

CHAPTER 17

What Happens in Decreolization

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The title of this paper should be read, not as an answer, but as a question. My intention is not to offer a definitive description of decreolization, but to indicate how much more we need to know about what is theoretically possible, and what actually does happen, in the course of decreolization. In reading Schumann and Stauble's very interesting lead paper for the LSA symposium, it occurred to me at several points that *this* was one of the biggest barriers to further understanding of the similarities between decreolization and second language acquisition (SLA). This is not to suggest that enough is known about second language acquisition. But since my native background and my research experience have been in the area of the creole continuum, it is the need to extend Schumann and Stauble's discussion of decreolization which has been more apparent to me. Hence the title of this paper and the focus of my remarks.

My discussion of the Schumann and Stauble paper will proceed as follows. In section one I raise several quibbles and questions about the decreolization model that Schumann and Stauble use, and suggest some ways of improving on it to

*I wish to thank Roger Andersen for the invitation to write this paper and my wife Angela for fruitful discussion of many of the ideas presented herein. It is also a pleasure to thank the following agencies that funded my research on the Guyanese Creole continuum, the results of which are drawn on at several points in this paper: the National Science Foundation (Doctoral Dissertation Grant GS-42475), the Danforth Foundation (Graduate Fellowship) and the University of Guyana Research and Publications Fund. I also wish to thank Professors William Labov, Dell Hymes, and John Fought who supervised the dissertation that grew out of this research and that is referred to at several points in this paper (Rickford, 1979). As usual, I alone am responsible for any weaknesses in this or the original work.

depict the theoretical possibilities for decreolization at the level of communities as a whole. In section two I am more explicitly concerned with comparisons between decreolization and SLA, but this time at the level of the *individual*, where SLA studies have been strongest and decreolization studies weakest. In my third and final section I move away from general processes and concentrate on Schumann and Stauble's comparison of negation in decreolization and SLA. This comparison is central to their claim that these are parallel processes.

QUIBBLES AND QUESTIONS ABOUT THE DECROOLIZATION MODEL

Schumann and Stauble lean heavily on DeCamp (1971a) for their working model of the decreolization process, as the following quotation from their paper indicates:

Decreolization is a process which occurs when certain conditions are present in a creole community. According to DeCamp (1971), the dominant official language must be the base language corresponding to the creole. The official language acts as the target language toward which the creole will develop. In addition, the formally rigid social stratification must have partially broken down, allowing the creole speakers to gain varying degrees of contact with the dominant group. This situation produces a series of lects ranging from the creole to the target language and is referred to as the post-creole continuum.

Since the authors' own research experience is in the area of SLA rather than in decreolization, it is quite natural that they should regard DeCamp's classic and frequently cited model as nonproblematic. But valuable and pioneering as this model is, it does have limitations, and these are worth pointing out.

In the first place, DeCamp himself (1971b:29-30) seemed to have regarded this model with a certain degree of tentativeness, for he cautiously introduced it with the words, "It *would appear* that a speech community can reach post creole status only under certain conditions" (emphasis added), and went on to emphasize, in the very next paragraph, that diglossia and the post-creole continuum "require further study." One of the areas that he felt needed further clarification was "the linguistic behaviour of both groups and individuals under varying circumstances"—a factor that is still not given sufficient consideration, neither in Schumann and Stauble's paper nor in so much of the decreolization literature. (More on this in the next section.)

Furthermore, Alleyne, 1971, which appeared in the same volume as DeCamp (1971a and 1971b), had suggested that from the very *beginnings* of those African/European contact situations in the New World that gave rise to creoles, "the creole was in fact everywhere only a major segment of a continuum of variation." Now if Alleyne's arguments and evidence are accepted—and they certainly seem to be widely respected—the DeCamp model would clearly have to be modified, at least for the Caribbean cases and the many situations that might have been similar. The breakdown in social stratification which, according to Schumann and Stauble (paraphrasing DeCamp), would allow "the creole speakers to gain varying degrees

of contact with the dominant group" (emphasis added) would have to be seen instead as allowing "more creole speakers" to do so, since some types of individual (such as house slaves) could be assumed to have had this opportunity all along.

In one sense, then, Alleyne's (1971) arguments necessitate a quantitative in addition to a purely qualitative view, and interestingly enough, it is primarily in a quantitative sense that some New World communities can currently be said to be decreolizing. (See Table 2.)

Another way of integrating Alleyne's insightful remarks, however, would be to suggest that even though the decreolization process itself might be several years old from the viewpoint of the community as a whole, it might be brand new from the viewpoint of the individuals who go through it in each successive generation. This much would be true if they did not simply borrow or acquire the intermediate outputs of their predecessors, but were active reshapers and restructurers themselves, articulating decreolization paths and producing intermediate outputs similar to those of their predecessors only because they were working from a similar starting point to a similar target.

Here is one area in which SLA studies are illuminating for those of us working on decreolization. In a decreolizing community, speakers are typically exposed to a whole range of intermediate or mesolectal outputs. As a result, when a decreoliz-

Table 1. A Model of That Type of Decreolization Which Transforms a Bilingual Community into a (Post) Creole Continuum

	<i>Basilect</i>	<i>Lower-Mesolect</i>	<i>Mid-Mesolect</i>	<i>Upper-Mesolect</i>	<i>Acrolect</i>
Stage 1	X				X
Stage 2	X	X			X
Stage 3	X	X	X		X
Stage 4	X	X	X	X	X

Note: Although this model (like the others below) depicts three mesolects, it could easily be adjusted to provide for more (or less). These models may also be idealizations insofar as they assume that it is possible to factor out the variation in creole-speaking communities into 2, 3, or n distinct "lects."

Table 2. A Model of That Type of Decreolization Which Involves Quantitative Shifts in the Percentages of Speakers Using Each Lect

	<i>Basilect</i>	<i>Lower-Mesolect</i>	<i>Mid-Mesolect</i>	<i>Upper-Mesolect</i>	<i>Acrolect</i>
Stage 4.1	33%	27%	40%	27%	27%
Stage 4.2	33	33	47	33	33
Stage 4.3	13	40	40	40	47
Stage 4.4	7	47	40	47	47

Note: The reason why the percentages in each row do not total 100% is because it is assumed (correctly, I believe) that each speaker is not restricted to a single lect, but may control 2 lects or more.

ing speaker eventually learns to produce a "new" intermediate output which is similar to outputs that others were producing before, it is difficult to determine whether this similarity is due to "independent parallel development" or to "diffusion," or both.¹ On the other hand, there are many cases of SLA in which different individuals from the same native language background attempt to learn the same second language, but with no contact with each other, nor with other speakers further along in the acquisition of the second language. To the extent that the stages on the "interlanguage continuum" are similar for *these* speakers, we can be assured that the interspeaker similarities in creole continua *could* be the result of "independent parallel development." (And of course, Schumann and Stauble do refer to earlier SLA studies, by themselves and other scholars, which show such similarities between different, independent, second language learners.)

If Alleyne's suggestions necessitate some modification of DeCamp's classic decreolization model, so also do the observations of Robertson (1979). Noting that a smooth continuum or transition seems to exist between creole *Dutch* and standard *English* in those parts of Guyana where the former is spoken, Robertson has suggested that DeCamp's first criterion is unnecessarily restrictive. Robertson notes that even if the creole and the standard are *not* lexically related, "the creole may undergo a process of relexification . . . and may participate in the general decreolization movement towards a new target" as long as the creole speakers have the potential for social mobility and are subject to acculturative pressures of the type which DeCamp's second criterion mentions.

Now if DeCamp's first criterion may be relaxed along the lines that Robertson has suggested, this would of course bring the decreolization process and the ordinary cases of SLA even closer, for the latter typically involve situations in which the source language and the target language are *not* lexically related.²

In addition to the preceding points, which have to do mainly with ways in which the decreolization model used by Schumann and Stauble might be updated, I wish to propose now some new ways in which it might be fruitfully extended. Even though the model views decreolization as the process by which a *bilingual* creole/standard language situation is transformed into a creole/standard *continuum*, it provides no stage-by-stage hypothesis about the way in which this might happen. One obvious hypothesis (ignoring, for the sake of argument, the implications of Alleyne, 1971) would be that of Table 1, in which the community is shown as gaining yet another lect in the direction of the standard at each successive stage, as the creole speakers gain increasing contact with the standard language.³

Note, however, that while the community's "decreolization" between time 1 and time 4 would undoubtedly involve language acquisition, each new lect serves as an *addition* to the community's linguistic repertoire rather than a *replacement* of any earlier lect. In individual second language acquisition, however, progress along the continuum seems to involve replacement of one interlanguage grammar with another, rather than addition to or expansion of the grammar of an earlier stage. Now it could be argued that this kind of difference between decreolization and SLA only arises because we are taking a community perspective for the former and an individual perspective for the latter. But as I've noted in passing, it is precisely

this difference in perspective that characterizes work in these respective fields, and it is one that needs to be borne in mind when comparisons are being drawn between their theoretical models and research findings.

Furthermore, for places like Guyana and the Sea Islands (Bickerton, 1973, and Rickford, 1979, for the former; Rickford, 1974, and Jones-Jackson, 1979, for the latter), where decreolization is usually assumed to have been on-going for some time, but where the creole pole still appears to be fairly similar to that of earlier generations, the preceding model is quite inadequate. For instance, a comparison of Guyana in the 1920's and the 1970's might show no change in the nature of the varieties represented, only a change in the relative numbers of people speaking each one. What we need instead is a quantitative model which indicates that the primary impact of decreolization might be in the declining proportion of people who speak the creole or basilectal variety, rather than in any decline in the "purity" of that variety itself. One way of integrating this kind of model with the one portrayed in Table 1 would be to see its successive stages as developments (4.1, 4.2, etc.) of stage 4 in Table 1, as shown in Table 2.

It should of course be obvious that if we took Alleyne (1971) seriously, there would be nothing like stages 1, 2, and 3 in Table 1, and decreolization would have to be seen as beginning from stage 4.1 of table 2.⁴

A final limitation of the decreolization model which Schumann and Stauble use is that it makes no provision for the situation that many people conjure up immediately when they think of decreolization—that is, the disappearance of the extreme creole or basilectal end of the continuum, followed by the disappearance of lower mesolectal varieties adjacent to it. This situation can be illustrated with a model like that of Table 3, which can be conceptually integrated with the model of Table 2 if we assume that stage 5 is a development out of stage 4.4, reached when the percentage of speakers at the basilectal end has dropped to near zero. Stage 5 would be a qualitatively new stage, at which the creole or basilectal variety atrophies for want of speakers—a process reenacted several times up the line, as in stages 6, 7, and 8. It is assumed, however, that between each of these qualitatively different stages, there would again be substages in which the leftmost lects show a quantitative decline in the number of speakers who use them; the substages would also be characterized by replacements of creole by standard forms (followed by restructurings?) in an increasing number of linguistic subcategories and environments.

Note, incidentally, that we could probably place the American Black English-speaking community at about stage 7 in this kind of model, and the Barbadian English-speaking community at about stage 6, if it is assumed that both of these communities possessed a truly basilectal creole variety at an earlier stage.⁵

It hardly needs saying that the model of Table 3, while fully representative of one popular sense of decreolization,⁶ and necessary to account for "decreolized" varieties like American Black vernacular English, would be similar, not to language acquisition, but to language death (Dorian, 1980). What might be less obvious is that communities might remain at a 4.3 or 4.4 stage for generations without passing to stage 5, partly because new creole speakers are born everyday with little

Table 3. A Model of That Type of Decreolization Which Involves the Disappearance of the Basilectal and Mesolectal Varieties

	Lower-		Mid-		Upper-	
	Basilect	Mesolect	Mesolect	Mesolect	Acrolect	
Stage 5		X	X	X	X	X
Stage 6				X	X	X
Stage 7					X	X
Stage 8						X

motivation or opportunity for decreolization, and partly because speakers who do manage to acquire "higher" lects nevertheless retain emotional ties with the basilect, and may shift into it (or into lower mesolects close to it) from time to time to signal informality, indicate solidarity with friends or fellow workers, make political statements, or perform numerous other "acts of identity" (LePage, 1976). The implications of this on the individual level are explored in the next section.

COMPARISONS BETWEEN SLA AND DECREOLIZATION AT THE LEVEL OF THE INDIVIDUAL

One of the difficulties one encounters when attempting to compare SLA and decreolization at the level of the individual is that while there exist several excellent longitudinal studies of SLA (such as Cazden et al., 1975; Wode, 1976; Felix, 1977; Molony, 1977; and Schumann, 1978a), there are not comparable studies of decreolization. For instance, Bickerton (1975)—the best-known study of decreolization—contains several fascinating observations and hypotheses about the nature of individual competence. However, its claims about the processes of change or development which individuals undergo in decreolization are not based on changes in the output of one individual (or more) sampled at several different times or stages of development, but on the different output of different individuals sampled at what was essentially *one* extended period of time (approximately the period between 1968 and 1970). This is also true of Edwards (1975), Washabaugh (1974, 1977), Escure (1979), Rickford (1979), Robertson (1979), and as far as I know, virtually every other existing study of decreolization or variation in a creole continuum.⁷ The only possible exception is a paper by LePage (1980), which, although it does not include systematic and detailed data of the type provided in Schumann (1978a) and the other SLA studies, does include an impressionistic report of the ways in which two Belizean Creole speakers appear to have changed over the course of eight to twelve years.

Now while cross-sectional studies (of the type so common in decreolization studies) can be quite insightful, they can also be frustratingly uninformative on some points and potentially misleading on others. For instance, when we ask a direct question about the extent to which individuals jettison lower lects as they acquire higher ones, it is possible to introduce concrete evidence of change in real time to answer this question for SLA (the longitudinal data in Stauble, 1978,

for instance), but not so for decreolization. With respect to the potentially misleading aspect of cross-sectional, nonlongitudinal data, Meisel et al. (1981) have noted that a sample of an individual's speech at one point in time may fail to give evidence of a structure which had appeared earlier and may reappear later but may have declined in use or disappeared temporarily at the time the sample was taken.⁸ These and other limitations of cross-sectional data mean that it is difficult to discuss what individuals do in decreolization with anything of the confidence with which researchers appear able to speak about what second language learners do in SLA—at least in the cases for which there is good longitudinal data.⁹

Nevertheless, I propose to draw on my native and research experience in the Guyanese Creole continuum to suggest what seems to be possible in decreolization at the level of the individual, and to compare this with what Schumann and Stauble have indicated to be true of SLA. And interestingly enough, it is the *differences* rather than the *similarities* between the two processes which are more apparent to me.

For instance, I believe that the pattern by which individuals decreolize is actually more like that of Table 4 (which is identical with the community pattern of Table 1 except that the acrolect is not introduced until the last line) than like that of Table 5 (which is presumably the pattern of normal SLA for individuals).¹⁰

The point is that while movement along the continuum may involve language acquisition in both cases, decreolization involves *extensions* of one's linguistic repertoire from an earlier stage while normal SLA involves *replacements*. In the decreolization process, I will hypothesize, individuals retain the competence in the basilect (or any lower lects) that they had at a previous stage, and add to this their newly acquired competence in a higher lect. In SLA, however, so far as I can tell, individuals do not retain the interlanguage that they had at an earlier stage when

Table 4. One Possible Model of Individual Decreolization (By Extension)

	Basilect	Lower-Mesolect	Mid-Mesolect	Upper-Mesolect	Acrolect
Stage A	X				
Stage B	X	X			
Stage C	X	X	X		
Stage D	X	X	X	X	
Stage E	X	X	X	X	X

Table 5. Another Possible Model of Individual Decreolization (By Replacement)

	Basilect	Lower-Mesolect	Mid-Mesolect	Upper-Mesolect	Acrolect
Stage A	X				
Stage B		X			
Stage C			X		
Stage D				X	
Stage E					X

they go on to another one, but instead replace it with a new interlanguage or a higher lect which is a closer approximation to the second language which is their goal.

One reason for this is that the basilect of the decreolizing speaker is not simply his or her "first approximation" to a second language, but his or her *native language*. Although it may become rusty through disuse over long periods of time, it can never completely disappear, I would suggest, at least not in the speaker's own lifetime, and not if there are opportunities to hear it spoken from time to time in the community.¹¹ For the ordinary SLA speaker, however, what Schumann and Stauble call the basilect is no native language: the native language is Spanish or German (to take two arbitrary possibilities) and the basilect is the first approximation to English or French or whatever is the target language of the SLA process. This is of course one obvious difference between the decreolization continuum and the SLA one that Schumann and Stauble create by analogy.¹²

Another difference between the two processes is that the decreolizing speaker may not *want* to jettison his or her basilect completely, even if this were possible. After all, he or she may live in a community where some of his or her closest friends and relatives may be restricted to the basilect, and shifting back into it may be an important means of reaffirming bonds of solidarity with them even though it may be valuable to the individual to add higher lects to his or her repertoire to gain access to better jobs and so on. I found evidence of this type in my (1979) study of the Guyanese Creole continuum, in terms of the ratings that respondents gave to basilectal, mesolectal, and acrolectal speech samples in a matched-guise test. On the job scale, estate (or lower class) respondents, who spoke basilectal/lower mesolectal varieties themselves, agreed with nonestate (or lower-middle class) respondents (whose speech was upper mesolectal/acrolectal) in associating acrolectal speech with the highest status jobs and basilectal speech with the lowest, as shown in Figure 1. But on the friend scale, the ratings of the two social classes were reversed, with the estate classes rating the basilectal speaker *most* likely to become a friend and the nonestate classes rating the basilectal speaker *least* likely to become a friend, as shown in Figure 2.

By contrast, individuals like Jorge and Juan, two of the consultants whose SLA data informed Schumann and Stauble's study, appear to have no similar attachment to the basilect and no comparable motivation for shifting into it once they are further along on the SLA continuum. Jorge and Juan were not in their native Colombia when their SLA was being monitored, but in the United States, in "an American public school where the entire curriculum was in English" (Stauble, 1978:35). The pressure within the school, and within the larger American environment, must undoubtedly have been to use English (the more and the better, the better), but to the extent that Jorge and Juan needed to reaffirm their Colombian solidarity with relatives and friends, they could have done so by using their native dialects of Spanish rather than their first approximations to English (their basilect). For individuals like these, there would appear to be little motivation to linger at any lower stage of the interlanguage continuum, or to return to it once a more advanced stage of competence had been attained.

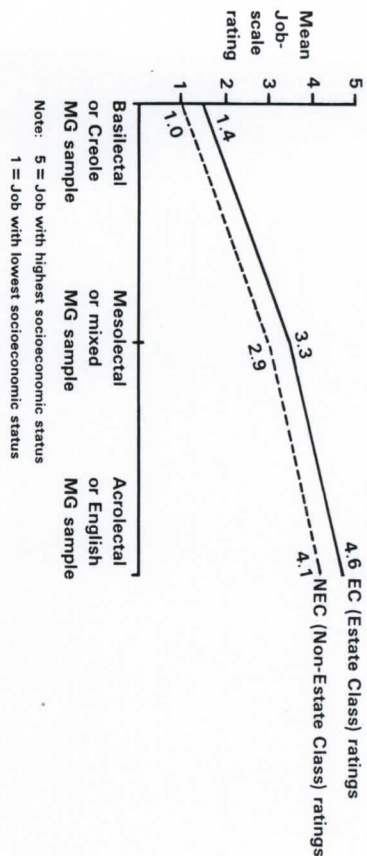


FIGURE 1 Mean Ratings of Matched-Guide (MG) Samples on the Job Scale

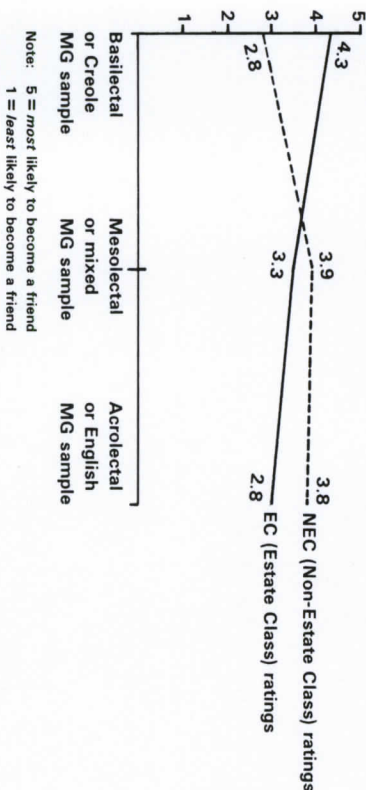


FIGURE 2 Mean Ratings of Matched-Guide Samples on the Friend Scale

This kind of difference between SLA and decreolization may also explain why it is possible for an individual to progress from a competence confined to the beginning of an interlanguage continuum to one pretty close to the end in *ten months* in ordinary SLA—as seems to have been the case with Jorge and Juan (Schumann, 1978a). I can think of no parallel cases in decreolization. Even if a speaker were to extend the range of his or her competence in the direction of the acrolect this dramatically in ten months, he or she would still be capable of shifting into lower lects and may well have the motivation to do so from time to time.

The only cases I know of in decreolization which show a transformation of linguistic competence as dramatic as that which Jorge and Juan show over ten months involve at least two *generations* (parents and their children). A typical case might be that of the Seymour family in Cane Walk, Guyana. Mrs. Seymour speaks a very strong basilectal/lower mesolectal variety of Creole, but isn't very good at upper mesolectal or acrolectal speech. Her husband, Mr. Seymour, whose first job had been as a cane-cutter, can speak a basilectal/lower mesolectal variety of creole too, but he rarely uses this in the interview situations, and apparently rarely uses it

at home. He is a relatively "big-time" contractor now, with many occupational and other contacts outside the village. He generally uses an upper mesolectal/acrolectal variety in his interviews with me, and it is primarily this which he has passed on to his youngest daughter, Katherine. Katherine, who goes to school at one of the country's best secondary schools in the capital city of Georgetown, speaks an almost flawless acrolect, and, interestingly enough, seems *incapable* of using the basilectal and lower mesolectal varieties of creole that her parents can use. In an experiment in which she was asked to "correct" English sentences to creole ones, she consistently came up with mesolectal forms and structures rather than basilectal ones (*en* as negator, for instance, instead of *mɔ*), and her mother laughingly commented in the background that "She can't . . ." (i.e., speak the real Creole). The gap between Mrs. Seymour and Katherine, insofar as it reflects real differences in *competence*, and inescapable evidence of decreolization at work, can be observed in other parent/offspring situations, but not in the same individual at two or more points of time.

It is interesting to compare the longitudinal evidence of LePage and Tabouret-Keller (in LePage, 1980) on this point. The authors report on two Belizean Creole speakers: GM, who was first interviewed in 1966 when she was 12, and who was subsequently reinterviewed in 1970 and 1978; and DGL, who was first interviewed in 1970, when she was 12, and who was subsequently reinterviewed in 1978.

With respect to GM, LePage and Tabouret-Keller conclude that, at the most recent stage, she "no longer commands the conservative creole and the broken up Spanish of her childhood so confidently." They note that the Anansi story that she had told in 1970 had a "far greater admixture of more standard forms" than the one that she had told earlier, in 1966; and that by 1978, when she "has ambitions, feels a much greater weight of responsibility," she "refers to Anansi stories and such like as things she used to enjoy as a child." The authors note that GM wouldn't tell an Anansi story on the last recording occasion, saying that she was "finished with all that kind of thing."

The second individual, DGL, also appeared to have changed over the eight-year period, in her interview speech at least. On the more recent occasion, the authors report, she was a teacher, and "much more on her guard linguistically" than she had been in 1970 when she'd been one of the "broader creole speakers." LePage and Tabouret-Keller add, however, that other Belizeans who had settled down as agricultural laborers or as factory workers had changed less.

In interpreting these reports, it needs to be borne in mind that what LePage and Tabouret-Keller are dealing with is *performance* data. By their own admission, what they have gathered is socially marked data which "reflects the groups which the individual believes exist in his society and his desire to be identified with or distinguished from them." Their impressions of how their informants have changed over the various time periods are based on the evidence of what these informants *did* in their interviews, not on what they *normally do* in their everyday lives, and still less on what they could do (i.e., the limits of their competence, which, insofar as it refers to one's native language, is felt by LePage and Tabouret-Keller to be "inaccessible" to the linguist).¹³ As a result, the most we

can conclude from the evidence of GM and DGL's reinterviews is that their awareness of the social prestige and stigma associated with the different varieties of Belizean speech seems to have increased and that they are (apparently) capable of using lects closer to the standard English end of the continuum (or at least lects involving more mixture with standard forms) than they had been previously. We *cannot* conclude—unless LePage and Tabouret-Keller have employed methods intended to assess the competence of their informants as far as possible in the direction of *both* poles of the continuum—that either GM or DGL have lost their *ability* to use more creole or basilectal varieties than they do in the interview setting, and it seems to me unlikely that this would have happened.¹⁴

In short, LePage and Tabouret-Keller's study provides no valid basis for me to revise my hypothesis that speakers in a creole continuum move upward to "higher" lects by expanding their linguistic repertoires rather than replacing one lect by another. Furthermore, one consequence of this is that decreolizing speakers might be expected to have a broader interlinguistic competence than the average second language learner who is not in a decreolizing situation. Instead of being "fossilized" at *poitins* on the interlanguage continuum, as Schumann and Stauble seem to suggest, decreolizing speakers might typically be expected to control *ranges* and to have more room for stylistic maneuver.

If we simply considered receptive competence, this would appear to be obviously true, since Bickerton (1975:196) has noted that Guyanese speakers, regardless of their productive competence, seem capable of processing/understanding virtually every variety across the creole continuum there.¹⁵ As Bickerton (1975:176) also notes, this is not true of normal second language acquisition (even though receptive usually exceeds productive competence even here).

Evidence from productive competence might be more tangible, however, and the data in Table 6 is of this type. What is shown here is the relative frequency of the main basilectal, mesolectal, and acrolectal preverbal negatives used by three Guyanese speakers in two recording contexts. The first is their spontaneous interviews (SI) with a fellow member of the Guyanese speech community, and the second their expatriate reinterviews (ER) with foreigners who were *not* members of the Guyanese speech community.¹⁶

Ustad shows the most dramatic shift between the two contexts. Although a quarter of his negatives in the SI involved the basilectal form *na*, as in:

1. If deh *na* fin' yuh, dem seh, 'Well, "If they don't find you, they say, yuh loss.' (BH 8)

there are no such forms in his ER. Instead, his negatives in this latter context are drawn entirely from upper mesolectal/acrolectal stock (e.g. *don't* and *didn't*), and many of them are noncontracted, as in:

2. The church *does not* take the money. (BH 84)

Magda, who uses a broader selection of forms in both contexts, doesn't shift quite as dramatically. But note that she is like Ustad in dropping basilectal *na* altogether in her ER style (from 7% to 0%), and in increasing the relative

Table 6. Relative Frequencies of Preverbal Negating Devices Used by Three Guyanese Speakers in Two Recording Contexts

	Basilectal <i>na</i>		Mesolectal <i>en</i>		Acrolectal <i>don't</i> <i>didn't</i>	
	SI (30)	27%	ER (12)	0%	53%	20%
Ustad	SI (30)	27%	ER (12)	0%	53%	20%
Magda	SI (68)	7	ER (16)	0	55	29
Reefer	SI (19)	100	ER (68)	0	56	9
	SI (19)	100	ER (68)	0	0	0
	SI (19)	100	ER (68)	0	7	0

Note: SI = Spontaneous Interview with Guyanese; ER = Expatriate Reinterview with two Englishmen and an American. Total number of tokens for each context (n's) given in parentheses. Note too that tokens of *don't* and *didn't* are classified as acrolectal because they were all used with correct time-reference.

frequency of her acrolectal forms (from 38% to 56%). Finally, we come to Reefer, who seems at first to exemplify the SLA concept of a fossilized speaker, since *na* is the only negative form that he uses in his SI. But in his ER we see the emergence of a few *don't*s—not a large percentage (7%), but enough to demonstrate that Reefer *does* know one of the mesolectal/acrolectal forms and is not confined to the basilect.

Any impression we might have that Reefer is in fact fossilized at the basilectal level is further dispelled when we consider his performance in an experiment in which he was asked to "correct" creole sentences into their English equivalents. (This was the counterpart to the English-to-creole correction test that I mentioned when discussing Katherine's competence.) Presented with the creole sentence, "e tek knife cut am," Reefer responded, "He take a knife and cut—na cut *am*—cut *him* or *her*. It can be him or her." While he continues to use the nonstative verb stem for anterior reference (cf. Bickerton, 1975), his choice of English equivalents for the object pronoun—and his discussion of the ambiguity of the creole form which is revealed in translation—demonstrate that he *can* control higher levels of the continuum as the need or desire arises. The only trouble is that the need or desire hardly seems to arise. For Reefer spends his time almost entirely in the company of the cane cutters with whom he works and among whom he is an acknowledged leader. Their everyday speech is basilectal/lower-mesolectal Creole, and, as Reefer says in response to a question from me about when it is appropriate to "talk properly" or use "good English":

3. Dey en gat *no* time when is more
best fuh taak, yuh know, good
English, an taak properly . . . Not
wid awee dis. We used to dis, yuh
know—patois language. (FI 7)

"There isn't *any* time when it is
better to talk, you know, good
English, and talk properly . . . Not
with us. We are used to this, you
know, patois language."
(Note: patois=creole)

Even when he is talking to higher-status management officials during labour disputes, he claims that he still talks in the "patois language" because "aal awee accustom da, yuh know" ("all of us are accustomed to that, you know"). There isn't space to do justice here to the richness of Reefer's expressed attitudes toward language use, but in general it seems to be true for him (as for others in the community) that language use is a definite act of identity, with social, psychological, and political connotations. This is demonstrated in his positive response to a question about the desirability of having school texts written in creole instead of English:

4. Awee na waan dem English man "We don't want the Englishman's teachin an ting da no mo, man. teaching and so on any more, man. Dem ting da mus done. (BH 44) Those things must end."

This brings me to my final point of this section. Schumann and Stauble make the claim (apparently based on Bickerton, 1975) that, in decreolization, "The degree to which the individuals have contact with target language speakers will determine which lect they speak. Thus, depending on the amount of contact they have, speakers' lects fossilize at various stages along the continuum." Now, although there is an undeniable connection between the contacts of individual speakers in the Guyanese continuum and the ranges (not points) of the continuum they can cover, I would hasten to add that the connection is not as simple as Schumann and Stauble make out here, but is complicated by interspeaker differences in attitude and motivation (to take only two relevant dimensions). Reefer, for instance, undoubtedly has more contacts with acrolectal or standard English speakers than other fieldworkers on the sugar estate—precisely because he is a leader, an intermediary in labor disputes, and so on. He can also use mesolectal, even acrolectal, forms and structures. But he apparently *chooses* not to do so, indicating that "contact," by itself, is not enough.

The interesting thing is that both Schumann and Stauble, in earlier publications which they did individually, have demonstrated an awareness of the rich interplay of social and psychological factors that affect language learning and performance. Although I could find equally good quotes from Stauble (1978), I will cite one relevant quotation from Schumann (1978b):

Even when there is sufficient social contact for second language acquisition to take place, for attitudinal and affective reasons there may be such psychological distance that "input" generated in the contact situation never becomes "intake" for the learner. (In Andersen, 1979)

If we can replace the simplistic reference to "contact" that Schumann and Stauble make in their paper in this volume with the richer set of constraining factors that they have both identified previously, I would wholeheartedly agree that this is indeed one area where decreolization and SLA are similar.¹⁷ In particular, I would wholeheartedly agree with the point made in the quotation from Schumann (1978b), except that I would probably prefer to say that psychological distance might prevent "input" from becoming "output" (instead of "intake"). The reason for suggesting this change is that the decreolizing speaker often has a wider compe-

tence than his/her everyday performance indicates—a point that I have tried to make several times in this section.

NEGATION IN DECREOLIZATION AND SLA

Schumann and Stauble's discussion of negation in decreolization and SLA is, as I just noted, central to their argument that these are parallel processes. The point of their discussion of this area of grammar is to establish that, in the course of both processes, change takes place initially by means of *replacement* of surface forms, followed by subsequent *restructuring* of underlying grammar and semantics. Without recounting all the details, I wish to say that I thought this point was well made. However, I felt that it could have been made even more interestingly and convincingly if the authors had drawn on quantitative data that they themselves had used in the earlier studies on which this paper was based.

Table 7, for instance, is based on data in Table 1 in Stauble (1978:36), showing Jorge's negation development over the ten-month period in which his acquisition of English (from a Spanish-speaking base) was studied. Table 7 is a simplification of Stauble's Table 1 data in two respects: (1) it is based on Jorge's data in only five of the twenty tapes—one for each of the SLA stages;¹⁸ (2) it is restricted to preverbal negation only, ignoring data on *no* and *not* phrases and negation in the copula and auxiliary, and collapsing the five columns of the original Table 1 which deal with preverbal *no*, *don't*, *doesn't* and *didn't* into the three shown in Table 7.

This simplification helps us to see the principal stages in the acquisition of English negation in stark relief. Compare the categorical use of preverbal *no* in the baslang with the categorical use of analyzed *don't doesn't*, and *didn't* in the acrolang, and note the neat intermediate stages, in which the *unanalyzed* equivalents of these forms are first introduced variably with preverbal *no*, then become categorical, and finally become variable again as they prepare to yield to the analyzed forms. The data could not have been better suited for illustrating Schumann and Stauble's point about the nature of the acquisition of negation in English; and the marvel, of course, is that it is not hypothetical, but factual, representative of the "living" longitudinal data which SLA scholars have at their disposal, and which I personally find so exciting.

Table 7. Jorge's Negation Development in Terms of Three Main Preverbal Negating Devices (SLA data)

Stage	Type	(n=)	No-V	Unanalyzed don't, doesn't, didn't	Analyzed don't, doesn't, didn't
Baslang	3	(20)	100%	0%	0%
Lower-Mesolang	5	(48)	42	53	0
Mid-Mesolang	7	(51)	0	100	0
Upper-Mesolang	17	(48)	0	8	92
Acrolang	20	(61)	0	0	100

Note: Adapted from Stauble (1978:36), Table 1.

For decreolization, it is possible to construct a comparable table, but as usual, we must depend on cross-sectional data. In Table 8, I set out the outputs of five Guyanese speakers whom I recorded between 1974 and 1976. I am drawing here on the data of their spontaneous interviews only,¹⁹ and am representing only the frequency of the three main preverbal negative markers (i.e., ignoring, for the present, the nature and frequency of their negated copulas, modals, and the like.)

Reefer's basilectal output in Table 8 is perfectly comparable to Jorge's basilectal output in Table 7, involving categorical use of the preverbal form *na*. Katherine's acrolectal output in Table 8 is also nicely comparable to Jorge's acrolectal output in Table 7, since she is virtually categorical in her use of analyzed *don't* and *didn't*. The intermediate mesolectal outputs of Sultan, Magda, and Ustad are not as neat as the mesolingual outputs of Jorge in Table 7, but there is a regular increase in the relative frequency of the analyzed *don't/didn't* variants as one goes from the lower-mesolectal Sultan to the upper-mesolectal Ustad.

Tables 7 and 8 will, I hope, enhance the authors' already well-made point that there are persuasive parallels between decreolization and SLA in terms of the central developments and changes that take place in the negation system. Although I agree that the general point is well established, I wish to raise some questions and make some additional observations on a few matters of detail.

In their survey of negation at the basilectal level of Guyanese Creole, Schumann and Stauble, drawing on the analysis of Bickerton (1975), note that the modals sometimes follow the general preverbal negation rule (*NA + kyan, NA + mos*), but sometimes have variants "modelled after the target language forms" (*kyan, mosn*). The impression that one receives from this discussion is that these two methods of negating the modals occur with approximately equal frequency, and that the *kyan* and *mosn* forms are unanalyzed—not conceived of by their users as consisting of modal + neg, but as monomorphemic chunks which are replacements but not restructurings of monomorphemic *na*.

With regard to the relative frequency of the two methods of negating the modals in the basilect, Bickerton himself (1975:43-44) does not give us any statistics, but he does suggest that the forms with preverbal *na* are not only older, but *rarer*, and that the *kyan/mosn* forms are the current basilectal norm. This would certainly agree with the indications of the preliminary analysis I have made

Table 8. Outputs of Five "Decreolizing" Speakers in Three Major Negating Categories

	(n=)	Na-V	Unanalyzed en	Analyzed don't, didn't, didn't
Reefer (Basilect)	(19)	100%	0%	0%
Sultan (Lower-Mesolect)	(57)	66	9	25
Magda (Mid-Mesolect)	(68)	7	55	38
Ustad (Upper-Mesolect)	(30)	27	0	73
Katherine (Acrolect)	(50)	0	4	96

Note: Based on the data of five Guyanese Creole speakers whose pronominal variation is examined in Rickford (1979).

of basilectal negation in my Guyanese Creole data; Reefer, for instance, uses *kyan* and *mosn*, but no *na* + modal forms.

With regard to the morphemic analysis of the *kyan* and *mosn* forms, Bickerton (ibid.) is equivocal. When he first introduces them, on pages 43-44, he suggests that the modals represent a "point of origin" for the English negative-placement rule according to which the negative is inserted "immediately after the first morpheme in the verb phrase, irrespective of subject-position," and that it is from this starting point that the English negative placement rule spreads to other areas/environments. Subsequently, on page 91, he seems to be retaining this bimorphemic analysis, because he refers to "post-modal-*n*" as a negative marker. However, still further on (pages 95-96), he invites us to assume "that Guyanese speakers acquire all quasi-English negators (*doon't*), *didn't*), *en*, not to mention *kyan*, *mosn*, and the rest of the negated modals) as indissoluble forms, monomorphemic sentence negators which are in process of replacing *na*." It is the latter position to which he finally seems to be persuaded, primarily because it "has parsimony on its side." As Bickerton notes: "If we suppose that the forms can be analyzed, we must then assume that the average mesolectal speaker has two negative placement rules, one for *na* and *en* (pre-VP placement), and one for *n't* (post-Tense placement), and that they operate without confusion . . . If, however, the forms are treated as indissoluble negative alternants of *na*, pre-VP placement can be retained for all forms of sentence negation."

I would wish to suggest very strongly, however, that parsimony aside, basilectal and mesolectal speakers of Guyanese Creole must be credited with a bimorphemic analysis of *mosn* and *kyan*. They certainly use the positive forms—*mos* and *kan*—frequently and appropriately enough, usually with deontic rather than epistemic function,²⁰ and in any field methods class, students would be penalized for failing to analyze *mosn* and *kyan* as bimorphemic (modal + negative) under the circumstances. Where the system does differ from Standard English is in the fact that the negative is incorporated in the modal, initially, as a bound morpheme, rather than a free one (*must not*), and that its form is *n* in the case of *mos*, and a combination of increased palatalization, vowel length, stress, and heightened pitch in the case of *kan* (which could not have been negated by postmodal *n* because the positive form already ended with a nasal). The significance of a suffixed/simultaneous negation rule for the modals, alongside the main preverbal negation rule, is not that great initially, because the stock of modals at the basilectal level is small (for instance, there is no *may*, *will*, or *shall*²¹) and the rule apparently applies only in the modal environments. By the time we are into the mesolect, however, the rule has become more productive, yielding other modal + neg forms (*would + n*, *should + n*) and helping to spawn other nonmodal negatives (*didn't + n* from preverbal *did*, *does + n* and *don* from preverbal *does*—the latter involving some suppletion). It is perhaps only at the highest acrolectal levels that the bimorphemic analysis of these forms will correspond exactly to that of Standard English (*mosn* as a contraction of two free morphemes, *must* and *not*, *doen't* as a combination of the third singular present-tense form of support *do*, and *not* as a

negative), but we lose nothing by recognizing that a rudimentary postverbal negation rule is alive and well (albeit restricted) right from the basilectal level. In fact, far from losing anything, we gain the value of a perspective in which the basilectal, mesolectal, and acrolectal systems are not quite as discrete as they would otherwise appear, and in which each bears a hint of what is to come at the next stage or level. This analysis is also quite in keeping with C. J. Bailey's (1973) wave model, according to which linguistic change begins in a very limited environment and spreads over time by acquiring new environments as well as new speakers: new points within the language and in geographical/social space.

I have a similar objection to the monomorphemic analysis of *don(t)* and *didn(t)* at the mesolectal level of Guyanese Creole. As Schumann and Stauble state, these forms (along with *en*), "are modelled on target language forms but . . . are not carriers of tense and negation but exist as wholistic chunks which are variants of preverbal NA." Now this appears to be true of Guyanese *en* at the mesolectal level, and of *don't*, *doesn't*, and *didn't* at the mesolingual level of SLA. In the transcripts of Alberto's SLA speech in Schumann (1978a), there are repeated instances of these latter forms being used contrary to the expected English time reference. With respect to Guyanese *en*, Bickerton (1975:99) notes that as a *have/be* negator, it is "employed indifferently with past and non-past reference"—a fact that is borne out in my own data on mesolectal Magda (but not acrolectal Katherine, whose *en*'s are always present tense).²² But a careful search, of both Bickerton's (1975) Guyanese data and my own, does not support the assertion that *don(t)* and *didn(t)* are tenseless at the mesolectal level. On the contrary, they appear to be used, right from the beginning, in the way that Schumann and Stauble suggest will be found only when the final barrier between mesolect and acrolect is being breached, i.e., with *didn't* "restricted to past reference" and "*don(t)/doesn't* to non-past reference." In Bickerton's Table 3.8 (1975:93-94), for instance, in which the relationship of Guyanese mesolectal negatives to equivalents in English is shown, mesolectal *didn* is shown as being used for English *did not* 85% of the time, for English *was/were not* 12% of the time, and for English *does not* or *am/are/is not* 0% of the time (i.e., *never*!). In at least 97% of all cases, the time reference of mesolectal *didn* was clearly past, as would be expected in English. In the same table, mesolectal *doon (=don(t))* is shown as being used for English *do not* 87% of the time, and for English *did not* only 11% of the time. It is this exceptionally small percentage of "deviant" cases that is exemplified in example 11 ("Some *don't* wore a shoe before coming here") in Schumann and Stauble's paper. I should add that virtually all of the *don(t)*, *doesn't*, and *didn't* tokens in the GC data that I examined in preparing this commentary were used with the appropriate time reference—nonpast for the former two, and past for *didn't*.

This does not mean, again, that the bimorphemic analysis of these forms in the mesolect is exactly what it is in the acrolect or in Standard English. Bickerton (ibid.) explores, for instance, the fascinating possibility that they may represent negative forms of mesolectal creole categories—iterative *does*, and anterior *did*—rather than instances of "empty" support *do*. It is difficult to resolve all the possibilities here, precisely because of semantic overlaps and increasingly subtle

semantic differences between the Creole and English systems at the mesolectal/acrolectal levels. But it is at least possible to try to resolve some of these possibilities for the negatives in the decreolizing system, given that we have some data on the corresponding *positives* in the Guyanese Creole continuum. For the SLA data that Schumann and Stauble use, the possibilities for further analysis (by the reader or anyone else) are sharply limited by the fact that we don't know anything about the form of the speakers' corresponding positives. When we read, for instance, that basilingual SLA speakers show "minimal overt variation" with *don't* and *isn't*, it would be ideal to also know whether they appear to have any consistent *do*-support or copula use in their corresponding positives.

There are, of course, respects in which *both* Bickerton (1975) and Schumann and Stauble (this volume) could have enriched and extended their analyses of negation in decreolization and SLA significantly. In relation to the semantics of the forms, none of these authors discusses the *function* of the negative utterances that they have recorded, as indicating nonexistence, denial, or rejection, for instance.²³ The different functions of the negated modals are also unexplored (cf. footnote 20). In relation to the syntax of negation, double modals have not been considered (Reefer uses "yuh *might kyaan* + V") nor has the "acquisition of English negative-concord rules," a topic that Bickerton at least mentions, but does not take up in his study of Guyanese tense/aspect.

My final observation is that the "levels" of the decreolizing continuum and their representative forms are not as neatly separated—when we look at actual language use—as they might otherwise appear to be. In Table 8, for instance, note how the "mesolectal" speakers span the continuum, and remember the evidence of Table 6 that some of these speakers can vary even more in a different context and style. I have tried to make a central argument in the second section of this discussion, that decreolization and SLA might differ in that the speaker involved in the former have more room for stylistic maneuver than the speaker in the latter. But I would be happy to be proven wrong on this point, and if Schumann and Stauble could extend their analysis of negation in SLA to include stylistic variation, their comparison of decreolization and SLA would be even richer and more valuable than it already is.

NOTES

1. As I have noted elsewhere (Rickford, 1979), the terminology of competing theories of pidginization is easily extended to analogous situations in decreolization.

2. It should be noted, incidentally, that in the creole continuum the creole is the basilect (contra Schumann and Stauble's claim in Section 2 of their paper that "the lect closest to the creole is called the basilect"). In SLA, the basilect is not the first language itself, but the first approximation to the second or target language.

3. Although DeCamp (1971a) listed a number of possible corrective pressures—radio, TV, education, internal migration, etc.—few scholars have tried to refine this list further or to establish a hierarchy of influence among these factors. In my (1979) study, however, I found that education was one of the closest correlatives of level of linguistic performance, and thus, presumably, one of the strongest factors encouraging decreolization.

4. Ewart Thomas, a statistician and fellow Guyanese here at Stanford, has taken a tremendous interest in decreolization as a result of discussions between us on the subject and we are currently trying to devise some mathematical models of the process. He has already suggested a simple way of computing a quantitative mean to indicate how far along on the creole continuum a community is at any stage. To calculate it, we assign the number 1 to the basilect, 2 to the lower-mesolect, 3 to the mid-mesolect, 4 to the upper-mesolect, and 5 to the acrolect, and multiply each of these by the percentage of speakers using each lect at any one stage, and total the resulting products. For instance, using the data from stage 4.1 in Table 2: $(1 \times 33) + (2 \times 27) + (3 \times 40) + (4 \times 27) + (5 \times 27) = 450$. This total is then divided by the sum of the percentages themselves $(33 + 27 + 40 + 27 + 27 = 154)$, and the resulting figure 2.92 indicates that the mean community usage at stage 4.1 is just short of the mid-mesolect (which is 3.0). By stage 4.3 the mean is 3.37, indicating that the community has shifted even further toward the acrolect, at this stage about halfway between the mid- and upper-mesolect.

5. Note that variation is still possible at stage 8, because the mode must provide for *intralectal* as well as *interlectal* variation, although it is assumed that the former will be more limited in extent and character than the latter.

6. The common interpretation of *decreolization* as involving the diminution and loss of the creole is in line with other English words in which the *de*-prefix gives the sense of something being taken away or removed from (e.g., *deplete*, *derail*) or the sense of an earlier action being reversed (*defrost*, *decode*). These examples are from *Webster's New World Dictionary of the American Language* (2nd College Edition). The *Oxford English Dictionary* contains an even richer and more detailed set of examples.

7. In some of these studies—for instance, Rickford (1979)—the data on some individual speakers represents an aggregate of samples recorded at intervals of several months or one or two years. The problem is that since radical linguistic change seems to take longer in decreolization than in some of the documented cases of SLA which have been reported, longitudinal studies of decreolization seem to require intervals on the order of five years or more (as in LePage, 1980).

8. It may even be of course, that it is the sampling situation (interview or elicitation) that is responsible for the absence of the structure, given that the individual may have several styles (varying according to the status of the interlocutor and other factors) and given that the typical data-gathering situation itself involves a context more formal and artificial than those in everyday life (Labov, 1972).

9. This is not to say that there may not be alternative interpretations of what is observed (Meisel et al. discuss several cases of this type) but at least we have some tangible real-time observations to begin with.

10. Other models besides these two are certainly possible; for instance, one in which a speaker might control no more than two contiguous lects at a time and must "drop" the earlier of the two before going on to acquire yet another. My intention was not to explore every possible model, but to indicate that there *are* different possibilities, and to discuss some of the main ones. Note also that, although Tables 4 and 5 both begin at the basilect, the same general predictions would hold if the starting point was at some level above the basilect. For instance, if a person is born into a mid-mesolectal family, his first stage would be depicted by an X at the mid-mesolect and one at the upper-mesolect, and so on.

11. Ewart Thomas, a Guyanese who has been living in the United States for several years (see note 4) has indicated in informal discussion that it usually takes him "about three days" to "adjust" to language use in Guyana when he returns home on visits. Although the competence may be rusty through underuse in the nonnative environment, it can be easily resuscitated. I should add that students of decreolization have not yet followed students of SLA in recognizing how valuable a resource migrants might be for the study of "what happens in decreolization" (although William Stewart had made a start in this direction several years ago by including in a research proposal plans to record South Carolina Gullah speakers who had migrated to New York).

12. See note 2 on this point.

13. This dilemma, of course, is not unique to LePage and Tabouret-Keller, but is a consequence of the "Observer's Paradox" (Labov, 1972) which all investigators face. In addition to utilizing Labovian and other techniques for reducing the effect of observation, so that we get closer to what informants normally do, it is necessary to devise ways of assessing what they *can* do. In my own work on Guyanese Creole, I used "correction tests" from Creole to English and vice-versa for this purpose, with revealing results (see discussion of Katherine above and Reefer below).

14. LePage and Tabouret-Keller report, interestingly enough, that G. M. says that her little daughter "speaks only creole." One would not want to deny the possible influence of peers, but it is unlikely that G. M.'s daughter would be speaking *only* creole if G. M. herself were unable to do so and used only some closer approximation to English in the home.

15. The claim requires closer empirical investigation, but it is certainly true that receptive competence outstrips productive competence in the creole continuum.

16. The foreigners were two Englishmen (Derek Bickerton and Michael Pye) and an American (John Holm). Their assistance is gratefully acknowledged. For most Guyanese, interaction with a White American or European is still one of the most formal contexts for language use (the residue of colonialism), although a number of individuals (like Bickerton) have succeeded in overcoming the negative effects of this factor in their own fieldwork.

17. The preceding quotation would apply, for instance, in the case of Reefer referred to earlier.

18. Selection was done on the basis of sample size. Within each level or stage, the tape with the largest number of negative tokens overall was selected as representative of that stage.

19. Reefer, Ustad, and Magda appear in Table 6. The reason for using their spontaneous interview data alone is to permit comparison with Sultan and Katherine, for whom we give only spontaneous interview data here.

20. That is, to indicate obligation rather than probability, and permission rather than possibility. My interest in this distinction has been stimulated by the work of Susan Shepherd, a graduate student in linguistics at Stanford whose (1981) dissertation includes a study of the acquisition and use of modals in Antiguan Creole.

21. *Should*, however, does occur at this level.

22. Bickerton (1975) makes the point—borne out in my data—that although *en* is used regardless of present or past reference in the lower-mesolect, it tends to be restricted to the present in the upper-mesolect. This is one of the interesting semantic aspects of variation and change in the system of negation that Schumann and Stauble overlook in their report on Bickerton's findings. In general, they concentrate on changes in *form* only—a point first drawn to my attention by Elizabeth Traugott in personal discussion.

23. I am grateful to Barbara Hecht, graduate student in linguistics at Stanford, for bringing me up to date on research dealing with these distinctions conducted by Lois Bloom and others studying child language acquisition.

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