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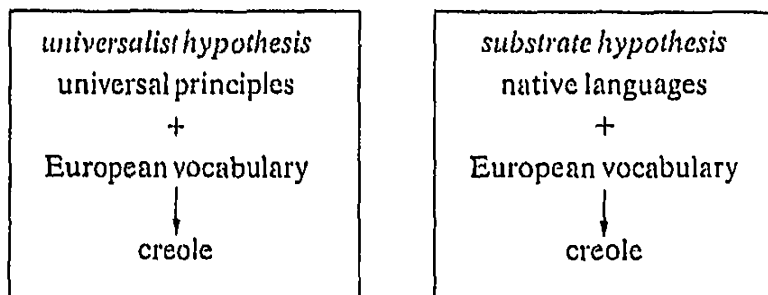
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Introduction

Problems in the Identification of Substratum Features in the Creole Languages

Pieter Muysken and Norval Smith

The dawning recognition that the Creole languages of the Atlantic and the Pacific could not be seen as simple versions of European colonial languages forced researchers to look at specific alternative hypotheses to account for the fact that the structures of the Creoles were different. Two of the most prominent of these hypotheses are inspired by the Romantic and the Historicist traditions, respectively: the universalist and the substrate hypotheses. The universalist hypothesis claims, essentially, that the particular grammatical properties of creole languages directly reflect universal aspects of the human language capacity (either a Chomskyan Universal Grammar (1981), a Bickertonian 'bioprogram' (1981, 1984), or a functional-pragmatic view (Givón, 1984) of this capacity). Creole genesis involves, then, the stripping away of the accretions of language history. The substrate hypothesis claims, on the other hand, that creole genesis results from the confrontation of two systems, the native languages of the colonized groups, and the dominant colonial language, and that the native language leaves strong traces in the resulting creole. Schematically:



One would think that this issue would be settled by now, given the clear set of alternatives and the extensive research of the last ten years. Nothing is farther from the truth. The same debate rages now as it did one hundred years ago, when Schuchardt reviewed Adam's (1883) book. Gilbert's contribution to this book surveys the early history of the issue.

Who, What, Where and Why?

Bickerton (1981, 1984) has emphasized the need for substratists (we will use this term, due to Holm, rather than the pejorative 'substratomanic' or the ambiguous 'substratophile') to demonstrate that speakers of a claimed substrate language were "in the right place at the right time". General appeals to parallels with Kwa or West African language patterns as characteristic of creole languages, are not sufficient to demonstrate anything. Although it may well be the case that such parallels are the result of substrate influence they cannot prove the substratist case if the same phenomena are also claimed by the universalists to represent the unmarked settings of various parameters.

What is clearly needed to demonstrate substrate influence is the conjunction of historical and linguistic evidence relating to *individual* languages. The historical evidence that is most significant and at the same time hardest to obtain is that relating to the initial period of slave utilization in a particular colony. Linguists themselves are dependent to a large extent on the work of anthropologists and historians for such information, and it is obvious that a large amount of interdisciplinary work remains to be done. In a number of cases, however, enough is known to allow a reasoned guess at the circumstances and the African groups concerned to identify the languages spoken by large groups of the earliest slaves. We will examine a case here where the historical conditions allowing for significant influence by one or two groups appear to be present.

This is the case of Berbice Dutch, where substrate influence is claimed by Smith, Robertson & Williamson (1986). The basis of this claim — though it is perhaps not a necessary condition it is a sufficient indication of significant historical contact — is a large percentage of basic vocabulary (27%) deriving from Eastern Ijò (in particular its Kalabarí dialect), a language spoken in the Niger Delta region of Nigeria.

Two vital historical facts are the following. First, the (private) colony of Berbice was founded in 1627. Unfortunately virtually nothing is known of the first fifty years of Berbice's history. However — and this is of some impor-

tance — it appears to have remained very small, for according to Van Kempen (1831) Berbice consisted of five plantations in 1666, and eight in 1714. Secondly, Dapper (1668) records that the principal slave-traders in the Calabar River were Dutch. Ryder (1965) argues that Dapper is referring to the 1640's which makes this of possible relevance for the early history of Berbice. When we add the fact of the high percentage of Ijò-derived basic vocabulary, which obviously requires a historical explanation, to these two historical facts we have a good testing ground for substrate influence.

We will bring forward two aspects of Berbice Dutch syntax as described in Robertson (1979), and discussed in Smith, Robertson & Williamson (1986), that seem to us to be good examples of substrate influence.

A first example is the positioning of the negative particle /ka(nɛ)/. This is located at the end of the VP:

yu [nimi dida kane]
 you know that not
 'you don't know that'

This positioning is not unique, as the Principe Portugese creole construction is similar (Gunther, 1973):

ci [kõse posá fa]?
 you know town not
 'Don't you know the town?'

However, comparison with the other Gulf of Guinea creoles, as well as occasional examples from Principe itself, shows us that this goes back to an original situation with two negative elements, the first one of which is in preverbal position:

amt [na sebé fa]
 I not know not
 'I didn't know'

Therefore the parallel with Principe Creole is only an apparent one. The Ijò languages differ from most other West African languages in that they are SOV in structure. Eastern Ijò has a negative particle *ka.

Eastern Ijò dialects

Kalabari	-áã
Kalabari	-ya (drum language)
Okrika	-ka (question)

	-ke (statement)
İbanı	-áá
Nkọrọọ	-ka, a

The negative particle goes after the verb in final position. As Smith, Robertson & Williamson (1986) remark it does not matter much whether the negative particle is cliticized to the V or the VP. In the formative period of a creole it does not seem likely that subtleties of this native would be readily apparent to Berbice Dutch speakers who were not also native Eastern Ijọ speakers. What is important is that this clitic was presumably perceived as adjoined to the VP, so that despite the VO word order in Berbice Dutch as compared to OV in Ijọ the position of the negative particle remains in fact the same.

In this case the phonological correspondence between Berbice Dutch /ka(ne)/ and Proto (Eastern) Ijọ /*ka/ strengthens the case for identity between the two cases. We have both formal and structural correspondence here. The /ne/ optional element in Berbice Dutch is quite possibly derived from Dutch *nee* /ne:/ "no". As a response this item appears reduplicated in Berbice Dutch as /nene/, but /kane/ is more usual here.

What is methodologically important in this case is that it would not be forecast by Bickerton's bioprogram. There is no reason to assume that the unmarked setting for the position of the negative element is in postverbal position.

In other words to *prove* substrate influence we have to look for "marked" structures appearing in both languages — the language potentially subject to substrate influence and the potential substrate language.

Then we have to find either structural parallelism or formal parallelism, or preferably both. If in the case of the Berbice Dutch negative just discussed the Eastern Ijọ negative marker had been replaced by a Dutch equivalent, we would still have been able to demonstrate the structural parallelism of course. Similarly if formal parallelism is retained without total structural parallelism this may be sufficient. An example illustrating this that we will not further discuss in detail would be the case of the Berbice Dutch plural. This is a general plural and is marked by the suffix /-ap(u)/.

This obviously corresponds to the Eastern Ijọ plural /-ap(u)/, which however is restricted to human nouns. The preservation of morphology through the creolization process is of course an obvious substrate effect but rather unusual. It needs to be seen to what extent the generalization to all NPs could

be triggered by linguistic universals.

The second aspect of Berbice Dutch syntax that we wish to discuss concerns the occurrence of locative postpositions in that language. We borrow heavily from the account of Smith, Robertson & Williamson (1986). Berbice Dutch has both prepositions and postpositions. It appears from Robertson (to appear) that prepositions are used in non-locative functions and postpositions in locative functions. Examples of locative postpositions are the following (from Robertson (op. cit.)):

war ben
house in
'in the house'

ɛke wari ɔndrə
I house under
'under my house'

Kwakwani anga
Kwakwani loc.
'at Kwakwani'

di banku bofu
the bench on
'on the bench'

di kreke anga
the creek loc.
'to the creek'

Eastern Ijò has *no* true adpositions. What it however does have is a class of expressions of the form N_1N_2 where N_1 is in principle any noun, and N_2 is a noun expressing spatial reference.

Similar expressions to those above are the following in Kalabari from Smith, Robertson & Williamson (1986):

wárí ɓtō
house inside
'at home'

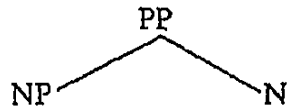
wárí mɛ' ɓtō
house the inside
'in the house'

ɿ warɿ suku ɸtō
 my house underneath inside
 'under my house'

ngada qgónō
 seat top
 'on the seat'

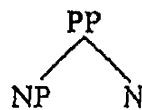
ori [okoló ɸtō] mú-tēē
 he creek inside go-has
 'he has gone to (the) creek'

Smith, Robertson & Williamson claim that an analysis of these NN constructions as PP is desirable in view of their adverbial status. Such an analysis has been proposed for Quechua by Lefebvre & Muysken (1986) where a PP can consist of an NP and a postposed N, because the [+N] feature of the head noun is neutralized in the projection and PP is supposed to be neutral, i.e. [N, -V]:

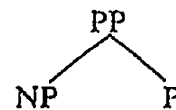


If this is the correct analysis of these Kalabarɿ structures this gives the following scenario:

Kalabarɿ



Berbice Dutch



Note that Kalabarɿ apparently does not possess non-locative adpositional structures, which would explain why Berbice Dutch follows the Dutch pattern (ultimately) in non-locative constructions.

Smith, Robertson & Williamson (1986) point out this is an especially striking case as Bickerton (1981) has claimed that an SVO language (as Berbice Dutch is) cannot *acquire* a set of postpositions. They conclude then that this pattern represents an inheritance from E.Ijɔ — an SOV language.

Note that in this case Dutch or Dutch creole locative elements have replaced the E.Ijɔ nouns of spatial reference in most cases. Note finally that Dutch also has postpositions:

het huis binnen
 'into the house'

These postpositions, unlike their Berbice counterparts, have a directional interpretation. They are always locative. It is not however to be excluded that they have played a role in the emergence of the Berbice mixed pre- and post-positional system.

So far we have looked at one construction which was rather easy to identify as a Berbice construction: negation, and one where we would have to postulate some kind of reanalysis between Ijò and Berbice Dutch: postpositions. We will now consider a case where neither Dutch nor Ijò have contributed to the syntax of Berbice Dutch: the placement of the verb. Ijò is an SOV language, as was mentioned before, and Dutch is an SOV language with a verb movement rule placing the verb in second position in main clauses. Berbice Dutch is neither, as is clear from examples such as the following. In fact it is rigidly SVO:

ek wa jefi -a kali kalf
 I ANT eat DUR little cassava bread
 'I eat little cassava bread'

Both the auxiliary particles and the main verb are found in the position right after the subject.

This may be explained through reference to some universal property that automatically yields SVO for a language such as Berbice, given some parameter. It may be due to effects of incomplete second language learning (cf. Meisel, Clabsen, and Pienemann, 1983), which would involve the reinterpretation of an SOV structure in which the verb has been moved to second position as an SVO structure by adult learners. It may also be due to SVO properties of an intervening Dutch pidgin, but then the question is why that pidgin is SVO.

The contributions to this volume

With respect to the papers included here, we can take the title of Mufwene's paper, which draws on a wealth of data from pidgins and creoles in Africa and elsewhere, as a motto: the universalist and substrate hypotheses complement one another. For one thing, the authors in this book most closely associated with the Government and Binding theory, certainly characterizable as universalist, take fairly substratist positions: Hans den Besten, Hilda Koopman,

and Claire Lefebvre. **Den Besten** traces the development of double negation from Nama to modern Afrikaans, while **Lefebvre** examines the evidence for relexification of the West African language Fon with French vocabulary, with Haitian Creole as the result. Her evidence is drawn from the lexicon and morphology, from constituent order, and from the structure of the noun phrase. **Koopman** compares verb complementation and other syntactic features of Haitian with those of French and a number of West African languages, arguing that there is sufficient syntactic similarity between the West African languages involved for it to be unnecessary for the Language Bioprogram to play a significant role.

Another thing to be noted is that there are a number of universalist hypotheses around. Three papers, each of which rejects significant substrate influence, illustrate this point. **Bickerton**, as was mentioned before, argues for a very specific set of properties of the language capacity of the child, which are activated when the linguistic input from the environment is restricted. His paper compares Haitian and Saramaccan with Yoruba and Vata, with respect to wh-movement and the distribution of empty categories, and concludes that the creoles and the West African languages do not really resemble each other. **Mühlhäusler**, on the other hand, examines the claimed influence of Tolai on Tok Pisin with respect to adjective-noun order, stressing a developmental perspective. He assumes a general set of universals of language development, which show up in language change, pidgin and creole genesis, and language acquisition. It is not possible to talk about Tok Pisin *sec*, only about Tok Pisin at a particular place and moment of time. **Seuren and Wekker** finally argue, in a very general contribution, that the universal properties of creole languages reflect underlying universal semantic structures. In this way, creoles are semantically more transparent than other languages.

A number of papers stress the fact that, contrary to the position taken by some universalists, creole languages did not emerge in circumstances of linguistic chaos. **Holm** reopens the debate on the autonomy of Hawaiian Creole English, claiming that Bickerton's use of data from elderly Japanese and Filipino speakers as representative of Hawaiian Pidgin English (1981) is inappropriate. There is ample evidence, according to Holm, for a regular, stable, and expanded early Hawaiian Pidgin English. **Alleyne** wants to shift the burden of proof regarding substrate influence onto the shoulders of the universalists. In Jamaica, the case he discusses, Twi was spoken next to Creole until the early part of the twentieth century. Therefore, he concludes, structural resemblances between Jamaican and certain West-African lan-

guages can best be explained by assuming substratum influence.

A final group of authors takes a more balanced position. **Hancock** stresses the need for careful historical and demographic analyses, and this leads him to conclude that the early creolized form of English along the Upper Guinea Coast formed one component in the various English creoles of the Atlantic region. Later on there was a varying amount of decreolization, depending on demographic factors. **Arends'** contribution provides an urgently needed demonstration of the lesson that creoles do not enter a situation of *stasis* once nativized. His quantitative analysis of Sranan copular structures covers two hundred and fifty years, and the general impression is one of large-scale restructuring. This implies of course that there are pitfalls in relying solely on contemporary data in creole studies. **Baker and Corne**, largely accepting **Bickerton's** view that creolization is a first generation process, make a good case for later adstrate influence on the syntax of Mauritian creole.

Synthesis

In this final section of our article, we will try and provide a synthesis of the various contributions to the volume. We are of the opinion that, despite the differences in approach of a number of authors, a greater uniformity of direction is beginning to make itself felt with regard to the question of the relative importance of universals and substrate influence in creolistics. We stress the word *beginning* in this connection.

The contrary findings reported in the two contributions by **Koopman** and **Bickerton** should provide us with a warning on the doubtful value of engaging in comparisons of languages — creole and African — without good reason for the selection of the particular languages. **Bickerton** himself (1981, 1984) has emphasized the very valid methodological point — referred to at the beginning of the article — that it is vital that the languages so compared should have been “in the right place at the right time”. In other words, there should be some evidence that a (preferably significant) body of speakers of a given (African) language were present in a given location where a given creole language developed. There is really too much variety — even among coastal West African languages — to utilize a few sample languages in a comparison. If this methodological point is to be taken seriously, then **Koopman's** conclusion that it is unnecessary that the LB play a significant role in the development of Haitian cannot be regarded as sufficiently proven. Of the West African

languages utilized in her comparison — Vata, Koyo, Bete, Dida, Baoule, Abey, Mahou and Moore — some, e.g. Abey, can not be demonstrated to have played any role in the peopling of early Haiti.

However, this point is equally applicable to Bickerton's article in this volume. If Koopman's article cannot be said to contain a sufficient proof against the necessity for the LB, it must also be said that Bickerton, who nowhere provides any evidence for a significant contribution of Vata or Yoruba speakers to the peopling of early Surinam or early Haiti, fails thus equally to deliver a sufficient proof against the substrate hypothesis as regards Saramaccan or Haitian.

Although, then, both articles are well-reasoned, detailed and exemplary of the type of discussion that is required — and sadly lacking hitherto — in this debate, neither comes up to Bickerton's own methodological criteria.

Three contributions deal with situations where there is sufficient historical evidence to justify comparing two languages with regard to their syntactic patterns. These are Den Besten (Afrikaans>Nama), Lefebvre (Haitian/Fon), and Mühlhäusler (Tok Pisin/Tolai). The first two articles seem to provide reasonable evidence to justify the suspicion that their positive conclusions regarding substrate influence are correct, and that these cases should be followed up to see if further evidence in this direction can be garnered. Mühlhäusler, on the other hand, demonstrates that it is not likely that Tolai had any effect on Tok Pisin in the small area of adjective-noun order, as had previously been claimed. It is of course just as important to weed out non-cases of substrate influence, as to discover genuine cases of this.

Mühlhäusler's contribution, as well as those of Arends and Baker & Corne, is also of importance to remind us of what is often ignored in creole studies, that is that "stable" pidgins and creoles are subject to the same processes of linguistic change (and interference (see Baker & Corne)) as are other linguistic systems. This change and/or interference may be rapid and thorough-going, as may be illustrated by the concrete example of the transition from Old English to Middle English.

Seuren and Wekker's contribution can be seen as providing the beginnings of a (lexico-)semantic dimension to the LB. Hancock, and Baker & Corne largely accept Bickerton's LB as the linguistic mechanism responsible for creologenesis. Alleyne, Koopman, and Holm, on the other hand, suggest that the LBH is not necessary to explain the particular cases of Jamaican, Haitian, and Hawaiian creole. Their argument, however, from particular cases is by reason of its very particularity not wholly convincing. The LBH

may not be *necessary* as an explanation of these particular cases, but it is there as a well-defined hypothesis, and as such cannot be ignored. Because of its presence the burden of proof for substratists has become more demanding whether they like it or not. Another reason for this is the greater concentration on the precise situation pertaining at the time of creologogenesis, as illustrated by Hancock's point concerning the need for careful historical and demographic analysis, which echoes Bickerton's own methodological strictures referred to above.

Gilbert demonstrates that the present debate between universalists and substratists is not a new one, but dates back at least a hundred years. Of course, the debate today is much more informed — and therefore potentially resolvable — as present creolists do not labour under the difficulties of their counterparts of a hundred years ago, who lacked both the data and the techniques for a satisfactory resolution of the question. We feel therefore that it is now time for a new approach to the study of creole languages. This feeling, inspired in part by the 1985 workshop in Amsterdam, amounts to a conviction that Mufwene is correct when he suggests that the Universalist and Substratist approaches are complementary. In other words, we feel that the truth, as is so often the case, lies between the two extremes. We suspect that there are both universal and substrate factors in creologogenesis — in varying proportions, depending on the precise social and historical circumstances obtaining at the period of creologogenesis itself.

We would conclude by pointing out again, that as far as the question of what constitutes satisfactory proof of the operation of one or other mechanism is concerned, it is clear that the exigencies of this have been increased for *both* mechanisms. Bickerton is methodologically in the right with his “right language in the right place at the right time”, but on the other hand, his claim that even if a candidate substrate language, fulfilling the above conditions, displays an identical syntactic phenomenon to that predicted by the LB, and occurring in the relevant creole, the presence of this in the latter is to be ascribed to the operation of the universal factors of the LB, can not be defended methodologically. Proof of the operation of the LB requires to be subject to the same level of strictness as proof of the operation of substrate factors. In other words, we must have evidence that a particular suitable (African) language was not available in the right place, or not available at the right time. This will mean in practice that, at least for the present, besides the cases where the operation of one or other mechanism can be reasonably demonstrated, we will be left with a residue of cases due to our ignorance

of the historical factors. It is to be hoped that this residue will be subject to constant reduction, as our knowledge of the relevant historical — and linguistic — data grows.

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