The haves and have nots: Sociolinguistic surveys and the assessment of speaker competence*

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ABSTRACT

A central concern in linguistics is assessing the linguistic competence of individuals or groups. Formal linguists usually accomplish this by the study of intuitions with little regard for observed usage, while survey sociolinguists usually depend on observations – especially the data of "spontaneous" interviews – with little regard for intuitions. In this paper I argue that survey sociolinguists need to make greater use of repeated recordings and elicited intuitions.

The existence of this need is illustrated in most detail by an attempt to replicate an earlier implicational analysis of pronominal variation in the Guyanese creole continuum. It is shown that with repeated sampling and the inclusion of elicited intuitions, the discontinuities on which implicational scaling depends disappear almost entirely. With a clearer idea of what speakers can say, however, the sociolinguistic interpretation of what they do say in the spontaneous interviews and recordings is rendered more reliable and revealing.

In the conclusion, some of the theoretical implications and methodological difficulties involved in extending the use of repeated recordings and elicited intuitions in sociolinguistic surveys more generally are discussed. (Sociolinguistic survey methodology, variation, style, implicational scaling, creoles)

INTRODUCTION: INTUITIONS VERSUS OBSERVATIONS AS DATA

Assessing the nature and limits of speaker competence, and of the grammatical systems which underlie that competence, is the stock in trade of very many linguists. In descriptive work of virtually any kind, in the delimitation of regional or social isoglosses, in the study of language acquisition and language change, linguists are faced with the challenge of determining which phonological distinctions speakers make and which they do not, which forms or constructions speakers have and which they do not, which semantic interpretations they accept and which they reject.

However, a major difference exists between those engaged in the study of language as a formal system (henceforth, formal linguists) and those engaged in sociolinguistic surveys of actual speech communities (henceforth, survey sociolinguists), with respect to the *kinds of data* which they use to explore speaker competence and grammatical structure.

For formal linguists, the primary data are the intuitions of an individual native speaker – usually the intuitions of the linguist about grammaticality and semantic relations in his or her language, less commonly the elicited intuitions of speakers of other languages. As Chomsky (1965:5) observed, "The grammar is justified to the extent that it correctly describes its object, namely the linguistic intuition – the tacit competence of the native speaker." It should be noted that the use of intuitions meant (and still means) introspection without check against a corpus, without observation in any systematic or extended way of the speech of others. And this goes together with an assumption of homogeneity as an intrinsic property of language,[1] and with the assumption that social meaning and diversity in the speech community are unimportant.

For survey sociolinguists, however, the primary data are not intuitions, but observations of language in use. The linguist's intuitions are avoided as a possible source of bias, and even the intuitions of "naive" native speakers are distrusted, on the grounds that introspective reports about language often diverge from observed usage and tend to be variable and unreliable. As Labov (1972a:199) observed, "In many ways intuition is less regular and more difficult to interpret than speech." This theme is echoed elsewhere in Labov's work, particularly in regard to the study of nonstandard or low-prestige dialects (Labov 1966:19, 180; 1972b:111), and it recurs in the writings of other sociolinguists (Bright 1978; Gumperz 1982:62; Milroy, J. 1979:95; Milroy, L. 1984:50; Rickford 1975:179; Romaine 1984:17; Trudgill 1972). For Labov, and for survey sociolinguists in general, concern with reliability and validity and interest in the diversity and social meaning with which language is used in everyday life require dependence on observations. And the primary observation instrument is a tape-recorded face-to-face interview with members of the speech community in which attempts are made to overcome the formalizing effects of observation. This type of interview has been variously referred to as "the spontaneous or free conversation interview" (Wolfram & Fasold 1974:48) and "the sociolinguistic interview" (Labov 1984:32). We will refer to it as the "spontaneous" interview in this paper.

Although the use of the spontaneous interview has been extremely productive for survey sociolinguistics over the years, I wish to argue in this paper that it is sometimes not rich enough to permit a reliable assessment of the limits of individual competence, and therefore not as dependable for comparisons between groups or theories about sociolinguistic processes as we normally believe. Specifically, I will propose that the evidence about individual competence which

we derive from a spontaneous interview needs to be supplemented with *repeated recordings* of the speaker in interaction with other interlocutors besides the original interviewer, and with systematic attempts to elicit his or her *intuitions*. The central evidence for these observations and proposals will be presented in the next section, where I discuss my attempts to replicate an earlier implicational scale analysis of Guyanese pronouns using recorded interview data. But I will present some preliminary evidence in this section, focussing on theoretical discussions in the literature and fieldwork experiences which indicate that we need to be cautious about inferring what speakers can do or normally do based on what they are observed to do in a recording context.

Among the earliest indications of this type were those of Labov himself, in the first work in which he elaborated on the concept of the sociolinguistic interview – his (1966) study of the social stratification of English in New York City. Noting that the interview context is relatively formal, Labov emphasized the importance of trying to elicit "casual speech":

> Now, within the interview, we must go beyond the interview situation, if we can. We must somehow become witnesses to the everyday speech which the informant will use as soon as the door is closed behind us: the style in which he argues with his wife, scolds his children, or passes the time of day with his friends (1966:99).

Labov's solution to this problem of the "observer's paradox" involved attending to potential casual speech contexts within the interview (like speech with a third person or speech on the topic of the danger of death) and attending to "channel cues" (like change in tempo or pitch). Casual speech was defined, procedurally, as speech in which these contexts and channel cues co-occurred. In later work (1972b:112), Labov restated the problem of eliciting casual speech as the problem of eliciting the vernacular, and promoted the value of group sessions as a means of surmounting it. His assumption (ibid.) that "Styles can be ordered along a single dimension, measured by the amount of attention paid to speech" has been challenged by a number of sociolinguists in recent years, as has his definition of the vernacular as a style "in which the minimum attention is paid to speech." We will address some of the relevant issues below, but for the moment it is enough to note that Labov himself had recognized that a speaker's performance in a particular observation context might be an inadequate representation of his or her "everyday" performance and full competence.

Early recognition of this problem was also evident in Hymes's discussion of Bloomfield's remarks about White Thunder's "atrocious" Menomini:

> To say this [i.e., that we must distinguish the potential equivalence of languages from actual inequalities] is not to reduce the actuality of White Thunder's Menomini to a mere list of what he may have been observed

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actually to say. No doubt his linguistic competence was deeper than any particular set of sentences he had uttered (1967:636).

The issue surfaced in a particularly explicit way in Bickerton's remarks on a dispute between Labov et al. (1968:223) and Loflin (1970) about the status of *have* in Black English (BE):

> in Labov's data, occurrences of *have* seem to be outnumbered by informants, i.e., there must be speakers who, at least while being monitored, never produced *have*. *If a speaker, given sufficient opportunity to produce a feature, fails to produce it, it seems not unreasonable to suppose that it forms no part of his grammar.* Assuming this would enable us to resolve the contradiction very simply: some BE speakers have *have,* and others do not (Bickerton 1972:19; italics added).

The italicized principle quoted here makes explicit what is implicit in the practice of many survey sociolinguists, but it raises the critical question of what constitutes "sufficient opportunity," and whether the monitoring we typically do is adequate for sampling a speaker's productive range (compare Andersen 1983:46; Gilman 1982).[2] Bickerton's remarks in a different context indicate much greater scepticism on this point, and are, I think, much better justified:[3]

> Evidence on this issue [the nature of style-shifting] is very hard to obtain, because of the impossibility of knowing what constitutes a speaker's total range, or even what part or quantity of that range he is utilising on any given occasion. A speaker may appear to be talking at his most acrolectal level, yet there is no guarantee that in, say, a job application or a court case he might not produce features yet more acrolectal. A speaker may appear to be talking at his most basilectal level, yet again, there is no guarantee that in, say, a fight with a neighbour or a meeting with a childhood acquaintance, he might not produce something still closer to the absolute basilect. Similarly, for a person one records on only a single occasion, one cannot be sure what one has tapped; Labov's 'channel cues' are of limited use in a creole system, for I have had speakers breathe faster, laugh, open beer-bottles, etc. while using what I could prove to be more acrolectal than their most spontaneous level of speech (1975:186–87).

The general sentiment of these remarks, and their specific suggestion that a one-shot sociolinguistic interview – no matter how ingeniously constructed – does not usually constitute "sufficient opportunity" for us to observe the limits of an individual's competence, are supported by my own research experience and that of others.[4]

One example from my Guyanese fieldwork will serve to illustrate the point. In the course of a three-hour interview with Mr. and Mrs. P. in a back room of their shop, I introduced the famous "danger of death" question (see Labov 1972b:113) and was treated to a number of exciting stories about incidents in which each of

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them had nearly died. Mrs. P.'s narrative about the time she got stuck in quicksand was particularly dramatic, and its climax, accompanied by sharp intakes of breath and other casual speech channel cues, might have been taken as representative of her "vernacular":[5]

> *and – am – a staat tu haala, kaa di mod de til hee! a de dong in di mod!*
> *sombadii di paasin, an di heer, an dem gu n kaal ii. An ii kom wi dii boot, an ii*
> *ṭek mii out . . . ar els a wuda ded in de!* (Sl21:417–421)
> [And – am – I started to holler, because the mud was till here (up to her neck)!
> I was down in the mud! Somebody was passing, and they heard, and they went
> to call him. And he came with the boat, and he took me out . . . or else I
> would've died in there!]

At another point in the same interview, however, as her husband was speaking to me, Mrs. P. turned aside to speak with one of her daughters, revealing in the process an even more basilectal vernacular. The daughter, who had been tending the shop outside, wanted to know whether she should extend credit to a customer who claimed that he had been sent by "Mary" (a friend of Mrs. P.'s) for a bottle of rum. Mrs. P.'s response, and the rest of the dialogue, went as follows:

Mrs. P.:	*maan, luk, . . . hou dem gu gi mi? dem gu – meerii hoo mii aredii.*
Daughter:	*na gi ii?*
Mrs. P.:	*aks am wa kain. tel am awii na gat eldooradoo, doo.*
Daughter:	[Returning] *eks em gu wok. eks em gu wok.*
Mrs. P.:	*gu gi am.* [Then, rethinking the wisdom of this] *. . . a wich waan a ii bodii? . . . tuu daala an ii waan haaf baatl rom? tikee na meerii sen am, maan.* [Pauses, then finally decides against it, sucking teeth] *. . . go, gi dii maan ii tuu daalaa, maan! meerii na gu sen am wid tuu daala se shi gu hoo faiv! . . . tel am meerii self gatu kom. hii oo mii nof-nof monii aredii.*
Mrs. P.:	'Man, look, . . . how will they pay me? They will – Mary owes me a lot already.
Daughter:	I shouldn't give him, then?
Mrs. P.:	Ask him what kind. Tell him we don't have Eldorado (a particular brand), though.
Daughter:	XM (another brand) will work.
Mrs. P.:	Go give him (it). . . . Which one of her friends is it? . . . Two dollars and he wants a half bottle of rum? What if Mary didn't send him? . . . Go, give the man his two dollars, man! Mary wouldn't send him with two dollars and say she'll owe five! . . . Tell him Mary herself has to come. He owes me a lot of money already.'

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That this sample of Mrs. P.'s speech is closer to the creole basilect than the preceding "danger of death" extract is clear from many features, but especially so in relation to the third masculine object pronominal subcategory. In the preceding extract, the form which Mrs. P. employs for this subcategory is *ii* – marked for (masculine) gender but not case, in the pattern characteristic of the mesolect (Bickerton 1973; Rickford 1979). In the latter extract, however, Mrs. P. categorically employs *am* for this subcategory (six times), approximating the basilectal pattern according to which object/nonobject case marking is distinguished (*am* vs. *ii*) but not gender.[6] Significantly enough, at no other point in this lengthy recording session did Mrs. P. use pronominal *am*, regardless of how involved she appeared to become as she launched into stories about ghosts (*jombii*) and floods in the area.

Labov (1972a:89–90) discusses a similar case from his fieldwork in New York City. His interview with Dolly R. is interrupted by a telephone call from one of her cousins, and the "intimate family style" which she uses in the telephone conversation contrasts dramatically with her "seemingly informal and casual" interview speech. As Labov notes:

> The contrast is so dramatic in the case of Dolly R. that we are forced to recognize the limitations of our methods of eliciting the vernacular: for some speakers, at least, our best techniques within the interview situation will shift the speaker part of the way toward the vernacular but there is no guarantee that we have covered the major part of the distance. We have defined a direction but not the destination (1972a:90).

Wolfson (1982:70) cites this last example to support her general claim that "there is no such absolute entity as natural/casual speech. If speech is appropriate to the situation and the goal, then it is natural. . . ." Others have supported the view that there is no context-independent "natural" style (Milroy 1980:59; Romaine 1984:21), and that it is questionable to define the vernacular as a style in which the "minimum attention" is paid to speech (Bell 1984:147–50; Macaulay 1981; Traugott & Romaine 1985). While I agree with some of these critiques, particularly of the vernacular as a minimum-attention style (see Rickford [in preparation] for elaboration), I remain convinced that the Observer's Paradox is real, its major supporting principles sound, and the need for techniques like spontaneous interviews and group recordings indisputable. My own fieldwork experience does not support Wolfson's claim (1982:70, also 1976:195–98) that spontaneous interviews are intrinsically unnatural speech events which rarely succeed in eliciting casual speech. Like Labov (1966), Macaulay (1982), and others, I have frequently recorded individuals in spontaneous interviews using the expressive features which Wolfson finds to be characteristic of performed (conversation) rather than interview narratives, and using casual registers similar to those which they were observed to use with

family members and friends outside the interview. Labov's original observation that a recorded interview with a stranger is likely to elicit the more formal end of an interviewee's stylistic range is a valid one, and unless we are to restrict our research to congeries of friends and family members (and even these may stiffen up when recorded), sociolinguists will need to continue to employ techniques like spontaneous interviews and group recordings to tap the less formal styles in an individual's repertoire.

What everyone seems to agree on (Labov 1972b:119; Romaine 1984:24; Wolfson 1982:71) is that the assessment of speaker competence requires a broad and varied data base. In keeping with this sentiment, I suggest that instead of abandoning existing methods, we attempt to extend and enrich them by making repeated recordings in different situations and by systematically eliciting speakers' linguistic intuitions. In the next section, I discuss the effect of using data like these in an implicational analysis of Guyanese pronouns.

IMPLICATIONAL SCALING AND INDIVIDUAL COMPETENCE

Implicational scaling was first introduced to linguistics by De Camp (1971), who demonstrated its value as a means of handling linguistic variation with data from the Jamaican creole continuum.[7] One of the most striking aspects of this demonstration was the fact that what looked like variation at the level of the community as a whole – variation in past tense marking between *didn't* and *no ben*, for instance – could be factored into invariance at the level of individual speakers; for instance, three speakers in his sample used *no ben* and four used *didn't*. In addition to this interspeaker distinction between haves and have nots, De Camp's technique revealed a significant relation between the features which speakers had and didn't have in their grammars; for instance, if speakers had English *didn't* instead of Creole *no ben*, they would also have English *granny* and *eat* (instead of Creole *nana* and *nyam*); if speakers didn't have *didn't*, they would also not have *child* (instead of *pikni*) and English interdental fricatives. One other significant feature of implicational scales – made explicit in subsequent work by Bailey (1973) and Bickerton (1973) – is that the feature distributions they portray are interpretable as synchronic reflections of diachronic processes, depicting the acquisition or diffusion of features as waves spreading across social (speaker-to-speaker) and linguistic (environment-to-environment) space.

The earliest and most common applications of implicational scaling were to analyses of variation within creole continuum communities; of these, Bickerton's implicational analyses of Guyanese Creole are best-known, and we will examine his (1973) analysis of pronominal variation presently as a case in point. But it is important to note that the technique is by no means passé and has by no means been restricted to creole situations. For instance, Gal (1979:102–4) uses it to depict the spread of German at the expense of Hungarian in the Austrian commu-

nity of Oberwart, Romaine (1980:234) uses it to illustrate the syntactic diffusion of *WH* relatives (at the expense of *TH*) in Scots English, and Zobl (1984:166–72) uses it to model the acquisition by French speakers of English rules for possessive *his* and *her*. What these situations share with creole continua is dynamism – their synchronic variability appears to reflect prior or ongoing change. What they also share with creole continua is interspeaker discontinuities; implicational scaling works best where recorded samples reveal fairly significant differences between haves and have nots, yielding a spectrum of lectal grammars which, although related as stages in an overall process of change, differ from each other in one or more respects.

The preceding points can be clarified by examining Bickerton's (1973) implicational scale for variation in Guyanese singular pronouns, shown in Table 1. The table represents variation in the form of Guyanese personal pronouns in nine singular subcategories (Third Masculine Possessive, First Subject, and so on) as used in recorded interviews by fifty-nine speakers. The columns numbered I to IX represent the pronominal subcategories; within each one, an index of 1 represents the most basilectal or Creole form, and higher numbered indices represent increasing approximations to English or the acrolect. The rows labelled A to U represent speakers' isolects, each one minimally different from its neighbour(s); isolect B, for instance, allows variation between *i* and *shi* as Third Feminine Subject where isolect A allows only *i*. There are twenty-one such isolects (A through U), ranging from the most basilectal (A) to the most acrolectal (U); the number of speakers whose recorded outputs conform to the pattern of each isolect is indicated in parentheses, but note that lects E and N include the outputs of a single speaker recorded on two occasions. The steplike line running diagonally across the Table separates basilectal indices from nonbasilectal ones and represents the course along which decreolization is hypothesized to occur as users of the basilect shift towards the acrolect; the order of the pronoun subcategories from right to left is the order in which the basilectal system is modified. The implicational pattern in this table is as follows: "A basilectal index ALONE in a given column implies the presence of similar indices in all columns to the left; while the presence of a non-basilectal index, alone or otherwise, implies the presence of similar indices, alone or otherwise, in all columns to the right" (Bickerton 1973:646). Data cells which violate this implicational ordering are circled; the scalability index (percentage of nondeviant cells) for this Table indicates that most of the filled cells (88%) conform to the predictions of the scaling model.[8]

Although this scale allows for split cells or intralectal variation (for instance, between *mi* and *mai* in column II), such cells account for only 26 percent of all filled cells. On the basis of this evidence, Guyanese speakers appear to be sharply separable into haves and have nots; and variation at the level of the community appears to be fairly well resolvable into invariance at the level of individual lects.

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TABLE 1. *Bickerton's implicational scale for Guyanese singular pronouns*

ISOLECT	I 3Mas Pos 1 = i 2 = iz	II 1 Pos 1 = mi 2 = mai	III 1 Sub 1 = mi 2 = a 3 = ai	IV 3Neu Sub 1 = i 2 = it	V 3Fem Pos 1 = i 2 = shi 3 = or	VI 3Mas Obj 1 = am 2 = i 3 = im	VII 3Fem Obj 1 = am 2 = i 3 = shi 4 = or	VIII 3Neu Obj 1 = am 2 = it	IX 3Fem Sub 1 = i 2 = shi
A (20)[a]	—	—	—	—	—	—	—	1	—
B (1)	—	—	—	—	—	—	—	1	12
C (4)	—	—	(12)	—	—	(23)	—	12	2
D (4)	—	—	—	12	—	2	—	12	2
E (2)	—	—	—	(12)	—	—	—	12	(1)
F (2)	—	—	—	(12)	12	—	—	12	(12)
G (2)	—	—	—	(12)	2	(1)	2	2	(2)
H (3)	—	—	12	2	—	2	3	2	(12)
I (2)	—	—	13	2	—	2	3	2	2
J (1)	—	—	23	2	—	2	—	2	2
K (2)	—	—	23	2	2	2	—	2	2
L (1)	—	12	23	2	2	(23)	3	2	2
M (1)	12	12	23	2	—	2	3	2	(12)
N (1)	(1)2	(1)	23	2	—	—	—	2	2
O (2)	(12)	12	23	2	2	3	—	2	2
P (1)	2	12	23	2	2	3	4	2	—
Q (1)	2	2	23	2	—	(12)(23)	—	2	2
R (1)	2	2	23	2	—	(23)	4	2	—
S (1)	2	2	23	2	—	3	—	2	—
T (4)	2	2	23	2	—	3	—	2	—
U (4)	2	2	3	2	—	3	—	2	—

[a]Numbers in parentheses indicate number of speakers exemplifying each isolect.
Source: Bickerton (1973:661). Scalability = 88.03%; Filled Cells = 74.6%.

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Scaling Cane Walk usage data

When I set out to replicate this implicational analysis in the mid-1970s with data from the Guyanese village of Cane Walk (a pseudonym), one of my central concerns was to avoid empty cells. Such cells – representing pronominal sub-categories which didn't occur in speaker's recorded outputs – constituted a quarter (25.4%) of the cells in Table 1, and although Bickerton (1971:478) had expressed confidence that empty cells would be filled "in accordance with the implicational series," it was obviously preferable to avoid such indeterminacies in the first place. Accordingly, not only did I prestructure my interview topics (by including questions about the speaker's mother, sisters, teachers, and so on) to encourage feminine pronouns and other forms which preliminary investigations had shown to be rare in ordinary discourse, but I also recorded each of the twenty-four individuals in my sample on at least two occasions, at intervals ranging from a few months to two years. In addition, I obtained recordings of several individuals in two additional contexts in which I was relatively or totally uninvolved: interaction with peer group members (for instance, at a party) and reinterviews with expatriate interviewers (two Englishmen and an American).[9]

One individual whom I recorded repeatedly in this way was Ustad, whose linguistic output I sampled in approximately ten hours of recording over a two-year period. This unusually rich and time-extended sample provides a good opportunity for us to see what difference, if any, repeated recordings have on our perception of individual competence. In Table 2, I present the relative frequency with which Ustad employed basilectal pronoun forms in four different samples:[10]

A. my initial, hour-long spontaneous interview with him (SI8), which is repre-sentative, in length and character (Labov 1984:32), of the typical spontaneous interview;

B. this initial spontaneous interview combined with all subsequent interviews in which I served as primary interviewer (SI14, SI15, SI19, FI7, FI8);

C. a recording of Ustad interacting with peers (relatives, friends, and neigh-bours) at a party in his home (SI73, 74); and

TABLE 2. *Ustad's basilectal pronoun usage in four recorded samples*

Sample	1 Sub	1 Pos	3Mas Pos	3Neu Sub
A (First JRR interv.)	.11 (98)	.23 (22)	.80 (5)	.52 (21)
B (All JRR intervs.)	.09 (313)	.22 (94)	*.17* (46)	.33 (76)
C (Peer group party)	*.39* (74)	.12 (8)	.55 (11)	.17 (6)
D (Expatr. reinterv.)	*.00* (30)	*.00* (2)	*.17* (6)	*.15* (13)

Note: Ns in parentheses. "Basilectal pronoun usage" refers to the relative frequency of the basilectal variant in each subcategory: *mii* in the First Person Subject and Possessive subcategories, *ii* in the Third Person Masculine and Neuter Possessive categories. Italicized frequencies in samples B, C, D differ significantly from equivalent frequency in sample A (chi square significant at the .05 level or better).

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D. a recording of Ustad being interviewed by the three expatriates referred to above (SI84).

Overall, Table 2 indicates that repeated recordings do alter our perception of Ustad's competence, since samples B, C, and D each differs significantly from A (statistically) in at least one of the four subcategories. The effect of the expatriate interlocutors is particularly marked;[11] not only are two of the four subcategories in sample D significantly different in statistical terms from their counterparts in sample A, but the remaining two represent categorical nonusage, giving us the misleading impression that Ustad *never* uses the basilectal forms therein.

In view of the powerful expatriate effect, and the fact that we only have expatriate reinterviews for one third of the sample, the expatriate reinterview data were excluded from Table 3, which represents the recorded outputs of all twenty-four speakers in my Cane Walk sample ("Mrs. P." occurs here as "Granny"), in peer group recordings, and spontaneous interviews with me, arranged in an implicational scale similar to that of Table 1.[12] The most striking feature of Table 3, apart from the fact that it contains no empty cells, is that it contains nearly three times as many split cells as Table 1 (71% vs. 26%), indicating that the competence of individuals is broader than previously supposed (as Escure [1981:32] also finds to be true), and that continuum grammars must provide for greater inherent variability.[13] Interestingly enough, however, the scalability figure for Table 3 (88%) is the same as that of Table 1, and the ordering of subcategory columns is also the same, except that the relative positions of the Third Masculine Object and Third Feminine Object columns are reversed. While repeated recordings enrich our view of individual competence, they leave us with sufficient interspeaker discontinuity for implicational analysis to remain viable.

Adding in and interpreting data from intuitions

The data in Table 3 are all usage data, however, representing the forms which speakers used in the various recording contexts, but not their conscious reports on or introspections about language. In the "Language Attitudes" and "Linguistic Competence" sections of a series of Controlled Interviews – data from which were not included above – I went on to elicit speaker's subjective evaluations of different varieties within the continuum, and their grammatical intuitions about the forms and structures of its polar lects, informally corresponding to "English" and "Creole" (see Winford 1985). The elicitation tasks which I used in the "Linguistic Competence" section included Correction Tests from Creolese to English and vice versa, questions about the acceptability of sentences representing different co-occurrence patterns, and questions about the semantic interpretation of specific forms. These "intuitions" are similar to those which formal linguists use, but differ from them in at least three respects: (i) they are elicited from a sample of community members rather than being derived from the linguist's own introspection; (ii) they are combined with and calibrated against

TABLE 3. *Implicational scale for morphological variation in Guyanese Creole singular pronouns (JRR's Cane Walk data)*

LECTS	Speaker No.	Name	I 3Mas Pos 1=hii 2=hiz	II 1 Pos 1=mii 2=mai	III 1 Sub 1=mii 2=ai	IV 3Neu Sub 1=ii 2=it	V 3Fem Pos 1=hii 2=shii 3=hor	VI 3Fem Obj 1=am 2=shii 3=hor	VII 3Mas Obj 1=am 2=hii 3=him	VIII 3Neu Obj 1=am 2=it	IX 3Fem Sub 1=hii 2=shii
A	4.	Reefer	1	—	—	(12)	—	—	12	12	12
B	11.	Darling	1	—	—	12	12	12	12	12	12
C	7.	Irene	1	12	12	12	12	12	12	12	12
D	12.	Nani	1	12	12	12	12	12	12	12	12
D	8.	Rose	1	12	12	12	12	12	12	12	12
D	1.	Derek	1	12	12	12	12	12	12	12	12
E	10.	Ajah	1	12	12	12	12	12	12	12	12
E	2.	James	1	12	12	12	12	12	12	12	12
E	9.	Sari	1	12	12	12	12	12	12	12	12
F	6.	Raj	1	12	12	12	12	12	12	12	12
F	5.	Sultan	1	12	12	12	2	12	12	12	12
G	3.	Florine	1	12	12	12	2	12	12	2	2
H	24.	Granny	1	12	12	12	2	23	12	(12)	(12)
I	14.	Magda	12	12	12	12	23	23	(12)	(12)	(12)
J	13.	Mark	12	12	12	12	(12)	(12)	(23)	(12)	(12)
J	22.	Ustad	12	12	12	12	(12)	(23)	23	2	2
J	17.	Sheik	12	12	12	12	(2)	23	23	2	2
J	23.	Oxford	12	12	12	12	23	23	23	2	2
J	16.	Kishore	12	12	12	12	23	23	23	2	2
J	20.	Claire	12	12	12	12	23	23	23	2	2
J	19.	Radika	12	12	12	12	23	23	23	2	2
K	18.	Seymour	12	12	(12)	2	23	3	3	(12)	(12)
L	21.	Bonnette	2	2	2	2	23	3	3	3	2
M	15.	Katherine	2	2	2	2	3	3	3	3	2

Source: Rickford (1979:380). Scalability = 88% (192/216); Filled Cells = 100% (216/216).

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the evidence of the spontaneous recordings (see below); (iii) heterogeneity is assumed, and they are designed to reveal competence across the lectal range of the continuum rather than in a single, homogeneous variety.

The results of the Correction Tests are particularly important for our present purposes. I devised these elicitation exercises – which are similar to Labov et al.'s (1968) classroom and vernacular correction tests in New York City – to see whether the limits of individuals' pronominal competence were really what they appeared to be from the performance data of Table 3. I had a sense, both as a native member of the Guyanese speech community and as a participant-observer in Cane Walk for more than two years, that some individuals could use other forms than the ones which had been attested in their informal recordings. And I wanted to test the common assertion that the competence of Guyanese speakers extends virtually across the entire continuum but is limited in actual performance either by social experience and prohibition (Bickerton 1975:197) or by socio-psychological choice (Edwards 1983).

In order to fulfil these aims, I needed a more direct method than the spontaneous interviews and repeated recordings, which were essentially attempts to create contexts in which speakers might *betray* the range of their linguistic competence (that is, reveal in spite of possible efforts to conceal). What I wanted now was a means of inviting speakers to *display* that competence more directly and explicitly. One way of doing this might have been to present speakers with pronominal variants besides the ones they had used in their recorded conversations and simply ask them: "Can/do you say X?" However, it was clearly preferable to have respondents demonstrate rather than merely report their competence. Alternatively, I could have asked respondents to give the basilectal or acrolectal equivalents of isolated pronoun forms. However, asking them to convert entire sentences in which the pronouns were embedded – as they were required to do in the Correction Tests – was preferable because it was both more demanding and more natural: Several variables had to be attended to simultaneously, and the process was closer to what speakers do in everyday life.

Portions of the Creole to English and English to Creole Correction Tests which I used in Cane Walk are provided below, along with the scene-setting introductions which I provided in order to increase the naturalness of the exercise.[14]

CORRECTION TEST: CREOLESE TO ENGLISH

I have a few examples of Creolese which people from different parts of the country gave me when I asked them the same question [about what Creolese is]. What I want you to do is listen to each of these Creolese sentences on the tape, and tell me how you would say it in good English:

Creolese sentence	English equivalent
(1) *ii tiif mi buk*	_____
(6) *ii de in di kichin*	_____

(12) *ii tek naif kot am* _____

(13) *dem bin tek stik biit am* _____

CORRECTION TEST: ENGLISH TO CREOLESE

Now leh we try it the other way around. I'm going to play a tape now with a man who stopped by the market in Port Mourant and asked a set of fishermen how he could find where his wife's brother lived. Only thing is, this man put the whole thing in fancy English, even though he born up Corentyne side heself, and grow up talking Creolese. When e finish, the men buss out wid laff. I gon play yuh wuh e seh, and then I want you to put it in the real Creolese or patois, the way he shoulda talk in the first place:

English sentence   *Creolese equivalent*

(1) Good morning, I hope you can help me.   _____

(2) We're looking for my wife's brother.   _____

(3) His real name is Boodhoo.   _____

(4) But I think they call him Morris Oxford.   _____

Table 4 shows the results of combining the intuitive data elicited in these Correction Tests with the production data from the interviews and tape recordings. The most striking aspect of this Table is how much broader the competence of individuals appears to be than it did in Table 3. Individual speakers now seem capable of saying almost everything, and split cells rise to 88 percent. Although I have retained the ordering of subcategories and speakers as in Table 3, the discontinuities on which Table 3 was constructed now disappear almost completely, and there is little basis for ordering the subcategories and speakers as in Tables 1 and 3. Nor is it possible to make dynamic inferences about which lects represent the most and least advanced stages of change, since there are virtually no distinct lects to begin with.

And yet it would plainly be wrong to use Table 4 to discredit or dismiss the interspeaker discontinuities revealed in Table 3. Those discontinuities were attested in hours and hours of recorded speech and represent realities in everyday behaviour within the community which even a casual visit would confirm. If we took the evidence of the Correction Tests to suggest that everyone really *does* say everything, and that there is not inter- and intraindividual variation with respect to pronominal usage, we would plainly be making a mistake. But if we instead take the Correction Test data as providing a closer approximation to what is sayable by individual speakers – "available" in the terminology of Hymes (1973:91ff) – it can be used to illuminate the recorded discontinuities in what is said ("occurrent" in the terminology of Hymes [ibid.]).[15]

Let us first consider cases in which forms appear to be sayable by individuals, but not said (available in competence but nonoccurrent in recorded spontaneous performance). For instance, Irene's Creole to English Correction Test yielded five acrolectal pronominal forms which she had never used in hours and hours of

TABLE 4. *Outputs of Cane Walk speakers with Correction Test results added to data from spontaneous recordings*

Speaker No.	Name	I 3Mas Pos	II 1 Pos	III 1 Sub	IV 3Neu Sub	V 3Fem Pos	VI 3Fem Obj	VII 3Mas Obj	VIII 3Neu Obj	IX 3Fem Sub
4.	Reefer	12*	1△	12*	12	1△	12*3	123	12	12
11.	Darling	12	12	12	1△	123	123	123	12	12
7.	Irene	12	12	12	12	12△	123	123	12	12
12.	Nani	1△	1△	12	12	12△	12△	12△	12	12
8.	Rose	1△	1△	12	12	12△	12△	12△	12	12
1.	Derek	12	12	12	12	123	123	123	12	12
10.	Ajah	12	12	12	12	123	123	123	12	12
2.	James	12	12	12	12	123	123	123	12	12
9.	Sari	1△	12	12	12	12△	123	123	12	12
6.	Raj	12	12	12	12	123	1 3	123	12	12
5.	Sultan	12	12	12	12	12△	12△	123	12	12
3.	Florine	1△	12	12	12	▽2△	▽2△	▽23	12	12
24.	Granny	1△	12	12	12	12△	123	123	▽2	12
14.	Magda	12	12	12	12	▽23	▽23	▽23	12	▽2
13.	Mark	12	12	12	12	▽23	▽23	▽23	12	▽2
22.	Ustad	12	12	12	12	123	123	123	12	12
17.	Sheik	12	12	12	12	▽23	123	▽23	12	12
23.	Oxford	12	12	12	12	123	▽23	123	12	12
16.	Kishore	12	12	12	12	▽2△	▽23	123	▽2	▽2
20.	Claire	12	12	12	12	123	▽23	▽23	12	12
19.	Radika	12	12	12	12	▽23	▽23	123	12	▽2
18.	Seymour	12	12	12	12	▽23	▽23	123	12	12
21.	Bonnette	12	12	12	▽2	▽23	▽23	▽3	12	▽2
15.	Katherine	12	12	12	▽2	▽23	▽23	▽23	▽2	▽2

Key: n = variants already attested in spontaneous recordings
n = new variants attested in Correction Tests
n* = new variants attested in expatriate interviews
▽ = basilectal variants not attested at all
△ = acrolectal variants not attested at all
Source: Rickford (1979:482).

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recorded interviews. Instances like this could conceivably be attributed to knowledge without command (see Hymes 1973:97), but the ease with which Irene handled the Correction Tests rules out this interpretation in her case. To understand what is going on, we need to take into account the local distinction between people who are field laborers on the nearby sugar estate (for instance, canecutters, weeders, shovel men) and those who have higher status jobs, usually unconnected with the sugar estate and often outside of the village itself (for instance, bookkeepers, contractors, shopowners). Building on local terminology, I refer to these groups as Estate and Non-Estate Class respectively, but they correspond in more general terms to Working and Lower Middle Class (see Rickford [in preparation] for elaboration).

Like all other individuals in Tables 3 and 4 with index numbers from 1–12, Irene is a member of the Estate Class (EC), and like certain other individuals within this class (for instance, Darling and Reefer), she systematically displays the basilectal part of her competence as a deliberate act of identity (Le Page & Tabouret-Keller 1985) with estate culture and lifestyle; in short, her competence extends beyond grammar to social appropriateness and politics. From Table 4, it is evident that she is familiar with nonbasilectal features and varieties, but she opts to use basilectal ones in her daily interactions as a means of asserting the value and integrity of EC culture and lifestyle and rejecting the Non-Estate (NEC) – and Georgetown Middle Class – orientation according to which fieldwork, *nansi* stories, and basilectal language use are devalued. In the face of the common assertion that field labor is "jackass work," she proudly affirmed that to those who are accustomed to it, *aal a da na notn!* 'All of that is nothing.' In response to a question about whether speaking "good English" would help one to get a better job and get ahead (to which eleven of the twelve NEC respondents said "Yes"), she replied negatively, and in response to questions about whether Creole speech should be used more on the radio and in schools, she gave an enthusiastic "Yes!" For her, as for Reefer, the use of the basilect is part of a larger sociopolitical statement that progress for those at the bottom does not involve adopting the behaviours and lifestyles of those at the top, but defying and resisting the dominant social order.[16]

At the other end of the spectrum of what is sayable but not said we might consider individuals like Seymour and Claire, whose English to Creole Correction Tests revealed that they controlled the appropriate basilectal forms in all but two subcategories. Like many other NEC members (those with index numbers 13–24 in Tables 3 and 4), these individuals accept the dominant social order and value system and seek their own advancement in it by suppressing the basilectal elements of their competence in everyday speech.[17] Bonnette, for instance, endorses the view that how you talk is important, asking rhetorically: "You ever find when you go somewhere with strange people – [from] the way you dress, or the way you look, people assess how to treat you . . ." And in response to the question about using more Creole on the radio and in schools, she was very

negative: "I don't think it would sound so nice, to hear it on the news." For NEC Cane Walkers like these, the use of mesolectal and acrolectal features is a way of distancing themselves from the indentured laborer and Estate Class status of their forefathers, and a way of helping themselves and their families to achieve social and occupational mobility outside the village.

There are a number of cases in Table 4, however, where the range of individual competence appears to be more narrowly limited, and these caution us against assuming that usage data are everywhere and immediately interpretable in terms of deliberate acts of identity, in terms of selection from a broad range of possible alternatives. Consider, for instance, Nani and Rose, two EC weeding gang women whose Correction Tests failed to turn up *any* new acrolectal forms, leaving them with more acrolectal gaps (five each) than any other speakers. For both of these individuals, the Creole to English test was a painful and embarrassing exercise, requiring a level of competence in standard English which they simply did not have.[18] When presented with the Creole test sentence, Rose would often remain silent before attempting the English "correction," sometimes concluding erroneously that the sentence was English enough as it was, instructing us to *lef da seem wee* 'Leave that as it is' or asking whether *ii ga ingglish moo dan da?* 'Is there anything closer to English than that?' Nani didn't make similarly explicit comments about the limits of her competence, but she laughed nervously as each Creole sentence was presented and kept protesting that she had household duties to attend to. Interestingly enough, both of these individuals raced through the English to Creole Correction Test with ease, revealing that the difficulty was not with the elicitation exercise itself, but with the kind of competence which the Creole to English test required.

At the other extreme is Katherine, who does not appear to have productive access to the basilectal variant in six of the nine subcategories. She is an upwardly mobile student at one of the best secondary schools in the capital, and her parents do not allow her to mix much in the village. Her mother laughed at her efforts to produce the "real Creolese" in the Correction Tests and said quite plainly that she didn't know it (this, incidentally, was not said with dismay). It is evident from Table 4 that her father, Seymour, commands the basilect far better than she does, but he has made little effort to transmit this aspect of his competence to his offspring, as a deliberate means of helping them to get ahead. Both in her case, as in the case of Nani and Rose, limited competence appears to be due to restrictions on "opportunities . . . for the use, and indeed the acquisition, of means of speech" (Hymes 1973:100). Claims that the productive competence of individuals in creole continua spans all possible forms and that their performance reflects a significant sociopsychological selection from this range (cf. Edwards 1983:300–1) may be valid for individuals like Irene and Claire, but not for those like Nani and Katherine. The difference between these sociolinguistic types is important and requires empirical investigation.[19]

Up to this point we have focussed on individuals, but the Correction Test data

can also enrich our understanding of similarities and differences among the pronominal subcategories. From the usage data of Table 3, for instance, it seems to be the case that the three-variant subcategories are treated as two-variant ones – but different two-variant ones – by the two social classes in this community. The EC members tend to use the basilectal and mesolectal variants (1,2) but not the acrolectal ones, and the NEC tend to use the mesolectal and acrolectal variants (2,3), but not the basilectal ones. The Correction Test data suggest that the gaps in these subcategories revealed by the usage data are very real, perhaps reflecting deeper discontinuities in availability. To translate this into more specific terms, consider the Third Feminine Object subcategory (column VI) in Tables 3 and 4. In the sociolinguistic recordings, none of the twelve EC members used the acrolectal variant (*hor*), and only two of the twelve NEC members used the basilectal variant (*am*). In the Correction Tests, there were still four EC speakers who could not supply the acrolectal equivalent for feminine reference *am* (for them, *shii* was a good enough English translation), and nine NEC speakers who could not supply the basilectal equivalent for *her* (for them, *shii* was a good enough Creole translation).[20]

This finding is in line with the course of decreolization which emerges from the implicationally scaled usage data in Table 3. Since the three-variant subcategories are among the first in which speakers shift from an invariant basilect, it is to be expected that people like Katherine who are furthest away from the basilectal end of the continuum will be least familiar with the appropriate basilectal forms therein. And even though these three-variant subcategories are among the earliest to be affected by the decreolization process, they are also among the very last to arrive at the stage in which the acrolectal form is invariant.[21] Therefore, people who are furthest removed from the acrolectal end of the continuum, like Rose and Nani, are also likely to be least familiar with the appropriate acrolectal forms therein.

Two other subcategories in which the Correction Test data confirm the predictions of the spontaneous interview data are the Third Feminine Subject (3Fem Sub) and Third Masculine Possessive (3Mas Pos) subcategories. Table 4 indicates that six NEC members did not productively control the appropriate basilectal form (*ii*) in the 3Fem Sub subcategory, and four EC members did not productively control the appropriate acrolectal form (*hiz*) in the 3Mas Pos subcategory.[22] If we omit the three-variant subcategories, which are special in the ways described above, the 3Fem Sub subcategory is the one with the most basilectal gaps, and the 3Mas Pos the one with the most acrolectal gaps. This is perfectly in accord with the course of decreolization which is inferrable from the implicational scale in Table 3, since the 3Fem Sub subcategory is shown there to be the first one in which decreolization begins, and the 3Mas Pos to be among the very last in which the process is completed.

Whether we take the perspective of individual speakers or pronominal subcategories, it is clear that the data from the Correction Tests, although different

in kind from the recorded usage data, confirm in many ways the indications of the latter and provide additional insights which would otherwise be difficult to obtain.

CONCLUSION

I have tried to demonstrate in the preceding sections that recording individuals on several occasions and eliciting their linguistic intuitions yields a richer and more accurate picture of their linguistic competence than the one-shot spontaneous interview does, and places our inferences about the sociolinguistic significance of their performance on more solid ground. In this section, I will explore some of the prospects, problems, and implications of using these methods in sociolinguistic surveys more generally.

Although the use of repeated recordings is by no means commonplace, the technique is compatible with current sociolinguistic survey methodology and has been used in some studies, particularly of code-switching and style-shifting (see for instance, Bell 1984; Bickerton 1980; Coupland 1980; Douglas-Cowie 1978; Hasselmo 1970:197–204; Labov et al. 1968; van den Broeck 1977). In his most recent publication on field methodology, Labov (1984:41) notes the advantages of maintaining continuous contact with individuals in a speech community and following up an initial interview with subsequent recordings, and this is of course very close to the spirit of the repeated recordings advocated in this paper.

Note, however, that Labov's repeated recordings – which he refers to as "continued interviews" – involve the same fieldworker who makes the initial spontaneous interview, and that netting the informant's "vernacular" and other styles is attempted primarily through varying the *topics* of conversation (1984:33–39). This approach has been, and will continue to be, important for many of us, but there is reason to believe that varying the interlocutors and audience in repeated recordings is likely to result in a richer harvest of the informant's linguistic range than varying the topic alone.

To begin with, when I asked my twenty-four Cane Walk informants about the appropriate conditions for the use of "Creole" and "English," each of them independently referred to characteristics of the *interlocutor* rather than the *topic, locale,* or any of the other variables commonly presented in the sociolinguistic literature. Some typical responses follow:

If yuh meet up to people who know the English, yuh got to speak it – at least if yuh know it. If yuh meet up people who can't talk in English, well, yuh got to speak *dey* way. (Derek)

Yuh use English when yuh meet important people dem. (Sultan)

Yuh got fuh be in de company, den yuh can talk dem correct English. (Ajah)

When yuh meet dese big people dem, yuh talk like dem, right? And when yuh
go to de other stage, yuh have to talk like de other people dem. (Clair)

Well, that would depend on the individual to whom you are speaking. Dey
might be a person who doesn't understand Creolese. Den you would have to
talk proper English to dem. But if we are all Guyanese, then let's talk
Guyanese, let's talk Creolese! (Seymour)

Well, when yuh meet up wid English people, and yuh know de lil twang, yuh
a try wid am. But if yuh na know, how yuh go talk? Yuh talk yuh own ting,
before yuh burst half-way and dem – dem laugh yuh! (Rose)

This does not mean that Cane Walkers do not sometimes display the "meta-
phorical switching" associated with change in topic and similar factors (Blom &
Gumperz 1972:424–26), but an ethnographic description of stylistic variation in
this community would have to give greater prominence to the "situational shift-
ing" (ibid.) associated with change in interlocutor, as the addressee-triggered
shift in Mrs. P.'s extract so clearly demonstrates.

Furthermore, Bell (1984:179), drawing on earlier research by Coupland
(1981) and others, shows that addressee-triggered style shifts are quantitatively
bigger than topic-triggered ones, and hypothesizes more generally that "style
shift according to nonpersonal factors derives from audience design." To the
extent that this hypothesis is confirmed – and it looks promising, so far[23] – we
would have a theoretical foundation for varying interlocutors rather than (or in
addition to) topics in order to plumb individual competence.[24] What would
remain to be done in each community then would be to discover what some of the
significant language-influencing distinctions among possible interlocutors are,
and build into one's field research a way of exposing informants to a feasible
subset of significantly different types. This could obviously be a time-consuming
process, which it may be difficult to carry out for more than a subset of one's
sample from a speech community. But repeated recordings of this type would
offer a valuable perspective on the evidence provided by the spontaneous and
continued interviews which one can make more easily and in larger numbers.
(Compare the independent evidence which "anonymous observations" provide
on the data of spontaneous interviews, as noted by Labov [1966:603]).

The barriers against the increased use of repeated recordings in sociolinguistic
surveys are primarily implementational, but the barriers against the increased use
of elicited intuitions are theoretical and conceptual. As noted above, sociolin-
guists have traditionally voiced a number of reservations about the value of
speakers' self-reports and intuitions about language, and the number of instances
in which intuitions (other than subjective reactions to speech varieties) are used
in sociolinguistic research are few.

And yet it is clear from the data presented in the preceding section and from
other recent developments in the field that sociolinguistics cannot make further

progress on some of its key issues until we overcome ideological barriers against the use of intuitions and begin involving speakers as active partners and participants in our enterprise rather than as passive objects of observation and analysis. For instance, Giles's accommodation theory (Giles & Powesland 1975) and Le Page's acts of identity theory (LePage & Tabouret-Keller 1985) both involve sociopsychological interpretations of what speakers *do* do against a backdrop of assumptions about what they *can* do, but as I have tried to show in this paper, an adequate conception of individual competence requires access to intuitions as well as observations.

A second area in which intuitions are quite clearly needed is in the study of variables beyond the phonological level at which variation models have been most successful to date. In the first place, tape-recorded sociolinguistic interviews typically do not yield enough tokens of morphosyntactic variables (Labov 1975; Rickford 1975). In the second place, issues of the semantic constancy and interpretation of the variants are more critical with syntactic and pragmatic variables and cannot simply be answered by output data (Gumperz 1982:31–37; Labov 1978:8; Lavandera 1978; Romaine 1981). The problem of deciding whether nontypical occurrences are performance errors also looms larger with syntactic variables (Milroy 1984:51). Sociolinguists who have begun to look seriously at syntactic and pragmatic variation have found it almost impossible to proceed without elicited intuitions, and the situation is unlikely to change.

In the face of our obvious need to incorporate the intuitions of native speakers in sociolinguistic work, the problems which have made us hesitant about doing so in the past need to be better understood and overcome. We cannot explore all the relevant issues here, but will briefly consider two salient ones – the problem of vernacular shifting and the problem of reliability.

The problem of vernacular shifting

The problem of vernacular shifting was first described by Labov as a general principle: "Whenever a subordinate dialect is in contact with a superordinate dialect, answers given in any formal test situation will shift from the subordinate towards the superordinate in an irregular and unsystematic manner" (1972a:213). Labov observed (ibid.:214–15) that this principle not only made it impossible to obtain interpretable results on vernacular correction tests (in which the subject is asked to correct standard forms into their nonstandard equivalents), but also made it difficult to work with bidialectal informants, "if indeed such speakers exist." Wolfram and Fasold repeated this argument, claiming in even stronger terms that: "It is very difficult for informants to divorce linguistic acceptability from social acceptability. A level of linguistic expertise is required which most non-standard speakers simply do not have" (1974:57). Milroy represents the most recent statement of this position, observing that a question about whether an unusual utterance in the Belfast vernacular represented a performance error was "unanswerable" and "hardly worth asking" since:

The response of bidialectal speakers to direct or indirect questioning about non-standard varieties is always conditioned by their attitudes to the social significance of both varieties and usually responses reflect knowledge of the standard rules. In the case of the Belfast "singular concord" rule, attempts to investigate further constraints ran up against these problems (1984:51–52).

It would be interesting to know more about the nature of the problems which Milroy and her colleagues experienced, because we have tended to repeat Labov's original warnings about the difficulty of eliciting the intuitions of nonstandard speakers rather than systematically attempting to elicit such intuitions, and pinpointing why and where successes and failures occur. In my Cane Walk work, for instance, Labov's principle of vernacular shift was generally *not* borne out; informants gave appropriate Creole responses on the English to Creole Correction Test despite the formal character of the controlled interview in which the test occurred. On the first two sentences, a few speakers did appear to be responding to standard rather than nonstandard norms,[25] perhaps under the influence of the immediately preceding Creole to English Correction Test, but in general, respondents were clearly trying to follow Creole norms. This is best illustrated by the responses which we received to the penultimate sentence in the Creole to English test, "But *we don't* know *where their* house *is.*" The italicized constituents are the ones in which grammatical and lexical differences between Creole and English are most marked, and the responses we received to each of these are shown in Table 5. Except for the single instance of *wii* in the first column, there is no evidence that speakers are being guided by English instead of Creole norms. Where speakers give mesolectal rather than basilectal Creole equivalents, this appears to reflect the limits of their productive vernacular rather than the effects of vernacular shifting.[26]

In general, my experience was that the Cane Walkers remained true to vernacular norms and were extremely insightful when responding to questions about their intuitions, not only on the Correction Tests, but on items having to do with co-occurrence restrictions, semantic interpretation, and usage.

Part of the reason my experience was so different from what the principle of vernacular shift would have predicted may have been the fact that I was myself a native member of the Guyanese speech community. This has not typically been true of the cases in which the principle of vernacular shift has applied most powerfully, and the fact that sociolinguists tend to use "insiders" for stimulating

TABLE 5. *Responses on an English to Creole Correction Test sentence*

Test sentence constituents	"we"	"don't"	"where"	"their"	"is"
Basilectal Creole	awii/miii 23	na 23	wi(said) 24	dem/ii 13	de 20
Mesolectal Creole	—	en 1	—	di/de 8	—
Acrolect/English	wii 1	doon 0	weer 0	deer 0	iz 0

spontaneous speech more so than for eliciting linguistic intuitions may have contributed to the sense that we have a problem here. Another factor in the relative success with which vernacular norms were elicited in Cane Walk may have been the fact that family members and friends were usually present when speakers' intuitions were being elicited; their presence seemed to have the same kind of vernacular norm-enforcing effect on intuitive judgements which Labov et al. (1968) and Blom and Gumperz (1972) observed peer group sessions to have on spontaneous production elsewhere.

It would be useful to vary each of these and other variables in future research to determine what constrains relative success in eliciting nonstandard speakers' intuitions. To continue to neglect such intuitions as a matter of general principle is completely unjustified and contrary to the spirit of Labov's (1972b:111) remark that the difficulties of eliciting such intuitions should not prevent us from trying to elicit them, but should remind us to interpret them in the light of observational and other data.[27]

The problem of reliability

The problem of reliability has been discussed primarily in relation to so-called syntactic dialects. The optimism of the 1960s that intersubjective agreement on intuitive syntactic and semantic judgements would be high and that unclear cases could be decided by clear ones has diminished considerably over the past two decades. As Labov noted over ten years ago (1975:14–32), inconsistency and variability in introspective judgements on critical sentences had become the order of the day, both among the linguists who produced data and theory at the same time, and among linguistically naive speakers. Subsequent reports by Carden and Dieterich (1981) and Bever (1985) indicate that the situation has not changed; the variability remains, and it is considered bad form to challenge other linguists' judgements or to question the reliability of introspective data.

This problem of reliability, and the related issue of validity – the extent to which introspective judgements line up with independent observational and experimental evidence (Carden & Dieterich 1981:585; Labov 1975:40) – are responsible in part for the relative neglect of intuitions by sociolinguists. But, again, the need for increased use of intuitions in sociolinguistic work makes this neglect intolerable, and what is needed is a concentrated effort to determine what kinds of intuitive judgements are more robust than others, what factors influence their variability, and what methods we might use for calibrating them against observational and other evidence.

Several useful suggestions and advances in this direction have already been made. Bolinger (1968) suggests that judgements about grammaticality per se are less reliable than judgements of what particular utterances mean, and this is supported to some extent by the results obtained from Vernacular Black English speakers about the grammaticality and meaning of stressed *bin* (Rickford 1975).

Wolfram and Fasold (1974:60–65) note that they have used structural elicitation methods with nonstandard dialect speakers with considerable success. Labov (1975) reports on attempts by himself and his students to elicit the intuitive judgements of native speakers on a variety of syntactic variables, to understand the constraints on their variability, and to increase their reliability and validity by combining their study with observational evidence. Carden and Dieterich (1981:585) continue the work begun by Carden (1970, 1973) of developing "adequate techniques for collecting introspective judgements from unbiassed subjects." Bever (1985) reviews work by himself and his colleagues (such as Carroll, Bever, & Pollack 1981) which is bringing us steadily closer to an understanding of intuitive judgements and the structural and nonstructural phenomena that govern them. If more survey sociolinguists were to join psycholinguists and generative linguists in this attempt to increase our understanding of intuitions and to enhance the reliability and validity of their use, the benefits to sociolinguistics and linguistics as a whole could be considerable.

In closing, I wish to summarize my primary points:

1. Survey sociolinguists have traditionally depended on spontaneous interview data for their conception of the limits of individual and group competence.

2. But a one-shot sociolinguistic interview – even with danger of death questions and the like – does not yield an adequate representation of what speakers *can* do, and needs to be supplemented with repeated recordings and systematic elicitation of speakers' intuitions.

3. Competence data which draw on repeated samplings and elicited intuitions display fewer discontinuities than data which do not, but it enriches the sociolinguistic interpretation of discontinuities in performance considerably.

4. Repeated recordings are likely to result in a deeper plumbing of speaker competence when they involve different interlocutors rather than (or in addition to) different topics. The main difficulty with using such recordings on a wider scale is the additional time, organization, and other implementational complications which this involves.

5. While elicited intuitions are valuable, and increased use of them seems vital to further progress in the field, their exploitation is not without difficulty. However, the difficulties which they pose – including the problems of vernacular shift and reliability – do not justify our ignoring or neglecting them. Attention to the instances in which these problems seem to be attenuated has already begun to yield prospective solutions, and more survey sociolinguists should become involved in ongoing efforts to understand intuitive judgements better and use them more fruitfully.

NOTES

1.   See Weinreich, Labov, and Herzog (1968:125) on similar assumptions in Saussure's concept of *langue*, and Hymes (1974:121) on similar assumptions in Feuerbach's *human essence*, critiqued by Marx.

2.   For instance, Le Page (1980:335) observes that GM, a Belizean speaker reinterviewed at the age of twenty-four (earlier interviews were at ages twelve and sixteen), "no longer commands the conservative Creole and the 'broken-up Spanish' of her childhood so confidently," but as I have noted elsewhere (Rickford 1983:307–8), it is difficult to justify such a strong statement about GM's competence on the basis of her performance in the reinterview. It is clear that GM's sociolinguistic awareness and the image she wishes to project to the interviewers have changed, but less clear that her ability to use Creole has diminished, especially since her daughter "speaks only Creole."

3.   Bickerton's work may not be considered sociolinguistic in the sense of "socially constituted linguistics" (Hymes 1972), but it is certainly so in the sense of Hymes's (ibid.) "socially realistic linguistics" – "work that extends and challenges existing linguistics with data from the speech community." On a similar point, compare Labov (1966:v).

4.   Compare Macaulay (1981:10): "Obviously no single interview can cover the whole range of a speaker's verbal repertoire. . . ."

5.   The phonemic transcription system used here is a modified version of the one devised for Jamaican Creole by Cassidy (1961). See Rickford (1987) for details.

6.   I say approximating rather than realizing the basilect in pure form because Mrs. P. uses one subject *shi* instead of *ii* in this extract.

7.   De Camp's larger aim was to effect a rapprochement between sociolinguistic and generativist methodology by arguing that systematic code-shifting between continuum levels should be considered part of competence, and by showing that implicational scaling could sort the variable data into invariant lects, and that the set of lects could be generated by conventional rules.

8.   But see Pavone (1980) for more stringent scaling tests and for a critique of the missing data in this table.

9.   I wish to thank the reinterviewers: Derek Bickerton, John Holm, and Michael Pye. The method is similar to that of van den Broeck (1977), whose Flemish informants in Maaseik were interviewed both by himself (semiformal situation) and by one of his university colleagues (formal situation).

10.   Numbers in parentheses are tape numbers. When making SI8, I was accompanied by an "insider," a teenager who lived near to Ustad and participated in the interviewing process both as questioner and (more actively) as listener and respondent. In SI14 and SI15, both Wordsworth McAndrew (a popular local folklorist) and I served as interviewers, McAndrew recording Ustad's account of a local Hindu festival for his weekly radio program.

11.   See Rickford (1983:308–9) for shifts in the use of preverbal *negators* by Ustad, Magda, and Reefer between their spontaneous interviews with me and their expatriate reinterviews.

12.   Table 3 does not indicate the relative frequencies with which the varients in each subcategory were used, for two reasons: (i) It was intended as a replication of Bickerton's pronominal implicational scale (Table 1), which did not include quantitative data; (ii) a frequency-valued implicational scale yields an unacceptability low scalability index (71%) for this area of the grammar and affects the ordering of the subcategory columns only minimally (Rickford 1979:415–17).

13.   For more discussion of the similarities and differences between Tables 1 and 3, and their significance for implicational scaling and decreolization theory, see Rickford (in press).

14.   References to local terms like "good English" and "Creolese" (=Creole) or "patois" were included to elicit acrolectal and basilectal norms respectively, and were clearly understood as such. The Port Mourant area and market settings are popularly associated with Creole speech and were included in the English to Creole Correction Test to enhance adherence to Creole norms.

15.   Although intuitive judgements of the type considered here can help us to get closer to the limits of individual competence, it is important to remember that intuitions are themselves performance data and that what is in competence can never be perfectly or exhaustively known. As Bever

(1985) noted, intuitions are behaviours, indirectly reflecting competence, at best, and must be viewed as the result of interaction among different systems for language use.

16. On the basis of the acrolectal gaps in Table 4, Reefer might appear to be more similar to Nani and Rose than Darling or Irene, but this is not really the case. Many of the items in Reefer's Creole to English Correction Test had to be excluded because they were provided by his brother Raj, who was present. Now this might seem suspiciously like an attempt on Raj's part to conceal Reefer's limited knowledge of the acrolect, but in his expatriate reinterview, Reefer spontaneously produced two of the four acrolectal variants which remain unattested in Table 4 (those in the 3Mas Pos and 1 Sub subcategories), and there are a number of other reasons to conclude that his typical nonuse of acrolectal forms is more a matter of choice than necessity.

17. This is even clearer when quantitative data are taken into consideration. For instance, although Seymour is listed in Tables 3 and 4 as using both the basilectal and acrolectal variants in the 3Fem Sub and 3Neu Obj subcategories, he actually uses the basilectal form only 2 percent of the time in each case.

18. The experience was embarrassing and uncomfortable for me, too. It is far more pleasurable to serve as captive audience for interviewees explications of their philosophies of life, their tall tales, and their danger of death stories than to escort them to the limits of their linguistic competence and see them flounder.

19. Both Labov (1972a:105) and Blom and Gumperz (1972) discuss cases in which speakers expressed sociopolitical identities with certain groups in the community and wanted to speak like them, but apparently could not because the relevant sociolinguistic variables were beyond their productive control. Cases like these present key challenges to the "acts of identity" model of sociolinguistic variation (Le Page & Tabouret-Keller 1985), even though that model does allow for speakers being unable to change their speech to accord with their desired identities.

20. The fact that there were more NEC speakers who did not know the basilectal form in this subcategory than there were EC speakers who did not know the acrolectal form is true throughout the pronominal subsystem, and probably more generally as well. Note that there are twice as many basilectal gaps in Table 4 (38) as there are acrolectal gaps (19). Contrary to what some have asserted, the people at the bottom clearly know more about the speech of the people at the top than vice versa. And there is at least one plausible explanation for this in the fact that the acrolectal forms are encountered daily in the mass media while the basilectal forms are not. People like Katherine who really do not mix much with basilectal speakers have little other opportunity to develop basilectal competence.

21. This is clearer in the abstract panlectal grid constructed from the implicational data in Table 3; see Rickford (1980:170–71).

22. They presumably control them receptively, but despite Chomsky's assertions that grammars should be neutral between speakers and hearers, we know from acquisitional and other evidence that receptive competence typically outstrips productive competence, and grammatical descriptions tend to be production-based.

23. In addition to the Cane Walk informant judgements, and the research evidence reported by Bell himself, note that Blom and Gumperz (1972:425), analyzing variation between Ranamal and Bokmal in Hemnesberget, Norway, had made a claim which is more general, but very similar to Bell's: that the effect of metaphorical switching derives from the relationship between language and social situation which is fundamental to situational switching.

24. Interestingly enough, Labov has always drawn a distinction between addressee- or situation-governed styles and topic-governed ones. He differentiates (1966:100–101) between *casual speech* ("the everyday speech used in informal situations") and *spontaneous speech* ("excited, emotionally charged speech when the constraints of a formal situation are overridden") and notes (1966:105) that his first three "Style A" contexts (speech outside the formal interview, speech with a third person, speech not in direct response to questions) represent *casual speech* contexts, while his latter two (childhood rhymes and customs, the danger of death) represent potential *spontaneous speech* contexts. I think it is fair to say that although survey sociolinguists often found the casual speech contexts less ambiguous and more dramatic in their effect than the spontaneous speech ones, they concentrated on the latter (hence the characterization "spontaneous interview") because these could be more systematically controlled by a single fieldworker in a single interview. (Vernacular-revealing interruptions like those which Labov had with Dolly R. and I had with Mrs. P. are serendipitous.)

25. For instance, Chauffeur offered this "Creole" version of the first English test sentence,

"Good morning, I hope you can help us": *gud mohnin, bodi, o duu hoop yu wil eebl tu help os;* and Jamalho offered this response to the second sentence, "We're looking for my wife's brother": *ai am lukin fu mi waif brodo.* But these examples of superordinate (acrolectal) influence were exceptional.

26. The lone *en* as negator is predictably Katherine's. All occurrences of *di* before *house* have been classified in Table 5 as instances of the mesolectal possessive pronoun, but in fact they are ambiguous with the level-neutral definite article "the." I should add that the reason the responses in the last two columns do not add up to twenty-four is that some respondents didn't follow the original subordinate clause structure, substituting clauses like *wi dem liv* and *wi fu fain di hous* instead.

27. On this point, compare Bickerton (1975:201–2), who notes that the intuitions of speakers in a creole continuum can be difficult to interpret in the early stages of an investigation, but can be fruitfully calibrated against tape-recorded outputs once the latter have been obtained.

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