

## REVIEW ARTICLE

### *THE COMPARABILITY OF NEW-DIALECT FORMATION AND CREOLE DEVELOPMENT*

*New-Dialect Formation: The Inevitability of Colonial Englishes.* Peter Trudgill. Oxford and New York: Oxford University Press, 2004, viii + 180 pp.

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This is an informative book about the development of colonial English varieties that the author also identifies as “Southern Hemisphere Englishes” (SHEs), spoken in New Zealand, his primary focus, in Australia, which is also very much discussed, in South Africa, and in the Falkland Islands. According to T, they all developed much later than Caribbean and North American Englishes, the other colonial varieties, which receive only occasional comparisons in this volume. T’s main argument is that they all derive most of their structural features, especially phonological (the focus of the book), directly from British English varieties. (Australian English influence on New Zealand English (NZE) is admittedly marginal, although it cannot be completely dismissed.) The process of the formation of SHEs amounts to what is identified in Mufwene (2001) as “competition and selection,” leading to “restructuring” (a phenomenon similar to gene recombination in biology). T acknowledges this process more explicitly toward the end of the book.

The phrase “new-dialect formation” in the title, whose apparent interpretation as an outcome of several structural changes becomes clearer only as the book unfolds, should not be interpreted as a kind of change in itself. As the rest of this review shows, it is the end-result of several form- and structure-changing processes that lead to language speciation. Readers should be critical of some of the author’s conclusions, which are inconsistent with the facts he presents. My own perspective is especially creole-based, because the author invites it through passing inaccurate criticisms that are not supported by the growing history-informed literature on the development of creoles. I show below that, whatever T’s reasons for his comments may be, they turn out to be unfortunate distractions from an otherwise useful contribution to historical dialectology.

The book is based on a rich body of data consisting of: (1) “recordings made for the National Broadcasting Corporation of New Zealand between 1946 and 1948...from people who were children of the first European settlers in New Zealand...about 325 speakers born between 1850 and 1900”; (2) “recordings [allegedly made by the author] of eighty-four speakers from thirty-four different locations in New Zealand, both North Island and South Island, who were born between 1850 and 1889, which [he] take[s] to be the crucial formation period for [NZE]”. According to T, “none of the speakers sounds like modern New Zealanders; some of them sound like English or Scottish or Irish people, and many of them sound like no one at all except themselves” (p. xi). Discussions throughout the book involve comparisons with English in the British Isles up to the nineteenth century and afterwards, which preempt any objections one may raise against

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usage of apparent time to prove evolution. Data about Australian and South African Englishes are drawn from published sources, those on the Falkland Islands from Sudbury (2000), and those on the English of Tristan da Cunha from Schreier (2003).

According to T, SHEs are so similar (not identical, since they did not always select the same variants) because they developed around the same time, drawing their structural features from more or less the same pools. He invokes the following categories of reasons to account for divergence between them and British English: (1) changes that have occurred in the UK but have not taken place in the colonies; (2) changes that have taken place in the colonies but not in the UK; (3) adaptations (chiefly lexical) to the new ecologies; and (4) contacts with other European populations (which is also true of North America and Australia). Contact with the indigenous populations matter only in those regions that were inhabited when the Britons arrived.

T's discussions of the changes could have been enriched by the population genetics notion of "population structure," which constrains, *inter alia*, patterns and media of interaction that permit them.<sup>1</sup> I can illustrate this better with the colonization of North America. During the seventeenth century, European interactions with Native Americans (NAs) amounted largely to trade transactions and negotiations of treaties conducted significantly in NA pidgins, whereas Europeans' communications with African slaves were initially intense and intimate (though certainly not egalitarian), carried on in European vernaculars. It would also take a long time before the NAs were less "demarginalized," forced by socio-economic changes to function in the new, European-based world order, in which English is the dominant language. This delayed assimilation seems to explain why NAs have apparently exerted no substrate influence on structures of North American English varieties. They have been shifting to English, gradually, while their children have acquired Native American English features and the parents have died with their nonnative accents, just like today's immigrants. Critical mass and absorption in the larger population seem to be important factors bearing on whether or not a particular "ethnic" group can leave its mark on the structure of a language. Clyne (2003) shows this to be clearly so in the case of Australia.

Likewise, nationalistic parochialism among European colonists, which lasted until the early twentieth century, must have reduced the extent of influence that continental Europeans could have exerted on structures of North American Englishes. Although the overall demographic proportion of these immigrants exceeded that of the Britons in America by the nineteenth century, the incremental pattern of their population growth, including the later immigration of some groups and their gradual absorption into the prevailing Anglo socio-economic structure, weeded out much of the substrate influence they could have exerted on their present English vernacular. This is unlike the experience of African slaves, who shifted to English earlier in American history and became more and more segregated with the emergence of large plantations. The perpetuation of that *de facto* segregation has preserved in the speech of African Americans some structural differences that have been obliterated or are less common among European Americans.

Although none of these observations undermines the rich documentation that *New-Dialect Formation* provides of the overwhelming British origins of features of colonial Englishes, some readers will wonder why there is no mention of, for instance, the Maoris in T's discussion of the evolution of NZE. Invoking population structure, one may well refer to this book to support, for instance, Poplack and Tagliamonte's (2001) thesis that the origins of African American English structural features lie largely in colonial English

(see also Poplack, 2000) and my own uniformitarian position that creoles have developed by the same restructuring processes observable in the evolution of other languages (Mufwene, 2001, 2005).

T's book also supports my hypothesis that variation in ecology accounts for variation in the way competition and selection among the features was resolved. Such ecological variation includes differences in the times and natures of the variants introduced to the colonies, in their statistical representation within the relevant populations, and in the features of the other languages with which English came in contact. Even the exceptions to contact-based evolutions that T invokes, namely, Newfoundland and Falkland Islands Englishes (p. 7), illustrate this observation, because they are still restructured varieties and not replicas of specific English dialects. I return to this point below.

Although T intends to argue, against "many creolists" (left unidentified), that "the socio-historical evidence [is not] decisive" (p. xii), the book proves just the opposite. For instance, in T's own words, "because of extensive if minority Irish emigration to Australia, we shouldn't be surprised if Irish English has left some other, non-phonological traces" (p. 19). He refers here to the presence in NZE of features such as *youse* for the SECOND-PERSON PLURAL PRONOUN, *come here till I kiss you* 'come here so that I can kiss you', and sentence-final *but*, as in *I don't like it but*, among others, which he takes to be neither local innovations nor importations from other colonies.

Also, while T is correct in arguing for a polygenetic emergence of SHEs, he has no strong argument for assuming a monogenetic hypothesis for creoles. Many of the assumptions on which his discussions are based have been disputed, if not proved wrong. A number of us who have read colonial history closely now subscribe to the following hypotheses: (1) creoles have not evolved from antecedent pidgins (Chaudenson, 1992; Mufwene, 1996); (2) until the early nineteenth century, some contact variety of Portuguese served as the general trade language on the West African coast (Huber, 1999); (3) it is doubtful that a creole was already formed in Barbados at the time this island became the dispersal point of slaves in the Caribbean and North America in the second half of the seventeenth century (Mufwene, 2000); and (4) the contribution of Barbados or the Caribbean in general to the North American slave population in the eighteenth century, the period during which most creoles seem to have developed much of their current basilects, hardly exceeded 15 percent for North America.

I know of no historical evidence supporting the hypothesis that English in Caribbean colonies developed later than Creoles; in fact, history suggests a different, bifurcated evolution. Irvine (2004) is right in arguing that acrolectal Jamaican English speech derives its structural features from more or less the same colonial feature pool as its basilectal counterpart, called *Patwa*. Many of the problems in the scholarly literature lie in gratuitous working assumptions which have ignored history, for instance, assuming a priori that any Caribbean English feature that differs from metropolitan English must have a creole origin. Contrary to T's claim (p. xii), sociohistorical considerations do not amount to considering "linguistic" (i.e., structural) evidence as "tangential." The shift of focus in creolistics simply reflects an intention to reevaluate an important ecological factor that was undervalued for too long. It is misguided to study the evolution of a language without also paying attention to the social history of its speakers. *New-Dialect Formation* derives some of its strengths precisely from the fact that T does not ignore the relevant history.

T also uses a number of arguments considered progressive in creolistics, although they, too, need more explicit articulation. For instance, he states that colonial Englishes were

the outcome of “*leveling* in which minority forms, socially marked forms and linguistically marked forms are lost” (p. 23). None of this makes sense unless one also bears in mind that markedness is ecology-sensitive. Moreover, it is important to articulate at least some of the factors that make particular variants in competition more, or less, marked (Mufwene, 1991, 2001). A universal account of markedness as suggested by the language typology literature cannot account for why specific populations would prefer seemingly more costly solutions to communication, and/or why they stick to options that are allegedly marked and putatively ought to be discarded. Such accounts are also uninformative or superfluous regarding language development and change, as they sometimes invoke options that do not apply in given specific settings.

Because T hardly discusses what is involved in markedness-dependent explanations, the following examples from the literature should be helpful. It would be irrelevant, if not inaccurate, to assume that “tall” is universally less marked than “short” simply because it is more neutral in English (or Germanic languages) to ask questions such as “How tall is XYZ?” rather than as “How short is XYZ?” In a contact setting, other factors come to bear, especially because questions about height are not asked in the English way in many other languages. Similarly, one cannot assume that combining numeral classifiers with quantifiers or adjectives is marked for a particular population of learners when this is the only option they have known before coming in contact with the new target language. It is the alternative option that may be marked to them. Similar considerations apply regarding the selection of sounds, which could explain why *very* can also be pronounced as [βeri] (with a bilabial fricative) in Gullah but only as [veri] in other American English varieties, and also as [beri] in Jamaican Creole. The bilabial fricative is rare in African languages, but the slaves had proportionally more contacts with the European colonists in coastal South Carolina than in Jamaica, which would have facilitated the selection of this sound, which is also attested in Irish English, in Gullah. Too many choices have been made in various cases of language change or emergence that are against typology-based predictions, which suggests that those typological considerations are often irrelevant to such ecology-specific evolutions. Even when they appear to be relevant, such as with the exclusion of some vowels from the phonemic inventories of creoles, the selections are due primarily to ecology-specific dynamics. For instance, Gullah has a schwa and /Φ/, whereas Caribbean creoles lack them.

On the other hand, T sounds more like an old-fashioned creolist when he explains “determinism” in the way SHEs have developed, invoking their “tabula rasa situation” which he considers “very ‘unusual,’” in part because of “the absolutely pivotal role played by young children” (pp. 25–7). A problem with this position is that T does not explain what makes their situation “unusual.” His explanation of the “tabula rasa situation” as that “in which there is no prior-existing population speaking the language in question either in the location in question or nearby” suggests that the term *tabula rasa* is misused in the first place. Although it is used to suggest that there was no influence from indigenous languages, one cannot deny the fact that the children putatively developed the second-generation varieties from the variable materials made available to them by their parents. T invites the reader not to assume any language shift, thus to ignore the role that continental Europeans who interacted with the British immigrants may have played in the selection of structural features into SHEs. Therefore, the focus should be on *continuity* of some of the linguistic materials brought from the British Isles. T’s invocation of children as a critical factor in the development of SHEs does not deny this continuity,

nor does it support the alleged “tabula rasa situation.” As DeGraff (1999) points out in the case of creoles, this generational connection makes both age groups contributors to the current structures of the new varieties.

It is important to emphasize here that, as shown by Slobin (2002) and Tomasello (2002), children innovate only minimally, especially during the early stages of language development. I submit that even in language contact settings children guarantee selective continuity of structural features. The selection is indeed driven by a principle that T invokes correctly from Keller (1994): “talk like the others talk.” The difficulty which T sees in the case of SHEs is that the adults who are emulated do not all speak in the same way. However, is this not true of any population? Hasn’t speaking the same language always been a matter of likeness (Chomsky, 2000), which means variation, rather than identity, in a language or speech community? *New-Dialect Formation* is an opportunity to think about how norms emerge and how the adults themselves participate in the same selection process, although less successfully, as correctly observed by T (p. 28). Still, that does not exclude them, as is evident in the development of pidgins and indigenized Englishes, where adult involvement is more obvious.

In chapter 2 (pp. 31–82), T demonstrates clearly that SHEs have inherited their vowels and rhoticity from nineteenth-century England. They are generally marked by “colonial lag,” a delay of “about one generation, in the normal progression and development of change” (p. 34). This is allegedly caused by the absence of a “common peer-group model in early New Zealand or the other colonies for children to aim at” (p. 35). Putatively, this explains why the New Zealand population studied by T sounds more like British speakers of the mid nineteenth century than of the late nineteenth century (p. 35).

I find in the author’s observation a corroboration of my hypothesis that creoles and other nonstandard varieties spoken today in former European settlement colonies could be among our best windows into colonial Englishes. Although no SHE variety is an exact match for a particular dialect in the metropole, the individual features seem quite conservative. Their recombination into new systems is obviously new. T’s observations about SHEs are just as true of Caribbean pronunciations of *garden* and *carry* with palatalized velar stops, of *little* with a medial glottal stop, of *pot* with [a] rather than with [ɔ], and of *going* as [gwayn], to cite but a handful of examples, although the absence of the schwa can clearly be attributed to (corroborative) African substrate influence. Chapter 2, like the rest of the book, should be helpful to creolists who are too eager to invoke substrate languages to account for features that distinguish creoles from their non-creole kin which have evolved from the same European languages. It should remind them not to impute, too hastily, any divergent features (exclusively) to substrate influence without double-checking the different input varieties to which the slaves were exposed.

Unfortunately, like many creolists, T confuses “origins” and “formation” (p. 31) in “new-dialect formation.” Identifying the former does not necessarily explain how the specific features that interest us about colonial language varieties became integrated into these new systems. This is when, among other things, the markedness considerations to which I alluded above come to bear on the restructuring process. Interestingly, the features need not all become part of the system at the same time, an observation that supports gradualism over abrupt development, especially one crediting children with the formation of the new varieties.

In chapter 3 (pp. 83–99), T presents “six key processes” that account for SHEs: (1) mixing, (2) leveling, (3) “unmarking,” (4) interdialect development, (5) reallocation, and

(6) focusing. My general impression is that these categories describe the changes more than they explain what is involved in “new-dialect formation” in terms of what accounts for the selection of specific variants over other competitors. For instance, it is too simplistic to describe “leveling” as “the loss of demographically minority variants,” especially since populations do not necessarily grow uniformly and, as observed above (and consistent with Chaudenson’s (1992, 2001) notion of “periodization”), different linguistic variants are not all introduced at the same time. We have still not figured out several factors that make a variant less marked than – hence, favored over – a competitor. The explanation provided by T based on Fiji Hindi, for instance, sheds little light on the subject matter. Thus, we have no explanation for why *h*-insertion (in *it* ~ *hit*, for instance) persists in almost all nonstandard varieties of English, including creoles, although it is hardly a dominant feature and is even stigmatized.

Chapter 4 (pp. 100–12) elaborates T’s thesis that NZE, like other SHEs, owes its current structural peculiarities to the selections made “at will” (p. 108; but see below) by the first generation of children born to immigrants, and not to the mutual accommodations made by the latter. Children simply ignored some of the variants (especially highly localized ones) produced by their parents (p. 111). T terms the outcome of children’s selections “apparent leveling,” as opposed to the “rudimentary leveling” produced by the parents’ mutual accommodations.

Although I generally agree with T’s invocation of selection as an explanation, questions arise about some implicit assumptions of his specific hypothesis: (1) Do mutual accommodations not contribute as much to selection as the children’s omission of some variants during language acquisition? (2) Don’t both processes amount to a reduction of the number and/or statistical frequencies of some variants, as T acknowledges in the next chapter (p. 114)? (3) Assuming – incorrectly – that a population grows in the artificially neat addition of a child population to the adult population, during a clearly identifiable period and in a rather abrupt way, doesn’t the proposed “apparent leveling” also depend on the proposed “rudimentary leveling”? (4) Aren’t both kinds of leveling part of the longer and slower process of norm emergence, which, I admit, must have been slower among adult (native) speakers of different metropolitan dialects? (5) How else would we explain norm emergence in communities speaking, say, (expanded) pidgins and indigenized Englishes, in which adult speakers seem to be the principal “creators” of the varieties?

Chapter 5 (pp. 113–28) is about the “crystallized” variety, which corresponds to the period that Baker (1993) identifies as “Event 3” in his discussions of the development of French creoles. T’s term is “Stage III.” He addresses the following question: “Why were certain forms retained and others lost?” His first comment is: “This is not a question that students of new-dialect formation have always thought to ask” (p. 114). Genetic creolists will certainly be shocked by T’s claim, aside from the fact they may prefer to ask the question with the interrogative “How?” T’s answer is “leveling”: children of Stage III are claimed to have been “in a more stable social situation . . . with a more restricted set of variants to choose from” (p. 114). He also invokes markedness to account for the selections. His general position is that “majority forms survive,” although “unmarkedness” – an unfortunately ambiguous and perhaps unnecessary word – “may sway the balance in favour of minority variants, provided, obviously, that they are a large minority” (p. 120).

Of course, only sociohistorical considerations such as those provided by T himself can inform us about the evolution discussed here. Markedness considerations can be very circular if all we say is that particular variants are selected simply because they are

unmarked. Interestingly, as T suggests in the previous quotation, there are various reasons why a variant can be unmarked (see Mufwene, 1991, 2001). Such considerations bear, for instance, on the following informative instance of congruence discussed by T himself: "In many other cases, of course, the variant from the English southeast did survive. However, my point is that this happened only if these variants also *had* support, as it were, from other areas of the British Isles" (p. 121). Clearly, these variants were not minority options, even if the colonists from the English southeast were in the minority. If a feature was shared by other speakers, there is no particular reason why it must be identified as a peculiarity of one particular group. It is also possible that when true minority cases were selected, they were selected for reasons that may make demographic statistics only an epiphenomenon. Similar evolutions must have occurred when what may now be considered a minority group was the most important among the earliest settlers. By the Founder Principle (see below), it recruited more carriers of its features among the later settlers, while the overall population grew only gradually, by installments of immigration and by birth. By the principle of Generative Entrenchment (Wimsatt, 1999), such features became more deeply rooted and more and more typical of the relevant colonial variety.

One should not downplay historical considerations; and markedness should not be invoked so casually as to dismiss cases that deserve better understanding of the process of competition and selection, the true general process that produces "leveling," which T also characterizes as "a *statistical composite* of dialect mixture" (p. 123). T is also correct in arguing that the process is "not random" (p. 126), if he means by this that selection is guided by principles that we do not fully understand yet. As suggested above, the usual invocations of markedness in the literature have not been explanatory. (See Mufwene, 1991, 2001 for more detailed discussions.)

In chapter 6 (pp. 129–47), T identifies "two rather different types" of "drift" in SHEs: the continuation of "linguistic changes that are already in progress in the common source," and inheritance from the common source of "shared *tendencies* or *propensities* which can subsequently lead to the development of similar but new changes" (p. 132). The former type is illustrated by "diphthong shift" and by a "fronted and lowered STRUT" vowel, whereas the parallel kind of innovation is illustrated by "HAPPY tensing," "glide weakening," and the "NURSE vowel."

This kind of explanation will not sound novel to creolists familiar with Chaudenson (1992, 2001), though the latter does not invoke the notion of "drift" at all. I am not sure whether the explanation of "drift" cited by T from Sapir (1921: 150), namely, "language moves down time in a current of its own making," is really what matters (p. 131). Readers interested in evolutionary processes are more interested in the mechanisms that eventually made SHEs different from the outcomes of the evolution of English in the UK and the USA, with "drift" then to be interpreted roughly as "divergence," a label describing the end result of changes rather than an explanation of how things evolved the way they did. I am not convinced that the distinction between the two types of "drift" is really adequate.

By T's own admission, some British English dialects showed tendencies for the developments now associated with SHEs. This suggests that the relevant "innovations" started in the British Isles, although differing ecological factors have disfavored these tendencies in the metropole but favored them in the colonies. Thus, the reader may be interested in three kinds of changes: (1) those evolutions in SHEs that started and have kept the same trajectories as in the metropole; (2) those evolutions that started in the metropole but have continued only in the colonies; and (3) those evolutions that are colonial innovations but

have no counterparts at any time in the metropole. (Those evolutions that started and continued in the metropole but are not attested in SHEs are of no particular interest.) In the third case, it will be informative to know which innovations are shared by all SHEs and which ones are not. I submit that each new ecology of dialect contact, factoring in the effects of periodization, creates a new “balance of power” among the competing variants, which sets up new evolutionary trajectories, accounting for differences not only between SHEs and present metropolitan English dialects but also among the SHEs themselves.

Much to my relief, T acknowledges this different interpretation in chapter 7: “the nature of the mixture [of ingredients] varied, at least somewhat, from one individual settlement to another” (p. 161). This is precisely what Chaudenson (1992, 2001) and I (Mufwene, 1996, 2001) have argued, namely, the variation that creoles of the “same” lexifier display in their structures reflects variation in the ecologies of language contact, factoring in also variation in the mix of substrate languages.<sup>2</sup>

In the final chapter (pp. 149–65), “Determinism and social factors,” T dismisses the adequacy of notions such as “prestige,” “status,” and “stigma” as explanations for structural changes leading to new varieties. “It is purely a matter of who interacts the most often with whom – a matter of density of communication” (p. 149). Although the detailed discussion that follows proves the author right (perhaps because prestige was not a relevant factor in the settings in which SHEs developed), one should beware of extrapolating from the categorical and general way T states his position on new-dialect formation in “*tabula rasa* situations” (p. 148). As argued in Mufwene (1991, 2001), factors that assign markedness values are not exclusively structural, and they can work in conflicting ways. Also, frequency and “density” of communication do not apply uniformly to all members of a population.

T contradicts himself when he invokes Croft’s (2000: 73) “nonintentional mechanism of selection” (similar to Mufwene’s 2001, 2002 “competition and selection”) as one of the explanations for new-dialect formation (pp. 150, 158), and also when he appeals to “altered replication” (identified in Mufwene 2001 as “imperfect replication”). The contradiction stems from his earlier assertion that children made the selections “at will” (p. 108).

In addition, T misrepresents the Founder Principle on p. 164, as shown below. I invoked it in Mufwene (2001: 28–9) “to explain how structural features of creoles have been predetermined to a large extent (though not exclusively!) by the characteristics of the vernaculars spoken by the populations that founded the colonies in which they developed.” I invoked it also to explain why the Bantu slaves, who became dominant in South Carolina in the mid eighteenth century, seem to have exerted less influence on the structures of Gullah than the Kwa populations that preceded them (2001: 44). I then clarify my position in the same chapter: “The Founder Principle does not preclude later influence as the ethnographic conditions of the contact setting changed during the gradual and protracted development of the new vernacular, especially during its basilectalization phase” (p. 76). T does not show how such an account is disputed by his study when he quips: “as Mufwene concedes (2001: 76), the founder principle works unless it doesn’t” (p. 164). What is more shocking is that he concludes the book with, among other statements, the following, which I find consistent with the Founder Principle: “The initial shape of New Zealand English, and thus by implication of the other colonial varieties, can be accounted for . . . in terms of the mixing together of different dialects of English from the British Isles, plus drift” (p. 164). Other than my reservation about the invocation of “drift” as an explanation, my only objection to this latter statement must be predictable

for some: the mixing is really not that of dialects but rather of dialectal features, which is more consistent with T's correct invocations, at a few places in the book, of selection as an explanatory mechanism. Consistent with the definition of *restructuring* I proposed above, features from diverse varieties have been reintegrated, variably from one contact ecology to another, into the new systems constituted by SHEs.

Overall, this book is useful to anybody studying the evolution of English in the colonies, including creolists and students of indigenized Englishes. It highlights, against the biases of its author, similarities in the restructuring processes that have produced them, especially those pertaining to the competition among variants in the inputs and to the relevance of sociohistorical considerations to understanding factors bearing on selection. It also underscores the significance of periodization and of gradualness in evolution, made more evident by the three evolutionary stages identified by the author. I simply wonder why, without checking the relevant literature, T assumes (p. 5) that the development of creoles, unlike that of SHEs, must be monogenetic. The blurb of the book also claims that "Trudgill here presents a controversial new theory about dialect contact and the formation of new colonial dialects." As this review shows, the book may be controversial, but the theory is not new, at least not to informed creolists.

#### NOTES

1. The notion of "population structure" should also explain why there is little influence of African American vernacular English on its European American counterparts, overlooking the recent ones (mostly lexical), for instance those spreading through the hip-hop culture, among the youth. Segregated populations, whose emergent varieties are particularized by unequal ecologies, can hardly influence each other directly, although they share a lot of structural materials inherited from related ancestors (colonial English varieties, in the case of North America and the Caribbean).
2. To be sure, T must assume that creoles are not dialects of their "lexifiers" and have not developed the same way as SHEs. However, whether or not they are dialects of their lexifiers does not determine that they developed by different kinds of restructuring processes (see Mufwene, 2001, and below). In any case, this is a question that has come up several times in genetic creolistics.

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