

SHORT NOTE

GENETIC LINGUISTICS AND GENETIC CREOLISTICS: A RESPONSE TO SARAH G. THOMASON'S "CREOLES AND GENETIC RELATIONSHIPS"

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In this note, I use the term *creole* as in Mufwene (2001) for some offspring of European languages which developed among primarily non-Europeans in Dutch, English, French, and Portuguese plantation settlement colonies of the 17th–19th centuries and have been disfranchised as vernaculars genetically unrelated to their lexifiers. The term *pidgin* is likewise used for those that developed in trade colonies of the same European nations, where contacts between Europeans and non-Europeans were originally sporadic. Those inclined to invoke pidgin or creole (PC) prototypes – to which I return below – are likely to identify them within this group, as did McWhorter (1998). Regardless of disagreements we may have about what counts, or does not count, as a PC, we certainly have fewer disagreements about this restricted set, on which I base my discussion below.¹

Genetic creolistics is used in the title of this note by analogy to *genetic linguistics* to suggest that the subject matters of both research areas are similar. I would even like to submit that the former should be considered a subfield of the latter, differing from it only in its focus on a specific group of languages. Both research areas are ultimately concerned with mechanisms and ecological conditions of language speciation – although we in creolistics have preferred using the term *restructuring*, as if the process were peculiar to PCs alone, and perhaps those other varieties imprecisely identified as “mixed” (Mufwene, 2001). Some cross-pollination between them should be welcome.

¹ Owens (1998) is perhaps an exception in arguing that vernaculars such as Kituba seem to stand up to McWhorter’s criteria better than Haitian Creole and the like, based on how many features they have lost from their lexifiers.

Actually, recent research on the development of creoles militates for reopening the books on some of the ways in which research in genetic linguistics has traditionally been conducted, especially regarding the role that changes in the ecology of a language (both system-internal and external) play in its evolution (Mufwene, 2001, Chapter 5).

Contrary to the myth that creolistics is a young discipline, I'd like to argue that genetic creolistics is just about as old as genetic linguistics is. An important difference lies in the fact that since the beginnings of both subfields of linguistics in the 19th century, creoles have typically been considered aberrations, irregular developments, therefore not worth the attention and time of serious linguists. Presumably, these varieties would not tell us anything about normal or natural language change. Such assumptions are unfortunately still identifiable in some modified forms in today's professional linguistics publications. Our enlightened precursor Hugo Schuchardt must be turning with disappointment in his grave on realizing that, like late-19th-century genetic linguists, some of us today would still ostracize him for wanting to treat PCs as regular, normal, and natural languages, therefore likely to enrich our understanding of Language (Schuchardt, 1884, 1924).

I submit that PCs can tell us a number of unexpected things regarding the architecture, functions, and evolutionary patterns of languages. Actually, due to various issues that have been raised about their developments, we are perhaps now in a privileged position to ask the following question: Does our inability to resolve the issues conclusively stem from the putative abnormality or unnaturalness of the language varieties or from the inadequacy of some working assumptions inherited from the 19th century? Interestingly, the 19th century is also where the conceptual foundations of genetic linguistics lie. While we have often been prompted implicitly or explicitly to compare our research methods against this research area (e.g., Chaudenson, 2001), what we have learned to date in genetic creolistics can also motivate us to re-examine the practice of genetic linguistics itself, as I do below.

Reading Sally Thomason's column (henceforth Thomason, 2002) in *JPCL* 17, pp. 101–109, I was struck by how she addresses some of Mufwene's (1998, 2001) and DeGraff's (2001a) data- and theory-based arguments against some of her positions. Without adducing any counter-evidence, she resorts to precisely the same kinds of stipulations that she has been criticized for (especially by myself) – though she denies doing so. The intended meaning of *stipulate* is of course 'accept or state a proposition without requiring that it be

established by proof'. For example, Thomason invokes "prototypical pidgins and creoles" (2002, pp. 105) but she does not give any examples thereof nor tell us how to identify them. I thought I might find this notion explained in Thomason (2001) but no clarification is provided there either, despite what I quote below. Yet, she knows (Thomason, 2001, p. 190) that McWhorter (1998) has been criticized not only for submitting such a concept but also regarding the accuracy of the membership of the category (see some commentaries in *Linguistic Typology* 5, nos. 3&4, 2001; Mufwene, 2000; DeGraff, 2001b). Presenting the concept as if it were part of the received doctrine in creolistics is more like dodging the issue than addressing it. It is not clear to this particular reader whether Thomason means McWhorter's best-exemplar prototypes or Mufwene's (2000, 2001) heuristic prototypes (i.e., "classic creoles" of the Atlantic and Indian Ocean, which have informed most of our theorizing). Such stipulating makes her position unfalsifiable, even if one were disposed to interpret charitably the following observation of hers: 1) "pidgins and creoles that best fit [her] definitions are prototypical" (Thomason, 2001, p. 160), and 2) "a prototypical pidgin emerges abruptly, in a new contact situation involving three or more languages, with limited contact and no full bi-/multilingualism among the groups in contact ..." (ibid., 197).²

Likewise, Thomason invokes "abrupt creoles" (assuming that it is enough to claim that they "developed without going through a stable pidgin stage," 2002, p. 106) but she does not identify a sample thereof. Thomason (2001) presents no example of this either. This presentation of facts is unclear to those of us who have argued that generally creoles have evolved directly and gradually from their lexifiers, without a transitional pidgin phase. I for one am curious which "abrupt pidgins and creoles" she has in mind. She proceeds to claim: "some mixed languages of both types [i.e., "bilingual mixed languages" and pidgins/creoles] seem to have emerged relatively abruptly,

² A re-reading of the history of European trade with Africa and Asia actually suggests that pidgins too developed gradually from varieties closer to their lexifiers (those spoken in the early stages by trained interpreters) to more and more divergent ones. (See, e.g., Bolton, 2000, 2002 regarding Chinese Pidgin English.) As trade and human contact increased in the relevant regions, more and more Natives used the European lingua francas without sufficient exposure to them and restructured them with more deviations. (I am not invoking the Americas here, because, interestingly, the colonial trade languages in this part of the world were indigenous, which is beyond the focus of my present discussion and cannot receive as much attention as the subject deserves.)

and some mixed languages of both types seem to have developed gradually” (2002: 107), but no examples are given at all to tell us which ones they are. And she punctuates, “Most pidgins still seem likely to have arisen relatively abruptly” (2002, p. 108), but she still adduces no single example to make her position verifiable or falsifiable. If she cannot acknowledge the above statements as stipulations, then I have one more reason for not being convinced by her intimation that “the famous [i.e., ‘time-honored’?] Comparative Method” (CM) is as reliable as she claims.

Thomason wants us indeed to believe that the CM has been so successful that it “has been altered only in relatively minor ways since” the 1870s and “is envied by many other historical scientists” (2002, p. 102). To my knowledge, there are quite a few historical linguists, apparently “good” (Thomason’s term) and respected ones, who have questioned the “famous” CM. Almost all the contributions to Aikhenwald & Dixon (2001) question its reliability and “the regularity hypothesis of sound change” (Thomason, 2002, p. 102). They argue that synchronic structural similarities among languages, on which the CM relies, may be due to common inheritance, mere typological coincidence, or diffusion. Likewise, Campbell (2002, pp. 146–147) includes an informative caveat about the CM. Overall, one must independently know the history of population movements and contacts among speakers of the relevant languages in order to sort things out. The position is echoed by Laks (2002). As Thomason (2001) herself acknowledges, contact has played an important part in the histories of all languages. Some of the chapters in Aikhenwald & Dixon (including those by Calvert Watkins, Randy Lapolla, James Matisoff, and Bernd Heine & Tania Kuteva) clearly show that the Stammbaums suggested by the traditional application of the CM do not do justice to the complex ways in which languages are genetically related. (This is precisely the position developed by Laks, 2002.)

There is thus no reason for being complacent about the traditional practice of genetic linguistics. From our struggles in genetic creolistics to understand the covariance of outcomes of language contact with patterns of social interactions, we can probably learn something on the coevolution of language and society. Genetic linguistics is far from being ahead of genetic creolistics in this respect. I can say this with confidence after discussing the issue with several genetic linguists around the world during the 2001–2002 academic year and checking several historical linguistics references before writing this response.

In the same vein, arguing that language evolution is “chaotic” (in the technical sense of being sensitive to initial and changing ecological conditions), Lightfoot (2002) argues that one cannot rely on the CM alone to establish whether syntactic structures of languages have been inherited from the same ancestor.³ If Thomason, who claims to speak on the behalf of (“good”) historical linguists, does not stipulate her dogma of intercourse-free speciation, then she should have articulated, or summarized, for us her criteria for determining the lower threshold of mixedness that disqualifies a language variety from being genetically related to other languages from which it has inherited, or with which it shares, most of its lexical, and therefore a great deal of the phonological and morphological, stock (ruling out cases of diffusion and borrowing). Hjelmslev’s (1938) so pointedly observed that all languages are mixed in some respect(s) and to some extent. In his footsteps, noting the influence of Frankish in particular, Posner (1996) remarks that if one proceeded literally by the CM, French would not count as a Romance language.

I have chosen this example because, as Trask (1996, p. 179) observes, the Romance languages are among the best documented cases of genetic classification, because Romanists have had the advantage of having massive documentation about Latin as the proto-language (Classical and Vulgar), which has facilitated their comparisons. (See also Campbell, 2002, p. 109.) They are ironically also one of those cases where the socio-economic history is quite well documented and favors invoking language contact as an important ecological factor. It is not by accident that anticipating Bailey & Maroldt (1977) on the development of Middle English, Schuchardt (1983), Meillet (1928), and Valkhoff (1960) argued that the Romance languages developed by “creolization,” though I disagree with their use of the term. As argued in Mufwene (2000, 2001), the concept of ‘creolization’ has no structural grounding (see

³ This is precisely the technical sense of genetic relatedness that I have assumed when questioning Thomason’s position that mixed languages cannot be genetically related to their lexifiers. This technical sense is after all not so different from the lay one that means ‘genetic kinship’. The lexifiers should of course be identified as parents, in the same way that my father and my mother are my progenitors but neither of them accounts alone for the composition of my genotype. Whether or not the reader agrees with my comparison depends largely on whether or not he/she accepts the genetic linguistics assumption of asexual filiation, which assigns only one parent to a language or group of languages.

also Thomason, 1997) and it represents no specific restructuring process or combination thereof.⁴

Just so that some of us will remember that historical/genetic linguists are no more unified than creolists are on a number of issues, let me note that Posner (1985) was among the first heretics to argue that creoles are dialects of their lexifiers. Trask (1996, p. 179) follows suit in stating that there are “two more Romance languages that are spoken only in the Caribbean”: Haitian Creole and Papiamentu. I have no idea whether or not they are “good” historical linguists, but their conclusions are based on similarities in the way Latin speciated into the Romance languages and the way that these diversified into what Posner (1996) identifies as the “neo-Romance” languages of the European colonies.⁵ For the record, they too invoke mutual intelligibility as a criterion, just as do Thomason & Kaufman (1988, pp. 168–169).⁶

An important question here too is whether a language can be related to another in a non-technical sense (see Note 3) and yet not be related to it genetically. Can Kituba, Lingala, and English be claimed to be related non-technically to French, simply because they have borrowed a lot of lexical items and phrases from this language? When lay people say that two or more languages are related, do they necessarily mean something different from the technical sense of genetic relatedness as shown by the CM or any other method? Or is it simply the case that they do not have enough information to distinguish common ancestry from “language convergence” (in the sense of *Sprachbund*)? Assuming that Thomason (2002) is right in suggesting that mixed language varieties are obviously related to their lexifiers in the non-technical sense but not in the technical one, let me note that she has still

⁴ The arguments are actually very similar to those adduced by Joseph (2001) against conceiving of ‘grammaticalization’ (to which I return below) as a specific diachronic process.

⁵ Although Mufwene (2001) makes allowance for such a position to be defended, Chaudenson (1992, 2001) proposes no such thing. He actually told me that I was daring in titling one of my papers “Some dialects of English are creole.” Recognizing that it is really up to speakers of a language variety to determine whether they speak a dialect of, or a separate language from, a genetically related language, I contented myself in Mufwene (2001) with noting that linguists have abused their expertise in suggesting that some offspring of some languages are more, or less, normal, hence more, or less, legitimate in the ways they have evolved.

⁶ To be sure, Thomason & Kaufman explain on p. 353, Note 1, that there are problems with using this criterion. However, they suggest that the situation is clearcut in the case of PCs, as they split abruptly, rather than gradually, from their lexifiers.

not addressed the following critical question that I posed above: What is the threshold of mixedness that a language variety cannot cross in order to qualify as genetically related to what for all intents and purposes must be considered its ancestor or a congener? She might also enlighten us more by identifying some of those boundary indeterminacy cases, which any categorization must naturally make allowance for.

Thomason claims that “When sources of pidgin and creole structural features can be identified with confidence, they usually come from substrate languages and/or from the operation of linguistic universals of various kinds” (2002, p. 105). Coming from a scholar of language contact who has often argued for the role of congruence (under the label of *convergence*) in the selection of PCs’ structural features, this statement is surprising. In any case, the best demonstrations of the kinds of substrate influence suggested by Thomason would have been provided by relexificationists. If, in the context of French creoles, Chaudenson (1992, 2001) and Corne (1999) have not yet dissuaded her from sweepingly downplaying substantial inheritance from the lexifier, DeGraff (2001a, 2001b) has done an impeccable job of falsifying this stipulation with more elaborate evidence from French and Haitian Creole. Barring faithful inheritance (which I think is disputable even in what Thomason would like us to think of as “non-mixed” languages), one can hardly deny the legacy of the lexifier in the vast majority of cases attributed exclusively to substrate influence, especially in the TMA systems (and negation in Haitian Creole). And, since there was language shift (by Thomason’s own admission) and we can assume that speakers of non-European languages targeted colonial varieties of European languages (however heterogeneous they were), the onus of convincing evidence should weigh more heavily on arguments of substrate influence – which even Chaudenson (1992, 2001) has not ruled out – than on arguments of inheritance from the lexifier. Targeting a particular language entails identifying aspects of its structures that are different from languages that one already speaks and doing one’s best to acquire them, though second language “acquisition” is even more imperfect than first language “acquisition.”⁷ This practical approach is based on the

⁷ I use scare quotes around *acquisition*, as around *transmission* below, because no native speaker is handed over a ready made system that they simply memorize and apply. They recreate their own system, known as idiolect, which generally replicates none of the idiolects that provided them primary linguistic data, though it shares features with all those idiolects

assumption by the learner that the target language is different from what they already speak. (Otherwise there would be no point in learning it, although there is also a concurrent assumption that the target shares some features with what they already speak.) To date, no study of naturalistic second language acquisition has provided evidence of any learner that would only acquire the phonological forms of lexical items in the target language ignoring all the semantic specifications and the combinatoric constraints associated with them, thus relying exclusively on the grammars of languages they already speak. The evidence has lied mostly in the form of interference which suggests partial influence. Consequently, regarding the development of creoles' structures, the null hypothesis should have been to assume grammatical inheritance from their heterogeneous lexifiers and to explain deviations from them by invoking substrate influence.

Thomason adds, "Notoriously, few structural features of prototypical pidgins and creoles can be traced directly and unambiguously to the lexifier languages" (105). First of all, this conclusion depends largely on the proportion of PCs' systems that have been investigated, notwithstanding the fact that the CM, which is presupposed by Thomason's claim, has hardly been applied, at least not using the right, nonstandard colonial varieties of the relevant European languages. One is also reminded here of the typical dictionaries of PCs, which are really designed as supplements of those of their lexifiers and include very little of the vast lexical materials the PCs and their lexifiers share. Being incomplete documentations of the lexica of the relevant PCs, they give an exaggerated impression of lexical differences between them and their lexifiers. One must, however, bear in mind that similar dictionaries of nonstandard dialects of the same European languages (would) likewise conceal the more numerous lexical materials that they share with their standard varieties. Otherwise, Chaudenson, Corne, and DeGraff lead us precisely to a conclusion quite contrary to Thomason's. Even Sylvain (1936), who has too often been invoked out of context to support the relexification hypothesis, leads us to their conclusion, viz., that in the particular case of Haitian Creole most of the features she discusses have diverse counterparts in nonstandard French. We should learn to overlook that infamous last sentence

on the family resemblance model. The terms *acquisition* and *transmission* are misnomers of convenience (Mufwene, 2001).

of her book – instigated apparently by her major professor – which seems to be the only part of it that most creolists who cite her have read (from quotations by other scholars).

The above studies are also the closest thing we can invoke about any attempts to apply the CM to PCs, though it is possible that Thomason has applied it (systematically) to some prototypical PC in a study that I do not know. There has just been an embarrassingly dominant tendency to compare structures of PCs to the standard varieties of European languages, precisely those that were not spoken in the settings where these new varieties developed. Likewise, we have also been misled by the significance of heterogeneity of the lexifier, as if the situation were unusual and unique to PCs (and other mixed languages). Like Chomsky (2000) and Mufwene (2001), Lightfoot points out that the situation is in fact also true of cases of language “transmission” not associated with PCs and that “acquisition” with modification is a selective process.

Neither Baker (1997) nor Thomason (1997) had a compelling reason for claiming that there was no target language in settings in which creoles developed (Mufwene, 2000). The alleged “fruitless”-ness of applying the CM to “mixed languages” is nothing but a foregone, a priori conclusion. When a language has inherited about 90% of its vocabulary from its nonstandard lexifier⁸ and shares so many aspects of its semantics and morphosyntax with it, would it not be time to also ask the following questions (again): Are our assumptions about “normal” language “transmission” (under conditions different from those of the development of creoles and pidgins) correct in the first place? Can any individual or population try to speak a particular language non-native to them by selecting only its lexical forms, without paying attention to the morphosyntactic patterns of their usage, and resort only to their native languages for this aspect of communication? As Chaudenson (2002) asks, is there any documented evidence of this in any other case of naturalistic language appropriation? Let me now add the following question: How can there have been a break in transmission of the lexifier when a PC shares so much of its grammar with its lexifier (typically a colonial koiné),

⁸ Saramaccan, other Surinamese creoles, and Berbice Dutch appear to be exceptions, although overall the position should be that the specific ecology of the development of creoles determined the extent to which languages other than their lexifiers have contributed to their vocabularies.

despite modifications that are to be expected from the learning process?⁹ One cannot ignore some basic structural similarities (not due to any universals) which obtain between the relevant European languages and the creoles that developed from them. The following features randomly stated off the top of my head as I write will suffice to illustrate this claim: 1) despite substrate influence, the phonologies of Haitian and Jamaican Creoles largely reflect how their lexifiers were spoken (e.g., the palatalized pronunciation of *cat* as [kyat] in Jamaican Creole is not particularly substrate, nor that of *yo* ‘them’ – etymologically related to today’s standard French *eux* – in Haitian); 2) the positions of adjectives in both creoles and in many others are more similar to what happened in their lexifiers than would have been the case if their grammars were the sole legacy of the substrate languages (this is especially significant because the category ‘Adjective’ is hardly part of the grammars of many substrate languages); 3) English creoles have definite articles where most substrate languages use a distal demonstrative (we don’t know for sure whether the use of the quantifier ‘one’ in the stead of the indefinite article is exclusive substrate influence); 4) the postposed “determiner” *la* in Haitian Creole does not lack affinity with a similar morpheme which has similar morphosyntax, semantics, and pragmatics in nonstandard (and even) colloquial French varieties, etc.¹⁰ An important question here that has typically been overlooked in the literature on the development of creoles is: Does “substrate influence” mean the same thing as “source of a structural feature?” To me

⁹ Some readers may want to invoke some anomaly in the fact that the structural features of a creole can be related macaronically to so many different dialects of the relevant European language. This is so obvious in the way Sylvain (1936) shows it. Aside from the fact that a similar objection has been raised against non-relexificationist substratist accounts, one must stop and ask oneself whether the same is not true of Romance languages in relation to Vulgar Latin, or of English in relation to the Germanic languages that were brought to England. Besides, as Meillet (1929) and Hagege (1993) have argued long before DeGraff (1999: Prolegomenon and Epilogue) and Mufwene (2001), *language transmission* and *language acquisition* are misnomers for the condition of availing primary linguistic data in the former case and the process of recreating a system, albeit unfaithfully, in the latter. Since idiolects are different in one respect or another, the input for every learner is heterogeneous. The difference in kind from the PC situation is more quantitative than qualitative.

¹⁰ One should realize now that, as Robert Chaudenson (p.c., 2002) once pointed out to me, even the term *lexifier* itself becomes questionable. It is more consistent with the approach I am disputing than with the one I argue to be the more adequate one. Well, I have continued using it for convenience sake. Perhaps by the time most of us agree that doing genetic creolistics is really doing genetic linguistics too, we will consider using similar terms such as *proto-language*.

the answer is clearly negative, especially when language “acquisition” is imperfect and linguistic systems are naturally osmotic. As often suggested by Robert Chaudenson, creoles are also precious windows into the structures of colonial varieties of the European languages from which they evolved.

I could be picky – or pickier? – and dispute many other aspects of Thomason’s column, but I will close only with one last comment. Her statement that “historical linguists are focusing on E-language” (Thomason, 2002, p. 106) is confusing. Due to his limited interest in it, Chomsky (1986) actually characterizes this notion ambiguously, viz., 1) as a set of utterances, expressions, linguistic behaviors produced by members of a community, and 2) as what Paul (1880) calls “Custom Language” (Weinreich et al.’s 1968 translation) – what I identify in Mufwene (2001) as “communal language” (i.e., a population of related idiolects). The former amounts not to some system but to the pool that provides the primary linguistic data that a linguist theorizes from – what Chomsky considers to be an epiphenomenon of (a) language. I am sure that no modern linguist conceives of a language as a set of utterances and that historical linguistics goes beyond using them as data (like other linguists) to infer changes in the system. Yet, although I agree that historical linguistics has focused on communal languages, I cannot resist siding with DeGraff (2001c) and Lightfoot (2002). A communal language is no more real than a species is; it is a construct. No changes occur in it that have not affected at least some of the idiolects from which it is extrapolated (Mufwene, 2001). Actually, one can go as far back as Paul (1880) to also note his clarification that the goal of historical linguistics has been to use data in order to highlight changes in the systems that have generated them. The primary reason why I resisted using DeGraff’s “E-Creole” in Mufwene (2001) was to avoid the ambiguity of Chomsky’s characterization of “E-language.”

In any case, what I see in Thomason’s column is the same thing I have observed several times before in creolistics, viz., an unjustified reluctance among many of us to accept the alternative that PCs are really as natural and normal as other “non-mixed” languages (if there are unmixed ones at all), and they have all evolved by the same kinds of restructuring (qua system reorganization) processes. I was, for instance, shocked to read in Bruyn (1996) and Plag (1997) that since many instances of “grammaticization” in the development of creoles are continuations of processes that started in their lexifiers, these are not really cases of grammaticization as documented in

other languages. The following quotation from Bruyn is particularly revealing:

The fact that *ben* does not function as either a particle or a lexical verb in the early stages of Sranan implies that there has been no language-internal development of a lexical item into a functional one. Rather, there must have been a kind of short-cut from the English participle, which has already some grammatical value, to the function of tense marker in the Creole language. To regard such a development as grammaticalization without further qualification would be unrevealing to the extent that in this case the process must be assumed to have taken place from one language into another, such that patterns of usage in English provided the starting point for a grammatical form in Sranan or other Creoles (30–31).¹¹

Such a claim seems to be justified primarily by a vested interest in maintaining the discontinuity (or break-in-the-transmission-of-the-lexifier) assumption which is shared by most current hypotheses of the development of creoles (Mufwene, to appear). It amounts to an appalling resistance to contrary conclusions that are clearly supported by the socio-economic history of the territories where creoles have developed. Perhaps it is consistent with Parkvall's (2001) concern that not assuming the evolutions of creoles to be unique in kind amounts to denying that there is a group of languages called creoles and putatively putting creolistics out of business.

There is yet every historical, non-linguistic reason for studying the same PCs as an interesting group of languages defined by the ideologies of particular conditions of trade and plantation settlement colonization of the 17th–19th centuries (see Chaudenson's, 1992, 2001 attempt to do so with his three unities of time, space, action). There are also plenty of linguistic reasons to hope that creolistics can make a contribution to general linguistics, in the same way that one can select any arbitrary set of languages and hope to make a

¹¹ The conclusion that “there has been no language-internal development of a lexical item into a functional one” doesn't of course follow if one assumes that creoles developed gradually from their lexifiers. The absence of *ben* as a tense marker from early Sranan texts (i.e., early 18th century) simply suggests that it had not yet emerged as an ANTERIOR marker or was simply not recognized as such by the authors of those texts. If creoles have evolved from their lexifiers by basilectization (the formation or consolidation of a basilectal sociolect) – which seems consistent with Bruyn's own observation about features missing from early Sranan texts – then the features considered particularly “creole” are later developments (Chaudenson 1992, 2001). If we can extrapolate from Baker (1995a, 1995b), these features would have emerged at different stages of the development of the creoles.

contribution to, for instance, linguistic typology and the study of language shift. As a matter of fact, a number of things we have been learning about PCs prompt us loud and clear to re-examine several things we thought we had understood about various aspects of Language, more specifically language evolution in the context of this discussion.

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