complexity, the more one can narrow down the set of possible interpretations and analyses and arrive at a consensus as to what might be the “correct” analysis in a given case, i.e., “[t]he languages with the most complex grammatical structures are most likely to supply information about the properties of universal grammar” (p. 211). She concludes that:

“[A]lthough it is quite possible that when we 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 these properties to us. /.../ 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. /.../ creoles are probably the worst source of data on universal grammar.” (p. 212)

Bickerton has also received criticism from the creolists **Sarah Grey Thomason** and **Terrence Kaufman**, who in their book *Language Contact, Creolization, and Genetic Linguistics*, argue that

“[T]he history of a language is a function of the history of its speakers, and not an independent phenomenon that can be thoroughly studied without reference to the social context in which it is embedded.” (*Language Contact...*, p. 4)

They argue that Bickerton's claim that languages are systems with structures, and that things incompatible with that structure cannot be borrowed, making languages resistant to interference is only relevant to borrowing proper, not to substratum interference, which

constitutes a different situation (*Language Contact...*, p. 15). Generally, Thomason and Kaufman argues that Bickerton dismisses substratum explanation far too easily, and that many – if not most – of the phenomena under scrutiny might well be the result of substratum interference.

Reactions: Biology

Lyle Jenkins of the Dana-Farber Cancer Institute in Boston thinks that 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. (p. 196)

Jenkins accepts Bickerton’s claims that whenever properties occur in the output that were not found in the input, then these properties are attributable to genetic coding. However, Jenkins questions some of the biological implications that seem to be the consequence of Bickerton’s inner core grammar:

“... LBH seems to imply that during normal language development the nervous system wires itself first with an unmarked /.../ 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 those choices conflict with the input language. /.../ Such reprogramming is not the usual case in genetics /.../ In known cases of gene rearrangement in development /.../ no examples have been reported of such rearrangements” (p. 197)

Jenkins does not want to dismiss Bickerton altogether, however, and suggests that studies of twins – who sometimes develop their own language – and deaf children might provide further information as to the nature of an innate mechanism.

Final Comments

As we have seen, Bickerton is rather heavily criticised. The main criticism from linguists and creolists seems to concern his claims as to the homogeneous character of pidgins and creoles, as well as his claims that substrate theories cannot account for creole expansion. Linguists – with other people – also doubt that HCE learners were exposed to HPE only, as Bickerton claims.

From neurology and biology (as well as other fields), it is pointed out that there is no evidence whatsoever to prove Bickerton’s claims that something either language- or species-specific might be at work in language creation or expansion. Although most commentators seemingly agrees with the notion of “innateness” to explain language learning, they argue that a general problem-solver could do the same job.

In the *Author’s Response : Creole is still king*, Bickerton answers to most of the criticism listed above, on a detail-by-detail basis not included here. He also expresses his disappointment with the research community, as it were, since, as he puts it, “many of the commentators stuck tightly to their own turf” (p. 212), instead of grabbing the opportunity to look outside their own discipline for inspiration, one of Bickerton’s main points being the cross-disciplinary character of his proposal.

One of Bickerton's main goals is obviously to bridge different disciplines, and although he seems somewhat disappointed with the result, it nevertheless seems as if he has succeeded in that feat, at least to a certain extent. Beyond doubt, he has stirred the scientific pot and his proposal appears to have created reactions galore among researchers from all the fields concerned.

This, I think, is always something desirable in research, and whatever the merits of Bickerton's Language Bioprogram Hypothesis might have on a detailed and specific level, it has beyond doubt done a good job in making people think in new ways and along new lines.

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