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ANALYZING VARIATION IN CREOLE LANGUAGES

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INTRODUCTION

As I prepare to discuss Gillian Sankoff's position paper for this session,[1] I am reminded of the story of the student who studied all about frogs for his zoology exam, but got a question instead about fishes. Undaunted, he began his answer by observing that, "Fishes are somewhat like frogs." Then he went on to present the material he had carefully prepared on the subject of frogs. After comparing Sankoff's presentation with my discussion, the reader may well conclude that she is dealing with fishes and I with frogs, or vice versa. For the bulk of my discussion centres around the two-page introductory section of her paper, expanding on themes which she touches on there about the nature and analysis of variation in pidgin-creole-speaking communities, emphasizing existing problems and research needs, and exemplifying these with recent research data on Guyanese and other Caribbean creoles. The issues which the rest of Sankoff's paper goes on to explore in such detail—such as the general nature of pidginization and pidgin-formation, and the socio-historical factors surrounding the crystallization of Pidgin English in the South-West Pacific—are dealt with only in a brief concluding section by me, and primarily as they exemplify aspects of the "how to analyze" problem, or its solutions.

One of the reasons for the difference in our respective emphases derives from the change which Sankoff made in her assigned title to allow for the discussion of processes as well as states, and pidgins as well as creoles. (This is explained in her opening paragraph.) This change of focus is in keeping with her rich and varied experience with Tok Pisin and the South-West Pacific and is, in my opinion, fully justified, not only by the fact that there was no session on pidginization at the St. Thomas Conference, but also by the fascinating

nature of the data and ideas which it allows her to include.

By the same token, however, I would prefer to deal rather more with creoles than with pidgins, and with the Atlantic area rather than the Pacific. Even though I was privileged to take a course on Tok Pisin some years ago (from Gillian Sankoff, no less!), I think I can claim to know a lot more about the Guyanese Creole continuum, which I have been studying, in a sense, from birth. Like the schoolboy in the above story, and like Sankoff in her own presentation, I think I can make my most valuable contribution with material that I have been associated with most intimately and most recently.

And, after all, fishes *are* somewhat like frogs...

1. THE EXTENT OF VARIATION IN PIDGIN-CREOLE COMMUNITIES

Like Hymes (1971: 299), Sankoff notes in her introduction that "the very extent and degree of variation in many "pidgin"- and "creole"-speaking communities has posed a challenge to current grammatical theory." She goes on to exemplify the kind of challenge involved by asking:

How, for example, can one write a synchronic grammar which includes a large set of closely related varieties, so closely related that they co-occur in the speech of even single individuals, so closely related that it is almost impossible, sometimes, to make discrete divisions among them, and yet so divergent that the polar varieties may scarcely be intelligible, so divergent that no one individual spans all?

To this big "How?" there is no single nor simple answer, but progress on this question will depend on the progress we make in solving the following sub-problems:

i) The problem of isolating the "varieties" in pidgin-creole continua
ii) The problem of assessing the limits of individual competence
iii) The problem of obtaining and interpreting native-speaker intuitions

There is a hint of each of these problems ((iii) in particular) in Sankoff's presentation. But they can all benefit from elaboration, and I will address each of them in turn below.

1.1 The problem of isolating the "varieties" in pidgin-creole continua

If we assume that pidgin or creole continua involve a "set of closely related varieties," as Sankoff suggests, and as many other people (linguists and laymen alike) seem to feel, then one of the analyst's first tasks is to tease the

different varieties loose from the tangle of variation.

At the simplest level, this might involve nothing more than providing examples of "different ways of saying the same thing" in the community (Labov 1972b). One of the earliest attempts of this kind, in the field of pidgin-creole studies, was made by Allsopp (1958), who listed the following nine realizations of "I told him" in the Guyanese Creole continuum:

1. ai tɔʊld hɪm
2. ai told hɪm
3. ai to:l ɪm
4. ai tɔl ɪm
5. ai tɛl ɪm
6. ai tɛl i
7. a tɛl i
8. mi tɛl i
9. mi tɛl am

This attempt was valuable insofar as it concretely exemplified the notion of a *continuum*, with an English pole (1), a Creole pole (9), and mesolectal variants in between (2-8).

However, if one attempts to relate actual variants to three or four co-existent systems, as Tsuzaki (1971) suggests is possible to do for Hawaiian English, and as Bickerton (1975: 12-14) attempts to do with Allsopp's nine variants of "I told him," one encounters grave problems about which variants (*mi, ai*, etc.) to assign to which system. As Bickerton (1975) concludes:

... it is not so much the number of systems in a "co-structure" as their presumed invariance and discreteness which makes the co-existent model inappropriate for analysing continua.

Insofar as the problem of isolating varieties is concerned, the technique of implicational scaling introduced into linguistics by DeCamp (1971) represented a significant breakthrough. DeCamp provided, in this technique, a means of sorting the variants in a continuum into a series of lects, which, while different from each other in at least one respect, were not discrete, but related as part of an implicational series, and as stages in an overall process of decreolization.[2] The scale in Table 1—described but not actually presented in DeCamp (1971: 356-7)—identifies seven idiolects in the Jamaican Creole English continuum, based on the actual outputs of Jamaican speakers.

Since DeCamp's seminal demonstration, linguists dealing with creole continua have leaned even more heavily on implicational scaling in the course of their analyses (Akers 1977, Anderson 1975, Bickerton 1971, 1973, 1975, 1977, Bickerton and Odo 1976, Rickford 1973, 1979, Washabaugh 1974, 1977, Woolford 1975). In the main, their scales differ from DeCamp's in several respects, however:

a) Recent scales tend to concentrate on the scaling of closely related items in a single subsystem at one level of the grammar (e.g., tense markers, or pronominal variants), while DeCamp's mixed variants form different levels (phonological, lexical, and grammatical).

TABLE 1

IMPLICATIONAL SCALE FOR THE JAMAICAN CREOLE CONTINUUM

Features	Idiolects					
	D	C	A	F	E	B
4	−	−	−	−	−	−
3	−	−	−	−	−	+
7	−	−	−	−	+	+
2	−	−	−	+	+	+
6	−	−	+	+	+	+
1	−	+	+	+	+	+
5	+	+	+	+	+	+

KEY to Features

	D	C	A	F	E	B
− (or "Creole") =	/d/	/t/	/d~ð/	/t~θ/	pikni	no ben	nana	nyam
+ (or "English") =	/ð/	/θ/	child	didn't	granny	eat		

Note: Based on a verbal description in DeCamp (1971: 356-357).

b) Recent scales provide for the possibility of having three, four, or even n variants within particular subcategories (e.g., *am, hi, h m* as masculine object pronoun), while DeCamp's scale was restricted to binary Creole/Standard alternatives (e.g., *no ben, was*).

c) Recent scales provide for variation at the level of the individual lect. While DeCamp's idiolects have either + *or* − under each feature, in recent scales they may have + *and* −, and any other values available, in at least some cases. (This allows for the speaker who says *am* and *h m*, instead of simply one *or* the other.)

d) Recent scales are used as a basis for the construction of anomaly-free panlectal grids, in which all of the possible isolects between the basilect and acrolect are specified, each isolect differing from the one next to it in just one respect. In turn, comparison of the implicational scale with the panlectal grid permits a precise overview of the entire continuum and the distribution of actual outputs thereon, which in turn may be explained by reference to socio-historical factors of the type which Sankoff explores in her position paper.

e) Recent scales and panlectal grids are used as a basis for specific hypotheses about the course of decreolization and the form of the rule

system, and for inferences about the nature and rapidity of change in the system, most of them according to principles of C.J. Bailey's (1973) "dynamic" wave model.

Table 2, depicting the panlectal grid which I constructed to cover the variation in nine singular pronoun subcategories in the Guyanese Creole continuum (Rickford 1979: 387), illustrates several of the characteristics of recent scales, and may be compared with Table 1 above (DeCamp's scale) to exemplify the points made above. The reader familiar with the literature on creole continua will, of course, recognise some similarities between this Table 2 and Table 6 in Bickerton's (1973) study of the same pronoun subcategories in the Guyanese continuum.[3] He or she may also recognize that most if not all of the points above represent *advances* pioneered by Derek Bickerton, either in the same study (1973: 642) or in subsequent work (1975: 202-203 in particular).

While these characteristics of recent applications of the implicational scaling technique do in general represent *advances* over the original applications proposed by DeCamp (1971), they represent a *retreat*, in one important sense, from the problem of analyzing pidgin-creole continua. What we aim to be describing when writing a grammar of a continuum is a set of *varieties* (refer to quotation from Sankoff's paper given above) which would involve combinations and interrelations of elements from different subsystems and levels (cf. Labov 1971). But what we have been increasingly restricted to is the analysis of *variants* and sets of variants within individual subsystems.

Now while it may be true that our analyses of individual variants may be more fine-grained and revealing than DeCamp's analysis of elements from a number of different levels, it remains true that people do not speak with pronoun systems alone, nor with copulas, nor with tense markers, but with combinations of all these subsystems and many more. The key problem, therefore, especially as we try to bring our analyses closer to the realities of language use in pidgin-creole continua, is to attempt descriptions of such combinations.

Progress to date has not been very encouraging. DeCamp (1973) defended his generalizing (1971) scale by suggesting that:

> One could think of general scales like mine, which mix different kinds of variables, as complexes of similar scales, each confined to one kind of variable. Thus a complex scale could be only the empirical consequence of several simple scales applying simultaneously.

However, attempts to apply this concept in actual studies of pidgin-creole continua have not met with much success. Bickerton (1973) reports, for instance, that there was not any significant correlation between his implicational scales for the copula and the pronominal subsystems in the Guyanese continuum: only ten speakers occupied similar isolects in the two scales, while

TABLE 2

(handwritten: Rate parameter, dynamic model: rℓ   −2.23   −0.21   0.54   1.90)

PANLECTAL GRID FOR DECREOLIZATION IN GC SINGULAR PERSONAL PRONOUNS
(CANE WALK DATA)

(handwritten: 3-variant subscales.)

Isolects exemplified by outputs of actual speakers (see bottom of Table, note ii)	ISOLECT No.	I 3MPOS 1=hi 2=hɪz	II 1POS 1=mi 2=maɪ	III 1SUB 1=mi 2=aɪ	IV 3NSUB 1=i 2=ɪt	V 3FPOS 1=hi 2=ʃi 3=hʌr	VI 3FOBJ 1=am 2=ʃi 3=hʌr	VII 3MOBJ 1=am 2=hi 3=hɪm	VIII 3NOBJ 1=am 2=ɪt	IX 3FSUB 1=hi 2=ʃi	Rule Changes occurring in each isolect
----	1.	1	1	1	1	1	1	1	1	1	----
----	2.	1	1	1	1	1	1	1	1	12	1→12/3FSub
----	3.	1	1	1	1	1	1	1	12	12	1→12/3NObj
Yes	4.	1	1	1	1	1	1	12	12	12	1→12/3MObj
----	5.	1	1	1	1	1	12	12	12	12	1→12/3FObj
Yes	6.	1	1	1	1	12	12	12	12	12	1→12/3FPos
Yes	7.	1	1	1	12	12	12	12	12	12	1→12/3NSub
Yes	8.	1	1	12	12	12	12	12	12	12	1→12/3ISub
Yes	9.	1	12	12	12	12	12	12	12	12	1→12/3IPos
Yes	10.	1	12	12	12	2	12	12	12	12	12→2/3FPos
----	11.	1	12	12	12	2	12	2	12	12	12→2/3MObj
Yes	12.	1	12	12	12	2	2	2	12	12	12→2/3FObj
----	13.	1	12	12	12	2	2	2	2	12	12→2/3NObj
Yes	14.	1	12	12	12	2	23	2	2	12	2→23/3FObj
----	15.	1	12	12	12	2	23	2	2	2	12 2/3FSub
Yes	16.	1	12	12	12	2	23	23	2	2	2 23/3MObj
----	17.	1	12	12	12	23	23	23	2	2	2 23/3FPos
Yes	18.	12	12	12	12	23	23	23	2	2	1 12/3MPos
Yes	19.	12	12	2	12	23	23	23	2	2	12 2/1Sub
----	20.	12	12	2	2	23	23	23	2	2	12 2/3NSub
----	21.	12	2	2	2	23	23	23	2	2	12 2/1Pos
----	22.	2	2	2	2	23	23	23	2	2	12 2/3MPos
----	23.	2	2	2	2	23	3	23	2	2	23 3/3FObj
Yes	24.	2	2	2	2	23	3	3	2	2	23 3/3MObj
Yes	25.	2	2	2	2	3	3	3	2	2	23 3/3FPos

Source: Rickford (1979: 307)

Notes: i) Arrows and underlined numbers depict the hypothetical course of decreolization in this subsystem, highlighting the locus of changes in successive isolects.

ii) Speakers whose outputs represent each isolect: 4=Reefer; 6=Darling; 7=Irene; 8=Nani, Rose, and Derek; 10=Raj and Sultan; 12=Florine; 14=Granny; 16=Magda; 18=Mark, Ustad, Sheik, Oxford, Kishore, Claire and Radika; 19=Seymour; 24=Bonnette; 25=Kishere.

thirteen did not. Comparisons of my (1979) scales for morphological and phonological variants in the pronoun system within the Guyanese continuum do not show any perfect correlations either.

We are thus no closer to the analysis of varieties as a whole than DeCamp (1971) was. This is one of the major unresolved challenges with which pidgin-creole continua present us.

1.2 The problem of determining the limits of individual competence

The quotation from Sankoff's paper given above refers to varieties which are "so closely related that they co-occur in the speech of even single individuals," and yet are "so divergent that no one spans all." These remarks imply that issues about the nature and limits of individual competence are crucial to the description of variation in pidgin-creole speaking communities, as, presumably, they should be in communities of any type. And yet we do not seem to find enough concern about these issues in conference presentations and in publications. We continually hear claims that a speaker is confined to one or more lects, or does not "have" some form or the other, when the data base is limited to a few minutes of recorded speech caught "on the fly," as it were, or elicited in a fairly formal interview.

Obviously there must be some upper limit on what contributes adequate data for the investigation of individual competence. Otherwise we could go on forever collecting data, always afraid to stop lest we miss something valuable in the process. But we need some lower limit too, some minimum level which ought to be satisfied before we consider our data a reliable and valid indication of what individual speakers can (and cannot) do. Maybe for a start we can accept the need to have about one hour of recorded interaction per individual speaker, and try to move in the direction of the "two, three, or even four hours of speech" which Labov (1972b) now sees as essential for a "detailed" study.

Even with this volume of speech we will need to prestructure our modules and questions to help us to get enough "natural" occurrences (at least five per cell) of the features we are interested in (cf. Labov 1972b, Rickford 1975). And in general we need to take far more account, in pidgin-creole studies, of the many innovations in sociolinguistic methodology pioneered by Labov and others. For instance, we need to extend our semi-formal interviews in the direction of vernacular or more informal speech by the use of peer group recordings and the like, and in the opposite direction by the use of reading texts, correction texts, and other formal devices (Labov 1972a). As the dates of these references indicate, these techniques have been available to linguists for a number of years. But we have not exploited them as fully as we might in pidgin-creole studies.

As an indication of the effects which different kinds of data might have on our impressions of the competence of individual speakers, and on our resultant

analysis, I wish to cite the example of Reefer, one of the leaders of the sugar workers whom I recorded on several occasions in the course of my recent study of the Guyanese Creole continuum (Rickford 1979). On the basis of several hours of speech in peer group sessions and semi-formal interviews, Reefer's pronoun pattern was classified as equivalent to isolect 4 in the panlectal grid in Table 2; that is, as restricted to the basilectal creole variants in all but the third masculine and neuter object, and third feminine subject subcategories. After my own interviews with him were completed, however, I arranged to have Reefer reinterviewed by two Englishmen and an American, and in this unusually "high" context, he came out, like other speakers who were similarly reinterviewed, with some forms he had not used before. The most striking case was the thirteen tokens of acrolectal a_1 as first person subject which he used on this occasion.[5] In *135* previous occurrences of this subcategory in his semiformal and peer group recordings, he had always used *mi* as subject, and I might almost have sworn, before doing the expatriate re-interviews, that the form a_1 lay outside of his productive competence. He also came out with tokens of acrolectal $h\iota m$ as masculine possessive where he had used only *hi* before.

On the basis of these new forms, Reefer's speech would have to be classified as representative of isolect 8 instead of 4. And in a controlled interview where he was asked to correct Creole sentences into English and vice versa, without any prompting, Reefer showed that his competence extended to almost the entire spectrum of singular pronoun variants. So much so, in fact, that the discontinuities in production on which implicational scaling depends were seriously called into question.

Not everyone's performance was equally dramatic. But it was generally true that the impression we had of each individual's competence varied somewhat according to the type of data collecting instrument we used (peer group recording, interview, written questionnaire), and the occasion on which it was used.[6] Ideally, we need to have a broad spectrum of different kinds of data—of the type we have for Reefer—for each speaker.

There is another aspect to this problem of determining the level of individual competence. This is the question of receptive or hearer competence and its relation to the productive or speaker competence about which I have been talking so far.

For instance, referring to the Guyanese continuum, Bickerton (1975:196) has claimed that "most Guyanese speakers, whatever their productive capacity, can process receptively any variety within the continuum," and he uses this as one of his arguments for assuming individual polycompetence. However, I am not sure that this claim of an all-encompassing receptive competence is valid. From personal experience and the reports of other people, I am not sure it is always true. I would certainly like to see some clear evidence from intelligibility tests on this subject (assuming that intelligibility tests are capable of giving clear evidence!). In any case, we still have to devise means of resolving

apparent differences between receptive and productive competence, and to decide how such differences will affect our analysis. With the exception of Bickerton (1975), creolists have *not* been attending to this kind of problem.

1.3 The problem of obtaining and interpreting native-speaker intuitions

Sankoff—quoting Mühlhäusler (1974:53) on this point—raises the problem of not having native-speaker intuitions to work with in the analysis of a pidgin (on the basis of the fact that pidgins are not supposed to have native speakers). A slightly different problem has been raised with respect to *creole* continua by Derek Bickerton (1975: 201) who notes that, "... there is one type of informant common in creole societies who will accept virtually any sentence he is given, to the inevitable bewilderment of the enquirer."[7]

While one cannot deny the reality of these problems, there can be nodoubt, for creole societies at least, that native-speaker intuitions must be and are available. It is our means of getting at them which need to be improved. This much is suggested by the anecdote in Reisman (1970: 138) in which one Antiguan speaker shows a conscious awareness of the semantic contrast between short and long vowels in Antiguan Creole. And in my recent study of Guyanese Creole, (Rickford 1979), I was pleasantly surprised by the clarity and consistency of native-speaker intuitions with regard to:

i) the social evaluation of samples of three primary speech varieties (English, Creole, and a mesolectal variety in between);
ii) the appropriate conditions for the use of these varieties in everyday life;
iii) the semantic interpretation of English and Creole sentences;
iv) the grammaticality of various sentences combining elements from the polar lects.

With respect to (i), note that ninety-two percent of the respondents in my controlled interviews (22/24) rated the "speakers" performing in acrolectal, mesolectal and basilectal samples in a matched guise test as holding the highest, medium and lowest prestige jobs respectively on a job scale.[8] With respect to (ii), virtually *all* the respondents independently zeroed in on *nature of addressee* (e.g., "when yuh deh in certain company," "when yuh meet dese big people dem," "if yuh meet up to people who know de English") as the single most important variable determining their use of different varieties. With respect to (iii), note that ninety-two percent (22/24) of the respondents interpret the *theoretically genderless i* in [ʃ tif mɪ buk] as referring to a male referent, but ninety-one percent (21/23) also agree that the theoretically genderless *am* in [meri hʌzban bit *am* jʌs bika:z i la:s i mʌri] is feminine. (The difference, of course, is that the sentence context for the former is neutral, while the presence of "Mary" in the head noun of the latter suggests a feminine interpretation for the following pronoun.) Finally, in relation to (iv), note that eighty-three per-

cent of the respondents (19/23) reject [ɪz aɪtɛl i tu tɛk am] as an acceptable Creole sentence. It isn't true that "anything goes."

I would suspect too that even speakers of pidgin languages—notwithstanding the fact that they may not be native speakers—must have intuitions or opinions of some sort about the language they speak (its social role, grammaticality, etc.). In this case, as in the case of creole-speaking communities, it is important that we not discount the existence or usefulness of intuitions on a priori grounds, and that we improve our methods of obtaining them.

Of course, while arguing for greater exploitation of intuitions in pidgin-creole-speaking communities, I am fully aware that they are subject to the same problems of reliability and interpretation as intuitions in other areas (Labov 1970, Rickford 1975). However, if we follow the advice of Labov (1972a) and Bickerton (1975) that intuitions be interpreted in the light of observations and other kinds of data, we can minimize the "possible sources of error," and maximize the richness of our analyses.

2. THE "VECTORIAL QUALITY" OF VARIATION IN PIDGIN-CREOLE-SPEAKING COMMUNITIES

In response to Sankoff's observations on the above subject (section (2) of her introduction), I wish to make two comments, one having to do with (i) the relationship between variation and change, and the other with (ii) the directionality of change in pidgin-creole-speaking communities.

2.1 The relationship between variation and change

Sankoff notes that:

The observation of Weinreich, Labov and Herzog (1968) that linguistic change in progress is manifested as synchronic variation has been given weight in virtually all studies of "pidgin"- and "creole"-speaking communities.

This is true, but it is only one half of the coin, and the other half is more controversial. Some variationists (particularly those working with the implicational/wave model) have expressed the view that the relationship between variation and change is perfectly symmetric. For instance, Bickerton (1975: 16):

... linguistic variation is the synchronic aspect of linguistic change and linguistic change is the diachronic aspect of linguistic variation.

But others (particularly those working with the quantitative/variable rule model) have been led to a weaker conception of the relation between variation and change: one which acknowledges that linguistic change entails variation, but also allows for the possibility of variation which is relatively stable and *not* indicative of change in progress (Fasold 1973, Berdan 1975, Cedergren 1975).

Now since we rarely have good "real time" data with which to evaluate this issue, i.e., samples or observations from earlier points in time which are systematic or detailed enough to provide for comparisons with the present, it is often necessary to depend instead on distributions in "apparent time," that is, to take the usage of the various age groups in a community as representative of the different periods in which they gained mastery of their vernacular (Labov 1966, 1972b). Unfortunately, however, distributions or correlations involving different *age* groups are rarely presented in studies of variation in pidgin-creole-speaking communities, although the work of Gillian Sankoff (Sankoff 1977, Sankoff and Laberge 1973, Sankoff and Brown 1976) on Tok Pisin constitutes a notable exception.

My own recent research on the Guyanese continuum (Rickford 1979) supports the view that the relationship between variation and change is not necessarily nor always symmetric, at least not if variation is interpreted as indicating ongoing change. With respect to morphological variation in the pronouns (e.g., between *mi* and *ai* as subject, and as exemplified in the column headings of Table 2), the age distributions suggest that change is in progress for the two main social classes, in *some* subcategories, e.g., first subject, third neuter subject, and third feminine possessive. But the situation across the different generations is also fairly stable in other subcategories. There is no evidence of change in the third neuter object in either of the social classes, nor in the third masculine possessive among the lower-class sugar estate workers.

It is certainly to be hoped that we will see more attention to age distribution in studies of pidgin-creole-speaking communities. Not only to provide additional evidence on this specific issue, but to permit more fruitful discussion of linguistic change in general—a topic which is often discussed in the field of pidgin-creole studies, but often in abstract terms.

2.2 *The directionality of change*

Sankoff notes that pidgin-creole research has "contributed to our understanding of ... the directionality of change," including "the way in which targets can influence linguistic change." But it is in this very field that we tend to make the mistake of assuming that there is a unidimensional flow of variation and change, in the communities we study, towards the acrolectal or standard language.

In this connection it is sobering to remember Haynes' (1973: 1) indictment of the concept of a creole continuum as:

... an abstract construct which places people in the Caribbean on their hillsides, rolling the stones of phonological, syntactic and lexical mastery to a European summit, getting there, but never quite...

The point of course is that this view is overly simplistic, as is the approach which relates variation to socio-economic status alone, and ignores the complex interaction of motivation and attitude, interpersonal relationships, and the whole host of other "extralinguistic" variables which creolists typically ignore (Haynes 1973: 123-130).

We need to take account, for instance, of the fact that while there are pressures in creole continua encouraging movement in the direction of the acrolect or standard language, there are also pressures favouring the basilect or creole language. In my Guyanese interviews, for example, Sari, a sugar estate worker in the weeding gang, noted that if she were to use English in her interview instead of Creole, the other community members who were present (especially the children) would laugh at her and say:

Look how she a Englishfy. She laka dem English duck!
"Look how she is 'Englishfying'! She is like those 'English ducks'!"

Apart from normal interpersonal pressures like these, designed to establish that one is not playing uppity or high and mighty, there are also the pressures which accompany increased national or political consciousness (cf. Devonish 1978), or a sharpened consciousness of ethnic or social class identity. These are reflected in the language of Reefer, the sugar estate leader discussed above. His interview and peer group performance remains close to the basilectal level, but he shows on other occasions that his competence is much broader. And his explicit comments on a number of issues—such as the following endorsement of a suggestion that school books be written in Creole instead of English—demonstrate that he considers language as an integral part of the socio-cultural revolution:

Me no waan-awe na waan dem English man teachin an ting da no mo man. Dem ting da mus done.

"I don't want—we don't want the Englishman's teaching and so on any more, man. Those things must end."

One other way in which we need to modify current notions is to take greater account of what Washabaugh (1974, 1977) has called *horizontal* variation. Unlike *vertical* variation, which can usually be related to pressures to avoid the basilect or acquire the acrolect, *horizontal* variation involves style shifting at what is essentially the same "level" of the continuum.

Finally, while we have consistently talked about decreolization in terms of movement from a creole language to a lexically-related standard language, Robertson (1980) has drawn our attention to interesting cases in which inter-

mediate forms develop between lexically different languages (e.g., between Creole *Dutch* and Standard *English* in Guyana). He notes that these cases will require a revision of our existing concepts of the creole continuum and the preconditions for its development.

All of these are areas in which our established notions about directionality and change in pidgin-creole-speaking communities are currently undergoing re-examination, or very much need to.

3. "PIDGINS" AND EARLY TOK PISIN

In this final section of my discussion I wish to make a few comments about the subject which occupies most of Sankoff's paper after the introduction. This is the thesis that "social historical circumstances have much to do with the nature and extent of variation found in 'pidgins' and 'creoles'." More specifically Sankoff argues that while the social contacts obtaining in pre-colonial periods in the Pacific area may have encouraged some degree of pidginization, it was only in the colonial and post-colonial periods that the social conditions for the crystallization of stable "pidgins" were satisfied. In the development of Tok Pisin, these conditions included: the high proportion of Melanesians to English speakers, the rapid turnover of Melanesian workers once their indenture periods were over, and the removal of English as a model between 1884-1914, during which time New Guinea was a German colony.

In general, this thesis is well argued, studded with a number of fascinating details (e.g., the information about the use of bilingual trade friends in the pre-colonial New Guinea situation), and bolstered by its reliance on some principles which are by now fairly well established (e.g., the argument originally made in Whinnom 1971 that pidginization of a target language is most likely to lead to a stable pidgin where speakers of different substrate languages must use the target language for intercommunication). What I wish to comment on is not so much the content of Sankoff's thesis, but the methodology it involves, in particular: (i) the reliance on written records, and (ii) the quantitative analysis of specific variables.

3.1 The Reliance on Written Records

Sankoff's paper is a fine example of an increasing trend in pidgin-creole studies to extend the analysis of variation backwards in time by the use of written records. (Compare Naro (1975).) This trend deserves great commendation and encouragement, because it provides our only hope of resolving many of the questions and debates in our field about how this or that variety originated, how stable or variable it was, which languages and what peoples were involved in its development, and so on. While linguistics does provide us

with some techniques for making inferences about the past based on present-day data (e.g., the comparative method, or the method of internal reconstruction), these are sometimes silent on the kinds of information we seek, and in any case, can always profitably be supplemented with textual evidence.

However, as those of us in pidgin-creole studies return to written records for evidence on various subjects, we need to remember the problems of interpretation to which such records are liable: problems of interpreting the phonetic value of orthographic symbols (a subject on which Bloomfield 1933, Lehmann 1962 and other texts provide some guidance), and problems of interpreting the linguistic comments of earlier observers (a subject on which we have fewer established guidelines). For instance, in assessing the claim of Barton (1910)—cited in Sankoff's presentation—that the trading dialect used by the Motu and the various Gulf tribes was "in some measure distinct from the widely divergent languages of either," we need to ask whether it is backed up with specific examples. In this connection, note some of the eloquent counter-arguments which Stewart (1968) makes to Read's (1939) claim that earlier records show Black Americans as using "good English":

The records which Read refers to are for the most part runaway slave advertisements... of course, the evidence which they supply is indirect (i.e., *they give impressions of the particular slave's competence in English, but no examples of that English*)... If these direct records say what Read interprets them as saying, then they are certainly at variance with what direct evidence (quotations in slave dialect) is available... The trouble with Read's conclusion seems to be that, in interpreting such advertisements, he did not consider the possibility that in the parlance of slave owners a term like "good English" might have meant something very different when applied to Negroes than it would have if applied to whites... (Emphasis added)

3.2 The Quantitative Analysis of Specific Variables

It is in the quantitative analysis which Sankoff provides of determiners and subject pronouns in the Queensland 1885 data that we see a particularly valuable (and novel) use of written records. Her analysis of determiner absence exemplifies the characteristics of a sound quantitative analysis: the isolation of a specific variable, the search among environments for possible variable constraints, and the tabulation of the frequencies with which variants of the variable occur in these environments, accompanied by explanations of the observed effects. It is interesting that the outputs of different individual speakers confirm the same hierarchy of effects, showing (yet again) that the application of the quantitative approach does not necessarily depend on group data, nor

involve the submerging of individual patterns.

Beyond the point at which Sankoff's quantitative analysis stops, we might try to write a variable rule capturing the regularities which are found in the nineteenth century samples of Tok Pisin (this would involve a prior decision as to whether the variation is better described in terms of determiner deletion or insertion), and to compare them with a quantitative analysis of modern Tok Pisin. This would provide a rare opportunity for us to study change "in real time" in a pidgin-creole-speaking community.

By and large, linguists in the field of pidgin-creole studies have not exploited the methodology of the quantitative or variable-rule approach as much as that of the implicational approach. In making a successful exemplification of the former, Sankoff's paper helps in yet another way to point out appropriate methods and directions in the analysis of variation in pidgin-creole-speaking communities.

NOTES

1. The research findings in Rickford (1979) which are referred to at several points in this discussion were made possible through grants by the National Science Foundation (Doctoral Dissertation Grant GS-42475), the Danforth Foundation (Graduate Fellowship), and the University of Guyana Research and Publications Fund. It is a pleasure to acknowledge the assistance of these agencies, and to thank Professors John Fought, Dell Hymes and William Labov, who supervised the dissertation. I also wish to thank Professors Derek Bickerton, Gillian Sankoff and Richard Tucker for reading and commenting on the drafts of various sections of the dissertation. As usual, these individuals should not be held responsible for any flaws in this or the original work.

2. Of course, the diachronic inferences were not emphasized by DeCamp (1971). And as I note in my summary of the differences between DeCamp's scale and later ones, the principles for making inferences about the nature and rapidity of change from implicational data were really developed in C.J. Bailey (1973).

3. There are a number of *differences* between my panlectal grid for GC singular pronouns and the one in Bickerton (1973): for instance, the fact that mine provides for more variation (47% of my cells are variable or split, compared with 19% in Bickerton's Table 6). But the important similarity, based on the evidence of our respective sets of data, is the fact that the order of the subcategory columns is identical except for the reversal of the 3MObj and 3FObj columns. This means that Bickerton's hypothesis about how the initial

stages of decreolization proceeds in this area is substantially confirmed.

4. In fact, while the implicational scale for the morphological pronoun variants includes thirteen qualitatively distinct lects, the implicational scale for pronominal vowel laxing includes only four quantitatively different lects.

5. Phonetically, six of these were realized as [aɪ] and seven as [a].

6. In Bickerton (1973), the outputs of one speaker in different recording contexts are sometimes classified as different lects and placed on different points of his scales. But shifts in topic or participants within one recording situation (to take only two possible examples) may also be viewed as different contexts by native speakers, even though these are not usually so treated by linguists. Whether to amalgamate the various outputs of individuals, or try to separate them into different strands, is still an unclear matter, and the current practice of different researchers differs (e.g., Rickford 1979 amalgamates, but Escure 1979 separates).

7. Bickerton (1973) also notes that "... the type of informant who bases his judgements on that section of the continuum with which he has most familiarity can be equally misleading, since his judgements will conflict sharply and at first sight irreconcilably both with those of the first type and with those of other informants whose responses are based on a different section of the continuum."

8. Speakers not only agreed on the relative rankings of the three matched-guise samples, but also on their specific ratings on the job-scale. Most impressive in this respect was the fact that twenty-two of the twenty-four respondents classified the basilectal sample as sounding like a cane-cutter.

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CREOLIZATION PROCESSES AND DIACHRONIC LINGUISTICS

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After giving support to Valkhoff's theory that South African Dutch was creolized in the seventeenth century, Loreto Todd makes a number of rather challenging statements in the last chapter of her short, but substantial survey of *Pidgins and Creoles* (1976:89-90): noticing that modern English compared with the language used before the Norman Conquest or French compared with the Latin of the classical authors show "features consistent with pidginization," she assumes that "many languages which have not been classified as creoles and whose histories are not known may also have undergone processes of simplification." If so, the creole universals derived from the close analysis of the world's pidgins and creoles might throw a new light on those historically poorly documented languages, and if, furthermore, "the processes of simplification and accomodation are similar wherever people, not rigidly bound by conventions of 'standard languages,' come into contact, then the linguistic features which are common to pidgins and creoles may prove as valuable a parameter in the study of the history of languages as the study of sound changes was in the past."

Such a view would tend to assume pidginization or language simplification and adjustment for a large number of cases where languages come into contact. This is precisely where the crux of the problem lies: at all times, when a group of speakers of a definite language of higher prestige managed to impose its language upon a larger mass of speakers of another language (mostly unrelated to theirs), either by administrative and political coercion, or by social and economical pressure, or by any other means, the speakers of language B would take over language A with some changes that reflect features of the phonology and the grammar of their own language. This is the basic assumption that underlies the *substratum* theory, which was used to explain why Latin $\bar{u}$ and Germanic $\bar{u}$