1. The Evolution of a Sociolinguistic Theory of Language

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1 The Language of Stereotypes and the Language of Science

The Oxford scientist who recently claimed that most English people were scientifically illiterate because they still spoke of the sun going round the earth, revealed his own ignorance of the way stereotypes – as discussed, for example by Schaff (1984) or by Cameron (1990) – are embedded in language. It is a fair bet that he himself still speaks of the sun “rising” and “setting.” In the following discussion of some of the work done on the relationship between individuals, linguistic systems, and groups, I emphasize the extent to which we all base much of the argument on stereotypes, which then do not serve us well enough as the basis of scientific models. Chief among them is the stereotype of “variation” in “a language.” Stereotypes are, however, important parts of our data. We must try to understand the language of former generations when they spoke of languages “changing” or “dying,” or being “genetically descended” from other languages, and so on. Anthropomorphic stereotypes are embedded in all these metaphors, and many more. In contradistinction to some sociological philosophy, I shall want to maintain that “languages don’t do things: people do things: languages are abstractions from what people do.” Of course, we are all making and using the abstractions.

2 An Expanding Universe

I have no space for an overview of the vast amount of work coming under the “sociolinguistics” or “sociology of language” umbrellas since the 1960s; most of the ground is covered in the other chapters of this book. Readers can also refer to the two encyclopedic volumes edited by Ammon, Dittmar, and Mattheier (1987, 1988) and to the earlier Dittmar (1976). The exponential increase has given rise to a number of new journals (see the bibliography). Verdoort (1988) reports that the proliferation of papers accepted by the sociolinguistics section at successive World Congresses of Sociology was so great that after Mexico (1982) it was no longer possible to publish them. The biennial British Sociolinguistics Symposium was offered over 200 papers for its 1994 meeting. All are “studies of language in its social context” but otherwise the spread of subject matter is considerable. Many are relatively atheoretical. I shall confine myself to what is for me and for some others, such as Labov, DeCamp, Trudgill, the Milroys, and Romaine, a central concern: What should a linguistic system look like if we want it to take full and explanatory account of all the contextual factors which make it meaningful to the users? And even within this framework I must omit discussion of the “communicative competence” that Hymes deems necessary, and I shall follow Fishman in keeping “the sociology of language” separate.

3 Some Progenitors

The complaint that early sociolinguistic work was sociologically uninformed is true; but then, we thought we were doing linguistics or, to be more precise, such long–practiced disciplines as historical and comparative philology, descriptive linguistics, dialect geography (Chambers and Trudgill, 1980,
and Trudgill, 1992, contain useful accounts of the bridges between the latter and sociolinguistics, with a forward-looking plea for "geolinguistics"). The claims of these disciplines to be "scientific" reformulated more than once since the eighteenth century, are of some importance here, since they have influenced the more recent struggles of the "social sciences" both to be recognized as sciences and to absorb linguistic models to that end. Max Müller in his mid–nineteenth-century Lectures on the Science of Language made a strong claim that comparative philology had now been firmly established as a science. It had led to the establishment of "laws" and principles according to which languages were said to change and dialectal differences to reflect stages in the operation of those "laws." The constraints on change were stated in terms first of the linguistic environment and then where necessary in terms of the social context, for example, migration and social contact. In the 1940s A. S. C. Ross proposed various mathematical "proofs" for these processes of change, and of postulated relatedness (Ross, 1950). The claim to be "scientific" has been urged strongly by Chomsky (1966), tracing the ancestry of generative grammar from Descartes and the Port–Royal grammarians and relating it to universal grammar and to brain structure.

In the eighteenth and nineteenth centuries, there was also a movement to re-establish rural "folk values" in reaction to the squallid urbanization which accompanied the Industrial Revolution. We can juxtapose Samuel Johnson seeking to regularize English in his Dictionary (1755) (though finally confessing that "to enchain syllables, and to lash the wind, are equally the undertakings of pride") to dialect poets such as Barnes and Burns. The latter part of the nineteenth century saw the start of large-scale writing down of dialect forms, either in dictionaries (as in Joseph Wright's English Dialect Dictionary (1896–1905)) or on maps. Mapping the geographical distribution of forms used led to the concept of the "isogloss," by analogy with isobars and isotherms: geographical boundaries between users of variant forms for "the same thing" in "the same language." Wenker, in Germany, collected his data between 1876 and 1887 by asking 50,000 village schoolteachers to write down their local dialect equivalent of each of 40 sentences, set out in a questionnaire in High German. (He was relying on the teachers for an accurate rendering of something of which they might in fact have only a partial knowledge.) Approximately the same method was used - though simply as a starting-point for a survey - by McIntosh in his Survey of Scottish Dialects (1952) and by myself in the Linguistic Survey of the British Caribbean, started with his help in 1952 (Le Page, 1952, 1957, 1958).

The French scholar Jules Gilliéron relied on the personal observations of a trained fieldworker, Edmond Edmont, visiting 639 locations in France and writing down the spoken forms for 1,500 items he elicited from the local informants he selected. Gilliéron's method, with some modifications, has since been used in dialect surveys of other European countries including England, and in the United States and Canada. Technical changes have influenced fieldwork practice and the analysis of data; in particular, methods of recording the responses of informants first on wax discs and then on magnetic tape. The increasing sophistication of electronic devices has led to recording longer stretches of spontaneous speech in place of single-word or sentence responses to questions about the names of things; tape-loop repeaters and sonographs have made possible minute discrimination in transcription.

4 Theoretical assumptions

Dialectology had already thrown into relief stereotypes and ideological presumptions which carried over into sociolinguistics. The older tendency had been to locate some "real old dialect" in rural areas in contradistinction to the "corrupt" speech of the towns. One of the major innovations of the 1950s, the move into urban dialectology, was partly a product of concern about the educational disadvantages of urban working-class children (see e.g., Bernstein, 1958; Dittmar, 1976) in Germany, in Britain, and the US. For rural surveys the typical informant was the nonmobile older rural male, the NORM. There was a pastoral implication of a static enduring landscape, so that one looked for reasons for language change rather than accepting (as one has to in urbanizing contexts) diversity as the base line and then searching for the way in which stereotypical concepts of homogeneous unchanging languages (with their connotations of social hierarchy and nationalism) come into existence. A static social structure was implicit in dialect geography; in order to compare like with like, the representative dialect speaker had to be relatively uncontaminated by education. The dialect areas outlined by isoglosses on the map were artifacts of the geographer; they had to be matched against such stereotypes as "southern dialect" or "Alemanic" or "langue d'oc," concepts which often related in the minds of outsiders to just one or two variables characterizing a complete, discrete system; whereas if one went on investigating features one found that isoglosses might converge and run parallel for part of the way and then
diverge again, so that "dialects" were neither uniform nor discrete. The extent to which social variables other than regional ones could be built into mapping was limited, but some surveys, including my own, attempted to label variants as, for example, "old-fashioned" or "rare" or "common."

The assumptions commonly made about the social correlates of linguistic differences vary from culture to culture: whether they are primarily geographical or ethnic or economic or social class or caste (see e.g., Berntsten, 1978; Khubchandani, 1983) or sex- or age-related (see chapters 8 and 9); whether they relate to "solidarity" or "conflict" or "power" (cf. Brown and Gilman, 1960); whether a person's "dialect" is a constant for them or whether we are all (as my own hypothesis suggests) to some extent linguistic chameleons, depending on the identity we are trying to project in any particular context. We find important contrasts between societies in which caste and class are underscored by religion (compare older and younger Sikhs in Britain as studied by Agnihotri (1979)) or by skin-color, and so on. We tend to make observations of other cultures in terms of significant differences in our own; but in Le Page and Tabouret-Keller (1985: chapter 6) we document how the self-ascription of some Belizeans to ethnicity and skin-color differed significantly from the ascription we would have made ourselves. A good deal of the criticism of sociolinguistic work made by sociologists has picked on the simplistic nature of the social structures against which we have matched linguistic variation. We also tend to hear other languages in terms of salient structural features of our own, and perhaps therefore overlook linguistic features – prosodic features, for example – which are socially significant to other speakers of "the same language." This is particularly likely if we come to a community as outsiders, rather than as an insider like Labov in New York or Trudgill in Norwich. Being an insider carries its own dangers of seeking to confirm one's own stereotypes.

Salient problems and possibilities of dialect geography are explored by Chambers and Trudgill (1980), insofar as "dialects of one language" are concerned. In the West Indies, and in creole studies generally, however, we have been thrown hard against the questions "what constitutes a language?" and "what should a linguistic system look like if it is to take full account of the social and psychological realities?"

5 The development of contact linguistics in the 1950s

Romaine's 1989 study of Bilingualism begins by pointing out that it would be odd to encounter a book entitled Monolingualism; that is the state presupposed by most modern linguistic theory, and yet bi- (or multi-)lingualism is a far more common state, and was for Roman Jakobson (1953) "the fundamental problem of linguistics."

The Oxford English Dictionary Supplement (1986) records the term "sociolinguistic" as first used by Eugene Nida in the second edition of his standard work Morphology in 1949; as a discipline, "sociolinguistics" is first referred to in 1939 – in T. C. Hodson's paper, "Sociolinguistics in India" (in Man in India, XIX, 94). The term was used by Martinet in his preface to Weinreich's thesis Languages in Contact (1953), where he went on to say (p. vii):

There was a time when the progress of research required that each community should be considered linguistically self-contained and homogeneous ... Linguists will always have to revert at times to this pragmatic assumption. But we shall now have to stress the fact that a linguistic community is never homogeneous and hardly ever self-contained ... linguistic diversity begins next door, nay, at home, and within one and the same man. It is not enough to point out that each individual is a battleground for conflicting types and habits ... What we heedlessly and somewhat rashly call "a language" is the aggregate of millions of such microcosms many of which evince such aberrant linguistic comportment that the question arises whether they should not be grouped into other "languages." What further complicates the picture, and may, at the same time, contribute to clarify it, is the feeling of linguistic allegiance which will largely determine the responses of every individual.

(my emphasis)

Chomsky's thesis, published four years later as Syntactic Structures (1957), was nevertheless predicated on the knowledge of that famous phantom, the idealized speaker-listener in a
homogeneous speech community with complete knowledge of its language. Much of the story of
“contact linguistics” in the early 1960s was of the search for a model which could accommodate both
Martinet's very pertinent observations about data and Chomsky's concepts of a grammar whose
adequacy could only be tested against that contradiction in terms, the intuition of the idealized native
speaker.

Weinreich wrote in terms of a pair of discrete systems interfering with each other in the usage of
individuals, citing Lotz (1950) to the effect that “every speech event belongs to a definite language”
and Jakobson, Fant, and Halle (1952) on “code-switching.” Weinreich supervised Beryl Bailey's thesis
(1966), a grammar of “Jamaican Creole” (as she and I decided to call it, thus giving it discrete status as
a language), written within the early transformational–generative model at his suggestion. In her
introduction she acknowledged the difficulty of trying to draw lines of demarcation between the Creole
and more standard varieties of English: “it is perhaps more accurate to think not of layers, but of
_interwoven co-structures_” (my emphasis). “This very fluid linguistic situation could provide material for
a fruitful and rewarding study on linguistic variation and the principles underlying it, but … this is an
attempt to describe one of the systems which lie at the core of this co-structure” (pp. 1–2). The
difficulties in fact carry over into describing contact systems in terms of “pairs of discrete languages
interacting,” and into any situation where a speaker is “bi-dialectal” – as most of us are. (See in
particular Gardner-Chloros, forthcoming.)

6 The 1960s

In both the US and Britain there had been a long tradition of anthropological linguistics allied to
structuralist descriptions of exotic languages; in the US it was associated with such names as Boas and
Sapir, in Britain (see, for example, Ardener, 1971), with Malinowski and J. R. Firth and his colleagues at
the School of Oriental and African Studies in London. Each had given rise to distinctive theories of
linguistic structure which helped to shape sociolinguistic research (see, for example, Hasan, 1992). But
prior to Bernstein, comparatively little attention had been paid by _sociologists_ to linguistics as an
analytical tool.

Ferguson's notable paper on “diglossia,” a functional analysis of different registers of “the same
language,” had appeared in 1959 (its application to Haiti, in particular, raised for creolists the critical
question of what constituted a language – see Le Page, 1989). Labov's work on the social dynamics of
Martha's Vineyard and then of New York City opened the eyes of some sociologists to the possibilities
of such “scientific” analytical methods. In the summer of 1964, the Committee on Sociolinguistics of
the US Social Science Research Council called a group of linguists and other “social scientists” together
for an eight–week interdisciplinary seminar, reported on by Ferguson (1965). In Britain, my own
department at York was set up in 1963/4 “for the study of language as a behavioural science,” and the
British SSRC began to take linguistic research under its wing. As Chomskyan theory appeared to sweep
away all concern with variation in language by focusing exclusively on “competence” in “a language,”
Hymes and his anthropologist/ethnographer colleagues reacted by requiring linguistics to complete its
scope by describing the “communicative competence” a speaker/listener needed to operate as a full
member of a language community – a knowledge of all the appropriate ways of using the language.
The first volume of Fishman's _Advances in the Sociology of Language_ (1971) brought together three
long retrospective and programmatic papers by Ervin–Tripp (1969), Grimshaw, and Labov on socio–
Linguistics, and – for half the book – Fishman himself, striving to keep “the sociology of language”
distinct. Ervin–Tripp's paper is of particular interest for me; she is concerned to build a model of the
_symbiotic relationship_ between the formation and dynamics of human groups and the formation and
dynamics of human language, as I was too. Towards the end of the 1960s Hymes and DeCamp came
together to organize the 1968 Mona conference (see Hymes, 1971).

7 Models of inherently variable systems: implicational scales

F. G. Cassidy, David DeCamp, and Beryl Bailey were three American linguists who helped me with the
Caribbean survey; British helpers included an Africanist (J. Berry), A. McIntosh, and A. S. C. Ross. Apart
from his mathematical expertise, Ross had in fact published an early “sociolinguistic” study (1954) of
differences between “Upper-class” ("U") and “non–Upper-class” ("Non-U") usage in England, a
distinction made famous for a while by Nancy Mitford and Ross in _Noblesse Oblige_ (1956), Mitford

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having furnished examples of each in *The Pursuit of Love* (1945) and in *Love in a Cold Climate* (1949).

At the end of his two-year stay in Jamaica DeCamp, who had earlier studied the English of San Francisco (1958/9), gave a paper to the first international conference on creole language studies (Le Page, 1961) on “Social factors in Jamaican dialects.” On his return to the US he, like Bailey, was caught up in the Chomskyan revolution as he analyzed his large collection of Jamaican material and tried to systematize in social terms the variation inherent in it. While Bailey resolved the problem by idealizing the usage of “older people and members of the lower levels of the social code” (1966:3) DeCamp treated his data as lying at different points on a linear continuum from broad Creole to more educated speech – what W. A. Stewart was to characterize as “basilect” to “acrolect.” For the 1968 Mona conference (Hymes, 1971:349-70) DeCamp presented a paper “Toward a generative analysis of a post-creole speech continuum,” in which he recognized that the “system” he was evolving resembled that of the Guttman psychological implicational scalogram (1944), as he tried to preserve the Chomskyan concept of a grammar while showing how individuals might move across a continuum of lects. The overarching “grammar of the Jamaican language” was to contain a series of grammars each differing by a single rule–change from its neighbors on either side; the “implication” was that a person affected by, say, rule change 3 had also been affected by changes 2 and 1. Each “lect” was to be seen as socially marked.

Bickerton (1971, 1975), rejecting variable rules (see below), subsequently took up this same model for “the Guyanese language,” and C.-J. N. Bailey used it (1973) as a base for his reformulation of nineteenth–century wave theory. These were brave attempts to build into generative theory both social and historical variation and to assess also (in terms of the degree of scalability of the data) the degree of relationship between the two ends of the continuum. But DeCamp was never able to fit more than 30 per cent of his data into such scales. Bickerton’s use of the model seemed to imply that “the Guyanese language” and the people using it were on a historical escalator moving steadily from Creole towards the more standard English of the urban educated classes (cf. Romaine’s (1982b) discussion of Labov, a model certainly not borne out by our own later work in Belize; it implied that all innovation came from the same direction, but much sociolinguistic work elsewhere has shown that this is rarely the case, and that one needs a multidimensional model. Moreover, it is frequently the case that change in a formal register is running counter to changes in more vernacular registers, which then ultimately either affect formal speech or lead to divergence and splitting.

The weaknesses of implicational scaling are discussed by Romaine (1982a), who nevertheless made use of it in her 1992 study of “Tok Pisin.” She diagrams the trajectory of, for example, a sound–change through lexical diffusion and across communities in Papua–New Guinea, reflecting different degrees of the influence of substrate and superstate languages on each member of the community. Variability of, for example, the pronunciation in “Tok Pisin” of initial p/f (as in /fo/ ~ /po/four – pp. 181ff.) shows up the various social factors which may have to be taken into account to “explain” why some groups of adults or of children have adopted the [f] pronunciation throughout their vocabulary while others have it in only one or two items. Years of schooling, and urban vs. rural location, are the most significant non–linguistic correlates. On a global scale, urban society in Papua–New Guinea must be towards one extreme of the diffuse–focused spectrum, while the highly–focused languages of the remote mountain valleys are at the other.

8 The observer’s paradox, and accommodation theory

One reason for the different solutions arrived at by Bailey and DeCamp may lie in what Labov has called “the observer’s paradox” – in which sociolinguistics resembles quantum theory: It is not possible to observe the behavior either of very small particles or of an interlocutor without affecting that behavior. In Le Page, 1968, I demonstrated the effect my own presence had at the start of a girl’s telling of an Anansi story compared with her delivery as soon as the traditional story–telling mode took charge. DeCamp was a white male American, Bailey a black female Jamaican–American. The selection of variants was to some extent at least a function of interlocutor and mode; interlocutors may incline to each other’s usage, as the accommodation theory of Howard Giles (1984) suggests. There are, however, important caveats about this concept, which I discuss below.

9 Labov, “acts of identity,” language variation and change, and variable rules
William Labov, after ten years as an industrial chemist, wrote his MA thesis as Weinreich's pupil at Columbia University; it was published in *Word* in 1963 as "The social motivation of a sound-change." It was based on data he collected in the summer resort of Martha's Vineyard, and demonstrated the linguistic acts of identity made by those who wished to be recognized as natives of the island as distinct from summer visitors and commuters. His doctoral thesis, *The Social Stratification of English in New York City* (again under Weinreich) was published in 1966. He had studied research methods at the Bureau of Social Research in New York; and while a graduate student had taken part in sociolinguistics meetings organized respectively by Gumperz, Ferguson, and Bright. In his thesis he approached sociolinguistic analysis primarily as a means of dealing with variation in a principled way – i.e., to account for it within the framework of categorical, invariant rules (Labov, 1971b: 461). In relating phonological variation and change to social class and social prestige in New York he had been preceded by studies of Black usage in Washington (Putnam and O'Hern, 1955) and by Ruth Reichstein's 1960 study (also in *Word*) of 570 girls at three socially differentiated Parisian schools. She quantified the incidence of the variants of three phonological variables and correlated them with the type of school, the *arrondissement*, the middle-class or lower-class occupation of the parents, and whether they came originally from Paris or from the provinces. She was able to show the direction of phonological change as affected by the usage of working-class districts near the center of Paris.

Labov's "stratified random sample" of New York speakers took account of their "ethnic origin" (New Yorkers, Italians, Jews, Blacks) and their assignation to a social class: Upper Middle, Lower Middle, Upper and Lower Working Class, on the basis of income, level of education, and occupation. (Trudgill's 1974 Norwich study followed Labov's schema fairly closely.) Labov introduced the variable of *style* into his analysis, recording formal and informal speech, reading aloud from a text, and reading a series of minimal word-pairs, to cover the range from least to most relaxed speech, a procedure we later followed in our Belize studies (Le Page and Tabouret-Keller, 1985). The "most relaxed" style was said to be marked by "channel cues" such as laughter, prosodic features, and speed. The assumption was made that the informants would be speaking "English."

Labov's most important innovation in the 1966 study was to quantify the incidence in different speech samples of variants of significant linguistic variables and then to write "variable rules." The quantities he plotted were not individual scores, but the mean scores for each social group in each style or mode. If we take as a simplified example his pilot study of the use of the variable "post-vocalic (r)," getting shop assistants in three different "classes" of New York stores to direct him first in a casual and then in an emphatic way to the "fourth floor," we can say that the group of shop assistants in each of the stores had an average incidence of some kind of rhoticity (r-coloring) which he calls (r-'), and an average complementary incidence of r-lessness (r-), in the loci where (r-) might occur – that is, where (-r) is present in "the lexicon of the language." A "rule" might then emerge in the following "explanatory" way within the grammar of "New York English" (NYE):

1. The variable (r) can occur in NYE in the following position [statement here as to possible loci] either as (r-) or (r-'), since a sound-change affecting it has not affected all speakers of NYE equally, for a variety of reasons.

2. Among a group of "working class New Yorkers" the mean incidence was observed to be in x percent of possible loci as (r-) and in y percent of possible loci as (r-) when they were speaking in a relaxed way.

3. Among the same group speaking in a more formal and deliberate way there was a higher percentage of (r-'), lower (r-).

4. The second score was in fact more like that of the mean relaxed usage of a middle-class group.

5. Therefore the motivation of the lower-class group when in a more formal context was to behave more like the middle-class group.

If we then divide the lower class into younger and older groups we may find that this tendency is more pronounced among the younger group. We might then form a hypothesis that a change is taking place in NYE towards a greater use of post-vocalic (r-') and seek a reason for the change – in this case, led by the middle class and possibly associated with the effects of literacy and an increase in spelling pronunciations (Labov did not advance this last possibility). An alternative hypothesis (again, not...
advanced by Labov) might be that within this society there is a mode of behavior appropriate to older people to which the younger group will switch as they age. Other hypotheses may be suggested if we divide the speakers by sex, or by ethnic group, or locality in which they live; and many of these have been tested in the work of Trudgill (1972, 1974, et seq.), Cheshire (1982), and other sociolinguists. Explanations may well be attitudinal, a feeling of social cohesion among a particular social group (see W. Edwards below) promoted by one or more of a number of factors (see, for example, both Labov's and Trudgill's 1972 concept of “covert prestige”) in the light of which we may form and test more general explanatory hypotheses about correlations between linguistic behavior and social or idiosyncratic psychological motivation – for example, in the case of Labov’s “lames” (1973a) and Milroy’s “weak ties” (1992), people who did not keep to the linguistic or social rules of the group with which they might otherwise have been closely associated.

There has been a good deal of discussion of the problems raised by variable rules (see Fasold, 1970; Lavandera, 1978; Horvath, 1985; Romaine, 1985). In a 1970 paper (“The study of language in its social context”) Labov set out his theoretical and methodological ideas and the discussion which had taken place around them. In a study of the statistical methods used, Cedergren and Sankoff (1974) proposed using a multiplicative rather than an additive probability model in order to retain the Chomskyan competence/performance distinction, to fit rule probabilities into competence and their frequencies into performance. But in spite of Labov’s (1969) insistence that they are part of the speaker’s knowledge of the group, part of a sociolectal grammar, they remain in fact statistical probability statements. Contrary to what Bickerton (1971) supposed in his objection to them and consequent support for implicational scales, they cannot be held to predict what a speaker will do on any particular occasion, however carefully the conditions are specified. The problems arise only if one forgets that sociolectal grammars are abstractions from a usually unspecifiable group of idiolects, however much they are buttressed from normative sources and stereotypically assigned autonomous status.

The question whether Black English, or any Creole, or indeed any one of very many vernaculars, should be treated as a discrete system or as a variety of some standard system can assume considerable ideological and political importance and has been tested in the US courts (see IGLSVL Abstracts, 1986, 1988, passim).

10 Non-standard English, Black English, Creole studies, and the 1968 Mona Conference

In the 1950s and early 1960s Basil Bernstein's “deficit hypothesis” – that working-class children internalized a ‘restricted code’ from their parents’ limited syntax and so did badly at school where middle-class teachers judged them in terms of their own “elaborated code” – gained wide currency among educationists in Europe (see Dittmar, 1976) and in the US (see Murray, 1983) where ambitious remedial programs such as Operation Headstart were conceived, particularly for Black children. Labov, who was opposed to the basic premises about the “deficient language” of these children, carried out for the US Office of Education and Welfare a study “of the structure of English used by Negro and Puerto Rican speakers in New York City” (see Labov, Cohen, and Robins, 1965). His well-known account of the variable rules he proposed for copula−less Black English constructions (Labov, 1969) derives from this study of Harlem street gangs. In his account, the Black English speaker’s copula−less constructions (e.g., “The man sick”) are derived from the standard grammar by a variable deletion rule which the speaker could apply wherever it was possible for prior contraction of the copula to take place. In fact, however, copula−less constructions are the norm in Caribbean Creole English, and it seems absurd to posit that the Black English speaker should start from a knowledge of the standard grammar and then apply a deletion rule to achieve the vernacular. It might be better to accept the use of such constructions alongside those of more standard English as examples of code−switching.

There were by this time a number of other educational, sociolinguistic, and sociological studies under way on Black English speakers by, for example, W. A. Stewart, J. L. Dillard, R. W. Shuy, R. Fasold, W. A. Wolfram (see bibliography). Work on pidgin and creole languages was expanding steadily; in the 1960s two volumes of my Creole Language Studies, B. L. Bailey’s Jamaican Creole Syntax, Cassidy’s Jamaica Talk, and the first edition of the Dictionary of Jamaican English (1967) were all published and came to the attention of the Americans. In 1968 Dell Hymes and DeCamp organized the second international conference on pidgin and creole languages at Mona, Jamaica (Hymes, 1971) and Labov wound it up with an extended survey, “The notion of system in creole languages.” Several speakers had approached
the problem from different directions: DeCamp's "Analysis of a post–creole speech continuum," Gumperz and Wilson's study of "convergence and creolization" on the Indo–Aryan/Dravidian border in India, Samarin's "Salient and substantive pidginization," Tszukai's "Co–existent systems in Hawaiian English." The solutions included code–switching between coexistent systems, DeCamp's implicational scalograms, and the concept of mixed codes. Labov had concluded that creole/standard language and pidgin contact situations could not be fitted into the Chomskyan paradigm, and that DeCamp was mistaken in trying to do so; he felt that some of the pidgins discussed "may be less than languages ... the notion of syntax begins to give way." To make a leap forward in time, in Mühlhäusler's (1986a, b) opinion (and my own) what has to give way is the stereotypical notion of "a language."

Studies of creoles and pidgins and of Black English have been important to theoretical linguistics, to the sociology of education, to social anthropology, and to sociolinguistics in both the US and Britain; see, for example, V. Edwards (1986), R. Hewitt (1986), S. Romaine (1988, 1992), M. Sebba (1993), and the work by W. Edwards cited below and by Bickerton, Fasold, Shuy, Stewart, Wolfram and others cited above. A number of writers (notably Thomason and Kaufman, 1988) have concluded (I think erroneously) that creole languages are a special case in language typology, standing outside the schema of "genetic relationship" regarded as the norm (see Le Page, 1994).

11 The individual speaker and social networks

L. Milroy's retention of the individual speaker at the center of her sociolinguistic study of three working–class districts of Belfast (1980, 1987a, b) took over an explicit concept of "social networks" as social structures which focus norms of behavior including language. She draws attention to the sense of belonging to a locality, in an urban context strongest in working–class districts (1987a: 15). She refers to the study by Blom and Gumperz (1972) of code–switching in Hemnes, Norway, between Standard Norwegian and varieties of the local dialect. They found the working–class networks more local and more tightly–knit than those of the elite, and the local dialect more focused. Both Blom and Gumperz, and Gal in her Oberwart study (1979), emphasize the role of close–knit networks in the maintenance of nonstandard language norms; they are also important in the maintenance of minority languages.

Milroy allowed her networks to define themselves as she was passed along them as "a friend of a friend." They were scored for density and multiplexity; "a network is said to be relatively dense if a large number of the persons to whom ego is linked are also linked to each other"; it is multiplex if, for example, "the same man may be connected to ego as co–employee, neighbor, kin and in many other capacities" (1987a: 50–1). The incidence of each variant of the chosen phonological variables was then correlated with these scores for each individual informant, the correlations plotted and then further examined for clusters significant for age or sex. Illustrated here is the strength or weakness of the ties within a community reflected in the degree of focusing or diffusion in the use of linguistic features within the group and the degree of attachment of individuals to the group, having either strong or weak ties.

According to Williams (1992) Milroy's assumption that the social group is an amalgam of individuals is "a rejection of the fundamental basis of sociological analysis" (p. 195). He had earlier (p. 13) cited Durkheim in support: "Collective representation, emotion, and tendencies are caused not by certain states of the consciousness of individuals but by the conditions in which the social group in its totality is placed" (Durkheim, 1938: 106). Williams is disparaging about all the sociolinguistic work he selects for discussion, finding implicit in it the sociological theories of the American J. Talcott Parsons, and no reflection of class solidarity and conflict. Labov's New York studies are among those condemned. In a 1992 paper, however, L. and J. Milroy attempt a careful integration of class distinctions and network relationships; they answer some of Labov's criticisms about the lack of explanatory power in the strength of network ties, and apply their ideas to the data about Black and White vernacular usage in Philadelphia published by Labov and Harris in 1986. They examine the role of strong network ties in resisting language change and of weak ties in bringing about change. In order to achieve this integration they adopt the analysis by the Danish Marxist anthropologist Thomas Højrup of social class as a "large–scale and ultimately economically–driven process that splits populations into sub–groups" characterized by what Højrup describes as "life models." In fact, a great deal of Language and Social Networks is about working–class solidarity in Belfast. The question of "sociolinguistic explanation" is discussed also by J. Milroy (1992a, b).
Social networks have since been used as exploratory tools by a number of sociolinguists – see, for example, Russell (1981), Bortoni-Ricardo (1985), Schmidt (1985), Lippi-Green (1989), Salami (1991), and W. Edwards (1992) in his study of “sociolinguistic behavior in a Detroit inner-city black neighborhood.” Edwards illustrates the fact that the cultural significance of variation in the incidence of variants may itself be a variable, and emphasizes that “attitudinal characteristics are as important as objective social characteristics in influencing linguistic behavior” (p. 112).

12 “Natural language,” participant observation, and quantitative studies

Methods of collecting data have been discussed in detail by L. Milroy in Observing and Analysing Natural Language (1987b), among others. The term “natural language” has two implications: on the one hand, that one is not working with the “unnatural” made-up sentences of grammar books; on the other, that one is bound to try to overcome Labov’s “observer’s paradox” as much as possible. (It is generally agreed that it is unethical to tape-record speakers without their knowledge.) Labov has tried to elicit a “natural” style by asking about exciting incidents in his informants’ lives. In his Black English work in Harlem he made some use, as we did in Belize, of radio microphones worn by informants while playing together. In his Philadelphia project he combined what he felt to be the advantages of his earlier survey methods in obtaining “a representative sample of opinions and attitudes from an enumerated population” with those of participant–observer procedures commonly used by social anthropologists and ethnographers, while reducing the distortions inherent in each: “by combining both methods, we can estimate the degree and direction of error in our final statement of the rules of the vernacular” (1981: 4). In Belfast L. Milroy also made use of participant observation, for example, sitting in the kitchen with the “friend” she was visiting, with the tape recorder running, while friends and neighbors dropped in and chatted.

In our Belize study (Le Page and Tabouret-Keller, 1985) we made use of lengthy participant observation of families together with recorded structured interviews of the children. It is necessary, however, to question the stereotype of “most natural language.” It is certainly true that in some contexts we monitor our speech more carefully than in others, sometimes to the point of hypercorrection (an important social indicator, as Labov (1972) pointed out). But we are always speaking to somebody even if it is only ourselves, and always (for the most part quite unconsciously) adopting a style, or trying to adopt a style, felt to be appropriate to our view of our relationship to our interlocutor. The whole process, and each style, are “natural,” part of our “acts of identity.” It is important not to equate this process with Giles’s theory of “accommodation”: we do not necessarily adapt to the style of our interlocutor, but rather to the image we have of ourselves in relation to our interlocutor.

Labov’s concept of stylistic variation was questioned by Cheshire (1982) in her investigation of variation within an English dialect. “Style–shifting,” a concept paralleling “code–switching,” was the framework for N. Coupland’s (1980) investigation in a Cardiff workplace, and the subject of a number of other investigations, some linked with Giles’s concept of “accommodation” (for example, in Giles and J. Coupland, 1991).

13 Acts of Identity

The hypothesis tested in our surveys of Cayo District, Belize, the northern part of St Lucia, and the Jamaican community in south-east London, derived from the problems of linguistic description which I had encountered during ten years’ living and teaching in the West Indies (Le Page, 1968).

It states that individuals create (the use of this word does not imply consciousness or “rationality”) their linguistic systems so as to resemble those of the group or groups they wish from time to time to be identified with, or so as to distinguish themselves from those they wish to distance themselves from. Their success in doing so is subject to constraints under four heads: their ability to identify the groups; the extent of their access to them and ability to analyze their linguistic systems; the strength of their motivation, which is likely to be multidimensional; and their ability to change their behavior – possibly mainly a function of age.

What we quantify here is the resultant of a multidimensional vector diagram. The hypothesis attempts to construct a general framework to connect individual motivation with the social and cultural
organization with which it is in symbiotic relationship, and with the linguistic symptoms of that relationship. Statistical studies then provide the basis for an evaluation of the weighting to be given to various motives.

"Projection, focusing, diffusion." The model of speaking and listening which accompanies the hypothesis is of speakers projecting an image (in a cinematic sense) of themselves in relation to their universe, and getting feedback from others as to the extent to which their images coincide – and then either collectively focusing, or allowing these images to remain diffuse. A pair of speakers may agree on a common usage which contains two more or less discrete systems. It was common, for example, in parts of Cayo District for parents regularly to speak to their children in a kind of Spanish and be answered in a kind of Creole, each accepting the generational status of the other, each having a passive competence in both "languages."

Cayo District had recently had a road built through it from the Guatemalan frontier to the coast. "Spanish" speakers had moved into the newly-developing land from the west, "Creole" speakers from the east, "Maya" speakers from north and south, "Carib" or "Garifuna" speakers from the southeast. None of these labels represented a wholly well-focused system; our aim was to discover what was emerging from the linguistic and ethnic mélange. There were complex political implications in the changes taking place.

Without needing to make any prior assumptions about social groups of any kind we were able (using a cluster analysis program) to see what clusters or groups the children formed according to the similarity of their scores on up to 25 variables – five linguistic variables (phonological and morphological) in five modes of behavior, most formal to least formal, including oral storytelling. We were then able to test for each child the extent to which membership of a cluster correlated with any one of a number of nonlinguistic variables, and thus to rank those for their importance in motivating the child's language. We were able to trace the linguistic trajectory of each child as it moved from more formal to less formal behavior. In a follow-up study of 40 of our original 280 informants eight years later we were able to discuss and check the validity of our 1970 conclusions about the general move towards "Creole" as the lingua franca of a newly independent Belize, and the shift in the forms which were now understood by the term "Creole," away from the older Creole of the coast; also, conclusions about the political, economic, ethnic, and cultural forces at work. We worked always from the individual child towards the clusters of their trajectories, and from these to possible matches with social stereotypes and social facts. There were many problems with what was an ambitious program (see McEntegart and Le Page, 1982); nevertheless, the model proved to be a powerful one.

Romaine (1982) contains a number of valuable papers on "Sociolinguistic variation in speech communities," including her own "What is a speech community?" a critical survey of issues raised by work of the kind discussed above, as well as a critique (McEntegart and Le Page) by our statistician of the methods we used in Belize and St Lucia.

14 Code-switching and "interpretive sociolinguistics"

Clearly, sociolinguistic studies in multilingual communities must include "code-switching," "code mixing," and the use of "mixed codes." A puzzle here, in spite of Lotz's (1950) confident assertion referred to earlier, is, as Martinet says, to know to which code a unit belongs, other than to the idiolect of the speaker, whether to one or other of two or more discrete codes, or to an interlanguage. Gardner-Chloros (forthcoming) has argued cogently, and has exemplified from her wide range of code-switching data, the "mythological" nature of the concept of the discreteness of linguistic systems.

Some work has concentrated on trying to specify the syntactic constraints on switching (see, for example, Sankoff, Poplack, and Vanniarajan, 1991), other work on the social context, topic and domain (e.g., Gardner-Chloros, 1991); some combines both approaches. The Workshops of the European Science Foundation's Code-switching and Language Contact Network (ESF, 1990–1) heard no fewer than 51 papers. Attempts to develop universally valid theories are well represented among them, notably by Myers-Scotton, who has developed since 1983 "a model to explain the social motivations of code-switching" which she refers to as "the markedness model" (MM), underlying which is the premise that "speakers use making code choices to negotiate interpersonal relationships" (1990: 58). The "negotiation principle" she proposes directs the speaker to "choose the form of your conversational contribution such that it symbolizes the set of rights and obligations which you wish to be in force
between speaker and addressee for the current exchange.” This principle is said to rest on an innate
theory of markedness and indexicality, and a “markedness metric” which “predisposes speakers to
assign readings of markedness to codes in the community's linguistic repertoire for specific
interactions in their community.” The syntactic rules for the overlapping use of the languages involved
are set out within a “Frame and process model.” Another starting point not dissimilar from my own is
that of Peter Auer, seeing bilingual individuals “using their verbal resources as social actors in everyday
exchanges” (1991: 320), and building a theory of code alternation on the theory of contextualization
developed by Gumperz (1982, 1992, etc.).

My heading here, “interpretive sociolinguistics,” is borrowed from Auer and Di Luzio (1984); it seems
an admirable term for work which starts from the observation of linguistic behavior and interprets it in
terms of social meaning, rather than starting from social structure and looking for linguistic correlates.
A good deal of work on code-switching is of this first kind, a model for which might be Blom and

15 Conclusion

Cameron (1990) has harsh things to say about the “naive” and “crude” models of society that
quantitative sociolinguistics uses; the way in which “the primacy of linguistic over social issues is
vigorously asserted” by, for example, Trudgill (1978) and Hudson (1980), and in which the quest for
“scientific status” has been part of the eschewing of sociological theory and acceptance of a vague
claim that “language mirrors society.” We need, she claims, a theory linking the “linguistic” to the
“socio”; without it we are “stranded in an explanatory void.” Ad hoc social theories are inadequate to
make good the lack.

My own “acts of identity” hypothesis was an attempt to provide just such a linking theory. The
multidimensional clusters of children are held to reflect the groups which they perceive in their society,
this perception then having the function of gravity in their galaxies, within the constraints of the four
riders. Our tests are to determine the extent to which the clusters they appear to form do match
against the objective realities of their society. Both the groups and the linguistic properties they are
endowed with are percepts of each individual and idiosyncratic, although clearly constrained by
objective realities. The activities of “projection” and “focusing” provide the energy through which
groups form. Nothing in this necessarily implies “consciousness” or “rationality” about the processes;
we use these faculties to rationalize our acts of identity post hoc. This may well be dismissed as “ad
hoc social theory”; it is rather a sociolinguistic hypothesis formed after many years of observing
multilingual societies. “Explanation” lies in the complex psychological motivation of the individual,
coerced by social forces. What is being “explained” is not linguistic variation – as both Dante (c. 1300)
and Martinet have observed, what needs to be “explained” is the formation of the concept of a
homogeneous “language.” In Acts of Identity we set out how we saw such a concept evolving from
observation of discourse, through the stereotypes denoted by such language-owning names as
“English” or “French,” to that of the most highly abstract and focused Chomskyan “grammar”; and how
actual linguistic behavior was influenced by the stereotypes as progressively it was named, formalized,
standardized, institutionalized, and totemized by a society.

Romaine (1984) challenges network studies on the grounds that they do not explain anything. But it is
not true that networks are “theoretical constructs”; rather, they evaluate the ways in which people do
relate to each other. They may not “explain” anything, but they give us insight into the way a society
works. One weakness common to much sociolinguistic theorizing is a stereotypical view of cause and
effect. Once both Fishman and I at a meeting in Quebec tried to point out that social ills there could
not be cured by forcing everybody to use French, that language use was a symptom, not a cause, of
their problems, and that at most language is in a symbiotic relationship with social factors. Nor is it
ture, as Cameron asserts, that speakers “inherit a system and can only choose from the options it
makes available.” Inherent within all linguistic systems are two fundamental characteristics which
stereotypically are lost sight of: Their units and processes have values arrived at idiosyncratically and
then negotiated collectively; and built into them is the capacity for analogical creativity, available to
and made use of by every speaker–listener, and a source of language change. A language is best
thought of as a game in which all the speakers can covertly propose and try out rules, and all the
listeners are umpires.
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