Language Acquisition in Creolization and, Thus, Language Change: Some Cartesian-Uniformitarian Boundary Conditions

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Content

Abstract

This essay prescribes some broad 'Cartesian-Uniformitarian' boundary conditions for linguistic hypotheses about Creole formation. These conditions make constructive connections between Creole studies, historical linguistics and language-acquisition research. Here 'Cartesian' has a mentalist sense, as in Chomsky (1966): I consider the formation of so-called 'Creole' languages to be ultimately reducible to the creation, in certain sociohistorical contexts, of certain idiolects (i.e., individual internal, or 'I-', languages) in the minds of the 'first "Creole" speakers'. To avoid circularity, my use of the term 'Creole' in the phrase 'first "Creole" speakers' combines some of its original ethno-historical senses: I use the word 'Creole', in this particular context, to refer to the non-indigenous people of African or European descent that were born and raised in the colonial New World, in opposition to those that were born and raised in the Old Worlds of Africa and Europe. The term 'Uniformitarian' evokes Neogrammarian

approaches to language change, as advocated, for example, by Osthoff and Brugmann (1878) and Paul (1890). It summarizes my fundamental working assumption that no *sui generis* or exceptional *linguistic* processes need to be postulated in order to explain the creation of these languages that have come to be labeled 'Creole': these languages were created by the same psycholinguistic mechanisms that are responsible for the creation of (I-)languages, and for linguistic diachronic patterns, everywhere else. Therefore, 'Creole' languages cannot be distinguished *a priori* from non-'Creole' languages on any linguistic-theoretical criteria – and 'Creole' languages can be genetically classified by the Comparative Method, on a par with non-'Creole' languages. Such assumptions go against popular claims about Creole genesis such as those in, for example, Thomason and Kaufman (1988), Lefebvre (1998) and Bickerton (1999). In establishing these Cartesian-Uniformitarian guidelines, I correct category mistakes that fail to distinguish explanations that apply to I-languages from explanations that apply to E(xternal)-languages and other social-group phenomena that I-languages are implicated in. One such category mistake concerns the ontology and time course of innovations in specific I-languages vs. their spread across populations of I-languages and into E-language communal norms. Then I investigate the possible – and impossible – contributions of first-language acquisition and second-language acquisition to 'creolization'. In particular, I take my cues from: (i) studies of language- and dialect-contact situations where children and adults seem to play observably distinct roles; and (ii) recent discoveries about instances of Sign Language acquisition and creation where the Primary Linguistic Data seem remarkably restricted. As for elucidating the limits on the restructuring capacities of children and adults, and their respective contributions to language creation and change, it is epistemologically safer to investigate instances of diachronic development that are more recent than the now unobservable early stages of 'creolization'. Throughout this essay, I use the term 'creolization' strictly as an a-theoretical abbreviation for the longer phrase 'development of these languages that, for sociohistorical reasons, have been labeled "Creole"'. In the perspective sketched here, creolization is just another instance of language change – or 'language evolution', in Mufwene's (2001, 2008) sense – the investigation of which is to shed light on Universal Grammar.

1. Background and Objectives

1.1. TERMINOLOGICAL AND CONCEPTUAL PRELIMINARIES: 'CREOLES', 'CREOLE GENESIS', 'CREOLIZATION', ETC.

What sorts of 'Creole genesis' hypotheses can, or cannot, be derived within a linguistic-theoretical framework that, contrary to widespread assumptions in Creole studies (see DeGraff 2005a,b,c), does not a priori posit any exceptional developmental patterns or psycholinguistic processes on the part of whoever 'created' these languages we call 'Creoles'? This is the main question that will concern me in this essay. Although I will be able to sketch only a few epistemological and theoretical preliminaries

toward a satisfactory answer, these preliminaries should at the very least help clarify the meanings of oft-used terms such as 'Creole languages', 'Creole genesis', 'creolization', etc., and their epistemological status vis-à-vis linguistic theory and theories of language change.[1] Such clarification is also meant to establish some basic boundary conditions for linguistic hypotheses about Creole formation, and, in so doing, contribute to constructive conversations between creolists, historical linguists and language-acquisition researchers – conversations that integrate Creole studies into mainstream linguistic-theoretical research. As envisaged in this essay, these conversations will evolve within a *perforce broad*, yet productive, set of methodological guidelines. I write 'perforce broad' because the actual structural outcome of each specific language-contact and language-change scenario depends on the complex particulars of *contingent* combinations of ecological factors – in the same way that no two developmental sequences, be it ontogenetic or phylogenetic, can be identical. All we can provide are broad boundary conditions that limit the space of our (thus far, much too unconstrained) hypotheses.

Pending the main discussion below, there is one starting assumption that gives this essay its 'Uniformitarian' orientation. But, before I can state this assumption, I must stress that in this essay the term 'Creole' and its process–nominal derivation 'creolization' are, to start with, strictly *a*-theoretical cover terms that do not presuppose any operational linguistic-structural criteria. For me, 'Creole' is an *ostensive* label that I use to point to certain speech varieties that, in sociohistorical and geopolitical terms, emerged, most typically in the Greater Caribbean, via contact between Europeans and Africans in the course of Europe's imperialist forays in Africa and the Americas in the 15th through 18th centuries. In this vein, the derived noun 'creolization' is used here as shorthand to label the sequence of sociohistorical events that led to the formation of these languages that were among the first to become known as 'Creoles'. Such ostensive definitions will allow me to stay away from the terminological circularity, uncertainty or vacuity that have long plagued the use of these terms in Creole studies and elsewhere. (For a variety of critiques, see, e.g., Mufwene 2001ff; M.-R. Trouillot 2002; DeGraff 2003ff; and various essays in Stewart 2007.)

Siegel (2007: 187–193) is a recent example of the too indiscriminate usage of 'creole'/'creolization' in so called 'postcolonial discourse'. There Siegel's ill-defined notions of 'creolization' in language *and* culture are all the more revealing that at least one of the references cited there, namely M.-R. Trouillot (2002), warns against such pêle-mêle use of these terms, which Trouillot bemoans as 'too sweeping' in absence of any 'agreement in defining creolization as an object of study' (p. 194f). In order to parse my arguments below, it is crucial to keep in mind that the ostensive and a-theoretical nature of my own use of the terms 'Creole' and 'creolization' goes against the grain of many (most?) linguists' received notions about

'Creole' and 'creolization'. And my own focus here is solely on the development of *linguistic*, not cultural, patterns.

What are these sociohistorical events that are usually claimed to characterize 'creolization' as, say, in the Caribbean, the *locus classicus* for the formation of plantation Creoles? To answer this question, let's consider Haitian Creole (HC), my native language, as a poster case. With at least 10 million speakers of whom a vast majority (at least 7 million) are monolingual HC speakers, HC is one of the most spoken and best documented 'Creole' language – 'the best described of French Creole dialects, if not of all Creole languages' with 'Haiti . . . becoming the light-house of Creoleness' (Valdman 1971: 202, 1979: 100, my translation; see Védrine 2002 for a recent bibliography of writings on HC). HC is also one of Haiti's two official languages, alongside French, and its only national language; that is, the only language that all Haitians in Haiti have in common, including at least 7 million monolingual HC speakers (M.-R. Trouillot 2002: 204). Given HC's well accepted status as a 'Creole' language under anyone's definition, HC data can certainly be used as a benchmark to test various hypothesis about 'Creole' structures and their development, even as we keep in mind that these hypotheses and their respective assumptions often contradict each other and do not establish any logically or empirically consistent class of 'Creole' languages.

In this paragraph and the next, I summarize my basic assumptions about the history of HC (DeGraff forthcoming). HC, like other Caribbean Creoles, developed mostly among Europeans and Africans via language acquisition by adults (L2A) and by children (L1A) in a complex mix of language-contact settings. The complex sociohistorical factors therein included a continuum of social divides and power asymmetries (Moreau de Saint-Méry 1797; Alleyne 1971; Baker 1982; Bickerton 1996; Singler 1996; Chaudenson and Mufwene 2001, etc.). One end of this continuum was characterized by drastic opposition and inequality between speakers of the European 'superstrate' languages and speakers of the African 'substrate' languages from the Niger-Congo area, with the Europeans and their descendants typically at the highest levels of the colonial socioeconomic and political hierarchy, and the Africans and their descendants at the lowest levels of this hierarchy. In this vein, the 'super-' in 'superstrate' and 'sub-' in 'substrate' refer to the socioeconomic differentials among the speakers of the corresponding languages, not to the chronology of their arrival in the locus of language contact. At the opposite end of the social-divide and power-asymmetry continuum, the superstrate and substrate speakers had relatively intimate interactions, especially during the colony's initial period of settlement in and around small farms and homesteads (*habitations*) whereby substrate speakers were outnumbered by, and in relatively close contact and interdependence with, superstrate speakers. Such relatively intimate interaction across class and racial lines – though most prevalent during the early-settlement period and much

more infrequent at the peak of the plantation economy – did continue throughout the colonial period among and around certain groups (e.g., mulattoes, domestic slaves, European indentured servants and proletarians a.k.a. 'petits blancs', etc.). These groups played a variety of 'buffer' roles (race-, class- and language-wise) between, on the one hand, the European capitalists and paid professionals and their descendants and, on the other hand, the African enslaved population and its descendents.

These continua would entail, *throughout colonial history*, corresponding continua of second-language (L2) learner varieties of the superstrate language – or, more accurately, *L2 varieties of the superstrate's colonial dialects and koinés thereof, many of which are quite distinct from contemporary (standard) varieties of the corresponding European languages as spoken in Europe and the Caribbean.* (The facts mentioned in the two italicized passages of the previous sentence, though often neglected by creolists, are of great importance in understanding the historical origins of Creole structures; see, e.g., Fattier 1998; Chaudenson and Mufwene 2001; Mufwene 2001ff; Siegel 2003ff.) These nonnative varieties, alongside native varieties, of the (koineized) dialects of the superstrate language would in turn become part of the linguistic ecology, including the language-acquisition input (i.e., the 'Primary Linguistic Data' [PLD]), for increasingly numerous cohorts of native (Proto-)Creole speakers. All these native and nonnative varieties together would thus seed a continuum of 'Creole' lects, from 'acrolectal' to 'basilectal', that is, locally developed varieties that ranged from the most structurally similar (the acrolectal) to the most structurally dissimilar (the basilectal) in comparison to the original European varieties and their koinés in the Caribbean. In the Haiti case, one side effect of the removal of most of the French population at the time of Haiti's independence in 1803 was the gradual flattening out of much of the variation that had previously been induced by contact with a range of French 17th/18 French dialects (a.k.a. 'superstrate') as spoken by the French colonizers. (See note 3 below. Also see the references in note 5 and DeGraff 2002, 374–94, 2005b,c for additional references and sociohistorical and linguistic details).

At this point, even the non-alert reader may point out that there are speech varieties that have originated outside the Caribbean (e.g., in Australia) and whose history is somewhat similar to that of HC in terms of language contact. Should these varieties be labelled 'Creoles' as well? Since I am skeptical about the validity of any linguistic-structural litmus test for Creole languages, I avoid circularity by intently narrowing my focus here on the Caribbean as the locus for the 'classic' Creoles. Alongside the (often exaggerated and over-rated) 'pan-Creole' similarities (see, e.g., DeGraff 2001b: 294–299), there exist substantial *dis*-similarities in both the histories and the structures of these languages that linguists call 'Creoles' (see, e.g., the 'parallel outlines of 18 Creole Grammars' in Holm and Patrick 2007). There also exist noteworthy similarities between the histories and develop-mental patterns of certain Creoles (e.g., in the Caribbean) and those of

certain non-Creoles (e.g., Romance and Germanic languages in Europe and the Americas). The implications of these similarities for the genetic classification of Creole languages are considered in a long series of publications (Meillet 1914 [1958] contra Schuchardt 1917; Hall 1950, 1958; Weinreich 1958; Goodman 1964; Valdman 1971ff; Posner 1985, 1996; Guy Hazaël–Massieux 1996; Chaudenson and Mufwene 2001; Mufwene 2001ff, etc.). I myself have discussed case studies related to HC (DeGraff 1995ff), some of which will be highlighted below, in section 2.3.3.

My intent in this essay – following, for example, Posner (1985, 1996), Guy Hazaël–Massieux (1996), Mufwene (2001ff), etc. – is to question whether the terms 'Creole' and 'creolization' have any linguistic-theoretical validity. Thus, we cannot start the argument by assuming that the set of languages known as 'Creole' across the world can be singled out on any specific developmental or structural criteria, especially in light of the fact that such definitions of 'Creole' have failed so far (see Ansaldo et al. 2007 for a recent 'deconstruction' of such definitions, contra McWhorter 2005, inter alios). As linguists, we are thus on safer epistemological grounds when we look at 'Creole' languages without assigning any theoretical baggage to the term 'Creole' – that is, without assuming that these languages around the world that have been labeled 'Creole' form a class based on any coherent set of strictly linguistic criteria, be they diachronic or synchronic (see, e.g., the critiques in DeGraff 2001a,b, 2003, 2004, 2005a,b,c and the data, analyses and references therein).

If you grant me this prudent epistemological stance, then we should not expect any specific sociohistorical or structural claim about any subset of languages known as 'Creoles' (e.g., Caribbean Creoles or French-based Creoles) to be straightforwardly extrapolated to all other languages known as 'Creole' across time and across space. Indeed Holm (2007: v), in his introduction to Holm and Patrick's (2007) *Comparative Creole Syntax: Parallel Outlines of 18 Creole Grammars*, warns that '[t]here has been mounting evidence that the similarities [across Creoles] are limited'. For now, I am assuming that the term 'Creole', after much 'mounting evidence', lacks any invariant linguistic-structural import (i.e., there is thus far no coherent and consistent 'Creole typology'). I thus consider it a fallacy to a priori expect any specific structural or developmental feature of a given Creole (e.g., Haitian Creole in the Caribbean) to necessarily have an analogue in some other Creole (e.g., Reunionese Creole in the Indian Ocean) 'simply' because both languages have been called 'Creole'.

My recent strategy has thus been to eschew as much prejudice as possible about the empirical and theoretical entailments of the term 'Creole', and to focus on the 'classic' examples of Creole languages in the Caribbean such as my native Haitian Creole as test cases that can help us constructively question the structural and theoretical validity of the term. The Haitian Creole data put me on relatively reliable empirical foundations. Besides Haitian Creole – with a history of formation that is steeped in

large-scale language contact – has often been enlisted to illustrate what should count as prototypically 'Creole' patterns of development and structure. It thus seems reasonable to use Haitian Creole to evaluate the *linguistic* criteria that have been claimed to partition the world's languages into Creole vs. non-Creole.

1.2. UNIFORMITARIAN BOUNDARY CONDITIONS

I will now spell out how my own use, in this article, of the terms 'Creole' and 'Creolization' relate to my basic 'Uniformitarian axiom'. This axiom appeals to the general sort of methodological considerations that were most forcefully introduced by the Neogrammarians:

> The factors that produced changes in human speech five thousand or ten thousand years ago cannot have been essentially different from those which are now operating to transform living languages. (Brugmann 1897: 2, on 'the many valuable contributions of William Dwight Whitney')

> [It is of] chief importance to understand that which is now before us, and that which belongs to the immediate past, investigating its growth and development. Its principle is this: to take as the starting-point what is known by experience, and to apply this to the unknown of the past . . . In this way we may hope to throw some light even upon those most remote periods in the history of our language. (Brugmann 1897: 32, again acknowledging his intellectual debt to Whitney)

> . . . the mental and physical activity of man must have been at all times essentially the same when he acquired a language inherited from his ancestors and reproduced and modified the speech forms which had been absorbed into his consciousness . . . (Osthoff and Brugmann 1878 [1967: 204])

I am thus assuming that the linguistic 'mental activity' of the agents of Creole formation in the Caribbean was 'essentially the same' as that of the agents of any other diachronic development. Therefore, the development of Creole languages cannot be reduced to some theoretically well-defined set of *sui generis* developmental processes that would apply in the development of all Creoles and of Creoles only, or whose application would result in a special typological class (a 'Creole' typology) identifiable on strictly structural grounds. Along with Mufwene (2001: 138, 2004: 460, etc.) and others, I believe that, given all available evidence thus far, 'creolization' is a (too) loose cover term for a variety of sociohistorical processes that does not correspond to any well-defined combination of linguistic developmental processes that would apply in *exactly* the formation of the languages known as 'Creoles'.

   In Uniformitarian fashion, I will therefore be assuming as one crucial boundary condition, for the much-discussed Caribbean cases of Creole

formation for example, that the aforementioned European and African expatriates alongside their 'Creole' (i.e., locally-born) descendants were cognitively on a par with one another and with language learners everywhere else and at any other time of human history. '*The great resemblance of all linguistic processes in the most different individuals is the most essential foundation for an exact scientific knowledge of these processes*' (Paul 1890 [1970: xlv], emphasis in original). Indeed, it can be reasonably assumed that the language-learning and language-creating capacities of our human ancestors have remained uniform across the species in the past few millennia (see Christy 1983 for a survey of Uniformitarianism in linguistics).

As I show below, the criteria that have traditionally been used (e.g., in Thomason and Kaufman 1988) to distinguish 'Creoles' from other languages are not empirically supported (see DeGraff 2005b,c for the empirical, theoretical and sociological failings of Creole Exceptionalism). Such findings are not surprising once we assume, in Uniformitarian fashion, that the human mind deploys the same linguistic cognitive processes when faced with the task of learning languages on the basis of PLD. The latter has the following two fundamental properties:

(i) The PLD for any given language learner is necessarily sparse in relation to the potential output of the target grammars.
(ii) The PLD for any given language learner originates from a necessarily heterogeneous population of idiolects.

These considerations bring us to our Cartesian boundary conditions for the study of language acquisition in creolization and language change.

1.3. CARTESIAN BOUNDARY CONDITIONS

When trying to elucidate the underlying processes that are ultimately responsible for Creole genesis, creolists often mention language-acquisition research, yet they cite this research in order to support conclusions that contradict each other's positions (see, e.g., Lefebvre 1998 vs. Bickerton 1999, to which we return below; also see note 4). Yet what we know of language acquisition should impose the *same* boundary conditions on all our models for language change, including Creole formation.

At first approximation, and in light of a variety of data and observations in language-acquisition, historical-linguistics and sociolinguistics research, it can be reasonably argued that language-change patterns in the history of all languages, including Creole languages, ultimately depend on innovations in *individual* instances of first- and/or second-language acquisition and on language use by individuals (see, e.g., Meillet 1919ff [1958]; Weinreich 1953; Trudgill 1986, 2002, 2004; Chambers 1995; Lightfoot 1999, 2007; Rizzi 1999; Kroch et al. 2000; Chambers et al. 2002; Jones and Esch 2002; Joseph and Janda 2003; Wexler 2003;

Winford 2003; Guasti 2004; Crain et al. 2006; Labov 2007; I. Roberts 2007 for extensive surveys and bibliographies). This too was adumbrated by the Neogrammarians:

> The human speech mechanism has a twofold aspect, a mental and a physical. . . . [O]nly through . . . knowledge [of this psychophysical mechanism] can the comparative linguist obtain the correct view of the way in which linguistic innovations, proceeding from individuals, gain currency in the speech community. (1878 [1967: 198])

Indeed it is through both first- and second-acquisition and through language use that innovations can in principle spread and 'gain currency' (i.e., stabilize), at the *group* level, into new tacit linguistic norms.

It can then be proposed that, in the case of Creole languages, the innovative patterns that resulted from *both* first- and second-language acquisition in the context of language contact spread through, and crystallize in, newly emerged speech communities, providing the latter with linguistic symbols of their new 'Creole' identity as part of new linguistic norms (see, e.g., Le Page and Tabouret-Keller 1985). In the case of *non*-Creole languages, one should ask whether second-language acquisition would routinely play as substantial a role in their formation as in the formation of *Creole* languages. Language contact and language shift of the sort that prevailed in the colonial Caribbean did play a key role in the history of many non-Creole languages, such as the Germanic and Romance languages in and outside Europe (Meillet 1914 [1958]; Weinreich 1958; Mufwene 2001ff, among many others).

What about non-Creole languages that, unlike Romance languages, have had no (apparent) language contact in their history? From the Cartesian I-language perspective on language change that I assume in this article, this question suffers from presupposition failure since there is *always* language contact – at the very least, contact among dialects and idiolects, which are perforce heterogeneous. This seems a reasonable assumption once we agree that languages with names such as English, French, Japanese, Kinande, etc., are convenient reifications built on top of necessarily heterogeneous idiolects that in turn cluster into distinct varieties, dialects, etc. Idiolects thus have ontological priority over community-level varieties. From this I-language perspective, 'language contact' happens *everywhere*, but at distinct levels of granularity, be it at the levels of 'idiolects', 'dialects' or 'languages', with the usual caveats about the dialect-vs.-language distinctions and implications thereof with respect to, say, the scale of structural innovations. For example, are we dealing with koineization/homogenization phenomena among structurally close dialects or with drastic structural innovations due to the typological distance between learners' L1 and the target L2. These are key issues some of which I, too briefly, touch on throughout this essay.

With these lengthy, but necessary, caveats and assumptions now established as boundary conditions on our speculations about Creole formation, this essay will go on, via many seemingly unavoidable detours, to try to constructively clarify some of the key terms that linguists use when talking about Creole languages and their diachrony, and the latter's (dis)connections with other diachronic phenomena.

In Section 2 ('Whence Creole genesis?'), I ask: What do various creolists working in different frameworks really mean by their respective uses of the phrase 'Creole genesis'? My answer to that question will suggest that fundamental, and often unrecognized, differences across (implicit) theoretical assumptions render certain uses of the terms 'Creole' and 'genesis' mutually incompatible – and even self-contradictory in some cases. There are three conceptual domains where contradictory assumptions are especially crippling as they pertain to the foundations of creolistics.

First, pertaining to the ontological status of Creole genesis: Are we talking about the creation of bona fide I(NTERNAL)-LANGUAGES qua 'system[s] of knowledge of language attained and internally represented in the mind/brain' (Chomsky 1986: 24) (cf. 'individual languages' in Neogrammarian parlance; see, e.g., Paul 1890 [1970: 456])? Or are we dealing with the creation of some socially defined types of entities (e.g., communal, national or 'E(xternal)-'languages) that, by definition, live outside the heads of individual speakers? Second, one we've identified and separated I-language vs. E-language levels of explanation, we can then constructively ask about the differences and relationships between (the tempo of) innovations in I-languages vs. (the tempo of) the spread of said innovations in populations of I-languages and the reflexes of such spread in communal linguistic norms. These are important issues to clarify given the oft-repeated claims that the (spectacularly fast?) rate at which Creole languages were created is one of the characteristics that make these languages exceptional. Third, pertaining to the agents of Creole genesis: Who, among children and adults, can 'create' the I-languages that go by the name of 'Creoles'?

In Section 3 ('On time machines and other expediments for creolists'), I highlight a small, but key, set of results from language-acquisition and sociolinguistics research that may help us set additional boundary conditions on our 'Creole genesis' scenarios, given a particular theoretical perspective on 'Creoles' and on 'genesis', one that is both Uniformitarian and Cartesian. There I will address one sort of constructive intersection between Creole studies and language-acquisition research, with implications for broader theories of language change. I will also address constructive connections between archival research on the early stages of creolization and theoretical investigation into the *universal* constraints that regulate

the development and structures of Creole idiolects on a par with non-Creole idiolects.

In Section 4 ('Integrating both second- and first-language acquisition in Creole-formation scenario'), I sketch a scenario for Creole formation that enlists and combines the aforementioned results from research in sociolinguistics and in first language acquisition (L1A) and second language acquisition (L2A) toward a 'Uniformitarian' model of Creole formation, one that integrates Creole formation within the larger realm of language-change and language-creation phenomena. This model is also 'Cartesian' to the extent that it adopts and promotes a mentalist approach whereby languages – or I-languages in Chomsky's terminology – are mental properties of individuals, properties that fall in the space defined by Universal Grammar (UG). This Cartesian assumption, like the Uniformitarian one, rehearses Neogrammarian caveats that seem forgotten by many creolists.

As it turns out, there has been a recent revival of the once-popular view that Creoles can be singled out as languages that emerged as early L2A interlanguages in a crystallized state of 'arrested development' (e.g., in Plag 2008a: 115, 'Creoles are conventionalized interlanguages of an early stage'). Section 4 ends with a refutation of such hypotheses.

2. Whence 'Creole Genesis'?

2.1. MAKING CONSTRUCTIVE USE OF THE DISTINCTION AND RELATION BETWEEN 'I-LANGUAGES' AND 'E-LANGUAGES'

2.1.1. The Ontological Priority of I-languages (and Innovations Therein) Once Cartesian-Uniformitarian boundary conditions are assumed for Creole-genesis hypotheses, any ontogenetic process that is postulated as a factor in Creole formation should, in principle, apply to the formation of *any* I-language, whether or not such I-language is called 'Creole'. This is the basic assumption in my own recent work (also see Mufwene 2001ff for an extended argument along related lines in a different theoretical framework).

Recall that for Neogrammarians at the end of the 19th century, it was already assumed that 'language . . . has its true existence only in the individual, and hence . . . all changes in the life of a language can only proceed from the individual speaker' (Osthoff and Brugmann 1878 [1967: 204]). Paul (1890 [1970: 7]) proposed the following as a basic methodological and theoretical assumption: 'The psychical organisms here described are the true media of historical development. What has been actually spoken has no development.' It was argued, for example, that the ultimate locus of 'exceptionless sound laws' is in individual minds, in 'psychical organisms' – I-languages, in modern terminology. Also recall Osthoff

and Brugmann's distinction between innovation and spread: 'linguistic innovations, proceeding from individuals, gain currency in the speech community' (1878 [1967: 198]) and their insistence on 'a science which undertakes extensive observations of the operation of the psychological factors which are at work in countless sound changes and innovations as well as in all so-called analogical formations' (1878 [1967: 198]). Here's another quite explicit statement of the Neogrammarians' Cartesian orientation:

> . . . some of the fundamental errors which dominated the entire older linguistics . . . originated in that very failure to recognize the fact that even the changes and modifications taking place solely in the external speech form and affecting only the phonetic expression of thought are due to a psychological process which takes place prior to the materialization of the sound by the vocal organs. (Osthoff and Brugmann 1878 [1967: 199])

More recently, David Lightfoot (1999, 2007) has updated the Neogrammarian objectives for historical linguistics in the framework of generative grammar:

> Identifying structural changes and the conditions under which they took place informs us about the conditions of language acquisition; we have indeed learned things about properties of UG and about the nature of acquisition by the careful examination of diachronic data. So it is we focus on changes in grammars, viewed as biological entities [i.e., I-languages-md], and we gain a very different approach to language change than the ones that focus exclusively on E-language phenomena, on the group products of cognitive systems rather than on the systems themselves. Linking language change to the acquisition of grammars in this fashion has enabled us to understand certain grammars better and has refined UG definitions. (Lightfoot 2007: 111)

Similarly taking I-languages as their main focus, Kroch (1989), Kroch et al. (2000) and I. Roberts (2007) have provided generative-syntax models of language change, which double as neo-Neogrammarian updates for historical linguistics. This rather explicit attention to I-languages in certain quarters of historical linguistics from the 19th century onward problematizes Thomason's (2002: 106) claim that 'historical linguists are focusing on E-language, not I-language, when they talk about transmission of languages from one generation to the next . . .' (see DeGraff 2001b, especially pp. 213–234, and DeGraff 2005b for additional historiographical details and references related to Uniformitarianism, and lack thereof, in linguistics in general and Creole studies in particular).

In 20th-century creolistics, Andersen (1983: 8, 15f, 41–44) has updated the sort of mentalist caveats that the Neogrammarians had put forward so forcefully. Andersen has asked us to distinguish between the group-level processes that contribute to the development of 'socially defined entities' and the individual-level 'psycholinguistic process[es] that [take] place in an individual's brain'. Andersen's 'socially defined entities' come into existence as 'the *spread* of innovations . . . lead[s] to a shared group norm' in the corresponding communities (p. 43, emphasis in original). Such 'social-group

phenomena [which] result in a *new* autonomous in–group linguistic norm' (Andersen 1983: 41; emphasis in original) seem related to one of the many definitions of 'E(xternal)-language' whereby 'certain regularities . . . hold among the population with reference to the language, sustained by an interest in communication' (Chomsky 1986: 19). As for Andersen's 'psycholinguistic process[es] in an individual's brain', they are in principle related to the creation and use of I–languages.

2.1.2. 'E-creoles' vs. 'I-creoles'
Following Andersen (1983), DeGraff (1999a: 8) makes a clear-cut distinction between 'E-creoles' and 'I-creoles' as useful theoretical entities in Creole studies: The formation of a given 'E-creole' is 'the establishment of a new community language' whereas the formation of the corresponding 'I-creoles' involves 'the development in individual speakers' minds/brains of a grammar that shows a certain typological distance from the grammars of the languages in contact'. The formation of I-creoles is ontologically prior to the formation of E-creoles. Indeed E-creoles result from the spread of particular I-creoles' innovative features through larger and larger subsets of I-languages in the 'Creole' community. E-creoles and I-creoles are thus related in important ways, and a rigorous theoretical handle on E-creoles and I-creoles as distinct, but related entities, seems a prerequisite for a comprehensive and coherent picture of Creole formation. We return to this crucial distinction in §2.1.3, in reference to the indispensable archival work on the early stages of creolization and how this work interacts with the theoretical investigation of Creoles qua I-languages.

How well have creolists as a group heeded (Neo-)Neogrammarian caveats for distinguishing explanations at the levels of I-language/ psycholinguistic phenomena vs. E-language/social-group phenomena? Not very well, it seems to me (DeGraff 2001b, 2005b). One of the persistent problems is a failure to exploit the constructive connections between the two levels of explanations – a failure that seems due to a category mistake in the very use of the terms 'Creole' and 'genesis'. To step ahead of my argument a bit, the indispensable archival, thus E-language-oriented, work on the early stages of creolization (of the sort most notably ushered by Mervyn Alleyne, Robert Chaudenson, Philip Baker, Barbara Lalla & Jean D'Costa, Jacques Arends, Guy and Marie-Christine Hazaël-Massieux, John Singler, Julianne Roberts, etc.) has often been (mis-)interpreted to be at odds with certain assumptions about the formation of I-creoles. Yet such archival research can, in principle, constructively interact with theoretical work in the I-language perspective. One handy illustration of such interaction is offered in §4.4.2 below where reference to archival data, *inter alia*, is used to disconfirm certain claims about the cognitive sources of Creole structures. This potential for constructive interaction between the descriptive archival and theoretical mentalist approaches can be further clarified and appreciated by revisiting some of the fundamental, yet

often unspoken, assumptions that are held by some of the creolists that have provided us with invaluable data and observations related to E-creoles.

Consider the much discussed 'gradualist' hypotheses in Arends (1989, 1993, 1995), Singler (1996), Mufwene (1999), Chaudenson and Mufwene (2001), etc. There it is claimed, in, for example, Arends (1993: 373), that '[t]he main conclusion is that creolization is not unigenerational, but rather a gradual process extending over a number of generations'. This gradualist hypothesis and the concomitant notion that a 'grammar' can be constructed in increments across generations bring to mind Hermann Paul's comment about 'Historical Grammar' as a system whereby 'descriptive grammars of different periods have been tacked together' (Paul 1890 [1970: 2]). The diachronic-cum-structural implications of creolists' own Historical Grammar have been made quite explicit:

> (1) One of Arends' points is that different parts of creole grammar emerge at different stages in a creole's history. Within that context, I think it appropriate to envision an initial period where many – but not necessarily all – elements of a creole's grammar are set in place. (Singler 1996: 197)

A number of questions must be raised in order to clarify the theoretical and empirical implications of (1): What is the locus of the 'grammar' in (1)? Is there a well-defined list of 'elements of a creole's grammar'? What inventory of 'elements of a creole's grammar' is enough to constitute a 'creole grammar'?

Let's first clarify and problematize the sort of 'Creole genesis' definition that seems to implicitly underlie the claim in (1), and the unspoken corollary assumptions about the ontology of 'Creole genesis'. Singler (1996: 196) tentatively defines Creole genesis as 'the creation of a language different from the lexifier language' while stressing, along with Mufwene (2001: 131) and many others, that 'the principal agents of [Caribbean Creole] genesis would have been African-born adults' (but see Mufwene 2004 for a more nuanced position whereby both children and adults participate in Creole formation, as argued in DeGraff 2002).

In one, perhaps too strict, interpretation based on my own I-language perspective which is not shared by Singler, the latter's position would seem to presuppose that it is principally *adult* learners who can 'create' languages different from their target. Does L1A by children always and uniformly produce identical replicas of its target, at all acquisition stages? Not so, according to the classic observations of Paul (1890 [1970: 15f]) and Meillet (1929 [1951: 74]) for whom language acquisition is actually language re-creation, thus the inherent possibility of structural innovations (e.g., parametric shift or 'grammatical inventions' in Rizzi's (1999) terminology) in *ordinary* instances of first language acquisition. These insights have been substantially confirmed by some of the recent L1A literature as surveyed in Crain and Lillo-Martin (1999), Rizzi (1999), Crain and Pietroski (2002), Wexler (2003), Guasti (2004) Crain et al. (2006), etc. The evidence from L1A seems so convincing that Crain et al. (2006) have

coined the phrase 'language acquisition is language change'. Key questions in that respect, to which we come back below, are, firstly, do adults and children innovate structures at the same rate and along the same developmental paths? And, secondly, what are the ecological factors that determine the eventual fate of these innovations? Many of the latter do not persist in the steady-state grammar of the learners, and those that do persist may not get transmitted to the population at large. As for the creation of new language varieties via language contact among adults, such creation also happens outside of creolization, as in the history of Germanic and Romance, which are not usually classified as Creole languages (see Mufwene 2001: 139–144, 2007 for some discussion).

Now, let's agree with Singler, for one moment, that 'Creole elements' are defined as those that make any Creole 'different from the lexifier language' (Singler 1996: 196). From that perspective, and given what we know of language acquisition in both adults and children, it is necessarily true that, from the *onset* of contact between Africans and Europeans in the Caribbean and throughout the subsequent contact period, there would have existed a continuum of language-learner varieties with various inventories of 'Creole elements', depending on learners' respective length of exposure to target language, their respective individual motivation and learning skills, their respective degrees of intimacy/distance vis-à-vis their models, their attitudes toward the target European(-derived) varieties, the typological distances between native and target languages, etc. (cf. Alleyne 1971 for related considerations for the Jamaican Creole case; also see §§2.2.2 and 2.3.5 below). No language learner, no matter how talented and no matter how intimate with speakers of the target language, could have acquired the target language overnight in one go. It is also necessarily true that, given the well-documented variations in the linguistic ecology of the Caribbean across time and space (as documented by Singler himself among others), the inventories of said 'Creole elements' must have fluctuated over time, independently of their cardinality. Therefore, from the initial period of a Creole's history, there would have existed ('basilectal') varieties structurally quite distinct from the target language (with many 'Creole elements'), alongside ('acrolectal') varieties relatively close to the target language (thus, with fewer 'Creole elements'). Abstracting away from the Creole-specific terminology in this paragraph, similar observations – about fluctuation in the structural distances between language-learner and target varieties – would hold of, say, the language-contact varieties that would eventually seed (Proto-)varieties of Romance languages out of Latin. We return to these points below in greater detail.

Nevertheless – and this is fundamentally distinct from the Cartesian assumptions of Paul, Wexler, Rizzi, Crain, Lightfoot, Kroch, I. Roberts, etc. – what the gradualists' phrase 'elements of a creole's grammar' in (1) presupposes as object of study is not any native idiolect or I-language: individual 'early Creole' speakers (or 'pre-/proto-Creole' speakers, if you

will) cannot wait around, beyond their respective life spans, for the unfurling of 'different stages in [their] creole's history' until 'elements of [their] creole's grammar are set in place'. The idea of 'an initial period where many – but not necessarily all – elements of a creole's grammar are set in place' is simply incoherent from a strictly I-language perspective that does not assume a predefined list of Creole 'elements': at any given time in any language's history, native speakers of that language – whether or not the latter is called 'Creole' – have *all* the necessary structural features of their idiolects *fully* 'set in place' and *fully* functioning in their respective individual minds. But, unlike the time course of any particular I-language (which is, by definition, coextensive with the lifespan of its speaker), the history of any 'Creole' as E-language goes beyond the lifespans of individual Creole speakers.

The theoretical objects of inquiry in gradualist hypotheses must, then, be some sort of community-wide and cross-generational grammar – either some 'Historical Grammar' in Paul's (1890) sense, or some E-creole in DeGraff's (1999a: 9) terminology, or some set of communal norms – each of these entities necessarily countenances variation across time and across space. If so, then gradualism as presented in (1) seems a straightforward consequence of Uniformitarianism and could hardly be the subject of debate: for any given 'grammar' X at any given time t, with 'grammar' defined in some historical/communal/social sense, it is a truism that 'elements of [X's] grammar emerge[d] at different stages in [X's] history [i.e., at times prior to t-md]'. Given that language acquisition is always language (re-)creation – hence, 'imperfect transmission' so to speak – it is also a truism that 'elements of [X's] grammar' change over time. There is therefore no well-defined static or teleological set of 'elements of [X's] grammar', whether or not X is considered a Creole. In other words, exactly one of the following is true:

(i) the statement in (1) applies uncontroversially to *all* languages: in this case, 'elements of [a particular] creole grammar', like 'elements of English grammar' is, as it should be, a strictly ostensive term without any fixed referent; otherwise

(ii) the statement in (1) suffers from presupposition failure: in Singler 1996, the phrase 'elements of a creole's grammar' is intended as an operational linguistic-structural term (i.e., to measure and compare different rates and degrees of 'creolization' in the Caribbean), but this phrase has no fixed referent. Furthermore, and on the meta-theoretical level, it must be noted, as Mufwene (personal communication, November 20, 2008) does, that 'Creolists have tried in vain to operationalize a concept that is not operational: they cannot agree on what a "Creole" is in the first place, or on whether there are any essential features that make a language a "Creole".' (See, e.g., McWhorter 2005 vs Ansaldo et al. 2007 for a recent sample of the debate on 'defining Creole' vs 'deconstructing Creole'.)

2.1.3. Findings about E-creoles and I-creoles Complement Each Other

This said, the socially defined sense of 'Creole' in Andersen's terminology – or 'E-creole' norms, if you will – adds one useful level of abstraction

on top of the notion 'I-creole'. This Historical-Grammar/E-language abstraction seems crucial for understanding the necessarily gradual evolution of group norms in the history of any speech community. From a perspective that distinguishes the E- vs. I-language levels of abstraction, the development of E-creoles is parasitic on, *inter alia*, the spread across time and across space of (some of) the innovative patterns produced in the I-creoles of the various individuals involved in Creole formation (cf. Andersen 1983: 8, 15f, 41–44; DeGraff 1999a: 9; Mufwene 2001: 2, 148–153). In the Cartesian perspective adopted here, the derivative status of E-creoles is nothing to be surprised by: any E-language is 'an epiphenomenon at best' (Chomsky 1986: 25). Recall Paul's (1890 [1970: 456]) statement that 'individual languages are the only ones which have any real existence'.

It is in the context of this complex relationship between E-creoles and I-creoles that careful archival work can shed the most light. Indeed the Historical Grammars of E-creoles can be partially reconstructed from the precious archival data and observations that are now being unearthed about the 'early stages of creolization'. These data and observations have made it possible to start elucidating in an empirically responsible fashion the spread of I-creole innovations across time and space from the earliest stages of creolization onward. Archival work on creolization is thus even more useful when its implications vis-à-vis the E- vs. I-language levels of explanations are clearly understood (see sections §2.2 below for some elaboration of this point).

The E-language theoretical level is also indispensable for Labovian linguistics and its insightful analyses of variable rules. The latter, as I understand them, are apprehended via their perforce-variable output across populations of speakers, even though they are ultimately rooted in mental properties of individual speakers. In other words, variable rules, no matter how they are ultimately represented in the grammar, are part of I-languages, with effects visible at the E-language level, alongside the variation due to differences *across* idiolects and dialects – 'nativelike command of heterogeneous structures is not a matter of multidialectalism or "mere" performance, but is part of unilingual linguistic competence . . . *absence* of structured heterogeneity . . . would be dysfunctional' (Weinreich et al. 1968: 101, emphasis in original). Labovian analyses confirm and substantiate the traditional insight that 'physical' E-language, which includes the Primary Linguistic Data (PLD) of language learners, is always heterogeneous, often in systematic fashion.

It is thus expected that E-creoles as well would be heterogeneous – with systematic (i.e., 'structured') heterogeneity. As for the early E-creoles in the colonial Caribbean, they would also manifest nonsystematic heterogeneity due to the vagaries of demographic factors and of the language-contact ecological factors such as the nature and degree of the typological variation among the varieties in contact, the social positioning and influence of their respective speakers, the nature of the contact (intimate vs. distant),

the proportion of the native vs. nonnative speakers of the emerging contact language, etc. Creole continua must have thus existed from the *onset* of language contact, as argued by many creolists starting perhaps with Alleyne (1971) (also see Moreau de Saint-Méry 1797; H. Trouillot 1955, 1980; Debien 1971; Baker 1982; Lalla and D'Costa 1989; M.-R. Trouillot 1990, 1995, 2002; Bickerton 1996; Chaudenson and Mufwene 2001; Mufwene 2001ff). Labovian linguistics is thus of crucial importance for understanding some of the ways in which languages, at both the I- and E-language levels, are constantly and gradually evolving, and why 'fully convergent grammars' (cf. Hale 1998: 1) are an unlikely (i.e., 'abnormal') outcome of language acquisition (see §2.3 below).

2.1.4. I-languages as 'Linguistic Fingerprints': No Two Idiolects Are Identical

Except in cases of language transfer in the bilingual speaker's mind (see the discussion in §4.2.2 on bilinguals as a limited conduit for language transfer in creolization), I-languages do not act directly on other I-languages. As Paul (1890 [1979: xxxvii, 7]) points out: '*All intercourse of mind with mind is merely indirect, and such intercourse depends upon purely physical conditions*' (emphasis in original). In the case of I-language 'intercourse', the 'purely physical conditions' in the 'external world' that directly influence the development of I-languages include the PLD that language learners are exposed to. This PLD is a subset of E-language in the Bloomfieldian sense – 'the totality of utterances that can be made in a speech-community' (Bloomfield 1925: 155).[2]

   The sets of utterances that language learners are exposed to in the course of developing their respective I-languages have a vanishingly small probability to remain identical across learners. The inventory and the statistical profiles of variations across learners' PLD sets always fluctuate. Since the language learner's trigger experience is always in flux, language change at the idiolectal level seems unavoidable (see Kroch 1989; Lightfoot 1999, 2007; Kroch et al. 2000; I. Roberts 2007, etc.). Given the inevitability of language variation – and the fact that even so-called 'homogeneous' speech communities are unavoidably made up of heterogeneous idiolects – language acquisition *always* obtains with the corresponding PLD coming from (somewhat) distinct idiolects. In other words, 'mixed PLD' is the only possible kind of PLD, *pace* Hale (1998: 3–5). Therefore, 'fully convergent grammars', in Hale (1998: 1) terminology, do not – and, *logically*, could not – exist. (I return to this issue in §2.3.1; also see DeGraff 1999b: 475, 529n6.) The mixed nature of, and changes in, the PLD sets within and across the experiences of individual learners are due partly to historical and demographic factors such as population displacements, migrations, the inventory and typological profiles of the languages in contact, the psychological and social distance between speakers of the languages in contact, the varying fluency of the speakers

providing the PLD, and so on. These factors all seem outside the scope of individuated I-languages narrowly defined; instead they seem, by and large, a communal, sociohistorical and contingent matter (also see Lightfoot 1999, 2007).

The inescapable finiteness of the PLD is yet another factor that is relevant to variation across triggering experiences even in the same speech community (Chomsky 1986: 31; Niyogi and Berwick 1997: 701, 713; Lightfoot 1999: 101, 2007: Ch. 6): given the sorts of variation mentioned above, plus the finiteness of the PLD, it seems statistically improbable that any pair of learners will ever be exposed to identical PLD sequences in the course of language acquisition. It thus seems unlikely that any two I-languages will ever be exactly identical to each other (Kayne 2000: 8). Even within one single relatively homogeneous speech community, the parameter settings of any pair of I-languages will be triggered by pairwise distinct PLD sets. (See §2.3.1 for more detailed observations in this vein.)

2.1.5. Abrupt 'Innovations' *Precede* Gradual 'Spread'

The spread of linguistic patterns in a(n expanding) population – or, in Osthoff & Brugmann's Neogrammarian parlance, individual innovations gaining currency in the speech community – is necessarily a gradual process (Weinreich et al. 1968; Posner 1985; Niyogi and Berwick 1997; Hale 1998; Lightfoot 1999, 2007, etc.). Thus, 'E-creolization' as well (i.e., the establishment of communal Creole norms, perhaps as part of a Creole identity; see §§2.1.2–2.1.3 and note 2) is a gradual process. E-creolization entails, in Andersen's (1983: 15) words, 'the spread of . . . acquired and innovated forms . . . to other members of [the] developing community'. This is yet another reason why the pro-gradualist statement in (1) is beyond dispute, once it is properly applied at the E-language level: every language qua socially defined entity has a history and a gradually evolving community, and its structural characteristics at any point in time must have each 'emerge[d] at different stages in [its] history'. In the diachrony of, for instance, Romance languages, new Latin-derived varieties gradually spread to developing communities via iterated instances of language acquisition by individuals that were brought in contact with one another. From our Uniformitarian perspective, E-creolization – the spread of I-creoles' patterns in the developing Creole community and the concomitant fluctuation and accumulation of innovative features across successive generations of I-creole speakers – is thus expected to also be gradual. Chaudenson and Mufwene (2001: 127) and Mufwene (2001: 10, 49–54) describe 'gradual basilectalization' as the temporally protracted increase and spread of Creole lects whose structures would increasingly diverge from the norms of the corresponding European varieties. Bickerton (1996: 316–319) develops a similar idea whereby the 'acrolectal' varieties (i.e., the Creole varieties most structurally similar to the European colonial varieties in the Caribbean a.k.a. 'superstrate' varieties) developed first, and the

'basilectal' varieties (i.e., those most structurally divergent from the super-strates) developed last. (But see the caveats in §2.1.2 about the concurrent development of basilectal and acrolectal varieties – and of other varieties in between these two poles – as *individual-level* L2A interlanguages across subgroups of learners from the onset of creolization onward.)

Let's now consider the sort of innovations that often constitute the creolist's main explanandum. These innovations and the 'restructured' idiolects they define ontologically precede: (i) the gradual spread of said innovations into the corresponding speech communities; and (ii) the reflex of these innovations in communal linguistic norms. Consider the creation or integration of (some of) these innovations into a given idiolect. This idiolect-level process is necessarily bounded by the lifespan of the corresponding speaker. *Cross-generational* gradualism vis-à-vis the time course of *individual* speakers' innovations is a contradiction in terms.

Individual innovations per se are perforce 'abrupt' to the extent that they are created in the lifespan of particular idiolects, and not across generations of speakers (cf. Paul 1890 [1970f]). Yet, it is only via repeated instances of their being (more or less exactly) reproduced via (more or less successful) instances of acquisition within and across generations that idiolectal innovations spread and become part of E-creoles. The process of spread is necessarily gradual and, hence, fundamentally distinct from the process of innovation, which is necessarily 'abrupt' in the aforementioned sense.

If 'Creole genesis' refers to the development and incorporation of structural innovations in particular idiolects, then such genesis cannot be analyzed as if it extended across generations (DeGraff 1999a: 8f, 1999b: 480, 484f). Cross-generational gradualism makes sense – as a useful truism – only if the term 'Creole' in the phrase 'Creole genesis' refers to E-language objects in the 'Historical Grammar' perspective described in Paul (1890 [1970: 2]). Once this E-language level is identified and kept distinct from the I–language level, the sort of archival data that have been unearthed from the early stages of creolization (by, e.g., Arends, Baker, Chaudenson, G. and M.-C. Hazaël–Massieux, Lalla & D'Costa, J. Roberts, Singler, etc.) can help us theorize about the underlying processes whereby innovations are created and enter into Creole speakers' I–languages and, *then*, spread into Creole communities' E–languages across space and across time. One model, outside of Creole studies, that studiously avoids the I- vs. E-language category mistake while systematically and constructively mining E-language data in archival texts for insights about I-languages is the work of Tony Kroch and his colleagues (see, e.g., Kroch et al. 2000).

2.2. DECONSTRUCTING 'GRADUALISM'

Hale (1998) forcefully articulates the methodological desiderata we need in order to clearly distinguish between the temporal characteristics of innovations vs. spread/diffusion:

(2) 'Change' [from the I-language perspective] is simply the set of differences between the source grammar[s] . . . and the constructed grammar . . . Note that 'change' therefore has no temporal properties, it is a *set* of differences . . .

Unlike change, diffusion through a population *does* have temporal attributes. Each step in such a diffusion requires that an acquirer come into contact with an innovating . . . speaker . . . and . . . learn [the relevant aspect(s) of –md] the relevant innovating grammar. That speaker, in turn, may end up with the [innovated feature(s)] and thereby act as a source for additional acquirers. Diffusion of a change through a population, therefore, can take decades, even centuries, depending upon the population being considered. (Hale 1998: 2f, 5f; emphases in original)

Innovations thus proceed from *individuals*, and thereafter these innovations gain currency through the *speech community*, spreading from individual to individual.

2.2.1. Gradual 'Change' in E-language Reduces to Abrupt Steps in I-languages

If Hale is right, then 'change events' that appear continuous and gradual actually consist, when viewed at the I-language level, of several discrete and 'abrupt' steps that are mediated via individuated instances of language acquisition by learners whose 'constructed grammars' either diverge from or converge with the 'source grammars' with respect to particular parameters of variation (cf. Lightfoot 1999, 2007). Divergence in acquisition underlies innovation whereas convergence in acquisition underlies spread.

Now consider cases of 'change' that are generally considered gradual: grammaticalization, individual-level gradations vis-à-vis variation/continuum effects, 'gradual basilectalization', generalization of innovative features through syntactic environments or by lexical increments (lexical diffusion or lexical drift), etc. From the perspective sketched in (2), these phenomena must be analyzed as the aggregate results of discrete idiolectal-level changes, alongside discrete 'transmission' of these changes from speaker to speaker within and across the relevant (sub-)communities. Even if certain innovations (e.g., grammaticalization phenomena) can be argued to arise through language use, it is only when the relevant innovative features are stably integrated, via acquisition, in (populations of) I-languages that they can 'get a life' in the grammar per se, as they become an integral part of speakers' native competence. In other words, gradual 'change event' must incorporate sequences of discrete steps that take place relatively 'abruptly' as each speaker develops and uses his or her I-language. The development of I-language and the subsequent changes that can occur therein (e.g., via analogical change, lexical diffusion, grammaticalization or post-vernacular language or dialect contact) can reasonably be considered 'gradual' phenomena (see §2.2.2), but the latter's gradualism is necessarily bounded by individual lifespans, and are therefore relatively 'abrupt' in comparison to cross-generational gradualism, as in the spread of certain innovations within and across communities – such spread 'can take decades, even centuries' (Hale 1988).

For example, a morphological or syntactic innovation will (appear to) progress gradually as each additional speaker who acquires the new feature extends it to more and more environments; each incremental step happens via an instance of 'innovative' acquisition where the innovation actually consists in extending the range of application of some previously existing 'innovative' feature to larger and larger classes of environments.

In a related vein, local gradual effects may also arise as each individual speaker in some community extends the application of some 'innovation' (e.g., some novel grammatical feature or some newly constructed inflectional paradigm) to more and more environments across his or her successive I-language states (see Mufwene 2001: 209n7 for some discussion). Related sorts of I-language gradualism are quite pervasive and constitute a main area of study for developmental psycholinguists working on L1A and on the gradual succession of interlanguages in the minds/brains of L2 learners (see, e.g., Wexler 2003; Crain et al. 2006, for related discussion vis-à-vis syntactic development in L1A, and Archibald 2000; Han and Odlin 2006; Liceras et al. 2008, for related issues in L2A).

2.2.2. Gradualism in Acquisition, in Creolization and in Age-Grading Effects

When external and psychosocial circumstances are most favorable to language learning, the early interlanguages in L2A, with their so-called 'pidgin(ized)' characteristics, are gradually replaced by systems that, in more and more grammatical domains, are increasingly stable, systematic and target-like. In certain cases, the learner may define 'target' and 'success' in such a way that 'successful' L2A for said learner will appear 'incomplete' in the ears of native speakers (Han and Odlin 2006, Siegel 2006: 22–24, 2007: 194f, etc.; see §2.3.5).

Keeping in mind Andersen's caveats about the individually vs. socially defined senses of 'pidginization' and 'creolization', here I use the term 'pidgin(ized)' vis-à-vis (early) interlanguages in a technical individually defined sense, that is, to describe the relatively restricted, unstable and unsystematic nature of certain interlanguages at the very early stages of L2A. This should not be taken to mean that 'pidgins', in their socially defined sense, must uniformly be restricted, unstable and unsystematic. Also note that not all 'pidginized interlanguages' (at the level of individual learners) lead to the formation of 'pidgins' (at the level of communal norms). This is reminiscent of the fact that not all I-language innovations lead to diachronic change at the level of the community (E-)language norms.

In Uniformitarian fashion, it can be reasonably surmised that early L2A interlanguages in the history of any Creole language, as in any other situation of language contact, were 'pidgin'-like, in the above-mentioned individually defined sense, even if such 'I(ndividual)-pidgins' never acquired communal norms. After all, no language learner – no matter how motivated, skillful and privileged PLD-wise – can become fluent in

the target language overnight. Besides, and as already mentioned in §2.1.2, because of differences, for example, in length and degrees of exposure to the target language, in learning skills, in motivation, in attitudes toward the target language, etc., adult learners could not all achieve in tandem comparable competence in the L2, thus the existence of variations (e.g. 'continuum' phenomena) from the very beginning of creolization onwards (see the aforementioned references to 'gradual basilectalization' in Bickerton 1996; Chaudenson and Mufwene 2001; Mufwene 2001). Presumably learners' I-pidgins (i.e., early-interlanguage varieties) have a potentially important role to play in all cases of contact-induced language change, with respect to, for example, levelling of morphological paradigms, over-regularization, analogical change, and so on – the sort of pervasive patterns of change that have often been (controversially) classified as 'grammar simplification' in phonological, lexical, and morphological domains (see, e.g., Bunsen 1854; Meillet 1919 [1958]; Weinreich 1953: 42f; Trudgill 1986, 2002, 2004; Kroch et al. 2000; Kerswill 2002; Winford 2003, and various entries in Liceras et al. 2008; I provide summaries and extended critiques with additional references in DeGraff 1999b: 491–499, 517–518, 2005c: 309f, and especially 2001b: 242–289). But given the aforementioned and well-documented variation across individual learners, it seems unlikely that the grammars of Creole language resulted by and large from the crystallization of early interlanguages (contra, e.g., Plag 2008a,b). This is an issue that I return to below, in §4.4.[3]

For now let's keep in mind the above I-pidgin caveats, and more generally the caveats regarding E- vs. I-language levels and the corresponding types of descriptions and explanations. At the individual level, the gradual ontogenetic development of interlanguages unfurls until the learner reaches some relatively steady state, which in L2A has been called 'fossilized' when the learner's (quasi-)final L2 grammar is structurally quite distinct from the target, i.e., when the learner's so called 'end state' produces many recurrent and persistent 'errors' (see Han and Odlin 2006 for a recent anthology of studies on fossilization). But, whether in L2A or in L1A, the gradually attained and relatively steady end-state is, in principle, always distinct from the target grammars: every attained grammar will manifest some amount of innovations, be they called 'grammatical inventions', 'errors', 'fossilization', etc.

Other cases of individual gradualism relate to language-variation phenomena where usage frequencies of competing variants change over the lifetime of individual speakers. Age-graded variation can also be due to late innovations due to external (e.g., language- or dialect-contact) factors, as documented in, for example, Sankoff (2004, 2006). Such individual-level gradualism is reflected in the structural and statistical profiles of the linguistic variations in the corresponding population of I-languages and, in some cases, in subsequent parametric change, as documented in archival texts and in sociolinguistics and historical linguistics. (For a sample of observations,

analyses and surveys in different theoretical frameworks, see, e.g., Paul 1890; Andersen 1983; Labov 1994; Niyogi and Berwick 1997; Lightfoot 1999, 2007; Sprouse and Vance 1999; Kroch et al. 2000; Sankoff 2004, 2006. Related studies in creolistics include Bickerton 1975; Rickford 1986; Sankoff 1994; see DeGraff 1999a,b and Mufwene 2001ff for overviews and critical discussion, and DeGraff 2005c for a case study in the domain of (para-)verbal syntax in Haitian Creole; also see §2.3.3 below.)

Crucially, the group-level processes responsible for *spreading* innovative features in a population of I-languages, with concomitant manifestation in the corresponding E-languages, must be kept distinct from the individual–level processes responsible for *creating* the innovations in the first place (cf. Paul 1890; Posner 1985; Niyogi and Berwick 1997; Hale 1998; DeGraff 1999a,b; Lightfoot 1999, 2007; Mufwene 2001: 15–21; etc.).

2.2.3. The Roles of Children and Adults vis-à-vis Innovations and the Spread Thereof

The following quote is one instance of the quite common confusion between I-language innovation and E-language spread: 'Children are unlikely to initiate change, since change is spread by social groups, and babies do not have sufficient group influence to persuade other people to imitate them' (Aitchison 1991: 179). Here, the '*initiation* [of] change', presumably by *individuals*, is conflated with the '*spread* [of change] by *social groups*'; this is a category mistake. Besides, all members of all social groups were children at some point, and there is absolutely no guarantee that as children they would have all succeeded in perfectly 'imitating' their older models (here I abstract away from the fact that language acquisition cannot be reduced to 'imitation', even if the latter plays an important part in all instances of learning; see, e.g., Chomsky 1959 and, more recently, Crain and Pietroski 2002; Wexler 2003; Crain et al. 2006, etc.). Totally perfect imitation is unlikely even in relatively homogeneous speech communities: once idiolectal variation, which is indeed unavoidable, is factored in, perfect imitation of all model idiolects becomes even more chimeric (see §2.3.1 for a more explicit statement of this issue). And, in certain cases, 'imitation' is by and large undesirable or impossible or both, because of a preponderance of nonnative or nonfluent models – for example, in the cases of Sign Language creation as in Nicaragua and in certain periods and certain locales in Caribbean colonial history and in the history of Romance and Germanic languages.

In the Sign Language creation cases to be discussed in §3.2, the initial PLD came almost exclusively from nonfluent adult speakers, and the varieties attained by the younger learners spectacularly surpassed their models in terms of stability, fluency, morphosyntactic combinations, etc. In one well-known case (in Nicaragua), it is the younger learners' varieties that provided the norms for the emerging community of fluent speakers.

A somewhat similar scenario unfurled in the Caribbean colonial context where those who, as children, acquired the local Creole varieties did not,

and could not, replicate the full gamut of the 'xenolectal' features of the nonnative among their models. Here I need to unpack some crucial assumptions about Caribbean colonial history – assumptions that we'll come back to in §3.2.2 and §4.

In certain space–time regions of colonial Caribbean history, the PLD for the native learners of the emerging Creole varieties was dominated by nonnative speech with a particular sort of social capital – or lack thereof – whereas the native speech of the locally born population (the 'Creoles') would acquire substantial social capital. The nonnative speech in the PLD would have included the Proto-Creole varieties spoken by the newly arrived African-born slaves (a.k.a. 'Bozals') who were assigned to field work on large segregated plantations and who would spend most of their time in each other's company. At the peak of the colonial enterprise in economically successful plantation-based societies such as Haiti (then known as Saint-Domingue), these African-born slaves at the lowest rung of the colonial socioeconomic hierarchy would constitute a numerical majority, and they would have had relatively little exposure to natively spoken varieties of European languages. In cultural and political terms, the Creole children, including the children of the aforementioned Bozals, would attain, via acculturation and accommodation, various degrees of 'group influence' – hegemonic power even – as they themselves quickly became targets of 'imitation' among the larger community, many of whom were aware of the socioeconomic advantages conferred to (near-)native Creole speakers. These and related observations are critically discussed – for various, and sometimes incompatible, ends – in, for example, Moreau de Saint-Méry (1797), H. Trouillot (1955, 1980), Davis (1975), M.-R. Trouillot (1990, 1995, 2002), Singler (1992, 1996), Bickerton (1996), Barthélémy (1997) and Chaudenson and Mufwene (2001) (also see §4).

The very moniker 'Bozal', etymologically from the Spanish word *bozal* 'muzzle', is symptomatic of the stigma associated with the colony's newly arrived Africans as compared to the 'Creole' slaves. In Haitian Creole, the adjective *bosal* means 'uncouth', 'wild', 'untamed', 'violent', or 'brutal' (Freeman and Laguerre 1996). The Bozal slaves were also called 'salt-water negroes', 'heathen slaves', 'horses', and so on (Moreau de Saint-Méry 1797 [1958: 55–59], also see Davis 1975: 191; Barthélémy 1997; Chaudenson and Mufwene 2001: 89–91). As a contemporary observer of colonial varieties of Haitian Creole, Moreau de Saint-Méry made a vivid distinction between the quasi-Creole varieties spoken by the Bozals and those native varieties spoken by the Creoles qua locally born: he claimed that Creole speech was 'often unintelligible when spoken by an old African' and that 'one speaks it all the more fluently that one learns it at a younger age' ([1958:80f], my translation). And it's the same Moreau de Saint-Méry who quantified the socioeconomic superiority of Creoles over Bozals: 'for all tasks, it is the Creole slaves that are preferred; their worth is always a quarter more than that of the Africans', which leaves little

doubt about the socioeconomic values attached to linguistic and other markers of Creoleness in the Caribbean colonial milieu. These values would certainly play a role in the elaboration and maintenance of the (non)prestige factors and the social networks that would determine differences in the rate and the direction of the spread of the structural innovations that were being introduced, via L1A and L2A, in the (early) colonial varieties of Caribbean Creoles.

Caveat: Singler (1996: 222) correctly points out that it is not a straight-forward matter to retrospectively estimate what would have counted as 'prestigious' with respect to *both* Europeans and Africans in the colonial Caribbean in light of the brutal competition between their respective sociopolitical and economic agendas. Yet, for Moreau de Saint-Méry and for many other contemporary observers, the Creole babies, as they grew up into fluent native Creole speakers, did accumulate 'sufficient group influence to persuade other people to imitate them'. (For a more comprehensive portrait of the historical and sociolinguistic context of the formation of HC and other Creoles, see works by Alleyne, Arends, Baker, Baker & Bruyn, Chaudenson, Chaudenson & Mufwene, Debien, Fouchard, G. and M.-C. Hazaël-Massieux, Singler, H. Trouillot, M.-R. Trouillot, Warner-Lewis, etc.)

2.3. ON THE FORMATION OF 'I-CREOLES' VIA PARAMETER-(RE)SETTING IN I-LANGUAGES – AS IN OTHER CASES OF LANGUAGE CHANGE

2.3.1. 'Poverty of the Stimulus' Arguments and 'The Logical Problem of Language Acquisition'

If 'Creole genesis' refers to the creation of a certain set of I-languages (our 'I-creoles'), then such creation is another instantiation of the so-called 'logical problem of language acquisition' that challenges every language learner, theoretical linguist, language-acquisition researcher, learnability theorist, and so on. (3) is a familiar, and quite rough, schema of this 'logical problem' in the tradition of generative grammar.

(3)

$$\text{UG qua } S_0 \text{ (initial state of language faculty)}$$

[universal principles alongside parameters with un-assigned, perhaps default, settings]

$$+$$

PLD (source of 'triggers' or 'cues' for the fixing of parametric values)

$$=$$

Idiolect-Specific Grammar qua S_s (steady state of language faculty)

[universal principles alongside parameters with values fixed]

'UG is . . . construed as the theory of human I–languages, a system of conditions deriving from the human biological endowment that identifies the I–languages that are humanly accessible' (Chomsky 1986: 23). Whereas UG, by hypothesis, provides the invariant 'hardwiring' in S_0 for Human Language, '[t]he transition [via exposure to contingent PLD-md] from . . . S_0 to . . . S_s is a matter of setting the switches' (Chomsky 1986: 146).

In any instance of naturalistic language acquisition – in the colonial Caribbean as in everywhere else – 'poverty of stimulus' issues arise for a number of reasons (see Ritter 2002 for one critical discussion of these issues). We must recapitulate these 'poverty of stimulus' arguments since they have so often been overlooked in Creole studies, especially by those creolists who evoke, as a cornerstone of their exceptionalist Creole-genesis hypotheses, a *sui generis* catastrophic 'break in transmission' due to 'limited input' (i.e. extraordinarily impoverished PLD) for the language learners responsible for Creole formation.

In the context of the equation in (3), the PLD are *always* 'poor' in the sense that they are necessarily finite. I–languages are recursive combinatorial procedures that produce exquisitely elaborate and multilayered abstract representations. Therefore, any finite set of observable data (i.e., a finite sequence of phonetic or gestural strings uttered in specific environments) is only a small and sparse subset of the potentially infinite output of the corresponding I–languages. The necessarily finite PLD set is plausibly compatible with a number of distinct I–languages. In other words, finite PLD sets inevitably underdetermine the recursive I–languages that can be associated with these PLD sets. This is one way in which language acquisition instantiates the more general 'Plato's Problem': 'how [do] we know so much, given that the evidence available to us is so sparse' (Chomsky 1986: xxvii).

Furthermore, the evidence available to the learner, even though sparse, *logically could not* originate from strictly identical I–languages. By definition, I–languages are specific to each individual speaker – they are 'linguistic fingerprints', so to speak (see §2.1.4). Therefore the PLD in any instance of language acquisition originate from I–languages that are perforce mutually distinct, even if subtly so. Therefore, the parametric values of the I–language attained in S_s in (3) could not *consistently* replicate the parametric values of (all) the I–languages that produced the PLD. Mufwene (2001: 193) puts it thus: 'Every new speaker replicates their target communal language imperfectly, starting with the trivial fact that they couldn't possibly replicate all the idiolects of which it is an ensemble and no idiolect replicates another.' Here, the PLD are 'poor' due to an embarrassment of riches, so to speak: given the inevitable heterogeneity of PLD sources, there is, in all likelihood, no single I–language model in the learner's environment whose parametric values the learner could *uniformly* and *exactly* match, no matter whether the PLD are 'truly degenerate' or 'well–formed', whether 'sparse' or 'redundant', etc. Combining the insights of Weinreich et al. (1968) and Kayne (2000) about idiolectal–level variability and distinctiveness,

respectively, it can reasonably be surmised that language learners also construct variable rules as part of their I-languages, but such variable rules will only approximate, not replicate, those reflected in the PLD, especially in cases when the PLD countenances variability of the nonsystematic sort, due to the unpredictability of the language-contact particularities, with 'language contact' interpreted in its most comprehensive sense to include dialect- and idiolect-contact.

2.3.2. 'Poverty of the Stimulus' in the Colonial Caribbean: a 'Sharp Break in Transmission'?

The heterogeneity of the PLD may well have been exacerbated in the language-contact situation of the colonial Caribbean, with a relatively high proportion of input that is nonnative, thus influenced by transfer from the many varieties in contact (see §§4.2–4.3 for further discussion). But the difference is surely a matter of degree, not one of quality. In other instances of language change as well (e.g., at certain key stages in the history of English and Romance), learners had to deal with their share of nonnative input even if such learners did not create languages that go by the label 'Creole'. Besides, Niyogi and Berwick's (1997) computational simulations for some of the population-level dynamics of language acquisition (under highly abstract conditions) teach us that, even in the strictly hypothetical case of a perfectly homogeneous set of I-languages free from 'foreign intrusion', it is still the case that parametric 'misconvergences' (i.e., structural innovations) by child learners will eventually create an heterogeneous population of I-languages, given certain reasonable assumptions about language acquisition (finite set of positive examples, attainment of a steady-state grammar after a finite learning period, etc.).

   In any case, alongside structural innovations in the grammars of Caribbean Creole languages, we do find robust evidence for structural correspondences between these Caribbean Creoles and both their superstrate and substrate languages – from Europe and Africa, respectively – with the superstrate correspondences far exceeding the substrate ones. Besides, the magnitude of structural 'discrepancies' in the history of certain non-Creole languages seems comparable, and sometimes even greater, than that of their counterparts in the diachrony of Caribbean Creoles. There is therefore no rigorous and reliable evidence to support the oft-repeated claims of a 'sharp break in transmission' in creolization, contra, for example, Bickerton (1984) and Thomason and Kaufman (1988).

2.3.3. English 'Out-Creoles' Haitian Creole

Consider, say, the lexicon of Modern English (or 'New English', hereafter abbreviated as NE), and compare it to that of Haitian Creole (HC). From this lexical comparative perspective, NE, a Germanic language whose lexicon is mostly of non-Germanic origins, appears much more mixed than HC whose lexemes, including affixes and basic vocabulary, are by

and large of French origins, in both form and function. And the same seems to hold for a variety of Romance-based Creoles such as the French-based Creoles of Martinique, Guadeloupe, Saint-Lucia, Dominica, Guyane, Mauritius, Réunion, etc., and the Iberian-based Creoles of Curaçao, Cape Verde, São Tomé, Casamance, etc. (Guy Hazaël-Massieux 1996: 37–55; also see Posner 1996: 90–96 where it is duly noted about Romance-based Creoles that 'their basic vocabulary is often well-nigh identical with that of their lexifier language'). From that perspective the lexicon of NE is more 'macaronic' and shows greater 'discrepancy' – or a sharper 'break in transmission' – than the lexica of HC and of other Romance-based Creoles. And we've now reached a seemingly absurd conclusion based on the very premises of those who propose a 'sharp break of transmission' as criterion for Creole formation: Modern English, which is genetically classified as Germanic, 'out-creoles' Haitian Creole, which has been claimed to be a 'prototypical', 'abrupt', 'true' or 'radical' Creole, one that is bereft of genetic classification (Bickerton 1984; Thomason and Kaufman 1988; Thomason 2002f, etc.). Such Creoles have been assumed to be created 'by a generation of children operating on a fluid, structureless, and probably macaronic input' (Bickerton 1999: 54). Q.E.D.

A related argument can be made by a brief comparison of morphosyntactic patterns in the history of NE and that of HC. We'll focus on three sets of now well-documented patterns: one related to verbal inflectional morphology, and two related to word order in the clausal domain, namely placement of (pronominal) objects and verbs (DeGraff 2005c).

First, verbal inflectional morphology: When verbs in NE and HC are compared to their respective etyma, it is a striking fact that both languages evince much less verbal inflectional morphology than their ancestors. For example, Middle English, on a par with French, could exhibit more than one inflectional affix per verbal stem, as in show*edest* with two consecutive suffixes (-*ed* for past and -*est* for 2nd-singular agreement); compare with French chant*ions* (-*i* for past imperfective and –*ons* for 1st-plural agreement). NE and HC do not allow such stacking of inflectional affixes, and the verbs in both languages show smaller inventories of verbal inflectional affixes than their etyma.

Second, verb placement: Both NE and HC are languages with Subject-Verb-Object canonical word order. In both NE and HC, the verb must be pronounced adjacent to its postverbal object when there is one, and clause-internal adverbs (underlined in the following examples) must be pronounced to the left of the verb: *John already knows Mary* vs. ⋆*John knows already Mary* and *Jan deja konnen Mari* vs. ⋆*Jan konnen deja Mari*. This is very much unlike the corresponding patterns in the ancestor languages. In both Middle English and French, the same class of clause-internal adverbs could intervene between verb and object, as in Middle English, *Wepyng and teres counforteth not dissolute laghers* 'Weeping and tears do not comfort dissolute laughters' (I. Roberts 1993: 250), and French *Jean (ne)*

connait pas Marie 'John doesn't know Mary' (with French *ne* often omitted in spoken varieties of French). Also contrast the adverb placement patterns in the aforementioned NE and HC examples *John <u>already</u> knows Mary* and *Jan <u>deja</u> konnen Mari* with their French counterpart *Jean connait <u>déjà</u> Marie*. Both HC and NE have the *<u>adverb</u>-verb-object* word-order pattern in the environments where French and Middle English exhibit the *verb-<u>adverb</u>-object* pattern. Furthermore, the *verb-<u>adverb</u>-object* pattern is not found in HC and NE whereas the *<u>adverb</u>-verb-object* pattern is found in both French and Middle English with nonfinite verbs – in addition to the *verb-<u>adverb</u>-object* with finite verbs.

Third, object placement: There too we witness an increase in the restrictions in word-order options: In HC, as in NE, object noun phrases, be they full noun phrases or pronominal, are uniformly pronounced to the right of the verb, except when they move to subject position as in passives or to the left of the subject as in *wh*-movement (NB: NE, unlike HC, show *Object . . . V* order in certain compounds as in *truck-driver*, *baby-sitting*, etc.) The range of *Object . . . V* patterns in HC and NE is noticeably smaller than that of their counterparts in French and Old English, respectively. Objects in French and Old English exhibit freer distribution than in HC and NE: for example, unlike in HC and NE, objects in French and Old English can be pronounced, among other places, to the left of the verb and to the right of the subject when the object is a clitic pronoun (the French and Old English object placement facts are actually much more complex; see, e.g., Kayne 2000 for French and Taylor and van der Wurff 2005 for Old English). Interestingly, HC's substrate languages, especially the Gbe languages and other languages in the Kwa family, join HC's Romance superstrate, and differentiate themselves from HC, in offering various constructions (e.g., in progressive, purposive, causative and inceptive clauses) where objects are pronounced to the left of their verbs and to the right of the subject, even though the canonical order is Subject–Verb–Object. As suggested by Enoch Aboh (personal communication, November 20, 2008; also see Aboh 2005, forthcoming), these *Object . . . V* constructions in Kwa are somewhat reminiscent of *Object . . . V* patterns in the aforementioned English compounds. Aboh (forthcoming) also discusses the loss of *Object . . . V* patterns (e.g., in verbal nominalizations) in Sranan and Saramaccan (Surinamese Creoles) notwithstanding their occurrence in both the English superstrate and most of the Kwa substrates (e.g., in Gbe).

What is notable here vis-à-vis the oft-repeated claims that creolization instantiates exceptional diachronic patterns is that, in the history of both English and HC (and of the Surinamese Creoles discussed in Aboh forthcoming), there has been a narrowing of options for verb and object placement alongside a reduction in verbal inflectional morphology, as compared to the corresponding patterns in the respective ancestor languages (for more extended discussion and additional case studies, see DeGraff 2000, 2001a,b, 2005c).

Lastly, the grammatical preverbal morphemes that express Tense-Mood–Aspect semantics in the extended projection of the HC verb phrase are all derived via grammaticalization from morphemes that were used in 17th/18th-century French verbal periphrastic constructions. Similar observations hold for the other French-based Creoles spoken in the Caribbean and in the Indian Ocean. (See DeGraff 2005c: 320–329 for data, analyses and references.) In this domain as well, the development of certain aspects of HC morphosyntax is reminiscent of that of English. Compare, say, the development of HC Tense-Mood–Aspect morphemes via the grammaticalization of French verbal periphrastic constructions with that of English modals via the grammaticalization of erstwhile main verbs (cf. I. Roberts 1999: 317).

Such systematic correspondences between HC and French in phonological, lexical, morphosyntactic and semantic domains militate against Thomason and Kaufman's, Bickerton's and others' postulation of an *exceptional* 'break of transmission', followed by *ab ovo* creation, in, and only in, Creole diachrony. Indeed, the development of HC's structural patterns appear to not instantiate the sort of extraordinary and abnormal break in transmission that would set the formation of HC apart from other instances of language change, contrary to exceptionalist hypotheses about Creole formation. Besides the HC patterns of development noted above can be explained by independently needed principles of linguistic theory, and similar developmental patterns are found in run-of-the-mill language-acquisition instances, and in social contexts that are not usually considered germane to Creole formation (DeGraff 2005c).

2.3.4. Caribbean Creoles Are Genetically Related to European Languages According to the Comparative Method

Before delving into the details of my argument about the genetic relatedness of Caribbean Creoles, I must insist on one important clarification to defuse some unwarranted criticism of my position: My ongoing arguments here and elsewhere do *not* make HC a 'variety [qua dialect –md.] of French', contrary to Siegel's (2007: 190) misrepresentation of my position. Romance languages are usually not considered as 'dialects' of Latin even though they are considered to have developed from Latin via 'normal' language change, that is, without any 'break in transmission'. In fact, I have argued elsewhere that there is good reason (both structural and sociological) to *not* consider HC a 'dialect' of French (see, e.g., DeGraff 1995, 2001b: 237), abstracting away from the fuzzy sociopolitical boundaries around the uses of terms such as 'language', 'dialect', 'variety', etc. Thus, HC and French, like French and Latin, can be reasonably claimed to be genetically related, even when they are *not* taken to be typologically similar or to constitute 'dialects' of the same language. Moreover, French and HC are mutually unintelligible, especially when spoken by monolinguals who cannot rely on the knowledge of a shared language for accommodating their interlocutors.

This said, core aspects in the development of HC grammar – with respect to, for example, sound patterns, morphology, vocabulary (both basic and nonbasic), verb and object placement, the development of grammatical morphemes – fall squarely within the range of developmental patterns that are commonly manifested in other instances of language change outside of Creole-related phenomena, as in the history of Germanic languages.

Furthermore, there exist French–HC morphological correspondences that straightforwardly fall in the category of correspondences that Meillet has called 'arbitrary associations' and 'shared aberrancy', which historical linguists have long used as privileged evidence to establish genetic relatedness. These French–HC 'arbitrary associations' include a variety of sandhi phenomena (e.g., certain cases of liaison), idiosyncrasies in affixal inventories (e.g., in the domain of inhabitant names or names of origins), suppletive forms (e.g., for the derivation of ordinal numbers), etc., as discussed in Dejean (1980), Fattier (1998), and DeGraff (2001a, 2002). Campbell and Poser (2008: 182) remind us that in establishing language family relationship 'Meillet's use of "shared aberrancy" in grammatical evidence is now standard practice among Indo-Europeanists and historical linguistics generally' (also see Joseph and Janda 2003). If so, any comparison of Caribbean Creoles with their European ancestors that does not take into account these 'shared aberrancies' looks like a methodological aberrancy, one that is shared across many writings in and outside of Creole studies.

When faced with the thorough documentation of lexical, morphological, and morphosyntactic correspondences between Caribbean Creoles (e.g., HC) and their European ancestors (e.g., French), those who promote exceptionalist views of Creole formation may argue that it is the tempo of Creole formation, which is claimed to be spectacularly faster than that of language change, that makes Creole languages exceptional (see, e.g., Siegel 2007: 174). In such a view, differences in the rates of formation of new languages are enough to make 'creolization' qualitatively different from 'language change'. Even Plag (1994) – who argues against the 'uniqueness' of creolization and who likens creolization to language change – contrasts 'impaired transmission' in creolization vs 'normal transmission' in language change, and assumes that '[c]reolization is unique [. . .] because of the fact that the changes [there] proceed very fast and affect many subsystems at the same time [whereas] natural change innovations are generally not as drastic, and tend to take much more time' (p. 18). But here we may be dealing with yet another confusion between innovations in I-languages vs. spread in E-languages. Indeed the (apparent) difference in tempo between the formation of Creoles and that of non-Creoles may reflect, not the actual tempo of I-language structural innovations in Creole vs. non-Creole diachrony, but two related time-based factors: (i) the speed at which I-language structural innovations spread within the E-languages of the corresponding speech communities (see, e.g., Mufwene 2001: 130,140 for an argument that 'Creoles did not develop more rapidly than

other languages'); and (ii) the artifact of written archives as a belated and sparse record of I-languages and their E-language manifestations across time. (I return to both these issues in §§2.3.5 and 2.3.7.)

In the meantime, and in light of now well-documented (dis)similarities in the diachrony of various patterns in HC, English and French morphosyntax, it can also be argued that along certain dimensions French and HC are more similar to each other than French and Latin are. As Meillet noted long ago, 'neo-Latin [i.e., Romance] languages fall into a typological class that is quite remote from the structural type represented by Latin' (1929 [1951: 80]). What Meillet had in mind, among other things, was that '[t]he use of word order in French and English to express relations between phrases is a creation of these languages: such innovation did not have any model in Latin or Proto-Germanic' (1912 [1958]: 148). According to Meillet, other features, in addition to the relative freedom of word order, that put Latin and French in distinct typological classes include: presence vs. absence of definite articles and of overt case morphology. In these particular domains, these innovations did produce even larger discrepancies in the emergence of Romance from Latin than those observed in the emergence of French- and Iberian-based Creoles (see Guy Hazaël-Massieux 1996: 37–46, for a related argument). As it turns out, for each of these binary distinctions in word order, case morphology and presence vs. absence of definite articles, French and HC pattern like each other, and both pattern differently from Latin. It can be similarly argued that, again along certain parameters, English and Caribbean English-based Creoles are closer to each other than English and Proto-Germanic are. (See DeGraff 2001b: 235–242, 291–299, 2005a,b, for more extensive treatments of these arguments for the HC case.) As I indicated in DeGraff (2005c) with references to supporting literature, such (dis)similarities are an artifact of what parameters we choose to compare, how and why.

A more general point is in order here: It's not only that the above-noted typological differences between Caribbean Creoles and their European ancestors are not substantial enough to demarcate 'creolization' from 'normal language change'. The argument is actually more pointed: given the state-of-the-art in historical linguistics (which still owes much to Meillet's insights on genetic relatedness), there is *no* sort of typological difference, or no sort of 'drastic [morphosyntactic] mismatch' à la Thomason (2002: 105), that would justify a claim against genetic relatedness, be it with respect to a Creole or a non-Creole language. This observation goes against Thomason's (2002) claim that the 'Comparative Method requires extensive matching in lexicon *and* structure' (p. 105, emphasis in original) – if it did, then Romance languages would be considered genetically *un*-related to Latin as they 'fall into a typological class that is quite remote from the structural type represented by Latin' with 'structures that are fundamentally different from their Latin counterparts', as observed by Meillet (1929 [1951: 80]).

What the above comparisons and observations suggest is that we still lack any reliable *structural* litmus test to differentiate between the formation of Creole and non-Creoles. Let's assume, without any preconceived bias, that:

> the rigorous criteria of the Comparative Method . . . include the establishment of recurring phonological correspondences in morphemes of identical or similar meanings, including much basic vocabulary, in sufficient numbers and complexity that chance is ruled out as the source of the correspondences; the establishment of systematic morphosyntactic correspondences; and a demonstration that reconstruction of a significant proportion of morphemes and morphosyntactic features is possible. (cf. Thomason 2002: 103; also see Campbell and Poser 2008 for a comprehensive survey and explication of these criteria and their historiography)

With such rigorous criteria in mind, all the available evidence – the inheritance, with modification, from French of the greater part of the HC lexicon including all of HC's basic vocabulary and grammatical morphemes, HC's phonological, lexical and morphosyntactic correspondences with French, and so on – puts HC, a 'prototypical Creole' if there ever was one, squarely in the scope of the Comparative Method, pace Thomason's (2002: 105) own conclusions that 'the Comparative Method would not permit identification of [Creole languages] as genetically related to [their] lexifiers [i.e., their European ancestors in the case of Caribbean Creole]' (cf. Thomason and Kaufman 1988: 8–12, 206, etc.; Ringe et al. 2002: 108; and the related debate in Mufwene 2003; Thomason 2003). As Mufwene (2004: 481) correctly observes:

> Thomason and Kaufman (1988) have also claimed that creoles cannot be genetically classified. None of those subscribing to this view has ever applied systematic comparisons of structural features of creoles with those of the nonstandard varieties of European languages that were targeted by the slave populations.

As far as I can tell, the very same *linguistic* tests that would classify Romance languages as descendents of Latin (as described in, e.g., Campbell and Poser 2008) would also classify HC and Romance-derived Creoles as descendent of Romance languages (cf. Meillet 1914 [1958] contra Schuchardt 1917; Hall 1950, 1958; Weinreich 1958; Goodman 1964; Posner 1985, 1996; Guy Hazaël-Massieux 1996; Chaudenson and Mufwene 2001; Mufwene 2001ff, etc.). Such linguistic evidence militates against any exceptionalist view on Creole formation, especially if we heed Campbell and Poser's (2008: 205) 'no non-linguistic evidence' admonition: 'Another valid principle permits only linguistic information, and no non-linguistic information, as evidence of genetic relationship among languages.' Once subjective non-linguistic factors are abstracted away, Caribbean Creoles can be comfortably accommodated in Stammbaumteorie tree-family models for genetic descent.

2.3.5. Nonlinguistic Confounds in the Application of the Comparative Method to Creole Languages

Here are four sorts of nonlinguistic evidence, among others, that have mistakenly, yet repeatedly, been levelled in favor of a 'sharp break of transmission' in the formation of Caribbean Creoles and against their genetic relatedness with their European ancestors:

(i)   large-scale L2A in Creole formation;
(ii) lack of a common language among any substantial subset of substrate speakers;
(iii) lack of a 'target language' for the African adult (quasi-)learners in the Caribbean – these learners did not really 'intend' to become fluent in the corresponding European varieties;
(iv) the abrupt and catastrophic emergence of Creole languages as compared to the slow and gradual formation of non-Creole languages, as in (say) Latin-to-Romance diachrony.

Not only are all four of these arguments excluded by Campbell & Poser's 'no non-linguistic evidence' principle, but they either fail to make the right cut between, say, Caribbean Creoles and Romance Languages, or they find no sociohistorical or empirical support in the available evidence. Let's look at factors (i)–(iii) in this section, then we return to (iv) in §§2.3.6 and 2.3.7. In §2.3.6, we revisit the notion 'break in transmission' as a (non)criterion for lack of genetic relatedness in Creole formation, and in §2.3.7 we take a fresh look at the rate of emergence of Romance languages in light of, inter alia, recent data in Adams (2007) about the regional diversification of Latin in 200 BC to 600 AD and the subsequent emergence of Proto-Romance varieties at a much earlier period than previously estimated.

For now, let's ask: Can 'large-scale L2A' be used as a defining criterion for *sui generis* creolization? I doubt it. L2A was substantially involved in the formation of both Caribbean Creoles and Romance Languages (on the latter, see, e.g., the painstaking details in Adams (2007: 281–289, 34f, 78, 113, 126, 365, 368f, 371f, 569f, 622, 701f, 716, etc.); also see Posner (1996: Ch. 3, 6, 7) for a broad overview of language-contact effects in the history of Romance). This is a challenge for definitions of 'creolization' that use L2A as a crucial criterion (see, e.g., Siegel 2007; Plag 2008a,b, and references therein) and for definitions of 'genetic descent' that stipulate 'an unbroken sequence of instances of native-language acquisition by children' as a necessary and sufficient condition (see, e.g., Ringe et al. 2002: 63; Labov 2007: 346). The well-documented role of L2A in the emergence of Romance languages may, at first approximation, also appears as a challenge for Labov's (2007: 344) distinction between 'normal linguistic development' and the formation of Creoles. For Labov (2007), 'unbroken sequence of [L1A] by children' (or 'transmission' in his terminology) determines Stammbaumteorie-friendly regular language change (cf. Ringe

et al. 2002: 63) whereas language- or dialect-contact via learning by adults (or 'diffusion' in Labov's terminology) determines 'wave effects' as in the formation of Creole languages (Labov 2007: 344, 346, 349, 371, etc.). For Labov, this is due to the diminished language-learning capacities of adults (see §3.2.1 below for another possible consequence of this language-learning difference between adults and children). But the fact that Romance languages, *on a par with Creoles*, saw L2A by adults (alongside L1A) play a key factor in their respective emergence constitutes a paradox for the positions of Ringe et al. and Labov. A corollary paradox is the fact that the evolution of Romance from Latin is usually taken as a poster case for the Stammbaumteorie family-tree model while Caribbean Creoles are often excluded from the scope of the Comparative Method (Thomason and Kaufman 1988; Thomason 2002, 2003; Ringe et al. 2002: 108). This paradox disappears once we accept the two following facts:

(i) Language- and dialect-contact (thus, contact-induced change, koineization, and so on), alongside L1A, seems quite pervasive in the history of *all* languages – be they labeled 'Creole' or not (Mufwene 2001ff). Such a way out of the paradox seems hinted in (my reading of) Labov's (2007: 383) suggestion that any difference between L1A and L2A with respect to 'transmission' of internal changes vs. 'wave' effects via language contact is, in all likelihood, a matter of *degree*. Note that the empirical patterns that are analyzed in Labov (2007) can be divorced from the exclusive role that Labov, following Ringe et al. (2002), attributes to L1A in the formation of Stammbaumteorie branches. Indeed all the case studies in Labov 2007 – with respect to L1A- and L2A-related differences in 'transmission' vs. 'wave' effects – concern changes in English varieties that, notwithstanding any difference in L1A vs. L2A, still belong to the same Stammbaumteorie phylum.

(ii) The proportion of L1A vs. L2A instances in any diachronic scenario falls in the class of 'non-linguistic factors' in the sense of Campbell and Poser (2008: 205), and, as such, it cannot be used as a criterion for determining genetic (un)relatedness. In Meillet (1914 [1958]) as well, language shift by adult populations does not necessarily entail a break in genetic relatedness. In light of the evidence from HC and other Caribbean Creoles, I must agree with Meillet and Campbell and Poser, and thus disagree both with Thomason and Kaufman's (1988) notion of 'abnormal transmission' – thus a break in genetic relationship – in Creole formation and with the stipulation of Ringe et al. (2002: 63) and Labov (2007: 346) that 'an unbroken sequence of instances of native-language acquisition by children' is a necessary and sufficient condition for linguistic descent. The latter stipulation, which excludes Creoles from Stammbaumteorie family tree, introduces a nonlinguistic *sociohistorically rooted* factor in the definition of genetic relatedness.

This stipulation seems off the mark, not only for Caribbean Creoles, but also for varieties of, say, Indian English and Haitian French, alongside other 'indigenized varieties' of European languages (e.g., 'World Englishes'), that are learnt as second languages in scholastic environments (Mufwene 2004). Ringe et al. and Labov would consider such varieties genetically unrelated to said European languages simply because they are often learnt via L2A. Yet these 'scholastic' varieties show systematic correspondences with their European ancestors, with which they are often mutually intelligible. For example, those educated speakers of Haitian French who learnt French in the classroom can usually converse with native French speakers from Europe, and so can native speakers of American English and nonnative speakers of Indian English. In a related vein, Mufwene (2001: Ch. 4, 2004) criticizes the classifications that turn into 'illegitimate offsprings' certain varieties of European languages spoken outside of Europe, including the aforementioned indigenized varieties whose nonnative models are generally classroom teachers. Mufwene's arguments follow Meillet's (1914 [1958]) views on genetic relatedness, and anticipate Campbell and Poser's (2008: 205) criticism of the use of nonlinguistic (e.g., ethnic) factors in 'outlandish' mistakes in the genetic classification of African languages. Guy Hazaël-Massieux (1996: 41) gives other examples of outlandish classification in African linguistics, some of which seem rooted in 19th-century doctrines of race and heredity. Ethnic factors have also played a role in the misclassifications of Creole languages, 'as commentators were only too ready to believe that speakers of obviously different ethnic origin would favour language of quite different type' (Posner 1985: 167).

Ringe et al. (2002: 64), citing Kroch et al. (2000), mention one example of a (presumably genetic) variety of English whose history was greatly influenced by L2A:

> One Middle English dialect of the northeast midlands is descended from the English learned imperfectly by Scandinavian settlers who were obliged to learn the language during the reconquest but had too little contact with native speakers to enable their children to learn a 'normal' dialect natively.

Ringe et al. describe this as an 'anomalous history', but presumably this Middle English dialect and its descendents would still qualify as being 'genetically descended' from English. If so, Ringe et al.'s definition of 'linguistic descent' would seem inconsistent with the facts of Middle English diachrony as documented in Kroch et al. (2000) – and with the Caribbean Creole facts as analyzed in this paper.

There is one other nonlinguistic difference that has been claimed as a key criterion for creolization. Consider the claim that in creolization, but not in other cases of contact situations, the adult L2 learners generally had 'no preexisting language in common', thus the need for a pidgin among substrate speakers (Bickerton 1984: 174, 176, etc.; also see Thomason and

Kaufman 1988: 148, 155, etc.). Among Africans in the colonial Caribbean, such a lack of any common language was, it has been claimed, the result of a deliberate strategy of ethnic mixing on the part of slave owners in order to preempt rebellions (cf. Thomason and Kaufman 1988: 148). As hinted at in Thomason and Kaufman (idem), the claim that enslaved Africans on any given Caribbean plantation would usually have no common language is an exaggeration. Given our assumptions above about I-languages as 'linguistic fingerprints', adult learners *anywhere* have pairwise distinct *idiolects*. Besides it is a well-documented fact that the enslaved Africans originated from a diversity of ethnic groups from the Niger–Congo area of Africa, bringing with them a diversity of E-languages as well. Yet, given the data collected by a variety of caribbeanists, including historians and demographers, it seems statistically unlikely that there would have been enough substrate languages (qua E-languages in the communal normative sense; see note 2) to consistently ensure that any subset of enslaved African slaves on any given Caribbean plantation lacked a common E-language. This is especially unlikely at the peak of land exploitation in the colonial Caribbean – with, for example, 480,000 African slaves in Saint-Domingue/Haiti in 1791, more than 90% of the total population (Singler 1996: 219f). Moreover slave owners and traders were mercantilist opportunists who at any given period tended to look for slaves wherever they could get them at relatively low costs, and whenever possible they would favor particular ethnic groups for certain purposes. According to Singler's (1996) detailed calculations, there is suggestive evidence that, for nearly 80 years after the founding of the colony, the majority of slaves in Saint-Domingue/Haiti were speakers of Gbe languages. Subsequently, it is speakers of Bantu languages who became numerically preponderant in the colony.

More generally, the slaves in the colonial Caribbean were often able to organize themselves into 'nations' that reflected, grosso modo, their ethnic origins. The descendants of such 'nations' include the contemporary *Nanchon* in Haitian Vodou whereby certain African-derived religious spirits, rites, dances, rhythms and drums belong to, for example, the 'Rada Nanchon' originally from the Allada people of Dahomey (present-day Benin), the 'Ibo Nanchon' from the Ibo people of present-day Nigeria, the 'Kongo Nanchon' from the Congo people, etc. This seems to suggest that at least some of the slaves on some of the plantations were, for an extended period, able to share and transmit – with modifications, of course – some of their common languages and cultural customs. Such observations contradict Chaudenson and Mufwene's (2001) repeated claims of abrupt deculturation-cum-acculturation among Saint-Domingue's African population. (For further related details and additional evidence on the protracted survival of African languages in the colonial Caribbean, see Moreau de Saint-Méry 1797: 63–70; Descourtilz 1809, vol. 3: 109–234; Debien 1971: 123, 220–234; Fouchard 1972: 433–436; Le Page and Tabouret-Keller 1985: Ch. 2; Singler 1992ff, H. Trouillot 1980: 55, 59,

62, 64–70; M.-R. Trouillot 2002; Warner Lewis 2003, etc; also see the related discussion and references in DeGraff 2002: 304 n45).

Notwithstanding the predominance of Gbe and Bantu among HC's substrates, it is varieties of French, including L2 varieties, that constituted the initial *lingua franca* of Saint-Domingue, and there were enough, and sufficiently distinct, L1s among the adult learners to produce a wide range of L2 varieties of the target French-derived varieties ('approximations of approximations' in Chaudenson and Mufwene's 2001 terminology). These L2 varieties would manifest distinct inventories of structural transfer from the various L1s (Singler 1996). Given the fact that contemporary HC varieties are relatively homogeneous and that the prevailing dialectal differences seem to *not* reflect structural differences among the substrate languages (Fattier 1998), one key question then becomes: How did the earlier array of mutually distinct L2 varieties with distinct inventories of substrate-influenced structures merge into a relatively uniform set of varieties? This is a question that we will take up below, especially in §4.2.2 (also see DeGraff 2002: 386–391).

There is yet another nonlinguistic factor that has been claimed as a crucial factor in creolization. This particular factor concerns an undocumented aspect of the psychology of Creole creators, namely their hypothetical goals vis-à-vis the European target languages. For example, according to Baker (1990: 111), 'in almost all situations from which a pidgin and (subsequently)/or a creole emerged . . . there was no TL [target language] as such, and . . . the real, if unconscious, target of participants was the development of a medium for interethnic communication (MFIC)'. In other words, Baker's claim entails that language learning, or rather language creation, by, say, Africans in some of the most formative stages in the history of Caribbean Creoles did not involve their 'seriously aspir[ing] to acquire something approaching native speaker fluency in [the corresponding European language]' (p. 107). Instead, the goal was to create a 'medium for interethnic communication (MFIC)' (also see Thomason and Kaufman 1988: 152, 352 n4 for related claims).

To start with, Baker does not spell out how exactly the psycholinguistic processes that underlie MFIC creation would differ from run–of–the–mill instances of L2A. It seems reasonable to assume that any effort at L2A is related to learners' desire to establish some sort of 'MFIC' between themselves and speakers of the corresponding L2, and such MFIC creation would, in turn, enlist (some of) the same resources that learners use to create their interlanguages in L2A. I wholeheartedly agree with some of Baker's conclusions in his 1990 'Off target' article; namely, that Pidgins and Creole are 'self-evidently successful solutions to problems of human intercommunication . . . drawing on the range of available resources, tailored to their users specific and changing communicative need'. But this may well hold for language learners' varieties in general, except in pathological cases (see, e.g., various chapters in Han and Odlin 2006). Siegel

(2006: 22–24, 2007: 194f) conveniently summarizes the evidence from language-acquisition research that L2A of the 'targeted' *and* 'successful' sort was involved in Creole formation, taking into account the psychosocial contexts that determine the L2 learner's own 'strategic' and 'adaptive' criteria for 'target' and 'success' (also see Mufwene 2001ff). In many cases, even L2 learners at the early stages of L2A would consider their learning successful when, for example, they 'learned enough of the target languages to be able to communicate but went no further, despite opportunities for further exposure to the languages' (Siegel 2007: 194). At this rate naturalistic L2A and MFIC creation seem indistinguishable along the relevant dimensions.

With respect to L2A in the specific case of the colonial Caribbean, it has already been pointed out that many of the Africans in the colonial Caribbean did share a common language at their arrival in the Caribbean, namely their native substrate language, as in the case of the Gbe speakers who numerically dominated Saint-Domingue's African population through much of the colonial period (Singler 1996). In this respect, the demographics of colonial Saint-Domingue strikes me as quite different from that of Mauritius as described by Baker for his MFIC scenario. In Saint-Domingue, for at least those Africans who already shared a common substrate language, there was less pressure for creating an MFIC for use among said substrate speakers, so their efforts at learning (approximations of) French varieties must have been relatively independent of any MFIC-related need vis-à-vis fellow Africans, and more attuned to the fact that approximations of French varieties had already accumulated social capital and gained currency as *the* colonial lingua franca (see §2.2.3).

Furthermore, the sociohistorical and empirical evidence available to us (e.g., first-hand reports from colonial Haiti and in archival and contemporary data on Haitian Creole, alongside colonial Creole texts from the earliest documented Creole varieties) suggests that sizable proportions of Africans in the colonial Caribbean exerted repeated and sustained attempts to achieve fluency in the corresponding European targets and that many of them did manage to systematically approximate substantial components of the target grammars (see §2.2.3 and §4). Linguists such as Thomason and Kaufman (1988) who argue that 'the label "genetic relatedness" does not properly apply when transmission is imperfect' (p. 10) and that genetic relatedness is to be determined 'based theoretically on the social fact of normal transmission rather than merely on the linguistic facts themselves' (p. 12) have not provided us with any rigorous operational measure to objectively and reliably draw the line between so-called 'imperfect transmission' as in the history of, say, Haitian Creole (a 'true'/'prototypical'/ 'abrupt'/'radical'/etc. Creole) and so-called 'normal transmission' in, say, Romance and Germanic languages (which are typically taken as uncontroversial examples of 'genetic' languages).

At any rate, in light of the available sociohistorical and linguistic evidence, we must follow Weinreich's (1958: 375) advice, contra, for

example, Thomason and Kaufman (1988), in consistently excluding hypothetical nonlinguistic factors such as (dis)continuity of transmission (e.g., 'successful L1A' vs. 'imperfect L2A in unsuccessful language shift') as criteria for genetic (non)relatedness:

> To use relative continuity or discontinuity of transmission as a *criterion* . . . would probably be unwise, since past circumstances of transmission cannot easily be reconstructed. The structures of the surviving languages are the only solid evidence available, and they are the only safe basis for assignment to a language family.

Adopting Meillet's (1914 [1958]) methods of genetic classification, Weinreich thus considers the 'solid evidence' of systematic correspondences between French and HC (e.g., the fact that HC basic morphemes, including grammatical morphemes such as those that mark tense and aspect, 'satisfy the definition of cognates with respect to French') and concludes: 'On the linguistic evidence, the French-based Creoles, then, are without a doubt branches of French'.

As for the issue of tempo, innovations in I-languages are necessarily abrupt. Therefore, the appearance of slower developments in non-Creole diachrony has got to be an illusion caused by language-external contingent factors that condition the rate at which those innovations persist and spread in *populations* of I-languages and, thus, into E-languages. As for the rate of emergence of said innovations in particular I-languages, it is inevitably upwardly bounded by the lifespans of individual speakers, whether these are speakers of Creole or non-Creole languages (see §2.3.7). We'll keep returning to this spread-vs.-innovation issue and the pitfalls of conflating the two concepts.

2.3.6. 'Break in Transmission': A (non)criterion in Creole Formation – Redux

Alongside the empirical implications of the above comparisons between diachronic patterns across the history of Creoles and non-Creoles, the language-acquisition schema in (3) bears one important conceptual implication regarding the often-asked question whether Creole genesis, and Creole genesis alone, instantiates an *extraordinary* 'break in language transmission' due to 'absence of well-formed input' as PLD for the language learners who created the Creoles. If our focus is on I-languages and on the processes that underlie their formation, then 'language trans-mission' is necessarily discontinuous, and there is nothing 'extraordinary' about 'break in transmission' in the sense that is captured in (3). As already mentioned in §2.1.2, such nonexceptional 'break in transmission' was noted as far back as in Paul's and Meillet's works in the late 19th and early 20th century. Each I-language, be it 'Creole' or not, is an individual UG-bounded recreation based on necessarily limited and somewhat conflicting evidence. If so, 'discrepancies' (or parametric differences)

between attained grammar and target grammars are actually quite normal; cf. Crain and Pietroski's (2002: 177–182) and Crain et al.'s (2006) examples of 'language change' in language acquisition.

Not all of the learner's parameter 'mis-settings' are retained in the learner's steady-state I-language. Take, for example, Lightfoot's (1999: Ch. 6, 2007: Ch. 5) cue-based model for language acquisition. There it is expected that, for certain parameters, the corresponding parameter-setting 'cues' may become available only after an initial period during which the relevant parameters are provisionally 'mis-set'. The greater the number of parametric innovations that are retained in the mature idiolects, the clearer we can observe – and here I borrow Singler's (1996: 196) definition – 'the creation of a language that is [markedly] different from the lexifier [qua target]'. Here I must note that I disagree with Lightfoot's (2007: Ch. 7) adoption of a quasi-Bickertonian catastrophic pidgin-to-creole life-cycle hypothesis, with creolization stemming from L1A with unusually impoverished input – a scenario that is not supported by the available sociohistorical and linguistic evidence that is surveyed in the present paper. Be that as it may, it is contingent sociolinguistic and demographic circumstances that determine how much the PLD fluctuate, and how many and which innovations from learners' intermediate grammars can survive into their steady-state I-languages and then spread to other I-languages and eventually into relatively stable communal norms (cf. Andersen 1983). These contingent factors include: migrations; language contact; the inventory of, and the typological variation among, the languages in contact; relaxation of linguistic norms due to, *inter alia*, low proportion of native or fluent speakers, high degree of language contact, etc.

2.3.7. Was the Formation of Caribbean Creoles Radically More Rapid Than the Formation of Romance Languages?

The aforementioned contingent factors bear on the question raised in §§2.3.4–2.3.6 whether the tempo of Creole formation (e.g., the rate of emergence of Caribbean Creoles) is *qualitatively* different from the tempo of non-Creole diachronic phenomena (e.g., the rate of emergence of Romance and Germanic languages), as commonly argued in Creole studies (see, e.g., Siegel 2007: 173). In both the Creole and non-Creole scenarios, innovations of any sort are necessarily 'abrupt' since they happen within the scope of individual instances of language acquisition or language use and since they are maximally bounded by the lifespans of the individual innovators. It is only the rate of spread of said innovations that will vary from case to case, based on sociohistorical contingent factors of the sort mentioned in §2.36.

Furthermore, in the long-ago case of, say, the Latin-to-Romance diachronic developments, it seems quite likely that the earliest varieties of (proto-)Romance went nameless and unrecorded for quite a while, thus giving us an inaccurate picture of the rate of their emergence (see, e.g.,

Adams 2007: 11f, 365f, 688f, 724–726). As Adams argues, the timing of Romance diachrony is often based on the written record, which notoriously lags behind the actual emergence of said varieties among their often illiterate and low-prestige speakers (cf. Meillet 1929: 127 [1951: 79]). The earliest (Proto-)Romance varieties would have evolved nameless for an extended period outside the purview of most scribes. 'Since most Latin texts are the work of an educated minority and since the educated tended to be influenced by the normative efforts of grammarians and purists, dialectal forms had little chance of making it into writing' (Adams 2007: 366f). This is one reason, among others, why Creole formation in, say, the 17th-century Caribbean, with archival texts that are almost as old as the earliest (Proto-)Creole varieties, can teach us important lessons about the emergence of (Proto-)Romance varieties in Antiquity (Mufwene 2001ff).

As it turns out, Adams (2007: 281–289, 365, 701f) provides some tantalizing evidence of Proto-Romance 'developments in micro-communities', some of it going back to the 1st century AD much earlier than the accepted date for the emergence of Proto-Romance. Such evidence suggests that some of the earliest contact-induced innovations emerged from the contact between speakers of Gaulish and Latin. These innovations were introduced among a small, isolated and rural population of Gaulish-Latin bilingual potters in Southern Gaul before any large-scale gradual spread into wide-spread (Proto-)Romance varieties – another good example of the need to distinguish between innovations (always unavoidable and always relatively rapid in the context of language acquisition by *individual* learners) and spread (sometimes inexistent, and always relatively slow when it does happen, within *populations* of learners).

Vis-à-vis alleged differentials in the diffusion rates of Creole vs. non-Creole innovations, one question worth asking about the Caribbean case, which we will not address here, is whether the multitude of the languages in contact, the relative absence of established linguistic norms in the plantation colonial context, coupled with the social status and upward mobility of the locally born, did favor a relatively high rate of diffusion for Creole innovations, especially those innovations that doubled as social markers in the speech of the locally born.

Be that as it may, the fluctuating proportion of nonnative approxima-tions of target varieties, alongside the aforementioned socioeconomic and demographic factors, throughout colonial Caribbean history would only affect the *contingent* aspects of (3), namely the PLD qua linguistic ecology of the language learner. Independently of sociohistorical (e.g., demographic, ethnographic and sociolinguistic) contingencies and their eventual effects on the PLD and on the constructed idiolects, the basic structure of UG – or its equivalents in other theoretical frameworks – must arguably be uniform across all instances of language acquisition: 'S_0 [in (3)] is a constant' (Chomsky 1986: 37f). Caribbean slaves were often miserably mistreated, but such mistreatment could not have deprived them of their

basic humanity, which includes their language faculty – their genetic endowment for language.

On a par with other diachronic phenomena, the formation of I-Creoles, as normal instances of I-language creation, does have group-level catalysts and group-level effects of the sort (too sketchily) considered here. This topic will be further elaborated in §§4.2 and 4.3. For now, it is crucial to keep these individual vs. social factors in mind in any discussion that tries to segregate Creole-formation phenomena from their non-Creole analogues.

3. On Time Machines and Other Expediments for Creolists

3.1. DO CREOLISTS REALLY NEED TIME MACHINES?

How are we to 'discover as much as possible about the actual process of creolization' (cf. Arends 1993: 377)? One influential gradualist school in Creole studies 'calls for a temporary decrease in theoretical activity in favor of the description and analysis of the process itself' via an archive-based '*empirical, historical approach*' [emphasis in original]; 'there is a clear priority for empirical investigation [of archival records] over theoretical speculation' (idem).

If one assumes in Popperian mode that theoretical hypotheses always play a role, explicit or implicit, in guiding one's empirical investigations, then it is not clear to me what sorts of 'theoretical speculation' must cede priority to 'empirical investigation' into archival records. If 'Creole genesis' includes the development of I-creoles (e.g., as a prerequisite to the creation of E-creoles), then we must deal with the following empirical limitations. Firstly, the original agents of the process – few of whom, if any, are thoroughly described in archival records from the early stages of creolization – are all dead. Secondly, archival records cannot be subjected to, say, grammaticality judgments and other sorts of experiments that are needed to investigate any complex sort of cognitive process.

These empirical limitations are usually familiar to linguists interested in historical phenomena. But not all such linguists argue against theorizing. In fact, the (neo-)Neogrammarian historical linguists we encountered earlier – from Hermann Paul to Tony Kroch, Ian Roberts, David Lightfoot, etc. – have put 'theoretical speculation' at the center of their research, with archival texts being used to test their theories.

In this vein, it seems unlikely that 'description and analysis of [any complex] process' can be helped by 'a temporary decrease in theoretical activity'. The aforementioned empirical limitations may not constitute any challenge to the creolist if 'the actual process of creolization' concerns solely 'Historical Grammar' (in Paul's 1890 sense) and if creolistics as an 'empirical, historical approach' aprioristically excludes any investigation into the psycholinguistic acquisition mechanisms that ultimately underlie

Creole formation. But Cartesian creolistics of the sort considered here cannot benefit from such arbitrary exclusion since it focuses explicitly on Creole idiolects (I-creoles).

In their colorful debate on the demographics of nativization and gradualism in creolization, Bickerton (1987: 231) then Singler (1993: 249, n5) have alluded to an hypothetical 'time machine in the cellars of [New York University]' that would allow creolists to directly observe Creole formation in real time. Fortunately, Uniformitarianism dispenses with time machines. Let's grant that the individuals involved in the formation of Caribbean Creoles had the same linguistically relevant genetic traits as contemporary humans. If acquisition of native languages by children (L1A) and of nonnative languages by adults (L2A) plays a role in observable instances of language change, then current results from language acquisition research should establish boundary conditions on our theoretical speculations on what children and adults could, in principle, contribute to Creole formation.

In other words, results from L1A and L2A research should both inspire, and impose *a priori* limits on, our Creole-formation scenarios. This seems a sound heuristic: much more is known and knowable about contemporary, thus observable and measurable, instances of L1A and L2A than about their counterparts in the early stages of creolization. The latter can no longer be investigated via direct observation and experimentation, notwithstanding our increasingly expansive archival databases. Compare, say, on the one hand, the richness and sophistication of modern psycholinguistic experiments and cross-linguistic databases in L1A and L2A research (see the data and surveys in Klein and Perdue 1992; DeGraff 1999b; Rizzi 1999; Archibald 2000; MacWhinney 2000; Wexler 2003; Crain et al. 2006, etc.) and, on the other hand, the limitations inherent in colonial archival documents for early Creole varieties. Guy and Marie-Christine Hazaël-Massieux and Philip Baker and Lise Winer, among others, have illustrated and discussed the precautions that are needed to correctly interpret and evaluate these early Creole texts. Although these early texts, with the right amount of care in interpretation, do constitute 'a source of invaluable insight into earlier periods in the evolution of creoles' (M.-C. Hazaël-Massieux 2006: 29), the fact remains that these documents were often put together by European observers who were part of the colonial mission in the Caribbean. These observers were, by and large, not native Creole speakers, and, unlike most contemporary creolists, they often overtly despised their accidental 'informants' (see DeGraff 2001a,b, 2005b, for some discussion). Here, one heuristic that would put us on less slippery epistemological grounds is to extrapolate from the better known (or the knowable) to the lesser known (or the unknowable), rather than vice versa, and to use the early Creole texts as data that bear on our theoretical hypotheses (see §4.4.2 for one attempt in this direction). As recommended by William Dwight Whitney, Karl Brugmann and many others, let us

'take as the starting-point what is known by experience, and . . . apply this to the unknown of the past' (Brugmann 1897: 2).

It is thus that the Cartesian/I-language approach and the Historical-Grammar/E-language approach to Creole formation constructively complement each other. There is absolutely no need for 'a temporary decrease in theoretical activity' in creolistics. The complementarity between theory and data, including archival data, is inescapable once we recognize that Creole formation, because it includes instances of (3), is *both*:

(i)   ultimately rooted in *invariant* linguistic capacities ('UG') located in the individual minds of language acquirers and language users; and

(ii) inevitably affected by the *contingent* sociohistorical factors that contribute to the make-up of the always-heterogeneous PLD.

Thus, in order to elucidate the processes underlying Creole formation it is necessary to couple theoretical work on UG with the analysis of all available data, including archival data, sociohistorical information, contemporary texts, grammaticality judgements from contemporary Creole speakers, etc.

3.2. UNIFORMITARIAN EXPEDIMENTS

3.2.1. Sign-language Emergence vs. Caribbean Creole Formation

What can the better-known facts of language acquisition teach us about the lesser-known facts of creolization? This question was addressed at great length from a variety of perspectives, going back to the 17th century.[4] In the rest of this essay, I will keep going back to studies that may be particularly informative about (limitations on) the respective contributions of child and adult learners to diachronic phenomena. Elsewhere (DeGraff, 2002, 2005c) I look at specific parallels between morphosyntactic developmental patterns in language acquisition and creolization.

In this respect, the papers of Kegl et al. (1999) and Newport (1999) on Sign Language acquisition and creation are fascinating because of their sharp observations on the distinct restructuring capacities of learners at various ages and on the (non)effect of nonnative PLD in language acquisition. What is worth stressing is that these studies, unlike our speculations about the ecology and time course of creolization in the Caribbean or, more generally, of language change in the (relatively) remote past, are based on developmental patterns that are directly observable, thus open to precise measurements and comparisons and to more empirically grounded analyses. These measurements, comparisons and analyses should help us better appreciate the kinds of restructuring that take place in language acquisition with nonnative PLD and the effects of age on such restructuring. Both of these issues seem relevant to the roles of L1A and L2A in creolization (and language change, more generally), especially as we evaluate popular Creole-genesis scenarios that categorically exclude either

children (à la Lefebvre 1998) or adults (à la Bickerton 1984, 1999) as agents of Creole formation.

Because most types of deafness are not genetically inherited, many Deaf children have non–Deaf parents with quite limited signing proficiency. It is the non–Deaf parents' nonnative, restricted and unstable – 'pidginized' signing, if you will (but see §2.2.2 for some methodological caution regarding the term 'pidginized') – that in many instances constitute the Deaf child's initial PLD (see Kegl et al. 1999; Newport 1999; Goldin-Meadow 2003, and references therein for additional details). In addition, Deaf children are usually exposed to their first signed utterances at a later age than their non–Deaf counterparts are exposed to their first spoken utterances. It is such factors that turn certain instances of Sign Language acquisition into rich sources of data for investigating the relationship between degrees of PLD impoverishment, learner's age and (innovations in) the structural profiles of the grammars constructed in L1A and L2A. In this vein, the studies of Kegl et al. and Newport may help evaluate linguists' claims about the roles of children vs. adults, and the role of nonnative input, in Creole formation in particular and in language change more generally.

At this point, it is important to stress that the individual-level and community-level scenarios of Sign Language acquisition and creation examined by, respectively, Newport and Kegl et al. do not, and could not, replicate the formation of Caribbean Creoles in all its details. For example, there are substantial ecological differences between the colonial Caribbean and the social matrix of the Sign Language emergence cases (see the caveats in DeGraff 1999a: 27, 1999b: 487f; Kegl et al. 1999: 205–207; Newport 1999: 164). One major difference between Creole formation and Sign Language formation as studied in Kegl et al. and Newport concerns the extent to which the learners' linguistic input and intake were impoverished due to sociohistorical vs. cognitive factors. In the colonial Caribbean, unlike in the Sign Language cases, there was no physiological impediment restricting access to PLD from any of the languages in contact. In any case, it is methodologically useful that these two sets of developmental phenomena – namely, Sign Language creation and creolization – are not exact counterparts of each other with respect to all the relevant variables. If they were, the aforementioned instances of Sign Language creation could not count as 'natural experiments' toward elucidating the psycholinguistic processes underlying the formation of Caribbean Creoles and other instances of contact–induced diachronic phenomena: experiments are more insightful when as many confounding factors can be abstracted away as possible, and when key variables (in this case, the profile of the PLD and the age of the learners) can be experimented with in order to estimate their role and effect in distinct situations (cf. Newport 1999: 161, 164).

In the case at hand, recall that the most relevant factors to be 'experi-mented' with vis-à-vis Creole formation include the impact of nonnative

PLD and effects of age on grammar restructuring through acquisition. In this respect, the Sign Language paradigm may well provide, in Kegl et al.'s words, 'a "cleaner" language-emergence case study' (207). In both Newport's and Kegl et al.'s studies, the Sign Language grammars of the learners' models are unstable and relatively undeveloped in observable ways. Hence, the learners' PLD are (almost) exclusively composed of 'pidginized' utterances. Both studies report measurable effects of learner's age on the stability and complexity of the attained I-languages: the younger the learner, the more stable and the more complex the attained grammar (cf. Hyltenstam and Abrahamsson 2000; Newport 2002, for literature reviews on maturational constraints in language development). What is the relevance of the conclusions of Kegl et al. and Newport conclusions for the familiar Creole-genesis questions?

3.2.2. What Can Sign-language Emergence Teach Us about Creole Formation?

In the studies of Newport and Kegl et al. studies, the child learners, unlike their adult counterparts, spectacularly outperformed their models, and created stable, systematic, fully-developed and complex I-languages on the basis of strikingly impoverished PLD produced by relatively inconsistent and less developed grammars. As these authors remark, these results confirm previous hypotheses that posit a maturational gap between the restructuring capacities of children and adults. This gap seems most evident in the initial stages of acquisition with PLD that are markedly impoverished. It seems quite possible that sustained exposure to robust PLD or acquisition in formal instructional contexts may attenuate the observable effects of such a gap. If so, the Sign Language results may appear more relevant to the 'plantation phase' of Caribbean creolization than either to its 'homestead phase' or to expanded pidgins like Tok Pisin (see DeGraff 1999b: 507f, 512, 530 n17, 18, for discussion and references).

In the Caribbean colonial context, the homestead phase (or *société d'habitation*) is the early settlement period in the colony's history. During that period, the Africans-to-Europeans ratio on most of the *habitations* was still quite low, with many Africans living in relatively close quarters with French speakers. The plantation phase (or *société de plantation*) is the subsequent colonial period when the African-born population in the colony spectacularly grew in number and became the numerical majority in an intensely segregated and brutally regimented system of land exploitation. (See Chaudenson and Mufwene 2001 for further discussion of *habitation* vs. *plantation*, and Bickerton 1996: 316–319, for related observations.)

It is mostly during the plantation phase of colonial exploitation that the colonial Caribbean's linguistic ecology would be dominated by nonnative interlanguages created in (so to speak) learner-unfriendly conditions – massive slave imports, brutal social regimens that entailed physical and psychological distance between African-born learners and fluent speakers

of target varieties, various sorts of marronage (i.e., slaves' escapes from plantations to create or join free communities of fugitives), high mortality rates, etc. It is not the case that such dire conditions existed at every phase and in every locale of the colonial language-contact situation. Indeed certain groups of language learners would have been exposed, in relatively close quarters, to a preponderance of (near-)native varieties of the superstrate language. These groups included:

(i)   language learners at the early-settlement and homestead stages of the colonial-exploitation enterprise – at the onset of the language-contact situation between Europeans and Africans in the Caribbean;
(ii) the locally born (i.e., 'Creole') descendents of (near-)native speakers of superstrate varieties;
(iii) the slaves (be they African- or locally born) who had to work for extended periods in close proximity with Europeans and their descendants – so-called 'house slaves' constituted one such group;
(iv) language learners who were either free (the *affranchis*) or of mixed race (the mulattoes); many of these *affranchis* and mulattoes were 'Creole' as well (i.e., locally born).

The groups in (ii)–(iv) were in existence, in varying proportions, throughout the colonial period. Such sociolinguistic factors are obviously related to the continuum phenomena that have been extensively studied by creolists.[5]

This said, for an extended period of colonial history, especially during the plantation phase, the linguistic ecology of the Caribbean was dominated by the native substrate languages and by the nonnative interlanguages spoken by the Africans arriving en masse in the Caribbean. These Africans, many of whom were destined to work on racially segregated plantations, were then exposed as language learners to various amounts of (a continuum of nonnative approximations of) European varieties – or the colonial koinés thereof. A key question for creolists is: How would the *société de plantation*'s linguistic ecology – with its mix of, *inter alia*, interlanguages with a wide range of xenolectal features from an array of native substrate languages – allow for the emergence of relatively stable and systematic sets of, by and large, *structurally similar* Creole idiolects with similar patterns of 'structured heterogeneity' in Weinreich et al.'s (1968: 101) sense? As a side effect of the structural convergence of these I-Creoles, there emerged corresponding sets of relatively fixed and consistent communal norms – E-creoles with established conventions as part of the emerging identity of the new 'Creole' community.[6]

In the Sign Language 'experiments', only children seemed capable of attaining stable, UG-consistent, native-like and fully developed I-languages, and they attained such I-languages relatively effortlessly after a relatively short exposure to the conspicuously impoverished and unsystematic PLD provided by nonfluent older models. It has also been argued, outside of

Sign Language research, that children, unlike adults, can *routinely* and *systematically* integrate, regularize and expand the (often inconsistent) structural innovations that nonfluent adults introduce in the PLD (cf. Slobin 1977; Trudgill 1986, 2002, 2004; Chambers 1995; Kerswill 2002; Labov 2007, etc.). These conclusions seem supported by a variety of results from research in language acquisition, sociolinguistics and neurolinguistics (see the discussion and references in DeGraff 1999b; we return to this issue below in §4.2.2 and §4.3).

4. Integrating Both Second- and First-language Acquisition in Creole-formation Scenarios

4.1. CREOLE FORMATION AS CREATION OF 'SYSTÈMES OÙ TOUT SE TIENT'

Creole-formation scenarios that drastically limit the contribution of superstrate languages (e.g., Lefebvre's Relexification Hypothesis) and those that drastically limit the contribution of both superstrate and substrate languages (e.g., Bickerton's Language Bioprogram Hypothesis) are undermined by one seemingly paradoxical basic property of HC – a so-called 'prototypical' or 'radical' plantation Creole, one that both Bickerton and Lefebvre have used as a poster case to illustrate their respective (and mutually incompatible!) theoretical hypotheses. Indeed HC's lexicon, morphology and syntax are substantially, although far from exclusively, French-derived, even at the level of grammatical morphemes and affixes and with respect to a variety of morphological idiosyncrasies such as affix distribution, semantic ambiguity in certain affixes, sandhi phenomena and suppletion (this is amply documented in, e.g., Valdman 1971ff; Dejean 1980; Freeman and Laguerre 1996; Fattier 1998; DeGraff 2000, 2001a,b, 2002, 2005c; also see related comments in §§2.3.3–2.3.5). As for the basic vocabulary of HC and other Romance-based Creoles, virtually all of it is from the Romance superstrate, as documented in, for example, Fattier (1998) for HC in particular, and Guy Hazaël-Massieux (1996: 37–55) and Posner (1996: 91–96) for Romance-based Creoles in general.

Given what we know about the cognition of word segmentation and the quite abstract relationship between lexicon, morphology, syntax and semantics, the systematic correspondences between HC and its superstrate at these four levels constitute a formidable challenge to any account that postulates as principal agents of Creole formation either African adult learners with extremely reduced access to superstrate speech (as in, e.g., Lefebvre 1998, 2008) or with no intent to become fluent in (approximations of) French varieties (Baker 1990) or Creole children whose PLD were from a macaronic structureless pidgin (as in, e.g., Bickerton 1999). These correspondences also constitute a formidable challenge to linguists such as Thomason and Kaufman who categorically exclude Creoles from the scope of the Comparative Method.

According to the Relexification Hypothesis, the creators of Caribbean Creoles were African-born adults who as language learners in the Caribbean overlaid phonetic strings derived from the target European language (the superstrate qua lexifier) onto the syntax and semantics of their native languages (i.e., the African substrate languages) with the grammar of the latter transferred virtually intact into the original Creole languages.

As Bickerton's (1987: 235) correctly points out in his criticism of the Relexification Hypothesis:

> You can't abstract words from the framework you meet them in and the properties that, in consequence, they trail with them. Those properties may be sharply reduced, as in early [L2A] or pidginization, but they are always there, and you cannot just peel them off like you would the rind from an orange.

As it turns out, Bickerton's argument against the Relexification Hypothesis doubles as an argument against his own Language Bioprogram Hypothesis. Caribbean Creoles usually have always had lexica that, to a great extent, are etymologically related to those of their respective European lexifiers. For example, recall from §§2.3.3–2.3.5 that the vast majority of HC lexemes and morphology – including basic vocabulary, grammatical morphemes, affixes, and morphological idiosyncrasies of various sorts – are all derived from French, with systematic correspondences with their French etyma in sound, structure and semantics. It is thus expected that many Creole words would have 'trailed with them' some of their original grammatical 'framework' from the European language, including a substantial subset of combinatorial and semantic properties. And this is exactly what we find in, for example, HC (Hall 1950f; Valdman 1971ff; Dejean 1980; G. Hazaël-Massieux 1996; Freeman and Laguerre 1996; Fattier 1998; DeGraff 2000, 2001a, 2002, 2005c, etc.). This contradicts: (i) the Language Bioprogram Hypothesis's claim that Creoles are by and large *ab ovo* creations by children exposed to macaronic and structureless pidgin input; and (ii) the Relexi-fication Hypothesis's claim that the grammars of Caribbean Creoles are essentially derived from Niger-Congo languages, with relatively little grammatical influence from the European superstrate languages.

Substrate influence – which, like superstrate influence, is well documented (see, e.g., Aboh 2006a, forthcoming, for case studies) – is also a problem for the postulation of a 'macaronic structureless pidgin' and a 'pidgin-to-creole life-cycle' in Bickerton's Language Bioprogram Hypothesis: substrate-derived structural properties of the sort documented in, for example, Aboh (2006a, forthcoming), like their superstrate-derived counterparts of the sort documented in, for example, Fattier 1998, would not survive a structureless pidgin. In any event, both superstrate- and substrate-influenced patterns in HC were '(re)created' by the agents of HC's formation, and these patterns were integrated into native I–languages alongside 'grammatical inventions' (in the terminology of Rizzi 1999). Such patterns pose a challenge for any Creole-genesis explanation that makes

exactly one of substrate influence, superstrate influence and innovations its sole explanandum at the exclusion of the other two (see Mufwene 1986 for related remarks).

4.2. CREOLE (I.E., CARIBBEAN-BORN) CHILDREN 'CREATED' THE FIRST CARIBBEAN 'CREOLE' I-LANGUAGES

4.2.1. The First 'Creole Languages' Were the I-Languages of the First 'Creole' Children

In light of the lexical and morphosyntactic profile of HC and in light of evidence from, and comments about, early HC varieties, the following can be deduced: In colonial Haiti (then known as Saint-Domingue) and elsewhere in the colonial Caribbean, there came to be speakers whose native I-languages exhibited stable integrated systems with pervasive superstrate inheritance, with robust signs of substrate influence and with various sorts of UG-compatible innovations (for samples of early HC varieties, see, e.g., Moreau de Saint-Méry 1797; Ducœurjoly 1802; Anonymous 1804; Descourtilz 1809; Carden et al. 1991; G. Hazaël-Massieux 1996; Fattier 1998; Valdman 2000; DeGraff 2005c; M.-C. Hazaël-Massieux 2008). These locally grown varieties manifested enough innovations to appear 'new' and to deserve new labels such as 'Creole'. At the same time, these 'new' varieties manifested enough correspondences with the lexifier to be considered genetically related to French (see, e.g., Meillet 1914 [1958] contra, e.g., Schuchardt 1917; Hall 1950, 1958; Weinreich 1958; Goodman 1964; Valdman 1971ff; Posner 1985, 1996; G. Hazaël-Massieux 1996; Fattier 1998; Chaudenson and Mufwene 2001: 94–129).

The first substantial group of fluent speakers of such early Saint-Dominguois/HC varieties – which were characterized by systematic correspondences with French at all levels of grammar – could not have been any subset of the African expatriates who, as adults, had been transported en masse to work as field slaves in Saint-Domingue's *société de plantation* at the peak of the colony's land exploitation (see note 5 for references). By then, these African-born field slaves – the so-called Bozals – could only have the narrowest access to French(-like) varieties and Creole(-like) approximations thereof. Given their limited exposure to such superstrate(-derived) varieties, they could not, *on their own*, innovate a full-fledged 'Creole' grammar with systematic structural correspondences with French, including an overwhelming majority of French-derived morphemes, down to French-derived affixes, French-derived grammatical morphemes and French-derived morphosyntatic idiosyncrasies. Besides, it's been estimated that as many as half of the newly arrived Africans would die within their first 3 years in the colony (see Debien 1971: 83f, 343–347; H. Trouillot 1980: 53, 57; Chaudenson and Mufwene 2001: 92). Presumably, those slaves would die without ever achieving anything like native fluency in any local variety, be it 'Creole' or not.

Taken together, these facts suggest that the basic morphosyntactic profile of HC, with the similarities noted above vis-à-vis its source languages, was established, not by field slaves on large-scale and radically segregated plantations, but by small-farm ('homestead') dwellers – before and after the 'sugar boom' shift to the large-scale system – and by other Saint-Dominguois who, like the homestead dwellers, often had relatively direct and intimate contact with speakers of French varieties, as well as with speakers of a continuum of (proto-)Creole varieties with varying degrees and various sorts of substrate influence (see the demographic categories (i)–(iv) in §3.2.2). These 'other Saint-Dominguois' included the free people of color (the *affranchis*) and the slaves outside the fields (the nonfarming slaves); see, for example, H. Trouillot (1955), Debien (1971: 85–133, 369–391) and Chaudenson and Mufwene (2001: 94–129). These relatively privileged Saint-Dominguois were also the ones most exposed to the acrolectal contact varieties that would have existed from the onset of creolization onward – the varieties with the most inheritance from native French varieties at all levels of grammar (phonology, bound and grammatical morphemes, basic vocabulary, lexicon, syntax, etc.). Many of these *affranchis* and nonfarming slaves were locally born, even though they would still be exposed, in various degrees, to the African ancestral languages throughout the colonial period (see Fouchard 1972: 436; H. Trouillot 1980: 55, 64–70; Warner-Lewis 2003; contra Chaudenson and Mufwene 2001: 91–93, 309).

The 'Creoles' (i.e., the Caribbean-born) among the *affranchis* and among the nonfarming slaves were the ones in the best circumstances to create or acquire the emergent Creole as their native I-language with stable parameter settings *deduced directly* via UG, that is, without influence from any prior native languages. In turn, the new communal norms corresponding to these locally created I-languages would constitute one important symbol of the locally born Saint-Dominguois' relatively prestigious Creole identity, which would help set them apart from the stigmatized Bozals at the bottom of the colonial socioeconomic hierarchy (see §2.2.3). In this respect at least, it seems fitting that the label 'Creole', qua language of the 'Creole' people, was derived from the ethnographic label of the locally born (i.e., those who generally were *native* speakers of this structurally innovative set of I-languages that would then become known as 'Creole' speech).

4.2.2. Creole Children's Role in the Homogenization of Colonial (Proto-)Creole Dialects

With the foregoing in mind, let us reexamine the issue of homogenization in the case of HC's development, from a variety of substrate-influenced interlanguages – with ethnically based structural variations – to a relatively uniform set of 'Creole' norms.

Substratist accounts predict, correctly I think, the existence, in the pre-homogenization period, of a complex array of proto-Creole nonnative

interlanguages influenced by a variety of substrate languages – Fongbe, Ewe, Akan, Gã, Gur, Efik, Ibibio, Igbo, Yoruba, Bamana/Malinke, Fula, Kikongo, etc. In this pre–levelling stage, there could *not* exist one single Creole variety, or a small set of Creole varieties, *with relatively homogeneous morphosyntactic profile(s)*.

In the Principles-and-Parameters framework, slight differentials in underlying parameter settings can have wide-ranging observable surface-level effects (Chomsky 1986: 151f; also see Kayne's (2000) comparative 'micro-parametric' work on, *inter alia*, dialects of French and Italian). Even in the presence of relatively similar substrate languages (among, say, the Gbe languages of the Kwa group), each distinct substrate language in the pre-levelling period would be related to a nonnative 'early Creole' variety – or an array thereof – with its own distinct grammar, as influenced by the parametric settings of the respective substrate qua L1 in L2A (see, e.g., Mufwene 1986: 138 for a sample of structural differences among the Kwa languages). Parametric differences would be even greater across language groups such as Kwa and Bantu. Compare, say, massive incorporation in Bantu and lack thereof in Kwa (see Singler 1996: 224f; and DeGraff 1999b: 501ff, for related comments). The typological mix of early Creole varieties would thus be radically different from the set of relatively uniform, though certainly not identical, grammars that characterize speakers of contemporary HC, across dialectal lines. Indeed, despite dialectal variations that reflect social–class and regional distinctions (see Fattier 1998 for extensive documentation), HC idiolects, which together provide the norms of Haiti's only national (E–)language, form a relatively stable and relatively homogeneous set of I–languages across virtually the entire population of Haiti (such relative homogeneity is also noted by Fattier on, e.g., p. XVII).

Given the inevitably unstable and divergent interlanguages – with ethnically marked L1-influenced structural variations – in the early stages of L2A at the onset of creolization, some 'dialect levelling' qua subsequent homogenization process must have played a central role past the onset stage of creolization. So who were the agents responsible for the levelling/ homogenization of (proto–)Creole varieties? In other words, who, in the colonial Caribbean, were the first learners to create or acquire their I-languages as part of increasingly larger communal sets of relatively homogeneous 'Creole' idiolects?

The creation of relatively homogeneous idiolects in language- or dialect-contact situations is one domain where younger learners are known to have the upper hand as compared to older learners. This is routinely observed in the case of immigrants' locally born children who eventually create, or converge on, some local, and sometimes koineized, target varieties, and systematically eschew dialectally or ethnically marked features of their parents' nonnative approximations of said local varieties (see, e.g., Trudgill 1986, 2002, 2004; Chambers 1995; Kerswill 2002; Labov 2007, and the discussion and references in DeGraff 1999b: 488f, 494f, 506, etc.).

As Trudgill (2004: 34) puts it, 'in the context of families moving from one dialect area to another, the phenomenon of total childhood accommodation to the new dialect is the object of so much and such widespread observation and comment on the part of nonlinguists that it does not really need scientific confirmation.' In this case, the nonlinguists' observations have gotten scientific confirmation via the careful measurements of the sociolinguists cited in this paragraph (see the recent survey in Labov 2007).

Measurable instances of homogenization by children have also been reported in the aforementioned studies of Sign Language acquisition from inconsistent PLD (see §3.2 above). In the Newport study, the PLD produced by the late-signing parents were often inconsistent in nonsystematic ways. Newport (1999: 168ff) argues that child acquirers whose PLD show unsystematic variability do not replicate such variability; instead the child learners pick 'predominant' patterns in the input and generalize them on the basis of productive rules, making their own output fully systematic. The child learner thus uses markedly sparse and inconsistent input to create 'a more deterministic rule system'.

Confirming Newport's result, Kegl et al. (1999) contrast the following in the history of Sign-Language emergence in Nicaragua:

(i)   the idiosyncratic and highly variable homesigns (*mimicas*) of previously isolated signers;
(ii) the relatively unstable 'pidgins' (*Lenguage de Signos Nicaragüense* – LSN) which were created by the homesigners in contact with one another; and
(iii) the stable Sign Language 'Creole' (*Idioma de Signos Nicaragüense* – ISN) that was created by the children immersed at a young age in the Deaf communities originally created by former homesigners.

(See DeGraff 1999b for further discussion; also see Hyltenstam and Abrahamsson 2000 and Newport 2002 and the references therein on the debate regarding critical periods in language development.)

Many sociolinguistic treatments of language variation and change have similarly argued that younger learners are essential to the homogenization of dialects in contact (see aforementioned work by Trudgill, Chambers, Kerswill, and Labov on the differential effects of L1A and L2A in dialect contact and koineization, and the discussion and additional references in DeGraff 1999b: 488f, 494f, 506, etc.). In Siegel's (2003) discussion of dialect-levelling in the diachrony of Hawai'ian Creole, 'it was the children who rapidly regularised these innovations [i.e., the results of language transfer via L2A by substrate-speaking adults] and integrated them into the grammar of the newly emerged creole.' After all, locally born children in these environments – unlike those who entered the contact situation as adults with somewhat restricted access to the target – would acquire the local (proto–)Creole varieties without any massive transfer from an array of mutually distinct prior native languages.

Such instances of L1A – in the midst of language contact – do not exclude the possibility that, alongside the Creole, some of the ancestor languages, at least for some period, would have been concurrently acquired by some of the Creole children. Such childhood bilingualism may have been another conduit for substrate influence – via contact between I-languages within individual bilingual minds. However, language transfer in child bilingualism seems much more limited in scope than language transfer in L2A by adults: the few available comparative studies suggest that children's bilingual acquisition follows a 'highly similar' course as children's monolingual acquisition (de Houwer 1995: 240–244, 248; Meisel 2008: 56). But there do exist instances of child bilingualism where transfer is more likely to happen, namely when one of the languages is weaker or acquired later than the other (Genesee and Nicoladis 2007; Paradis 2007; Ionin 2008), which must have been the case in the rapidly changing linguistic ecology of the colonial Caribbean. This would have happened, for example, when some Caribbean-born children of enslaved Africans would be first exposed to their ancestral language(s), and then, with some delay, to Creole varieties which they would acquire as L2s. The picture from the bilingual-acquisition literature is, not unexpectedly, quite complex, with many theoretical and empirical controversies (see, e.g., the papers in Haznedar and Gavruseva 2008 for a sample of contemporary case studies and competing analyses). Besides, in the colonial Caribbean cases of bilingual acquisition, we simply do not know what subsets of the languages in contact and how much input from each language were available, in what order, to these bilingual Creole children at various stages in creolization.

Be that as it may, eventually the Africans' Caribbean-born descendents – perhaps the most socially mobile sector among the colony's people of color (see §2.2.3) – reached a stage when they had relatively little use for the ancestral African languages, whose socioeconomic utility and social capital would have rapidly decreased vis-à-vis the superstrate language and the local Creole. In any given colony, the latter was the only native language that the Caribbean-born would *all* have in common across colonial space and that would distinguish them from their stigmatized Bozal ancestors and counterparts. The widespread stigmatization of Bozal speech would have been yet another catalyst for the suppression of substrate-specific xenolectal features by the locally born and whoever could try to imitate them (see §2.2.3). This scenario illustrates one way in which group-level sociolinguistic factors, which take scope over populations and communities within, interact with the cognitive and ecological factors that are relevant to individual learners (e.g., the characteristics of L1A vs. L2A and the corresponding PLDs) and how such interaction influences the developmental course of both I- and E-languages (see Trudgill 1986, 2002, 2004; Chambers 1995; Lightfoot 1999, 2007; Mufwene 2001ff; Kerswill 2002; Sankoff 2002, 2004, and Labov 2007 for surveys of such interaction between individual- and group-level processes in language- and dialect-contact situations).

4.2.3. Creole Children's Innovations

In addition to, or perhaps as a factor in, their contributions to dialect levelling, children may also play a central role in grammatical innovation(-cum-expansion) in areas of the grammar for which the relevant triggers in the PLD are somewhat sparse. In the studies of both Newport and Kegl et al., the PLD provided by nonfluent late-signers are somewhat lacking in certain sorts of productive combinatorial morphosyntax (e.g., in the functional layers associated with Tense-Mood-Aspect and with nominal projections) whereas the native patterns of the young learners do not lack such combinatorial capacity. This is clearly illustrated in Newport's case study of the young native signer Simon whose parents – his language models – are nonfluent late-signers:

> '[W]hen the context required a combination of two morphemes, [Simon's] parents virtually never produced such combinations. . . . Simon did not acquire the exclusionary pattern of his parents, but rather freely combined two inflections.' (Newport 1999: 169–70)

Newport's observations find parallels in Kegl et al.'s comparison of late-acquired LSN (see (ii) in §4.2.2) to natively acquired ISN (see (iii) in §4.2.2): the latter shows an increase of, *inter alia*, morphological marking of person agreement, locative marking, and object classification (e.g., size and shape classifiers).

Certain developmental patterns in Creole formation offer intriguing parallels with the above Sign Language patterns vis-à-vis the emergence of combinatorial morphosyntax via L1A. Some of the most detailed archival data on the early diachrony of any Creole language come from Hawai'i in the form of '[c]ourt records, newspapers, personal and business correspondence, popular books, school literary journals, and Creole plays . . . encompass[ing] Hawai'i's entire modern era [1778 to the present]' (S. J. Roberts 1999: 46; also see S. J. Roberts 1998); but see the caveat in §3.1 above. S. J. Roberts (1999: 65) concludes her study of the diachrony of the Hawaiian Tense-Mood-Aspect system with the following suggestion: 'the integrated [Tense-Mood-Aspect] system was a native-born innovation, although it built on the developments of earlier nonnative speakers'. One watershed feature of the nativized Hawaiian Tense-Mood-Aspect system is that certain complex Tense-Mood-Aspect combinations in the archival data are only found in native speech (S. J. Roberts 1999: 60).

S. J. Roberts's observation is reminiscent of Newport's and Kegl et al's conclusions vis-à-vis young native signers' grammatical expansion based on the markedly restricted input that is provided by nonfluent late learners. In these particular instances of Sign Language and Creole formation, younger learners appear to create productive structural combinations that are not manifested in the PLD provided by their older models. This is not surprising given the ubiquity of 'grammatical inventions' in run-of-the-mill instances of L1A (Rizzi 1999; Crain and Pietroski 2002; Wexler 2003; Crain et al. 2006, etc.).

4.3. BOTH ADULTS AND CHILDREN 'CREATED' CARIBBEAN CREOLES, EACH IN THEIR OWN WAY: AN L2A–L1A CASCADE

Who were the 'central' agents of Creole formation? In substratist accounts like Lefebvre's Relexification Hypothesis, Creole-formation agents were mainly the African-born adults, who basically kept their native grammars intact while replacing their native lexica's phonetic shapes with strings derived from superstrate speech. In 'universalist' accounts like Bickerton's Language Bioprogram Hypothesis, Creole-formation agents were mainly the Caribbean-born (i.e., 'Creole') children, who basically created their I-languages virtually *ab ovo* (i.e., from the scratch of a macaronic and structureless pidgin), thus with little grammatical influence from any of the languages in contact. Such exclusionary accounts systematically and erroneously gloss over interesting interactions and differences between L2A and L1A and the implications of such interactions and differences for Creole formation.

In my own recent work, I have revisited the interaction between L2A and L1A in creolization. Putting old wine in new bottles, I have dubbed this interaction an L2A–L1A 'cascade' relationship whereby L2A and L1A play *indispensable, distinct and complementary* role at various stages of creolization, with the substrate-influenced output of L2A playing a key role in defining the PLD in subsequent L1A. The latter is the process whereby Creole I-languages were created directly from UG, with the resulting parameter-settings based mainly on triggers in the PLD, without massive influence from any prior substrate idiolect in the acquirers' minds. This does not exclude the possibility of bilingual acquisition and language transfer therein, in addition to language transfer via the substrate-influenced patterns in the PLD to native Creole speakers (see §4.2.2). Note that this L2A–L1A cascade relationship naturally allows both L1A and L2A to contribute to Creole development, each in its own principled way. This 'cascade' scenario is thus fully compatible with the possibility of L2A effects (e.g., simplification) in Creole formation and of both superstrate and substrate influence in various components of Caribbean Creoles' grammars, subject to UG constraints (cf. Mufwene 1986).

Many creolists take it as established – and correctly so, it seems to me – that substrate influence via L1 transfer in both L2A and substrate-creole bilingualism did play a role in the development of Caribbean Creoles (see the discussions, surveys and references in DeGraff 1999b; Mufwene 2001ff; Siegel 2003, 2006; Winford 2003, and the references and caveats in note 4). It appears incontrovertible that L1 transfer in L2A has played a pervasive role in many instances of language creation and change, beyond creolization per se – and perhaps in *all* instances of language change if we include instances of dialect and idiolect contact in the relevant class of 'language contact' phenomena and if we consider the influence of late-acquired dialects on the PLD in L1A as yet another important factor in language change (see, e.g., Trudgill 1986, 2004; Chambers

1995; Kerswill 2002; Labov 2007 for the influence of late-acquired dialects in various cases of language change, including koineization, new-dialect formation, etc.). It thus seems reasonable to include 'language contact', in this fine-grained sense, as a contributing factor vis-à-vis all diachronic phenomena. Such contribution a fortiori includes the influence of substrate languages in Creole formation, all the more so if we assume, in Uniformitarian fashion, that Africans in the Caribbean context of Creole formation are cognitively on a par with language learners everywhere else. It thus seems likely that some sort of L1 transfer on the part of Niger-Congo speakers in the colonial Caribbean was instrumental in 'creating' certain, but by no means all, aspects of Caribbean Creoles (see Aboh 2006a,b, forthcoming for recent case studies).

Yet, by hypothesis, it is only the locally born (the so-called 'Creole') children that could have been responsible for Creole genesis (e.g., in the Caribbean) in the well-defined and narrowly technical sense of 'Creole genesis' as the creation of (native) 'Creole' I-languages. These Creole children would have been in the best position to 'inherit', modify and integrate, via their PLD, both superstrate- and substrate-derived structural properties, along with structural innovations from prior instances of both L1A and L2A, into their own native Creole varieties. Because of their relative prestige as native speakers of Creole varieties with higher social capital, these Creole children, once turned adults, would have also been in the best position to subsequently influence the spread of (further approximations of) said 'inheritance', along with the accumulated grammatical innovations, in the developing Creole community and its expanding E-language and the norms thereof (see §2.2.3 above and the references therein and in note 5).

If Creole children 'create' Creole languages, qua relatively stable and homogeneous I-languages, it is the interlanguages of substrate speakers, qua adult learners of superstrate(-like) and Creole(-like) varieties, that would have been the major conduit into the incipient Creole for substrate-derived patterns and L2A-based innovations. Among the latter stand out various sorts of morphological simplification as in the aforementioned case of inflectional erosion in the verbal domain, and as documented outside of creolization per se (see §2.2.2). There is thus a well-defined and narrow sense in which adult speakers of the substrate languages did 'create' the basis for certain patterns in the Creole, namely via various sorts of structural innovations in their interlanguages. Some of these innovations would have reflected structural patterns from the learners' native languages while others would have reflected general L2A strategies (see note 4 for references and caveats).

Some of the best known cases of substrate influence in Caribbean Creoles include serial verb constructions, nonverbal copula-less predication in matrix clauses, clefts with predicate copying (data in §4.4.1), and so on. These constructions are found both in the Kwa languages and in Caribbean Creoles, but (apparently) not in the relevant European

languages. Yet the available evidence (e.g., the lexical and morphosyntactic profiles of HC and HC's pervasive correspondences with French at all levels of grammar) suggests, contrary to Lefebvre (1998), that substrate grammars did not have an overriding all-exclusive influence in the development of the African adult learners' nonnative interlanguages and in the development of the native Creole languages of the subsequent generations of locally born speakers. The overriding substrate influence posited by Lefebvre is unexpected in any Uniformitarian framework whereby the African-born and the locally born (i.e., the 'Creole') learners in the colonial Caribbean were cognitively on a par with other language learners, especially considering the exposure to superstrate data that these learners did have in various temporal and spatial regions of Caribbean history and geography (see, e.g., (i)–(iv) in §3.2.2). Given, say, early and contemporary Saint-Dominguois/Haitian Creole's lexical and morphosyntactic profile, these learners did manage to go beyond the sort of L1 influence and simplification patterns that are most pronounced in the very early stages of L2A. There is also first-hand colonial reports whereby Africans in Saint-Domingue were deemed to be so fluent in French that they could teach the language to some of the French-born illiterate patois speakers who came to the Saint-Domingue from far-flung provinces of France as indentured servants (Chaudenson and Mufwene 2001: 66f, 80f, 90f, 111). The available sociohistorical and linguistic evidence thus contradicts any hypothesis whereby Caribbean Creoles are the results of some crystallization of early-L2A interlanguages. This issue is elaborated in the very next section. (DeGraff 2002: 374–394, 2005c, provides additional sociohistorical and linguistic details in support of the HC-formation scenario that is sketched in this section.)

4.4. CREOLES AS 'CONVENTIONALIZED INTERLANGUAGES OF AN EARLY STAGE'?

Despite the data and observations mentioned above and in previous work, it has been recently argued, again, that 'Creoles are conventionalized interlanguages of an early stage' (Plag 2008a,b; see Pelleprat 1655 for one of the earliest approaches of this general sort). In this hypothesis Creoles stand out as languages in arrested development, with Creole creators by and large remaining stuck in some early stage of L2A. This hypothesis, though popular, has hardly any basis in sociohistory or in language-acquisition research: in light of early Creole texts and given what we have learned from colonial Caribbean history, language acquisition and sociolinguistics, it seems reasonable to assume that, at every single historical stage *from the onset of creolization onward*, there always existed a continuum of L2A-based lects – from basilectal to acrolectal – with, in turn, a continuum of European-derived and Caribbean-grown varieties as L2 targets (Moreau de Saint-Méry 1797; Alleyne 1971; Baker 1982; Lalla and D'Costa 1989; G. Hazaël-Massieux 1996; Bickerton 1996; Baker and Bruyn 1999;

Chaudenson and Mufwene 2001; Mufwene 2001ff; M.-C. Hazaël–Massieux 2008, etc.).

Since Plag's is the most recently (re)formulated exceptionalist and a-historical hypothesis about Creole formation, let's spend the last stretch of this essay evaluating its (de)merits.

4.4.1. 'Interphrasal Information Exchange' in Contemporary Creoles

Let us first consider the claim that Creoles almost entirely lack 'contextual inflection' (i.e., overt inflectional morphology that is 'dictated by the syntax') due to the lack of 'interphrasal information exchange' in the processing capacities of the creators of HC qua speakers of early interlanguages in L2A. For example, 'the instantiation of agreement procedures or structural case assignment requires the most advanced processing procedures and occurs therefore only at later stages' (Plag 2008a: 124). But such a constraint throws the syntax baby with the morphology bath water. Interphrasal information exchange may be a necessary condition for contextual inflectional morphology, but it is certainly not a *sufficient* condition for the appearance of such overt morphology. Therefore, the lack of, say, overt agreement morphology and morphological case cannot be taken as the unambiguous reflex of a lack of 'interphrasal information exchange'.

This said, we do find a variety of Creole languages that exhibit instances of *overt* inflectional morphology that are syntactically conditioned, thus falling in the class of 'contextual inflection'. In my native HC, there is a class of verbs with short and long forms where the short-vs.-long verb-form alternation has syntactic correlates: in one case (e.g., with *gen* vs. *genyen* 'to have') the short form cannot be used when the object is a pronoun or the trace of a *wh*-phrase); and in another case (e.g., with *fè* 'to make' vs. *fèt* 'to be made'), the long form is used as a passive whereas the short form occurs in the active (for details and data, see DeGraff 2001a: 74f, 2007: 102,112; also see DeGraff 2001b: 294–299 for related discussion). There is also a phonological discourse-related correlate to the HC's short-vs.-long alternation: the long form is required when the verb is emphasized. In Mauritian Creole, the short-vs.-long verb-form alternation depends on whether the post-verbal phrase is an argument or an adjunct. There the generalization *grosso modo* is that the short form surfaces only when the verb is followed by an overt argument and that the long form surfaces elsewhere – for example, when the verb is followed by a *wh*-trace or an adjunct (Syea 1992). Moreover, instances of the Mauritian Creole long forms have been recently documented in Verum Focus constructions, which 'emphasize the truth or falseness of the proposition expressed by the sentence' (Henri and Abeillé 2008).

Independently of one's precise analysis, these facts suggests that the inflectional suffixes that characterize the long forms in HC and Mauritian Creole are sensitive to both the *syntactic context* of their occurrence, including the syntactic status of surrounding elements (e.g., *wh*-traces vs. NP-traces vs.

overt NPs, arguments vs. adjuncts), and the *information structure* of the utterance (e.g., emphasis, Verum Focus). Henri and Abeillé's (2008) provide a clear summary of the syntactic constraints on the Mauritian Creole short-vs.-long morphological alternation, which affects nearly 70% of verbs: 'As far as syntactic constraints are concerned, verbal alternation is but another example of verb sensitivity to argument realization'; furthermore, '[short forms] and [long forms] of Mauritian verbs are clearly syntactically driven and encode argument realization sensitivity on the verb'.

Yet, about the Mauritian Creole short-vs.-long verb alternation, Plag (2008a: 126) writes: 'It is not entirely clear, though, whether these types of morphology are indeed of the contextual kind. In all these cases, the inflection marks the kind of construction the verb is part of.' But it seems to me that one of the entailments of the proposition in the second sentence of this quote is that the sort of morphology that is at stake here (e.g., the short-vs.-long alternation) is of the 'contextual kind', with 'context' including *inter alia* the syntactic environment (e.g., 'the kind of construction the verb is part of'). Indeed the latter provides some of conditions that 'dictate' the appearance of the relevant morphology on the verb (cf. Veenstra 2007).[7]

Now, focusing on HC alone, it manifests many other syntactic phenomena with characteristics that must have emerged from the processing of interphrasal information exchange by the creators of HC, and these are characteristics that are found throughout natural languages, including 'Creole languages' under any definition. Here's a small sample (also see the data in §4.4.3):

(i) <u>Subcategorization and selectional information:</u> As in other languages, HC verbs impose subcategorization and selectional restrictions on their complements. One such verb is *konte* 'to rely' which selects for the preposition *sou* 'on' as in (4):

(4) *Mwen   konte   ★(sou)   wou*
     I   count   on   you
     'I rely on you'

Other examples of selectional restrictions are found in the well-documented domain of Caribbean Creoles' serial verb constructions where each verb in the series restricts the range of the other verbs (see, e.g., Veenstra 1996).

(ii) <u>Clausal complementation:</u> Alongside the pattern in (4), clausal embeddings of various sorts are among the kinds of information that HC grammar must specify vis-à-vis the subcategorization and selectional information of certain verbs. For example, verbs like *kwè* 'to believe', among many others, can take tensed clausal complements, as in (5) where 'ANT' stands for 'anterior tense', whereas verbs like *vle*

'want' can take either tensed or untensed complements, as in (6) (see Sterlin 1989; also see §4.4.3 below for the pronominal co-reference patterns that obtain in the tensed vs. untensed complements and implications thereof for the Creoles-as-early-interlanguages hypothesis):

(5) *Jan   kwè   Mari   te   di   li   te   konte   sou   Bouki*
     John believe Mary ANT say 3sg ANT count on Bouki
     'John believes that Mary had said that (s)he counted on Bouki?'

(6) a. *Boukinèt   te   vle   pou   Bouki   te   genyen*
         Boukinèt ANT want for   Bouki ANT win
         'Boukinèt wanted for Bouki to have won'
     b. *Boukinèt   te   vle   Bouki   (★te)   genyen*
         Boukinèt ANT want 3sg   ANT   come
         'Boukinèt wanted for Bouki to win'

(iii) <u>Cross-clausal dependencies</u>: HC shows typical 'long-distance' filler-gap dependencies, as in *wh*-movement constructions where grammatical information, including the subcategorization and selectional information of the sort exemplified in (4)–(6), must be 'exchanged' *across clauses* in order to associate the lowest trace *t* of the moved *wh*-phrase (in the lowest embedded clause) with the overt *wh*-phrase (in the matrix clause). Consider the example in (7). There information must be exchanged between <u>sou ki moun</u> 'on which person' in clause-initial position and the trace *t* in sentence-final position.

(7) <u>Sou ki   moun</u>   Jan   kwè   Mari   te   di   li   te   konte   t ?
     On which person John believe Mary ANT say 3sg ANT count
     'On whom does John believe that Mary had said that (s)he counted?'

(iv) <u>Subject-object asymmetries</u>: In HC, *wh*-movement patterns manifest distinctions that seem parasitic on 'information exchange' between the position of the *wh*-trace and the area (around) where the *wh*-phrase is pronounced. To wit, the presence vs. absence of *ki* in (8) in a position following the *wh*-phrase. In this contrasting pair, the morpheme *ki*, which is a-theoretically glossed as 'WH', surfaces in a position following the *wh*-phrase when, and only when, the *wh*-phrase is extracted from the subject position.

(8) a. *Ki*   *moun*   ★*(ki)*   *renmen*   *Mari*   *?*
         Which   person WH   love   Mary
         'Who loves Mary?'
     b. *Ki*   *moun*   (★*ki*)   *Mari*   *renmen*   *?*
         Which   person WH   Mary   love
         'Who does Mary love?'

(v) <u>Grammatical phenomena in subordinate clauses that are triggered by the choice of the subordinating verb:</u> The possibility of 'long-distance' *wh*-movement depends on certain properties of the verb that selects the clause that contains the *wh*-trace. This is a clear example of a grammatical constraint that relies on information exchanges across clauses (i.e., between the embedding and embedded clauses). For example the long-distance movement in (7) is not possible if one of the verbs that intervene between the trace and its antecedent is *chichote* 'whisper' (a nonbridge verb) instead of *di* 'say' (a bridge verb), as discussed in Koopman (1982: 216). To wit, the ungrammaticality of (9) as opposed to the grammaticality of (7):

(9) ★ *Sou <u>ki</u> <u>moun</u> Jan kwè Mari te chichote li te konte <u>t</u> ?*
'On whom did John believe that Mary had whispered that (s)he counted?'

(vi) <u>One more example of a grammatical constraint that applies across (embedding and embedded) clauses:</u> As documented in Piou (1982: 131), the constraint in (v), which bans *wh*-movement across nonbridge verbs, also applies to the predicate-copying construction where the predicate head (e.g., *bo* 'to kiss' in (10)) is pronounced both in a focus position toward the beginning of the clause and in situ in a clause-internal position between subject and object. (In (10), *se* is a-theoretically glossed as 'FOC' for 'focus marker'.)

(10) *Se   bo   Jan   kwè   Mari   te   di/★<u>chichote</u>   li   te   bo   Mari*
    FOC kiss John believe Mary ANT say/whisper 3sg ANT kiss Mary
    'It's kiss John believes that Mary had said/★whispered that he/she kissed Mary?

4.4.2. 'Interphrasal Information Exchange' in the Early Stages of Haitian Creole and in Other Creole languages

Here is one of Plag's posited characteristics for 'very advanced' interlanguage: 'question formation involving inversion, which would correspond to a very advanced [L2A] stage, seems not attested in creole languages' (2008b: 321). What Plag fails to note is that clausal embedding, which is another of his hallmarks for 'very advanced interlanguage', is pervasive among Caribbean Creoles, alongside (long-distance) predicate clefts with the copying of the predicate head across clauses. Clausal embedding also seems to exist in other languages that have been called 'Creoles' outside the Caribbean, and these other 'Creole' languages also show predicate clefts with predicate copies distributed across clausal boundaries (see Holm and Patrick 2007 for some of the relevant data). Yet, according to Plag's hypothesis such embedding is possible only in the very last stage of L2A, the one where we find the so-called 'S'-procedure [with] Embedded S'

(2008a: 123f). The productivity of clausal recursion and interclausal information exchange in such a wide variety of Creole languages further undermines the 'Creoles as early interlanguages' hypothesis.

Furthermore, the facts in (5)–(10), and also in (11) below, all involve what may well be one basic property of Human Language, namely recursion, and the facts in (4)–(11) certainly involve fundamental characteristics of HC grammar such as subcategorization, selection, clausal embedding, (long-distance) *wh*-movement, (long-distance) predicate clefts, co-reference properties of pronouns, etc. These or related characteristics, which crucially depend on the capacity for interphrasal information exchange, are found in *all* Caribbean Creoles.

Furthermore, it seems incompatible with Plag's hypothesis that most of the above characteristics are also instantiated in the earliest (proto-)Creole texts available to us such as those in Moreau de Saint-Méry (1797), Ducœurjoly (1802), Anonymous (1804), Descourtilz (1809), Arends (1989–1995), Carden et al. (1991), G. Hazaël-Massieux (1996), Baker and Bruyn (1999), Valdman (2000), and M.-C. Hazaël-Massieux (2008), keeping in mind the usual caution regarding archival records in general and the extra caution that is particularly warranted for archival records from the colonial Caribbean (see, e.g., Guy and Marie-Christine Hazaël-Massieux's and Baker and Winer's words of caution and §3.1 above). Among our earliest available texts is the one in Carden et al. (1991), which consists of court depositions dated 1671 from Martinique. This very early sample of Martinican Creole already contains features that are found in contemporary Caribbean French-based Creoles. The 1671 Creole text exhibits instances of subcategorization and selectional restrictions, clausal complementation, purposive clauses, relative clauses, etc., all of which necessitate interphrasal information exchange so they can obey grammatical constraints that apply across phrasal domains.

As for the predicate-copying pattern in (10), it is arguably the result of transfer from the Kwa substrate, so it must have entered the Creole relatively early, at the time of language contact between the substrate and superstrate languages.

Therefore, the facts in (4)–(10) above, and also in §4.4.3 below, all of which involve interphrasal information exchange, cannot be explained away by claiming that they are all 'later developments' in HC's history (cf. Plag 2008a: 126). Actually such a claim would entail that the earliest HC varieties lacked subcategorization and selectional restrictions, clausal embedding, long-distance *wh*-movement, etc. Not only there's no evidence for such a drastic qualitative gap in the history of HC, but also this would entail that the earliest HC varieties, very much unlike contemporary HC, lack basic features of Human Language. This is certainly an anti-Uniformitarian claim that needs substantial evidence as support – evidence that has yet to be provided.

In any case, the observation that some of the Caribbean Creoles' cleft constructions and related grammatical constraints – all of which involved

clausal embeddings – emerged under the influence of transfer from the Kwa substrates directly contradicts the claim that these Creoles are crystallized version of early interlanguages. According to the logic of Plag's own assumptions regarding Processability Theory in L2A, the Kwa substrates could not have influenced Caribbean Creole cleft constructions if the Kwa speakers qua Creole creators were still in early L2A stages during creolization. According to the 'Developmentally Moderated Transfer Hypothesis' as described by Plag:

> L1 transfer will not occur across the board, but when the structure to be transferred is processable within the developing L2 system. That is, the interlanguage processor must have the very procedure at its disposal that is required for the processing of the L1 structure to be transferred. (2008b: 317)

In this light, consider again the fact that the aforementioned constructions with clausal embeddings and cross-clausal grammatical constraints (e.g., long-distance predicate clefts) were influenced by substrate languages qua L1s. Such cases of substrate influence suggest that the Creole creators who were so influenced must have been in the most advanced L2A stages – the stages whose hallmarks include 'fully-developed S[entence]-procedures' and 'sentence embedding' procedures. Indeed, according to the Developmentally Moderated Transfer Hypothesis, the corresponding L1's constructions (e.g., long-distance predicate clefts and grammatical constraints therein, all of which presuppose 'S-procedures' and 'sentence embedding' procedures) would have had no effect on early interlanguages since the latter lacked such procedures. Thus, the cleft patterns could not have been transferred into the then-emerging Creole if the Creole creators at that time were still at some early interlanguage stage. (Incidentally, substrate influence via L2A in the emergence of predicate clefts in Caribbean Creoles is one more reason to reject any facile claim that adult L2A, thus language contact, always entails *global* simplification of the resulting grammars. Cf. note 8 below.)

Plag (2008a: 127) entertains the possibility that, in light of HC's morphological profile (with some contextual inflection and many French-derived affixes), Creole formation in the case of HC may represent 'more advanced stages of [L2A]'. But if this possibility holds for HC – a language that has often been claimed to be a 'prototypical'/'radical'/ 'abrupt'/'true' Creole on sociohistorical or structural grounds (Bickerton 1984; Thomason and Kaufman 1988; Lefebvre 1998; Singler 1996; M.-R. Trouillot 2002, etc.) – then more advanced stages of L2A must have also played a formative role in the formation of other Caribbean Creoles. As it turns out, all Caribbean Creoles, as far as I know, show similar hallmarks of interphrasal information exchange of the sort illustrated in (4)–(10) above, including information exchange across multiple clauses as in long-distance *wh*-movement and predicate clefts (see, e.g., Holm and Patrick 2007). Besides, it can be reasonably argued, on both empirical and

sociohistorical grounds, that HC's basic morphosyntactic profile, including the French-derived affixes that Plag considers as symptoms of more advanced stages of L2A, was established, not 'well after the creolization period' (contrary to Plag 2008a: 126), but as part of the creation of the earliest Creole idiolects (DeGraff 2001b: 229–232, 291–294).

Plag (2008a: 115) carefully notes that his hypothesis may not be 'entirely true' and that 'there are a number of creole properties that cannot be explained under this hypothesis.' What the ongoing observations suggest is that the grammars of Caribbean Creoles, on a par with natively acquired human languages elsewhere, are *fundamentally* unlike the putative early interlanguages described in Plag (2008a,b).

4.4.3. The Argumentation for the 'Creoles as Early Interlanguages' Hypothesis Is Empirically and Theoretically Inconsistent

Plag's hypothesis is theoretically problematic as well – perhaps, even self-contradictory. For example, consider the following claim:

> The argumentation [in Lefebvre 1998: 273–275 and Veenstra 1996: Ch. 3] for the assumption of exceptional case marking in these two languages [Saramaccan and HC] in spite of the absence of overt case marking follows theory-internally from an analysis of certain types of complements as nonfinite. The analyses rest however exclusively on syntactic phenomena (scope and referential properties, passivization facts etc.) and not on morphological marking, and has therefore no bearing on the problem of inflectional morphology discussed in this column. We find no change in the morphological shape of embedded subject pronouns in these languages.

The problematic statement in the above passage is the claim that Lefebvre's and Veenstra's analyses of (exceptional) Case marking in HC and Saramaccan have 'no bearing on the problem of [contextual] inflectional morphology' because they rely solely on *syntactic* phenomena. Overt inflectional morphology is taken by Plag (2008a) to depend on interphrasal information exchange. Such exchange, it seems to me, would then rely on *syntactic* information that is transmitted across syntactic domains – indeed, syntax is the very component of grammar that organizes the *phrases* where information is to be exchanged. If so, any analysis of HC and Saramaccan that reveals syntactic phenomena that require the passing of information through nodes in the syntax (e.g., phenomena such as Case marking, quantifier scope, passive movement, co-reference patterns, etc.) certainly bears on the claim that the creators of these languages lacked the *syntax*-related processing capacities to handle such information exchange.

As it turns out, both Lefebvre and Veenstra's arguments for exceptional Case marking are based on, *inter alia*, the (im)possibilities of pronominal (co-)reference patterns *across* the boundary of the clausal complements introduced by *want*-type verbs. Consider, say, the HC contrast in (11) which is documented in Sterlin (1989; cf. Lefebvre 1998: 273):

(11) a. *Mari$_i$ vle   li$_{j/*i}$ genyen*
          Mary   want   3sg   win
          'Mary$_i$ wants him$_{j/*i}$/her$_{j/*i}$ to win'
          (i.e., not: 'Mary wants herself to win')
      b. *Mari$_i$ vle   pou   li$_{j/i}$ genyen*
          Mary   want   for   3sg   win
          'Mary$_i$ wants for him$_{j/*i}$/her$_{j/i}$ to win'
          (i.e., OK: 'Mary wants herself to win')

In (11a) with a complementizer-less clausal complement, the reference of the embedded subject pronoun *li* '3sg' is obligatorily disjoint from that of the matrix subject *Mari*. In (11b) with a clausal complement introduced by *pou* 'for', the embedded subject *li* is free to refer either to *Mari* or to some other individual in the domain of discourse. In order to block co-reference in (11a) between *Mari* in the matrix clause and *li* in the embedded clause, the HC speaker must be able to 'share information' between these two clausal domains. Furthermore, the referential range of the embedded subjects in (11) is sensitive to the morphosyntactic realiza-tion of the embedded complementizer layer. Lastly, the contrast in (11) obtains with a subclass of verbs that take clausal complements (e.g., *renmen* 'love', *pito* 'prefer', *krenn* 'fear', *mande* 'ask', *bezwen* 'need', etc.). Similar contrasts hold vis-à-vis Saramaccan's perception verbs. (See Sterlin 1989 and Veenstra 1996: Ch. 3 for further details on the relevant constructions in HC and Saramaccan, respectively.) The facts in (11), which implicate both the syntax of clausal complementation and the co-indexing of noun phrases across clausal boundaries, thus reflect productive instances of interphrasal information exchange of the sort that is excluded in early interlanguages as analyzed by Plag (2008a,b).

The problem is more general: In many theoretical frameworks, inflectional morphology is one (relatively superficial) reflex, *among others*, of the sort of *structural* relations that underlie various types of interphrasal information exchange. These types of information exchange are robustly attested in the grammars of Creole languages. In principle, such structural relations include c-command, sisterhood, Spec-Head relationships and other relations that enter into 'agreement' relationships, whether or not they have overt morphological reflexes.

Furthermore Plag's assumptions go against recent L2A results whereby syntactic representations in (early) interlanguages are shown to be relatively independent of their morphological reflexes. In such frameworks, overt morphological markers can go missing in the surface representations even when the relevant abstract features are active in the syntax and are shared by the relevant syntactic nodes. In such dissociative models, absence of overt morphology does not necessarily imply absence of the underlying abstract syntactic representations (see, e.g., various papers in Liceras et al. 2008; also see Aboh 2006b for one hypothesis where core syntactic

relationships can be dissociated from their morphological reflexes, and where the 'visibility' of overt inflectional morphemes at the syntax-semantics/ discourse interfaces plays a key role in accounting for the loss of inflection in language contact situations; see note 7 below).

The claim that Creoles' lack of contextual inflection reflects early interlanguages' lack of interphrasal information exchange amounts to a cherry-picking and a (too) superficial analysis of the relevant facts: this Creoles-as-early-interlanguages hypothesis ignores a substantial array of grammatical phenomena and theoretical observations, including some germane L2A data and observations, all of which weigh against the hypothesis that 'Creoles are conventionalized interlanguages of an early stage'.

4.4.4. 'Local Simplicity' in Creole *and* Non–Creole Formation

Consider the claim that:

> . . . 'local simplicity' . . . indicate[s] that in certain areas, it can indeed be shown that the creole grammar is simpler than that of its input languages, with 'simpler' being rather crudely defined in terms of either markedness, or number of forms, features or morphosyntactic distinctions being expressed. (Plag 2008a: 117)

Creoles are certainly not exceptional in that regard. Indeed the same local-simplicity claim can be, and has been, made about language change in general. Consider for example the pervasive instances of over-regularization, morphological erosion, paradigm leveling, rule loss, 'emergence of the unmarked', etc., as documented by historical linguists of various theoretical persuasions (see DeGraff 2001b: 242–289 for a summary, with critiques and references). The evolution of Latin into Romance and the history of English (e.g., the transition from Middle English to Modern English) also provide examples of 'local simplicity' differentials in, say, the domains of inflectional 'morphosyntactic distinctions'. It has often been argued that such rise in 'local simplicity' can be accounted for by L2A in language- and dialect-contact situations (for L2A as a factor in the reduction of morphosyntactic distinctions, see, e.g., Bunsen 1854; Meillet 1919 [1958]; Weinreich 1953: 42f; DeGraff 1999b, 2005c; Kroch et al. 2000; Trudgill 1986, 2002, 2004; Kerswill 2002; Liceras et al. 2008; also see §2.2.2). That language contact (thus, L2A) did play a role in the emergence of Romance languages and in the history of English is beyond any doubt, but what is doubtful is that the development of these languages would have gone through a historical stage characterized by widespread varieties that looked like conventionalized very-early interlanguages (e.g., without clausal embeddings of any sort). As far as I can tell from the cases of local-simplicity increments in the history of, for example, Germanic and Romance, prior conventionalization of numerically dominant early L2A interlanguages does not constitute a necessary condition for local-simplicity effects in the history of these languages. Therefore, and in keeping to

Uniformitarian boundary conditions, it cannot be claimed that local simplicity in Creoles is 'due to the nature of creoles as conventionalized interlanguages'.

Increments in 'local simplicity' do not set Creoles apart from non–Creoles (DeGraff 2001b: 242–289). On the contrary, it should remind us that it is quite reasonable to consider 'creolization' to be on a theoretical and empirical par with 'language change'.[8]

5. Envoi

The 'L2A–L1A cascade' perspective on the development of Caribbean Creoles that I'd like to advocate gives a 'central' role to L1A as the locus of creation of 'new' I-languages by 'Creole' (i.e., the locally born) children in the early stages of creolization. This perspective also gives a 'central' role to L2A in *all* its stages as the locus for the creation of innovative substrate-influenced interlanguages that, in turn, influenced the PLD that fed into subsequent instances of L2A and L1A. In the early stages of the formation of the new Creole community, both the adult learners' inter-languages and the Creole children's I-languages are 'new' in the sense that these inter- and I-languages are not, and could not be, identical to either the L2A-learners' respective L1s (the substrate languages) or the L2A- and L1A learners' models (i.e., the superstrate(-derived) varieties that provided the corresponding PLD at the early stages of creolization). This 'new'-ness is inevitable since language acquisition is neither relexification, nor language transmission *sensu stricto*: both L1A and L2A entail the (re-)creation of abstract representations (mental grammars) hypothesized on the basis of relatively superficial and necessarily limited and heterogeneous target utterances (see §2.3), thus making way for structural innovations at every stage of the recursive L2A–L1A cascade relationship described in §4.3.

In the framework that I have sketched above, both L1A and L2A play a recursive role in creolization in the Caribbean: For socioeconomic and political reasons, the Creole children, once they become older, will be among the most influential PLD sources in subsequent instances of L1A and L2A. As for L2A, the interlanguages therein will, throughout the period of large-scale arrivals of enslaved Africans, provide input for subsequent generations of L1A and L2A learners.

Any sociohistorical and demographic differences among various cases of contact-induced language change and creation – whether or not the results are called 'Creole' – will have an effect, not on the psycholinguistics of the L2A–L1A cascade per se (language-acquisition mechanisms are the same everywhere), but on the PLD that native learners will use in creating their new I-languages. For instance, sociohistorical and demographic factors will influence the proportion, fluency and heterogeneity of nonnative utterances of the PLD (cf. Baker 1982: 852–856; Lightfoot 1999, 2007; etc.). At the opposite poles of the PLD's native/nonnative continuum, there will be: (i) instances where most of the PLD come from native

speakers; (ii) instances where most of the PLD come from nonnative speakers. Once we incorporate into our language–acquisition and language–change scenarios the reality of dialectal and idiolectal variation and the facts of population displacement and migrations (i.e., once we factor the effects of dialect- and idiolect-contact via the influence of late-acquired – or 'post-vernacular' – grammatical features, as in the work of, e.g., Trudgill, Chambers, Kerswill, Labov), there may well be vanishingly rare instances, if any, where *all* patterns in the PLD exclusively reflect natively acquired grammars. If so, then 'language contact' at various levels of granularity is indeed a key factor in virtually all diachronic phenomena.

My hunch is that one of the major methodological roadblocks in Creole studies is that, for far too long and perhaps for ideological reasons, creolists have focused solely on isolated factors in the diachrony of Creole languages – either 'substrate influence' or 'superstrate inheritance' or 'innovations' or 'simplification' and so on – while paying too little attention to the sociohistorical details of said diachrony and to the *entire* lexica and grammars of Creole languages, including all their innovative aspects and all their potential correspondences with both the substrate and the superstrate languages (see, e.g., Mufwene 1986ff; DeGraff 1995ff; and M.-R. Trouillot 2002, for related critiques). This too narrow focus has, it seems to me, inadvertently prevented many creolists from considering Creole formation as the normal creation by language learners of holistic and well-integrated mental grammars constrained by the principles of Universal Grammar – 'systèmes où tout se tient'.

Accounting for only one (perceived) aspect of Creole languages is only part of the Creole-genesis story, a part that must be integrated with what we know about the overall structure of each Creole language and with what we know about language acquisition and language change outside of 'creolization'. In this essay, I was only able to establish some broad Cartesian-Uniformitarian boundary conditions and to sketch, with very large brush strokes, what I suspect to be the basic makeup of the full story of Creole formation as a normal instantiation of 'language change' that is driven by language acquisition with always finite, thus 'limited', and heterogeneous PLD. This story centrally implicates as 'agents' of Creole formation *both* child and adult learners. These two age-groups' respective capacities and limitations for language (re-)creation can be further elucidated by theoretically informed collection and analysis of all available data, including archival data, and by theoretically motivated investigation of, and experimentation with, observable instances of first- and second-language acquisition amidst a variety of historical and ecological contingencies.

From this Uniformitarian-Cartesian perspective, 'creolization' is on a par with 'language change' everywhere else, with 'Creole' languages straight-forwardly falling under the scope of the Comparative Method and contributing enlightening data toward our understanding of the sorts of diachronic patterns that are regulated by UG.

6. Acknowledgements

Short Biography

Michel DeGraff is Associate Professor of Linguistics at the Massachusetts Institute of Technology, Cambridge, Massachusetts, USA. He obtained his PhD in 1992 at the University of Pennsylvania with a dissertation entitled 'Creole Grammars and the Acquisition of Syntax: The Case of Haitian Creole'. His research interests and scholarly publications mostly concern the development and structures of 'Creole' languages, with focus on his native Haitian Creole. DeGraff is also interested in the joint study of language change and language acquisition and how such study may help elucidate the mental bases of language development in individual speakers and across generations of speakers. This is the topic of the collaborative anthology that he edited on *Language Creation and Language Change: Creolization, Diachrony, and Development* (MIT Press, 1999). This topic is revisited in his 2005 paper 'Word order and morphology in "creolization" and beyond' in *The Oxford Handbook of Comparative Syntax*, edited by Guglielmo Cinque and Richard Kayne. These works are part of a long-term project that documents why 'Creole' languages cannot be distinguished in any fundamental way from other languages on structural or developmental grounds. This argument is further explored in two contributions to the journal *Language*: 'Against Creole Exceptionalism' in 2003 and 'Against Creole Exceptionalism (redux)' in 2004. From DeGraff's nonexceptionalist perspective, the synchronic and diachronic properties of individual 'Creole' languages can in principle shed light on the Human Language faculty, without any sui generis stipulation about a 'Creole typology' or about 'Creole genesis'.

In a related vein, DeGraff has also examined the politics of Creole studies, namely the ways in which the production of 'knowledge' about Creole languages and Creole formation often reveals less about Creole languages per se than about issues of power, ideology, and geopolitics in the (post-)colonial history of the 'New World'. Details of this argument are presented in DeGraff's 2005 article 'Linguists' Most Dangerous Myth. The fallacy of Creole Exceptionalism' in *Language in Society*. This argument is being developed more extensively in a book–length manuscript.

In addition to this manuscript on the politics of Creole studies, DeGraff is presently collaborating with the Beninois linguist Professor Enoch Aboh (University of Amsterdam) on a comparative study of the grammars of Haitian Creole and the Gbe languages of West Africa (from the Kwa family of Niger-Congo).

Notes

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[1] For more details, see DeGraff (1995ff).

[2] One could call the Bloomfieldian (1925: 155) postulate for 'language' the 'physical' sense of E-language, as suggested to me by Salikoko Mufwene (personal communication). This 'physical' sense of E-language can be contrasted to the 'abstract' sense of E-language as the set of normative regularities – or socially sanctioned linguistic conventions – that can be extrapolated from the utterances of (certain members of) a speech community. It is this 'abstract', and subjective, sense of E-language that often enters into the use of language as 'act of identity' (cf. LePage and Tabouret-Keller 1985; also see the concept of 'common language' in Paul 1890).

[3] Presumably, continuum patterns will evolve, and in some cases *not* evolve, based on contingent sociohistorical factors. For example, the elimination of the French-born population in colonial Haiti through the 1791–1803 independence war – an unparalleled event in Caribbean history – removed one major factor of language change in the history of Haitian Creole, namely the major portion of socially prominent native French speakers. The latter, because of their socioeconomic prominence, had previously exerted a continuum-inducing influence on the pre-independence Creole speakers they interacted with.

[4] There is a great deal of work going back to the 17th century that, at least in principle, relates 'creolization' and 'acquisition'. Witness the age-old dogma that Creoles are the results of 'abnormal' language transmission via 'imperfect language acquisition' and, as such, stand apart from 'normal' languages, which undergo 'normal' transmission. Pelleprat 1655 [1965: 30–31, 34] offers early antecedents of the 'imperfect acquisition' view. The Language Bioprogram Hypothesis (Bickerton 1984, 1999) and the Relexification Hypothesis (Lefebvre 1998) are two representative theories whereby Creole formation instantiates radical sorts of *non*-acquisition vis-à-vis target-language structures (for critiques, see DeGraff 2001a,b, 2003, 2004, 2005b,c, forthcoming).

Overviews of past and present proposals on the relationship between creolization and acquisition can be found in many surveys, anthologies and monographs and the bibliographies therein. Here is a very partial list: Bickerton (1981), Andersen (1983), Adone (1994), Adone and Plag (1994), Faingold (1996), Véronique (1994), Wekker (1996), DeGraff (1999c), Mufwene (2001ff), Kouwenberg and Patrick (2003), Winford (2003), and Lefebvre et al. (2006).

Great care must be taken to avoid circular arguments whereby certain creolists, in order to support their (controversial) Creole-genesis hypotheses, uncritically enlist (controversial) claims by certain language-acquisition researchers who, in turn and as uncritically, enlist the *same* Creole-genesis hypotheses as support for their own (controversial) acquisition-related claims. One clear example of such circular reasoning can be found in Sprouse (2006) and Lefebvre (2008). Lefebvre enlists, in support of her Relexification Hypothesis theory, alleged instances of so-called 'relexification' in cases of L2A as investigated by, for example, Sprouse (2006). Lefebvre (2008: 98) writes: 'relabelling is claimed to be extensively used during the first stage of acquisition of a second language (Sprouse 2006)'. Lefebvre also cites Sprouse (2006) as follows:

> Sprouse (2006: 170) further states that *'Full Transfer can be restated in terms of Relexification'* and that *'Relexification is at the core of the second language instinct, accounting both for the L2 initial state and for the frequent failure or failure-driven revision to effect convergence on the target language.'* (2008: 95; emphases in original article)

But there is well–documented evidence that in L2A, the learner's grammar is routinely restructured to approximate that of the Target Language (TL). It is indeed claimed by Sprouse (2006: 175) that in 'canonical L2 acquisition',

> The learner at stage G_0 is confronted with TL input that G_0 cannot license. Morphosyntactic 'parameters' can be revalued, yielding G_1. G_1 is confronted with TL input that it cannot license; revaluation yields G_2, etc.

It seems quite likely that substrate speakers in the colonial Caribbean were, at least occasionally, exposed to TL input that their G_0 could not license. On such exposure, 'revaluation' would have yielded a G_1 distinct from G_0. Such TL exposure cum revaluation must have *routinely* happened, judging from the available historical and linguistic evidence surveyed in the main text and in DeGraff (2002), which contradict Lefebvre's claims about the origins and the profile of HC.

Yet, in assuming a sharp contrast between Creole formation and what he calls 'canonical L2 acquisition', Sprouse adopts from Lefebvre (1998), *without any independent evidence or argumentation*, the problematic sociohistorical assumption that 'lexifier language input is rare or absent for many in the emerging creole speech community' (2006: 175). Compare with Lefebvre's (1998: 36) claim that 'speakers of the substratum languages have very limited access to the superstratum data' (also see Lefebvre 1998: 57, 65, 386, 394, etc.). Given Lefebvre's (2008: 95, 98) uncritical use of Sprouse's (2006) controversial proposal (based on an unsubstantiated distinction between L2A in Creole formation vs. 'canonical L2A') and given Sprouse's equally uncritical adoption of Lefebvre's own controversial assumptions (based on unsubstantiated sociohistorical and language-acquisition scenarios for the formation of HC) the combined argumentation is circular and does not rest on any reliable and independent evidence. As it turns out, Sprouse and Lefebvre's claims contradict well-documented facts related to the sociohistory and the linguistic patterns of Caribbean Creole formation as in the HC case.

As for the L2A data provided by Sprouse (2006) in support of 'relexification', they actually contradict Lefebvre's (1998, 2008) claims regarding how 'relexification'/'relabeling' operates in Creole formation. For instance, Sprouse's (2006: 176) examples of the 'relexified' German of native English speakers manifest pieces of German inflectional morphology (as in, e.g., *Trinken wir ein Bier*) that have no counterpart in English and, thus, could not have arisen from the 'relexification' of English morphemes with German-derived phonetic strings (keeping the meaning of 'relexification' to the sense defined in Lefebvre 1998).

The examples in Sprouse (2006) certainly illustrate the well-known process of language transfer in L2A, but they certainly do not conform to the relexification/relabeling hypothesis as described in Lefebvre (1998, 2008). Actually Sprouse's examples illustrate the sort of empirical differences that have been previously pointed out between transfer and relexification (see, e.g., DeGraff 2002: 369–373; and Siegel 2006: 31, 38). I concur with Siegel's (2006: 38) assessment that 'there is actually no clear evidence of what could be called relexification as "a tool for acquiring a second language"'. (See DeGraff 2002 for an extensive critique of the empirical and theoretical problems in Lefebvre 1998.)

[5] For further relevant historical and sociolinguistic details on the colonial Caribbean and related issues, see Moreau de Saint-Méry (1797), Descourtilz (1809), H. Trouillot (1955, 1980), Alleyne (1971), Debien (1971), Fouchard (1972), Baker (1982), M.-R. Trouillot (1990, 1995, 2002), Singler (1992ff), G. Hazaël-Massieux (1993), Bickerton (1996), Barthélémy (1997), Baker and Bruyn (1999), Chaudenson and Mufwene (2001), Mufwene (2001ff), Warner-Lewis (2003), M.-C. Hazaël-Massieux (2008), etc.

[6] Here I abstract away from the existence, in both Creole and non-Creole communities, of regional and class-based continuum-like variations (cf. Fattier 1998 for the HC case); such variations in contemporary Creole speech do not seem to be the continuation of structural variations that would have been transmitted to the early Creole varieties from their structurally distinct substrates. (Also see §§2.3.1, 4.2.3 and note 3.)

[7] Heartfelt thanks for Enoch Aboh for bringing to my attention the relevance of the short-vs.-long verb-form alternation with respect to Plag's hypothesis. In Aboh's (2006b) account of inflectional erosion in language contact, the relative lack of contextual inflection in Creole languages can be related to whether or not such inflection is active at the syntax-semantics/discourse interface. In language-contact situations, pieces of morphology that are interpreted in the

semantics or discourse module are more 'visible' than those that are inactive in these modules. Thus, pieces of inflection such as subject-agreement affixes on verbs, which are strictly 'by-product of core syntactic configurations (e.g., specifier-head vs. complement-head relations)', have no semantics- or discourse-related contents, and are thus 'hardly visible' to language learners in language-contact situations (Aboh 2006b: 235). The fact that the short-vs.-long verb-form alternation seems to play a role in the syntax-discourse interface (e.g., with respect to emphasis in HC or Verum Focus in Mauritian Creole; Henri and Abeillé 2008) seems, at first approximation, to give additional empirical support to Aboh's hypothesis about the role of the syntax-semantics/discourse interface in language-contact phenomena. Aboh (2006b: 235) claims that 'the loss of inflection in a language contact situation should by and large be dissociated from issues related to second language acquisition proper'. In the I-language perspective adopted here (also see Weinreich 1953), the ultimate venue for language contact, language transfer, structural innovations, loss of inflection, etc., *is* language acquisition. Such an I-language perspective suggests that language acquisition and loss of inflection in language-contact situations cannot be dissociated, contra Aboh (2006b: 235.).

[8] See DeGraff (2001b: 242–289) for a variety of empirical and theoretical challenges for the age-old beliefs that Creole grammars show maximal global simplicity. These beliefs, as surveyed in DeGraff (2005b), go back to the colonial era. McWhorter (2005) is among the most recent exponents of these beliefs. As far as I can tell, the complex challenges to these 'simplicity' claims (see, e.g., DeGraff 2001b: 242–289) still stand (see Ansaldo et al. 2007 for related challenges). More recently, Aboh (forthcoming) provides examples that illustrate how language transfer in language-contact situations, and the concomitant (re)combination, in the emerging contact languages, of substrate and superstrate grammatical features can actually induce an *increase* of complexity in such contact languages (cf. DeGraff 2001b: 250–259 for similar arguments). One straightforward example is the use of retroflex stops in Indian English, a phonological feature that makes Indian English phonology more complex than its British English counterpart (DeGraff 2001b: 236). Also see the debates in the journal *Linguistic Typology* (vol. 5, no. 2/3, 2001; and vol. 8, no. 3, 2004) for vivid samples of related controversies. Itkonen's (2009) review of Dahl (2004) is among the latest contributions to the 'complexity' debate. Miestamo et al. (2008) also offers recent contributions to keep fueling these 'complex' controversies; some of the papers there are indicative of the diverse sorts of empirical rigor and theoretical sophistication, or lack thereof, that go into recent efforts at elucidating ill-defined, and often conflicting, notions of complexity. The Pidgin- and Creole-related simplicity claims in the Miestamo et al. collection are particularly revealing when evaluated against the empirical contributions in, for example, Jahr and Broch (1996) and Thomason (1997a). The latter, for example, invites us to a 'wider perspective' on contact languages, as advertised in its title, with 'structural descriptions [. . . that . . .] provide a strong antidote to the still common view that all pidgins and creoles have similar and simple structures' (Thomason 1997b: 6). Interestingly neither of these two collections is cited in Miestamo et al. (see DeGraff 2001b: 252–256 for previous discussion of similar omissions in complexity-related proposals).

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