

Phonological Features in Afro-American Pidgins and Creoles and Their Diachronic Significance. Comments on the Papers by Holm and Carter

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It is a pleasure to comment on these papers by John Holm and Hazel Carter because they are both well written and well researched, and because the study of pidgin-creole phonology is an important but neglected topic. As Holm noted in the conference version of his paper, recent argumentation against Africanisms in Afro-American varieties has been syntax based, and the phonological similarities of these varieties have not been considered significant for theories of their genesis and development. This critique, of course, does not apply to Alleyne (1980) or Boretzky (1983), but the neglect of phonology in creolistics is very real, and it goes beyond the substratist-universalist controversy. For instance, none of the fourteen articles in Mynsken and Smith (1986) deals with phonology; and only one of the twenty-one articles in the first six issues of the *Journal of Pidgin and Creole Languages* deals with phonology. Even in this volume, phonology-based papers are clearly in the minority.

It is important to counteract this underrepresentation because generalizations based on data from only one domain are invariably contradicted or challenged by data from other domains. For instance, Bickerton's (1975:18) claim that variable rules would be inappropriate for the Guyanese situation because of the extensiveness of invariant patterning there is more valid for morphosyntax than phonology (Rickford 1979), a function of the sociolinguistic generalization that phonological features tend to show gradient social stratification, while grammatical features tend to show sharp stratification with more pronounced differences between groups (Wolfram 1969:121).¹ A second example is Labov's (1972:322) classic characterization of sociolinguistic variables as "alternative ways of saying the same thing," which turned out to work well for phonology but not for morphosyntax (Lavandera 1978). A third example involves the issue of Africanisms, the focus of this conference. While the source of many pidgin and creole syntactic features may be a matter of controversy, no one could fail to concede after reading Alleyne (1980), Boretzky (1983), and Kihim (1986)

that some of the distinctive phonological features of Afro-American pidgins and creoles represent the influence of their African substrates.

But it is not enough to introduce phonological features simply as a foil to syntactic ones. Having agreed that it is important to consider all domains, we still have a great deal of hard thinking and research to do about the ways in which syntax, morphology, phonology, and the lexicon line up in terms of susceptibility to diffusion and in terms of their significance for our theories of creole genesis and development.

Let me quickly sketch some of the issues which occur to me. First, Holm noted in an earlier version of the paper in this volume that "borrowing is generally considered to occur most readily on the level of the lexicon and least readily on the level of syntax, with phonological borrowing occupying an intermediate position." But Weinreich (1953:67) noted at least three different nineteenth-century and early twentieth-century opinions about the relative susceptibility of the various domains to borrowing or interference. Although everyone agreed that words were lent and borrowed most readily, Whitney (1881) felt that suffixes and inflections came after words, with sounds last; Daurzat (1938) felt that sounds and syntax came after words, with morphology last; and Pritzwald (1938) suggested that phonology came after lexicon, with morphology and syntax last. Weinreich essentially disagreed with all of them, noting that before meaningful comparisons were possible, we would need to devise means of formulating the degree of integratedness of a system and measuring the affected portion of each domain. I think he meant by this that while it is relatively easy to point to instances of borrowing with respect to individual features, it is harder to come up with reliable measures of the overall degree of influence in an entire domain and to compare relative influence across domains meaningfully.²

Second, one difference between phonology and syntax—and it is one that challenges Holm's general theme that both dimensions must be given equal weight in untangling issues of creole genesis—is that because of the myriad combinations into which even a small inventory of phonemes can enter, it is difficult to argue that a language lacks "sufficient" phonological distinctions. No one ever says of Hawaiian or Ndjuka, for instance, that if they had more than five or seven vowels, they would be able to differentiate more words or concepts. In syntax or semantics, however, it is conceivable (although admittedly controversial) that a language might be underequipped for the range of uses which native speakers require, and it is, of course, this limitation which Bickerton (1981) postulates as a condition for his hypothetical bioprogram to "kick in" as a pidgin is nativized. Neither Bickerton nor anyone else has argued for the operation of a corresponding phonological bioprogram, however, and

it is partly in the absence of any such argument that local substratal effects in phonology seem less controversial. Of course, other considerations involving universals do apply to phonological features, as Holm notes.

A third issue is that creoles appear to differ from pidgins in their greater phonological flexibility or capacity for morphophonemic variation, especially insofar as the reduction of tense-aspect and other grammatical morphemes is concerned (Labov 1990; Mühlhäusler 1986:206). This is one area in which work on Afro-American varieties with an eye to the potential effects of substratal, superstratal, and universal influences is very much needed. Bendix's (1983) paper on sandhi phenomena in Papiamentu, African, and other creole languages is one of the only recent works dealing with this subject. I should note that detailed study of such phonological variability will require recorded, naturalistic, connected speech data, and not simply the citation forms readily available in published works. It will also require better recording equipment and more careful attention to the recording process.³ The extra effort and expense are likely to be worth it. For instance, Caribbean creoles share with American vernacular Black English a systematic but apparently unique rule by which initial voiced stops in tense-aspect auxiliaries can be variably deleted (*da ~ a*, *go ~ o*, *dō ~ o*, and so on; see Rickford 1980). This is an obvious candidate for substratal African influence, a possibility increased by the fact that Nigerian Pidgin English displays similar variability (Nicholas Faraclos, personal communication).

Phonological variability in African and Afro-American varieties is in fact a recurrent theme in the papers by Holm and Carter. It seems clear that we need to study such variation more carefully and directly than we have in the past, and that creolistics as a field needs to use more sophisticated means of analyzing and accounting for variation. This is an issue to which I will return in discussing these papers individually, especially Carter's.

Holm's Paper

Mechanism for the Retention of Substrate Features

Holm's general approach is commendable in two respects: his openness to other influences besides substratal transfer and his reference to the transfer model of Weinreich (1953), whose taxonomies and insights are too often ignored by students of language contact. With respect to other influences, however, one wonders why universals are artificially restricted to universals of adult second language acquisition when the relevance of first language and other universals

has been so forcefully argued in recent years (see, for instance, Bickerton 1983, a rebuttal of Valdman 1983, which Holm cites approvingly; as well as papers by Mufwene and others in Smith and Mynsken 1986).

Weinreich's transfer model is also more complex than Holm suggests, in at least two respects. First, in addition to the processes of overdifferentiation, underdifferentiation, and phone-substitution which Holm mentions, Weinreich (1953:18–19) mentions a fourth possibility: reinterpretation of distinctions, which is more difficult to identify because its effects are almost invisible on the surface. Second, and more important, Weinreich insists that structural correspondences between two languages only help to establish what is *possible* in terms of interference or transfer between them; what *actually* happens in particular cases is also determined by nonstructural factors relating to individual language users (e.g., which other languages they know) and to the social statuses of their communities (e.g., size and relative prestige). Thomason and Kaufman (1988:35) agree: "It is the sociolinguistic history of the speakers, and not the structure of their language, that is the primary determinant of the linguistic outcome of language contact."

Holm is not alone in minimizing or ignoring the nonstructural factors; even a prominent sociolinguist such as Trudgill (1986), while overtly tipping his hat to Weinreich, overwhelmingly favors structural over nonstructural factors as explanations for the effects of interdialect contact. But I think Weinreich's more genuinely sociolinguistic approach was right; we need such an approach to explain why some of the phonological adaptations we find in pidgins, creoles, and other cases of second language acquisition do not match what we might predict from the straightforward operation of substrate effects. For instance, Mühlhäusler (1986:141) shows that there are some "surprising" differences between the shapes of English loanwords in Tok Pisin's principal substratum language, Tolai, and their cognates in early Tok Pisin: English 'biscuit' comes out *bisket* in early Tok Pisin, but *parikeri* in Tolai, and English 'strong' comes out *strong* or *sitrong* in early Tok Pisin, but *korong* in Tolai. Similarly, some of the adaptations English loanwords undergo in West African languages ('step' realized as *sitēmbu* in Yoruba; Carter 1988:239) are not found in the English-based Caribbean creoles for which such languages were substrates. In both cases, the explanations for the differences undoubtedly involve both structural and nonstructural factors, including the fact that the source language speakers have borrowed a few words into their essentially unchanged native systems, but the pidgin and creole speakers have participated in a different sociolinguistic process involving mutual linguistic accommodation and language shift (Thomason and Kaufman 1988).

Coarticulated Stops

With respect to the coarticulated stops /kp/ and /gb/, the case for their being derived from African substrates is clear in view of the factors cited by Holm: their presence in a number of relevant West African languages,⁴ their absence in relevant European languages, and their rarity or markedness worldwide. Holm does a good job of marshaling evidence from Alleyne (1980:50), Boretzky (1983:60), and other sources to show that coarticulated stops occur not only in Saramaccan (contra Bickerton 1981:122) but in other creoles, including Ndjuka, Príncipe Creole Portuguese, Krio, Liberian, Nigerian Pidgin English, and Gullah. At the same time, the fact that these coarticulated stops are relatively rare and have a limited functional load, being mainly restricted to African-derived words, reduces their significance.

Although the "Africanness" of these coarticulated stops may be "straightforward," there are some open questions about their development and distribution in Afro-American varieties which Holm does not mention. One is the fact, noted by Alleyne, that /gb/ is more common than /kp/ in non-African-derived Saramaccan words; that is, we have more examples like *gboto* 'boat' than *kpinii* 'squeeze out.' Why should this be? Alleyne speculates that "there may have been some interlingual identification between African [gb] and European [b], arising perhaps from the absence of [b] in some of the African languages or dialects used in the contact situation" (1980:50). This may well be, if relevant West African languages without [b] exist, but another explanation may simply be that [gb] is commoner than [kp] among West African languages. Southern Bantbara and Maninka, for instance, both have /gb/ but not /kp/ (Welmers 1973:48).

A second, related issue is that in Saramaccan these coarticulated stops did not simply replace European-derived /p/ and /b/, but also (perhaps more commonly) /kw/ and /gw/, indicating that the interlingual identification between West African and European consonant systems was not as straightforward as we might otherwise have imagined.

Finally, there is the synchronic variation in several of the African and Afro-American languages between [kp] and [kw], and [gb] and [gw], to which Alleyne, Boretzky, and Holm all refer. This may relate to the phonetic fact, noted by Welmers (1973:47), that the coarticulated stops are often heard as involving a w off-glide because the bilabial release is slower or weaker than the velar. And this in turn may account for the interlingual identification between the coarticulated and labialized stops referred to above.

One open issue Holm does mention is the question of whether the words

with coarticulated stops in Gullah and in Krio, Liberian, and Nigerian Pidgin English, which are primarily if not exclusively African derived, represent recent (nineteenth-century) borrowings. This seems very likely in the case of the West African varieties, but less so in the case of Gullah. I agree with Mufwene (1985:157-58) that coarticulated stops are marginal in Gullah, and I share his fascination with the issue of why they were preserved in some but not other words. But Saramaccan, in which the possibility of recent borrowings is slimmer if not nonexistent, poses similar problems, clearly the result of irregular retention from an earlier stage. I do not see why a similar explanation could not apply to Gullah, and I am not convinced by existing arguments or evidence that the Gullah features represent more recent borrowings. Why should nineteenth-century Gullah speakers have been better disposed to adopt such a distinctively non-English feature than their seventeenth- or eighteenth-century counterparts, whose exposure to English would have been more limited and whose familiarity with African languages greater? In any case, it is an intriguing issue.

Palatalization

With respect to palatalization, one cannot help but be impressed by the wealth of evidence Holm introduces, from a variety of languages both past and present, and by his readiness to admit superstratal and other influences. And yet, with the exception of one or two cases—such as the distribution of alveopalatal [ʃ], [dʒ], [ʃ], and [ʒ] in São Tomé Creole, which suggests stronger influence from Southern Kongo than from standard Portuguese—one is almost embarrassed by the richness of possible influences. With substrates, superstrates, and universals all likely to yield the same result in so many cases, influence from any one source is especially difficult to prove.

In future work on this variable it may be useful to consult Bhat's (1978) comparative study of palatalization, as much for its summary data on over one hundred instances of palatalization in a wide variety of languages as for its recommendation that the three constituent processes of palatalization (tongue fronting, tongue raising, and spirantization) be distinguished. As Bhat notes (51-54), tongue raising tends to affect apicals, and the height rather than frontness of the following vowel is important; tongue fronting, by contrast, tends to affect velars, and the frontness of the following vowel is more important than its height. These generalizations help us to anticipate the solution to the mystery of why Jamaican has /ky/ and /gy/ in words like /kyaad/ 'card' when, according to Holm, "they would not be expected because they do not have a high front vowel." Since velar palatalization is an instance of tongue fronting, we expect

the frontness (rather than height) of the vowel to be crucial, and, of course, Holm's sources show this to have been the diachronic conditioning in this case. In a similar vein, while it does not appear to affect the substantial point (originally Alleyne's), it is technically incorrect to describe the back vowels which follow /tj/ and /dj/ in Surinamese *tjuba* and *djombo* as "nonpalatalizing," because the crucial conditioning in the case of tongue-raising palatals like these is vowel height, not frontness.

An important source of evidence in several of the cases of palatalization Holm discusses is the existence of synchronic variability in the creoles, as attested in lexical doublets such as Lesser Antillean *tɔwɪn* ~ *twɪn* 'kitchen' and triplets such as Netherhollands *biri* ~ *biji* ~ *bisi* 'a little bit.' Based on my experience in transcribing spoken texts (Rickford 1987), I suspect that morpho-phonemic variability of this type is even commoner in connected speech than is usually reported and that it was so even in the past, at least since creolization took place.

A related point about palatalization—and it applies to other features, too—is that despite its ubiquity, written records of creole speech from earlier times are often limited, like modern written representation, in giving absolutely no orthographic indication of its existence (using *cai* instead of *cyai* or *kyai*). The moral, of course, is to be cautious in the use of such written records. But it may also be helpful to study the synchronic relations between Afro-American speech and its representations in popular literature and the media to get some idea of how much we can depend on such representations in reconstructing the past. In popular representations of vernacular Black English, for instance, consonant cluster simplification is often indicated, but not the neutralization, before nasals, of [ɪ] and [e].

Holm's concluding section is, like most of his paper, evenhandedly and judiciously argued. The following statement, however, comes as a bit of a surprise, since the focus of the paper up to this point has been on substrate influence on phonology: "Bickerton's implication that substrate influence can occur on the level of phonology and lexicon but not on the level of syntax must be rejected as both illogical and implausible." What is lacking is any explicit theoretical justification for the expectation that substrate influence in any one domain should imply or guarantee similar influence in other domains. Holm may have had in mind a principle of the type which Thomason and Kaufman (1988:60) adopt on the basis of their research:

If a language has undergone structural interference in one subsystem, then it will have undergone structural interference in other subsystems

as well, from the same source. Not necessarily in all subsystems . . . lexical interference may be negligible in cases of interference through shift; and considerable structural interference may occur without including externally-motivated changes in the inflectional morphology. But we have found *no* cases of completely isolated structural interference in *just one* linguistic subsystem.

However, the authors do go on to note that in borrowing, "limited phonological restructuring can occur without concomitant syntactic restructuring" (60), citing the case of Nguni dialects of Bantu, which have borrowed clicks from Khoisan (and which retain them primarily in Khoisan loanwords) but show no syntactic interference from Khoisan.⁵ Given the similarity between this Nguni case and what some have suggested about coarticulated stops in Afro-American creoles—namely, that they are largely restricted to African words and *may* represent recent borrowings—we should probably be cautious about invoking Thomason and Kaufman's principle as justification for Holm's more general conclusion.

Carter's Paper

Carter's paper is more difficult to discuss than Holm's because it covers more subtopics. Carter focuses on areas in which the phonetic facts and appropriate phonological analyses are more open to question (vowel length, quality, and tone), includes more new data ("new" to creolistics, at least), and raises the issue of variability more acutely. Moreover, Alleyne's (1980:35–43) discussion of Afro-American vowels, the springboard for Carter's paper, is silently present throughout the paper. I found it necessary to read and reread Alleyne's discussion to understand Carter's more fully, and in what follows I will probably appear to be commenting on his argumentation and evidence as much as hers, but hopefully not unduly so.

Alleyne's Hypothesis

The starting point for Carter's paper is a hypothesis elaborated in Alleyne (1980:38–43), which she summarizes as follows: "The earliest form(s) of Afro-American had no phonemic vowel length distinction, and that in varieties such as Jamaican, vowel length was introduced through the influence of English." At the risk of going over some of the ground Carter covers, I wish to consider Alleyne's proposals in a little more detail.

Alleyne (1980:38, 76) characterizes the earliest Afro-American phonological system as a four-tiered one:

	i	u
e	o	
ε	ɔ	
a		

The lax mid vowels [ɛ] and [ɔ] are described as having a low functional load and occurring mainly in African words (39–41), so the system is essentially three-tiered (i/u, e/o, a). In addition to lacking a productive tense/lax distinction for the mid series, the system also lacks any vowel length or tenseness/closeness distinction for the high vowels, any means of distinguishing between 'beat' [i] and 'bit' [ɪ], or between 'fool' [u] and 'full' [ʊ].

What is Alleyne's evidence for this claim? Not early written records or texts (none are cited), but the fact that modern varieties such as Saramaccan and Ndjuka, generally the most conservative and least Anglicized varieties of Afro-American, appear to have a similar system. For instance, both 'big' and 'see' have the same vowel (*bgi*, *si*) in these varieties, as do 'root' and 'pull' (*liin*, *piu*).⁶ Although these varieties do have some long vowels ("double" by Carter's analysis), these are seen as "a later development as a result of the coalescence of two vowels after the application of a vowel epenthesis rule and an intervocalic liquid deletion rule. Thus 'self' → *selef* → *seépi*" (1980:39).

Modern Jamaican, Guyanese, and Krio vowel systems are more complex than this, but Alleyne sees their additional distinctions, once again, as representing postgenesis developments, in these cases the result of the continuing influence of English. The primary evidence for this claim is the synchronic variability between forms like [kɾil] and [kɾilɪ] 'creel' in Jamaican and [beɟ] and [beɟɪ] in Krio, which he sees as "residues of the earlier lack of phonemic distinction" (42).

Carter's Hypothesis

While agreeing that Krio and West African Pidgin English (WAPE) provide some support for the absence of vowel length in early Afro-American, Carter suggests that vowel length variability was present in Afro-American from early on, and that this in turn was a function of variable or competing West African subsystems rather than English influence. English, she argues, is unlikely to have been the source of phonemic vowel length in Jamaican and similar varieties because the Great Vowel Shift, which transformed distinctions of length into distinction of quality, was essentially over at the time these varieties were

being formed (seventeenth century on). Furthermore, Jamaican's putative long vowels are better analyzed as double vowels with HL tone pattern (*íi*, *áa*, *úu*). This makes them less likely to have come from English, which has neither double vowels nor distinctive tone, than from African languages, many of which do have such features.

Much of Carter's paper is devoted to a survey of African languages. This is very interesting, but it is easy to get mired in detail. One essential point is, however, that most of the relevant African languages have double rather than phonemically long vowels; thus their speakers assimilate perceived length in English loanwords (in stressed syllables which occupy a whole foot, for instance) with double vowels, as in Yoruba *fínnu* 'film.' Another key point is that vowel doubling and vowel length are highly variable in African languages—from one language, dialect, and style to another, as well as in diachronic changes and synchronic morphophonemic variation within a single language. Such variability is evident today in Jamaican Kunnina ([kinzu ~ kinzɪn] < Kongo [kinzú] 'pot'), and it suggests to Carter the existence of two competing subsystems among the mid-nineteenth-century Angolan and Sierra Leonean immigrants from whom Kunnina is derived: one in which only short vowels are allowed, and one in which double or long vowels are also permitted. She suggests that the variation in Jamaican English itself (e.g., /asliɪp/ ~ /asliɪp/) might have originated in the same way, through the clash between African subsystems which permitted phonemic double or long vowels and those which did not.

In the remaining sections of this discussion I will comment on various aspects of the alternative hypotheses, beginning with narrower issues about the length and quality of English, West African, and Afro-American vowels and concluding with larger issues relating to their variability.

Length of English Vowels during the Formation of Afro-American

Carter, I think, proves her case that "the development of the qualitative differences which were to replace phonemic length as distinctive . . . [in English] was well on its way in the sixteenth and seventeenth centuries." This is clear from her demonstration that Jamaican /ii/, /aa/, and /uu/ do not correspond to Middle English (ME) long /i:/, /a:/, and /u:/, respectively, but to the outputs of ME /e:/, /a/, and /o:/ (and to other specified vowels), raised and otherwise modified by the Great Vowel Shift.

Equally justified is her conclusion that "Jamaican is therefore at least as likely to represent perceived differences of quality as of length, and in some

cases clearly does so." To this I would add, however, that those vowel quality differences are even more ubiquitous than she suggests, and that they occur in Guyanese and Gullah as well, despite the tendency of Alleyne (1980:35, 41–43) and others to represent and discuss these vowel systems as if length but not quality were the only phonetically discernible and phonemically significant feature. For Gullah, this is clearly not the case, since Turner (1949:15–20) distinguishes between [i] and [iː], [e] and [eː] in terms of relative retraction and closeness or height.⁷ In Guyanese, comparable differences of vowel tenseness or height also exist (see Allsopp 1958:6; Rickford 1979:191–93, 1987:8–9). Even Devonish (1989:75), who opts for the following normalized representation of GC vowel phonemes, recognizes that in the high and mid pairs the vowels are distinguished by relative tenseness or peripheralness as well as by length.⁸

ii	i	u	uu
ee	e	o	oo
aa a			

Jamaican also seems to have a comparable combination of length and quality; Cassidy and Le Page (1980:xxxix) describe /i:/, at least, as "long, high, tense," in contrast with /i/ "short, high, lax" (emphasis added). It is all too easy, given the prevalence with which Cassidy's phonemic orthography is used for Caribbean creole English vowel systems, to forget that /i:/ versus /i/ often represents a difference of vowel quality as well as length.

Having agreed on these two counts with Carter, however, I wish to disagree with her argument that "Alleyne's hypothesis requires that English should have still maintained the length distinctions at the time when Jamaican, Guyanese, and Gullah began to acquire or develop them." I think what is fundamentally at issue—in the difference between the Surinamese and non-Surinamese varieties, and in the inferences we can draw from this and other evidence about the development of Afro-American—is the *number* of vowel distinctions involved and their *variability* rather than just their *nature*. Jamaican, Guyanese, and Gullah clearly share distinctions between /i:/ and /i/ (and comparable pairs) with English, but not Saramaccan and Ndjuka (reportedly). Whether these distinctions are interpreted primarily in terms of quality or length, the more decreolized varieties could have acquired them after their initial formation, through their longer and more intimate association with English. The phonetic issues are not without interest and significance, but deciding them one way or another does not materially affect the larger issues.⁹

Jamaican (and Guyanese) Vowels: Double or Long?

A similar argument applies to Carter's analysis of Jamaican as possessing double vowels (with two syllabic nuclei) rather than long vowels (with a single, long syllabic nucleus). It is a refreshing and attractive analysis, especially in view of the neat way it links up with historical facts (e.g., relevant African languages have a similar system) and accounts for some of the synchronic features (e.g., Jamaican /dwiit/ derived from /du it/ to maintain syllable count). But there are reasons to be cautious about it. In Guyanese, at least, /maatá/ and /biitá/ both have, to the ears of this native speaker and others I have consulted, two syllables, not three, the initial syllable consisting of a single long vowel, or as Devonish (1989:89) analyzes it, a single complex nucleus with two moras. It would be good to have acoustic or native speaker support for the double vowel analysis in Jamaican and to know how extensively it applies in the lexicon. Is /dwiit/ 'do it' really two syllables? Does this analysis apply also to /iin/ as a variant of /in/ 'in'? Moreover, as Devonish (personal communication) has asked, how come Jamaican attests only the HL tone pattern in these adjacent double vowels (/máata/) while the full spectrum of possibilities (HL, LL, LH, HH) is reflected in words with nonadjacent vowels?¹⁰ Contrast Saramaccan, in which adjacent double vowels and alternative tone patterns are better established, as in *dɔɔ* 'door', *gɔɔ* 'grow', and *dóon* 'drum' (Alleyne 1980:41). Finally, the Jamaican pattern of assimilating English adoptives does not really match that of languages like Yoruba with true double vowels. Yoruba *fínnu* 'film' exemplifies the Yoruba tendency to assimilate English stressed vowels which occupy a whole foot as double, but the comparable Jamaican item does not have a double vowel (*flim*, *fin*); contrariwise, Yoruba *leta* 'letter' represents the Yoruba tendency to assimilate English stressed vowels which do not occupy a whole foot as short, but Jamaican *bíita* 'beater', which has a double or long initial vowel rather than a short one, could not have been derived according to the same principle.

Again, these are interesting phonetic and phonological issues in their own right. But whether we analyze Jamaican as possessing long or double vowels, or both (as Shona does), we can ask why it makes such distinctions where Saramaccan and Ndjuka do not. (Recall that Surinamese double vowels derive from elided liquids and do not match the putative Jamaican double vowels in distribution.) Was Afro-American originally invariant and lacking in length and quality distinctions for the high and mid vowels, as Alleyne claims?

Variability and Other Issues

Interestingly enough, Carter's position on early Afro-American vowel length is more prototypically Alleyne-like than Alleyne's, insofar as it stresses African rather than English origins and variable/complex rather than invariant/simplified initial systems. Throughout Alleyne's 1971 paper (and in many parts of Alleyne 1980), the thesis is advanced that differences in native West African systems and differences in opportunities and motivation for acculturation to English meant that early Afro-American must have been variable from its very inception. Consider, for instance, the following passage:

In neither lexicon, phonology nor syntax was the Proto-Afro-American dialect stable, uniform or durable. . . . It is likely that in any Afro-American community there would have been, from the very beginning, considerable intra- and interdialectal variation. . . . Fluctuation in the phonological systems of Afro-Americans during the formative period was also due to differences in the native phonological systems of Africans . . . fluctuations on the morphophonemic level may also have existed as a result of dialect differences within the English of the contact situation. (Alleyne, 1980:75-76)

Carter certainly reveals enough variability within and across the native vowel systems of relevant African languages to make it likely that early Afro-American was not invariantly lacking in length and quality distinctions and that some of the variation in modern Jamaican, as in modern Kumina, might reflect competing West African systems.

Having essentially agreed with Carter (and with prototypical Alleyne) on this, however, I must confess that a number of issues remain unresolved. I will mention only two.

First, why do Surinamese varieties lack distinctions of vowel length, doubling, or tenseness while Jamaican, Guyanese, and Gullah do not? Carter does not address this issue, but to follow through on her line of argumentation, we might suppose that the relevant substratal systems for Suriname did not permit phonemic vowel length or doubling, while at least some of those for other parts of the Caribbean did. It is unlikely that such a neat, demographically based vowel system differentiation can be established (for instance, we know from Hancock 1969:17 that "at least one-third of the slaves imported into Surinam were from Angola," and from Carter's paper that Angolan Kongo includes double vowels), but in order to pursue the question we need a list of the relevant

contact languages for each territory and information on their vowel systems, especially with respect to length and doubling.

Alleyne's argument that the Surinamese varieties differ from the others because of their more limited exposure to English is a simpler one, although it is still a puzzle why the former should lack distinctive vowel length and quality despite possible reinforcing substratal influences (see n. 9). One solution, although not the type Alleyne would favor, I think, is to recognize a tendency for pidgins and early contact varieties to lose vowel length and other phonological distinctions found in both their lexifier and substratum languages (Heine 1975:3; Mühlhäuser 1986:148) and to suggest that the Surinamese and West African varieties are frozen at a "pidginizing" stage of this type. But since the Surinamese creoles are as complex as other Caribbean varieties, sometimes more so in many other features, this "solution" raises new problems and puzzles of its own.

Second, I think it will be necessary to recognize more intrasytemic morpho-phonemic variation in Jamaican, Guyanese, and Gullah (perhaps also in Saramaccan, Ndjuka, and Sranan) than either Carter or Alleyne seems prepared to accept, and to analyze it more directly, using more sophisticated means. Both authors seem to regard morphophonemic variation between tense and lax or long/double and short vowels as exceptional, but I suspect that a closer analysis of connected speech will reveal it to be more widespread (see Cassidy 1961:42-43, and Carter's own description of Kumina), and that quantitative or implicational approaches will better reveal its nature and conditioning (De Camp 1971:358-62; Rickford 1979, chap. 7). For instance, according to Alleyne (1980:41), "some words whose cognates in English may have had the shorter (lax) vowel acquired [in Jamaican and Guyanese] long vowels: [i:n] 'in,' [wi:k] 'wick,' [i:f] 'if.'" One is likely to infer from this that only the long or tense vowel variants persist. But I know from my native Guyanese experience that the short/lax vowel variant is actually more common. The following entry for /iin/ in Cassidy and Le Page (1980:234) suggests as much for Jamaican: "A common dial pronunc of *in* (though the usual pronunc at all levels is /in/ [in])." Diachronically, Carter still seems to envisage the formation of Jamaican as due to competition between two or more different but invariant West African systems. However, her discussion of the modern reflexes of those systems reveals more inherent, intrasytemic variability. Her fascinating Kumina data display considerable inherent variability, although the terms "confusion" and "chaotic" imply a view of it as exceptional and unprincipled which is probably unjustified. I would like to see more creolists wade into the analy-

sis of such inherent variability directly and with more sophisticated analytical tools. The process should be informative and insightful in both synchronic and diachronic terms.

Notes

1. For instance, with only a few exceptions (such as the use of /a:/ instead of /ɜ:/ in words like 'God'), speakers at all levels of the Guyanese continuum share non-English or creole pronunciations such as /tʌv/ for 'town' (compare British/American SE /taʊn/) or /fɛtɪf/ for 'fairy' (with high tone or prominence on the second syllable instead of the first; see Devonish 1989:92). But they are more sharply stratified in terms of their everyday use of morphosyntactic creole features such as the use of anterior *bin* or focus/cleft-marking *a* (*a tɪf iɪ tɪf di bʌk*).
2. Relevant to this issue too is Thomason and Kaufman's (1988:37-40) observation that the relative susceptibility of a domain in one language to influence from another depends on whether borrowing or language shift is involved, the former favoring the lexicon, the latter favoring phonology and syntax. Creole genesis is a subtype of language shift.
3. Uher or Nagra reel-to-reel tape-recorders, capable of high-fidelity recording speeds of $3\frac{3}{4}$ or $7\frac{1}{2}$ rps, are ideal. Even the best cassette recorders are limited insofar as they have fixed (slow) speeds. Researchers need to be prepared to spend as much, or more, on their recording equipment as they do on their computers, recognizing that the former critically limits the quality of the data to be analyzed.
4. By "relevant" African and European languages, I mean here and throughout this paper those languages likely to have been involved in the formation of Afro-American varieties, such as Twi, Yoruba, and Mende, on the one hand, and English, French, and Dutch, on the other.
5. Sarah Thomason, in fact, reiterated this point and this example in the discussion period following Holm's paper. She also observed that prestige constraints impinge more powerfully on syntax than phonology, with the result that nonstandard African-derived features are less likely to persist in syntax than in phonology.
6. The system also includes at least one diphthong (/ai/) and a nasality distinction.
7. Turner's discussion is couched in phonetic terms, but it is evident throughout that the distinctions are intended to be phonemic. For instance, after discussing Gullah [i] and [ɪ], Turner observes that "in Fante, [i] and [ɪ] likewise belong to separate phonemes" (1949:19). Furthermore, the texts at the end of the book exemplify relevant phonemic contrasts, for instance between [bit] 'bit' and [bit] 'beat' (262).
8. Devonish notes (1989:75), as Cassidy and Le Page (1980:xlv) and Alleyne (1980:41) also do, that differences of tenseness, height, and length "coincide in GC

as they do in many languages." His argument for accepting length as phonemically criterial in GC is the fact that the pair [a:] and [a] differ only in length. Note too that it is an idealized basilect, omitting [ʌ] (compare Rickford 1987:9).

9. Of course, since many of the relevant West African languages have a tense/lax or close/open distinction for the high vowels [i/i:, u/u:], the mid vowels [e/e:, o/o:], or both (Weimers 1973:20-21; Bender-Samuel 1989:23, 56, 128, 414), it is difficult to understand why the Surinamese and West African English-based pidgins and creoles do not make a similar distinction. Alleyne (1980:43) seems to be aware of the potential problem here but turns it on its head. According to him, "although African certainly has [e], Afro-American uses [e] in English cognates such as 'bed.'" He uses this observation as evidence that English vowels at the time of early Anglo-African contact were distinguished by length instead of quality. Since Carter effectively repudiates the latter claim, the puzzle still remains, suggesting that substratal influence cannot tell the whole story.
10. Although I have not yet seen it, Hubert Devonish (personal communication) informed me that Wittle (1989) has analyzed Jamaican as having long (one-syllable) vowels rather than double vowels (with two equally prominent syllabic nuclei).

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