11   Discourse Variation

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Mais le discours sied mal à qui cherche du sang

Cyrano de Bergerac

Linguists have generally been suspicious of the notion of discourse. Chomsky influentially declared that the proper study of linguistics was sentences and that studies of language use were irrelevant (Chomsky 1986), and so it is perhaps ironical that the literature on discourse analysis should begin with the work of Chomsky's mentor Zellig Harris (1952, 1963) whose work starts from the principle that language consists of small units that are combined into larger ones: phonemes are combined to make morphemes, morphemes are combined to make words and phrases, and so on. Harris was attempting to find "some global structure characterizing the whole discourse" by identifying "a pattern of occurrence (i.e. a recurrence) of segments of the discourse relative to each other" (Harris 1963: 7). Harris believed that "such relative occurrence of parts is the only type of structure that can be investigated by inspection of the discourse without bringing into account other types of data, such as relations of meanings throughout the discourse" (Harris 1963: 7). However, Harris found that he could not avoid "judgment of the meanings of morphemes" (1963: 72) but "we try to apply [this operation] sparingly, in a way that leads to fewest applications" (1963: 65). In practice, Harris was principally concerned with what Halliday and Hasan (1976) call *cohesion*, represented in an abstract schema.

   Harris's definition of discourse was "any connected linear material . . . which contains more than one elementary sentence" (1963: 7), echoed by Stubbs, in his textbook as "the organization of language above the sentence or above the clause" (1983: 1). This reflects what Linell (1982) calls "the written bias in linguistics," since the appropriateness of the term "sentence" in describing spoken language has still to be demonstrated (O'Connell 1988: 258–9; Macaulay 1997: 140). A range of definitions of discourse is examined by Schiffrin (1994:

20–43), distinguishing between structural (formalist) and functional approaches. Since the purpose of this chapter is not to examine discourse analysis per se, but the results of studies that show discourse variation, it is not necessary to define the term narrowly. People use language, both spoken and written, for a variety of purposes and all these uses can be examined under the heading of discourse. The study of discourse variation is the attempt to find patterns of language use that characterize the spoken language of a definable group in a specific setting. In this chapter I will not deal with written discourse.

However, that does not eliminate all problems. One of the most common functions of discourse is to communicate something, but the proper study of linguistics is not communication. (In this case, I agree with Chomsky.) Linguists are concerned with the use of language in communication, but that is a very different thing. To take an obvious example, conversation analysts (e.g. C. Goodwin 1981, Sacks 1992, Schegloff 1992) and psychologists (e.g. O'Connell 1988) have shown the significance of pauses and silence in communicating. However, there can be no *linguistic* analysis of silence, though pauses may be a guide to linguistic units. Similarly, the use of gestures and facial expressions can be crucial to communication (Kendon 1990, McNeill 1992, McNeill and Levy 1993), but they form part of a different system than that of verbal communication.

Investigating variation in discourse presents different problems from those in examining variation in other aspects of language. Ever since Labov's pioneering work on Martha's Vineyard (Labov 1963) quantitative methods have been used in sociolinguistic investigation. Usually the items counted have been phonological or morphological, following Labov's view that the most appropriate candidates to be treated as linguistic variables are those that occur frequently and are less liable to conscious manipulation (Labov 1966). For this purpose, relatively small samples of speech may be adequate, and even such unnatural speech acts as reading out a list of words may provide useful information on differences between speakers. The investigation of spoken discourse, however, requires evidence collected in settings where the nature of the speech event is clear and the roles of the participants can be established. The study of discourse also usually requires larger samples of language use. It also requires many methodological decisions that are not crucial in studying other kinds of variation.

It is also fair to say that the study of discourse variation is at an elementary stage. There is no general agreement on methods of collecting or analyzing data, on what features are suitable for investigation, on how to identify possible discourse features, and what significance to attach to the use of a particular feature (Tannen 1994). Even when a feature is relatively easy to identify formally (e.g. tag questions) it may be less easy to determine what functions the feature performs. Moreover, unlike phonological and morphological variables, for discourse features there is seldom a context in which one variant or another must occur. Instead, the most important aspect of a discourse may be the frequency with which some feature occurs. Since samples are often of unequal

size, it is essential for comparative purposes that the raw count of occurrences be converted to a frequency of, say, number of occurrences per thousand words. Where samples of equal length have been analyzed this is not necessary for internal comparisons but has to be calculated for comparison with other studies.

1   Two Approaches to Discourse Studies

Much of this chapter will be taken up with examining individual studies, not only for what their results show but also to find out what lessons can be learned from the approach employed. This emphasis will be most evident in the section on quantitative methods. Since the field is so broad, it would be impossible to provide a comprehensive survey of all the work that has been done. Instead, I will try to provide examples of different kinds of investigations and the methods involved. Since this is obviously not a complete review, no inferences should be drawn from the failure to mention any individual work. Finally, there are many works on discourse analysis that are not cited because they do not deal with *variation* in discourse.

The two approaches to the study of discourse that I will be concerned with are the ethnographer's observation of communicative practices and the sociolinguist's examination of language use, principally from a variationist perspective. The first approach requires that the investigator spends time in the community, observing behavior, identifying speech events in all the complexity set out by Hymes (1974). Sociolinguists, on the other hand, have concentrated more on the characteristics of texts, either written materials or transcripts of recorded speech. Some investigators have combined both approaches. I will not be directly concerned with the Foucauldian notion of discourse as "grouping of utterances or sentences, statements which are enacted within a social context, which are determined by that social context and which contribute to the way that social context continues its existence" (Mills 1997: 11) or what Gee (1999: 7) calls "Big D Discourses . . . to enact specific activities and identities," though any discourse variation will have social consequences. I will also not attempt to cover the field of discourse analysis as a whole but only deal with studies where there is variation in language use that can be identified with membership in a socially determined group.

There is a further problem, however, in limiting the scope of a chapter on "discourse variation" since almost all variation will be in "discourse," but phonetic, phonological, syntactic, etc., variation are examined elsewhere in this volume. In a sense, therefore, "discourse variation" refers to the messy bits that most other contributors steer clear of. Researchers have investigated a wide range of features under the label "discourse," though I will not attempt to deal with all of these. Moreover, much of the variation in discourse involves categories such as social class, gender, and age, etc., that are also dealt with in

other chapters, but the kind of variation examined here is different from that in the other chapters. Finally, some studies of variation in discourse compare differences in language use between different groups or subgroups, while others present accounts of language use in particular societies without making explicit comparisons.

2   Ethnographic Studies

The ethnographic study of discourse is exemplified in the pioneering work of Malinowski (1923, 1935) in his aim to link up "ethnographic descriptions with linguistic analysis which provides language with its cultural context and culture with its linguistic interpretation" (1935: 73). Also influential was the collection of articles edited by Bloch (1975) dealing with political language and oratory. More recent collections on similar topics are Brenneis and Myers (1984) on political language in Oceania, Watson-Gegeo and White (1990) on conflict resolution in Pacific societies, and Hill and Irvine (1993) on the notions of agency, responsibility, and evidence in a range of societies. Duranti (1994) describes the political use of language in a Samoan village. Hanks (1990) examines spatial reference and deixis in a Mayan community. Urban (1991) describes myths and ceremonial rituals in South America. Besnier (1995) deals with emerging literacy on the small Polynesian island of Nukilaelae. Other investigators have reported on characteristic styles of speech: Errington (1988) on Javanese; Katriel (1986) on *dugri* speech in Israel; and Tannen (1984) on New Yorkers' dinner conversation. Moerman (1988) provides a rare example of conversation analysis in a non-western society, Thailand. Studies such as these provide illustrations of language use that can be used for comparative purposes and also provide models for examining discourse in other societies.

One area in which ethnographic studies have been particularly influential has been the investigation of the situations under which children's language development takes place. Ochs' (1988) work on language socialization in Samoa and Schieffelin's (1990) on the Kaluli show that caregivers' behavior towards young children may be very different from that of mainstream American parents. Ward (1971) and Heath (1983) had already shown that ethnic and social class factors also affect child-rearing practices in the USA. These studies show that the pattern of attentive caregivers using child directed speech and making every attempt to understand the young child's initial tentative utterances (e.g. Snow and Ferguson 1977) is far from universal.

Ethnographic studies have also been influential in reporting gender differences in discourse. The most influential has been Robin Lakoff's work (1973, 1975) in which she reported on her own intuitions and as a participant observer of the behavior of middle-class women in the USA and listed a number of features that she claimed were characteristic of "women's language." Lakoff's

work stimulated numerous empirical studies of these features in attempts to support or refute her claims. In particular, there has been strong resistance to her suggestion that women's language "submerges a woman's personal identity, by denying her the means of expressing herself strongly, on the one hand, and encouraging expressions that suggest triviality in subject-matter and uncertainty about it" (Lakoff 1973: 48). Some of these studies will be discussed below.

Keenan Ochs (1974) reports that in a Malagasay community on Madagascar it is the women and not the men who are more outspoken and do not mitigate their speech in expressing criticism or anger. Irvine (1973) shows that among the rural Wolof in Senegal it is the lower caste griots who use the more elaborated speech style while the higher caste nobles take pride in a kind of linguistic incompetence. Basso (1990) observes that the Western Apache prefer silence to speech in encounters where they are unsure about their interlocutor. Bauman (1983) describes the distinctive ways of speaking among Quakers in seventeenth-century England. Studies such as these are a valuable corrective to universalistic claims about speech behavior that are ethnocentrically based.

Ethnographic studies depend directly on the accuracy of the investigator's observations and interpretations, and these will be affected by the role played by the investigator in an interactive context (Duranti and Goodwin 1992). Until another investigator visits a similar Malagasay or Kaluli community, the observations by Keenan Ochs and Schieffelin are likely to remain unchallenged. If challenges do arise at some future time, then a verdict on who is right will depend largely upon the credibility of the researchers. In the case of Lakoff's claims about women's language in the USA, those who were unconvinced usually did not resort simply to counter-claims, based on their own experience. Instead, they attempted to test those claims against empirical evidence collected systematically for the purpose.

3   Sociolinguistic Studies

The most commonly used method for collecting information on language variation has been "the sociolinguistic interview" (Labov 1966, 1981). Wolfson (1976) and Milroy and Milroy (1977) adversely criticize the quality of speech recorded in interviews. I argued against this negative view (Macaulay 1984, 1991) by showing that useful samples of speech could be recorded under these circumstances. Schiffrin (1987) also shows that important discourse features can be studied on the basis of interview data.

Despite the adverse criticism, sociolinguistic interviews can provide valuable evidence of more than phonetic or phonological features, particularly where the same interviewer conducts all the interviews so that there is some consistency in the approach to the interviewee. The role of the interviewer, however, is heavily biased in favor of being a receptive listener rather than an equal

partner in the conversation between "intimate strangers" (Gregersen and Pedersen 1991: 54). In an ideal sociolinguistic interview the interviewee is essentially a monologuist, telling stories, reminiscing, offering opinions, and so on. Clearly, individuals differ in the ways in which they take advantage of this opportunity (Macaulay 1984, 1991, 1999), and one of the important factors will be how the interviewee perceives and reacts to the interviewer (Dubois and Horvath 1993, Eisikovits 1989, Laforest 1993, Macaulay 1991, Schilling-Estes 1998). This is not simply a matter of "audience design" (Bell 1984) since the contribution of both participants is critical and the interviewer's interest in and rapport with the interviewee can have an important effect on the quality of speech recorded (Macaulay 1990, 1991, 2001). Such factors will affect any findings on the use of discourse features.

One alternative is to set up group interviews (Eckert 1990, Gregersen and Pedersen 1991, Labov 1972, Labov et al. 1968). In group sessions, however, there is a much greater chance of extraneous noise and unless each speaker is recorded on a separate track from an individual microphone there is always a risk that it may be difficult to separate out the contribution of each speaker unless their voices are clearly distinct. It is also difficult to arrange a systematic set of group interviews by speakers chosen on the basis of their membership in a particular social category and the results may be disappointing because of the unnaturalness of the speech event (Gregersen and Pedersen 1991: 56). This makes it difficult to obtain comparable samples of speech.

There is a form of data-collecting that lies between the monologues of individual interviews and the polyphony of group sessions. This is to set up a situation in which two speakers, who know each other and who are from the same kind of background, talk to each other in unstructured conversations in optimal recording conditions. This avoids the danger of accommodation (Giles and Powlesland 1975) to the speech of an interviewer, perhaps from outside of the community (Douglas-Cowie 1978) or from a different sector of the community (Rickford and McNair-Knox 1993). Naturally, speakers may react differently to the artificiality of the situation but the method permits the systematic collection of extended samples of speech from a selected sample of the population. The resulting data set will provide materials for comparison between categories of speakers recorded under similar conditions and therefore appropriate for an analysis of any differences that may emerge.

These methods require the cooperation of the speakers and there is always the indeterminate effect of the recording situation. Moreover, many aspects of language use are unlikely to arise in these settings. For some features, it is possible to count their occurrence in a given situation through participant observation. For example, it is possible to observe compliments or apologies (Holmes 1990) and to make a note of the sex of the participants and make an estimate of their age. However, the frequency with which these will occur in the observer's presence will be affected by the sex and age of the observer and the situations that can be observed. It is also possible to obtain information on language use by means of self-report questionnaires.

4   Qualitative Sociolinguistic Studies of Discourse

Labov's earliest work on discourse was his analysis of oral narratives of personal experience (Labov and Waletzky 1967), a work that continues to dominate the field, as a volume celebrating the 30th anniversary of the paper's publication shows (Bamberg 1997). Most of the more than 50 celebrants testify to the usefulness of Labov and Waletzky's model but few have much to offer in the way of additions or improvements.

Labov's (1972) next venture into discourse analysis was to demonstrate that there was no evidence that the kinds of characteristics Basil Bernstein (1962) found in his *elaborated code* represented "a subtle and sophisticated mode of planning utterances" (Labov 1972: 222) and that we need to discover "how much of middle-class style is a matter of fashion and how much actually helps us express ideas clearly" (Labov 1972: 222). The challenge is one that has not so far been met.

The first major work showing variation in narrative following Labov is Barbara Johnstone's (1990) study, based on 68 stories collected by Johnstone's students in Fort Wayne, Indiana 1981–85. One of Johnstone's (1990) main findings is that there are gender differences and these are worth quoting at length:

> While women's stories are about social reality, men's stories are about individual reality. [66] . . . women's stories tend to be "other oriented," underplaying the protagonists' personal roles and emphasizing social community and mutual dependence. [66] Fort Wayne men tell stories which make statements about their own character and abilities. Men's stories are about events in which their skill, courage, honor, or sense of humor was called upon and successfully displayed: hunting and fishing, fights, successfully solved problems on the road or in the military, clever pranks and clever reactions in awkward moments. [66–7] Men's stories are about skill rather than luck. [67] Though women do on occasion tell stories about their personal exploits – getting the better of authority or pulling off a prank – their skill is always abetted by luck, and more often their stories are about experiences that were embarrassing or frightening or taught them a lesson. [67] When a woman is not the protagonist of her story, the protagonist is either male or female; while men do not tell stories about women's skill, women do tell stories about men's. [67] When men act alone in their stories, they are almost always successful; when women act alone, the outcome is usually negative. [67] Women use more personal names in their stories than do men, even when their audiences are unfamiliar with the names. Men provide more details about objects. [68] The result of these differential discourse choices is that women's stories typically create a storyworld populated with specific, named people engaged in interaction, while the storyworld created in men's stories is more often silent, and the characters are more often nameless.   (Johnstone 1990: 66–8)

Holmes (1997a) drew similar conclusions based on a sample of 30 same-sex conversations recorded as part of the Wellington Corpus of Spoken New Zealand

English. Holmes found that the stories reflected the different daily preoccupations of men and women (1997a: 286): "The women focus on relationships and people, affirm the importance of their family roles, family connections and friendships. The men focus on work and sport, events, activities and things, and affirm the importance of being in control, even when they don't achieve it."

Neither Johnstone nor Holmes gives numbers to support their claims. Kipers (1987) recorded 470 conversations in the faculty room of a middle school in New Jersey. Although Kipers does not give information on the amount of speech recorded, she does provide figures on the distribution of topics. She found that in female-only conversations the most frequent topics were on house and family (28 percent), social issues (21 percent), work (14 percent), and personal and family finance (12 percent). For male-only conversations the most frequent topics were work (39 percent), recreation (28 percent), and miscellaneous (11 percent). (The latter category consisted of "telling jokes," "the weather," "book read by the conversants," and "quitting smoking.") In mixed-sex conversations the most frequent conversations were work (25 percent), home and family (22 percent), and social issues (15 percent). This suggests that the men adapted more to the presence of women than the other way around. The greatest differences in individual topics are that their own children take up 9 percent of the women's conversations, 5 percent of the mixed-sex conversations, but less than 1 percent of the men's. In contrast, spectator sports occupy 13 percent of the men's conversations, 4 percent of the mixed-sex conversations, but none of the women's conversations.

Coates (1996) gives a detailed account of women's talk, showing the features used and drawing inferences as to the significance of this kind of speech. Although Coates also collected equal numbers of men's conversations she does not present any information on it in this work for comparative purposes. Nor does she give quantitative information that would allow comparison with other studies. In a short article (Coates 1997: 126), she discusses one comparative aspect of men's and women's speech: "the research I have carried out focusing on single-sex friendship groups shows that all-female groups of friends typically choose to organize talk using a collaborative floor, while all-male groups typically choose a one-at-a-time floor." With roughly equal samples of speech, to devote over 300 pages to women's talk and just over 20 pages to men's talk does seem to be redressing the male bias in research with a vengeance.

While Coates (1996) is an illuminating examination of language use, the concentration on the speech of one gender may have the effect of creating or reinforcing stereotypes. To claim that a certain feature is characteristic of one gender may seem to imply that it is not characteristic of the other, but unless both genders have been investigated under the same conditions, there is no evidence that it is not characteristic of both. This is an endemic problem with gender studies and may lead to misinterpreting or misrepresenting the data, as happened with claims about girls' precocity in language development (Macaulay 1978).

Even in a comparative study such as Holmes (1997a) there can be a problem, such as that of distinguishing between minimal responses as providing positive feedback or as neutral, non-committal responses. As Holmes (1997a: 290) admits, "any interpretation will be subjective," though there may be prosodic or paralinguistic evidence to support the interpretation. The more the investigator approaches the data with preconceptions about gender differences the greater the risk of biasing the subjective interpretation in one direction or the other.

Even Holmes' valuable *Women, Men and Politeness* (1995) in which she presents evidence to show that "in general, women are more polite than men, and that in particular they are more positively polite or linguistically supportive in interaction" (1995: 29) is not immune from this criticism. Holmes is careful to make it clear that her evidence comes from middle-class speakers in New Zealand and that the results may not generalize elsewhere, but that is not where the problem lies. For example, Holmes refers to the interviews collected for the Wellington Social Dialect Survey and she explains (1995: 34): "As far as we were concerned, the more talk the better. Being polite in this context meant being prepared to answer questions fully and at length. It soon became apparent that the least cooperative and polite participants were the young Pakeha males (i.e. those of European origin)." This statement attributes a motive to the young Pakeha males that may not be justified. There are many possible explanations for a respondent's reticence, including his/her perception of the task, the topics raised, the attitude of the interviewer, etc. Even the same interviewer can be either more or less successful in getting a respondent to speak at length (Dubois and Horvath 1993, Laforest 1993, Macaulay 1984, 1991, 1996). To treat all interviews as equivalent speech events is to ignore the complexity of the situation (Macaulay 2001).

Holmes (1995: 36–7) cites two other studies using artificial tasks in which "the women interviewees were more cooperative and polite, contributing substantially more talk overall than the men" (1995: 37). On the other hand, Holmes observes that "when talk offers the possibility of enhancing the speaker's status, men tend to talk most" (1995: 37). The point is not whether Holmes is correct in her interpretation but rather to note that there is a leap between the data and the interpretation. This caveat is important when evaluating studies of this type and even more so in quantitative studies.

Ochs and Taylor (1995) examined 100 past-time narratives told by seven two-parent families. They define a "story" as "a problem-centered past-time narrative" (1995: 100). They found what they construe as "a commonplace scenario of narrative activity at family dinners":

> First, mothers introduce narratives (about themselves and their children) that set up fathers as primary recipients and implicitly sanction them, as evaluators of others' actions, conditions, thoughts, and feelings. Second, fathers turn such opportunities into forums for problematizing, with mothers themselves as their chief targets, very often on grounds of incompetence. And third, mothers

respond in defense of themselves and their children via the counterproblematizing of fathers' evaluative, judgmental comments.   (Ochs and Taylor 1995: 116)

They conclude this gloomy report with the comment:

"Father knows best" – a gender ideology with a deeply rooted politics of asymmetry that has been contested in recent years – is still in reverberating evidence at the two-parent family dinner table, jointly constituted and re-created through everyday narrative practices.   (Ochs and Taylor 1995: 117)

If Ochs and Taylor's findings are generalizable, it would appear that Victorian "family values" continue to thrive in the present-day USA.

Blum-Kulka (1997) also studied dinner table conversation, comparing Jewish American families, American Israeli families, and Israeli families. Although her primary interest was in socialization and sociability, she points out a cross-cultural difference in choice of topic (1997: 55): "The American preoccupation with health (i.e., sports, physical fitness, health foods) is absent from the Israeli conversations. Israelis, on the other hand, tend to topicalize food and language more than Jewish Americans do." Blum-Kulka also found gender differences: "only in the Israeli families did we find men actually engaged in talk about shopping and preparing food" (1997: 88). There was a difference interpreted as dominance: "We have seen that in the Jewish American families the fathers raise more topics and talk more than the mothers and that this gender balance is reversed in the two groups of Israeli families" (1997: 90).

Findings such as these reinforce the warning given by Cameron et al. (1989): "It needs to be borne in mind generally that 'women' do not form a homogeneous group" (1989: 91). Freed and Greenwood (1996), in a small-scale study of same-sex dyadic conversations, report (1996: 21): "Our findings on the distribution of *you know* and the use of questions in same-sex friendly dyadic conversation show that it is the specific requirements associated with the talk situation that are responsible for eliciting or suppressing specific discourse forms, not the sex or gender of the speakers, or some abstract notion about the relationship of the speakers, or their group membership." This observation, though limited to only two features in the speech of a small number of speakers, should be kept in mind when strong claims are made about group differences in discourse. James and Clarke (1993) and James and Drakich (1993), in their critical reviews of research on interruptions and on amount of talk, found no significant differences between the sexes despite the many claims to the contrary.

Eggins and Slade (1997) examined three hours of casual conversation collected during coffee breaks in three different workplaces. They found that "the most frequently occurring stretch of talk in the all-male group was teasing or sending up (friendly ridicule)" (1997: 267). They did not gossip and tended to talk about work or sports rather than personal details. In the all-female group there was a predominance of gossip ("broadly defined as talk which involves pejorative judgment of an absent other" 1997: 278) and storytelling. There was

no teasing. They discussed "quite personal details including boyfriends, weddings, marriages, children, and relatives" (1997: 268). In the mixed group of men and women "amusing or surprising stories dominated the conversation" (1997: 268), there was some joke-telling and some teasing. This is a small sample on which to base strong claims, but the results are interesting, so it is to be hoped that Eggins and Slade will explore this area further.

Schilling-Estes (forthcoming) points out the problems that can arise because one speaker's voice may be "fraught with echoes of the voices of others," not only in quoted dialogue (Macaulay 1987a) but in other subtle ways: "If people are continually uttering the words of others, then how are variationists to know which utterances they should count as a speaker's own and which they should not, especially given that bits of prior text are uttered in voices that may have been obviously altered to reflect source voices and sometimes not?"

Coates (1999) also looked at changes with age in the discourse of teenage girls, based on transcriptions of conversations among four white middle-class girls in London, recorded by themselves from the age of 12–15. This is a remarkable data set, judging from the examples Coates cites in her paper, as the girls range over a wide spectrum of topics, which do not vary greatly as they grow older, with one exception. In later conversations the girls provide "information of a highly personal nature" (1999: 126), which makes them more vulnerable, and "the ludic aspect of their talk decreases" (1999: 137). Coates reports that she was amazed at how much the 12-year-olds' talk differed from that of her female friends, and also how much the girls' talk changed over the years. As Coates remarks, "there is a dearth of research in this area" (1999: 142), which seems surprising, given the interest in all aspects of gender differences. This is an excellent example of small-scale research carried out with imagination, energy, and good will.

Another investigator to make use of resources close at hand is Morgan (1989, 1991, 1999), who recorded members of her own family. Morgan points out that most accounts of African-American English have been based on the speech of adolescent males who participate in the street culture. Morgan provides intergenerational information on how "as African-American girls grow into women, their everyday conversations often involve the expression and defense of social face" (1999: 37). Morgan (1991) also shows how indirectness is used and evaluated differently by African-American women than would be the case in mainstream society. Morgan's work, like Coates's, provides an insider's perspective on language use that would be hard to match by survey methods. Given the problems of obtaining valid samples of speech, this is an approach that would be even more valuable if the researchers would make more of the raw data available for comparative purposes.

Of course, all such samples are biased. As Mitchell-Kernan (1972) comments on her choice of informants from the mothers of children in pre-school playgroups in Oakland, "a sample which selects on the basis of the presence or absence of pre-school children is, of course, age biased" (1972: 13–14). Yet Mitchell-Kernan's description of "signifying," "loud talking," and "marking"

remains one of the best accounts of speech behavior among African-American women. In discourse studies, the quality of the interaction recorded or reported is more important than any objectivity gained by random sampling methods.

Efforts using questionnaires to gain information about the use of features that are hard to record have not been noticeably successful. De Klerk (1992, 1997) used questionnaires to gain information on the use of taboo words and expletives by teenagers, but the results of self-report studies of this kind tend to be unsatisfying because one would like to know from actual examples who says what when. Hughes (1992) observed her speakers as well as administering a questionnaire and found that "their use of taboo or swearwords is an integral part of their language" (1992: 297) as anyone who had encountered them in their daily activities would know. But Hughes also found through her questionnaire that the women were more cautious about potentially blasphemous words such as *Jesus*, *Christ*, and *God*. This is the kind of information that can only be obtained through direct questioning.

Bates and Benigni (1975) used a questionnaire to investigate pronoun use in Italy, following up the original study by Brown and Gilman (1960). Their most interesting finding was that the lower-class speakers reported that they were more likely to use the formal terms (*Lei* and *voi*) than the upper-class speakers. Bates and Benigni offer an interesting explanation of this finding in social and political terms. This is a good example of the use of questionnaire methods not least because Bates and Benigni administered the questionnaires in a way that allowed respondents to explain and comment on their choices.

5   Quantitative Studies of Discourse Features

One of the first quantitative studies of a single discourse feature was Dubois and Crouch's investigation of the use of tag questions during the discussion sessions after papers at a small academic workshop. Contrary to Lakoff's claim (1973: 53–5) that women are more likely to use question tags than men, they found that all 33 tags in the sessions were produced by men (Dubois and Crouch 1975: 293). Interestingly, these results are sometimes reported (Cameron et al. 1989: 77, Holmes 1995: 84) as that men used *more* tags than women, not that *only* the men used tags.

Holmes (1984, 1995), using a balanced sample of men's and women's speech in New Zealand, found that women used more tags (51) than men (36). Cameron et al. (1989), examining examples from 25 speakers recorded as part of the Survey of English Usage (Svartvik and Quirk 1980), found that the men used almost twice as many tag questions (60) as the women (36). They give as a possible explanation that two of the men had known that they were being recorded, so perhaps "their speech reflected a concern to elicit as much talk as possible from other participants" (Cameron et al. 1989: 85) and increased their use of tag questions. This illustrates the problem of using surreptitious

recording, when one of the participants knows about it. It also is part of a general problem of using "confederates" in interactional research (Duncan and Fiske 1985). More importantly, it illustrates a search for an explanation when the results are not what the investigators expected. This is legitimate but it draws attention to a fundamental problem in quantitative research of discourse features. To what extent can we trust the figures? In many cases, there will be no additional information available to the investigators that might help to explain apparent anomalies, but that does not mean that there may not be factors that skew the results. The most reliable way to check on unknown factors would be to carry out similar studies on equivalent populations, but few scholars are interested in replicating what they themselves or others have done.

Holmes (1984) introduced a refinement by classifying tag questions as either (1) epistemic modal (focusing on information); (2) challenging; (3) facilitative (encouraging the listener to speak); and (4) softening. She showed that women were more than twice as likely to use facilitative tags as men. Cameron et al. (1989) found that the proportion of facilitative tags was greater for women than for men but the total number of facilitative tags was greater for men (perhaps for the reason given above). Cameron et al. also report that the task of classifying the tag questions "was not unproblematic" (1989: 83), partly because most utterances are multifunctional. They also did not find intonation an infallible guide to the different categories of tag questions. They found that Holmes's framework "compelled us to make a somewhat artificial choice" (Cameron et al. 1989: 84) between categories. This point underlines one of the problems with replicating another investigator's method. While the method may appear straightforward when originally reported, attempts to repeat the procedures often raise questions about how to deal with borderline cases.

Erman (1993) reports on an examination of the speech of 22 speakers in the London-Lund corpus (Svartvik and Quirk 1980) for the use of what she calls "pragmatic expressions." The pragmatic expressions she tabulates are the phrases *you know*, *you see*, and *I mean*. She found that almost twice as many of these expressions occurred in same-sex sessions compared with mixed-sex sessions (Erman 1992: 228). She also found that the men used considerably more of these expressions than the women. She also found a functional difference in that "the women tended to use pragmatic expressions between complete propositions to connect consecutive arguments, whereas the men preferred to use them either as attention-drawing devices or to signal repair work" (1992: 217). Erman's conclusion is "that pragmatic expressions, although sometimes nearly depleted of semantic meaning, serve a number of communicative functions; they facilitate the speaker's encoding of the message as well as the addressee's decoding of it and serve interpersonal as well as textual ends" (1992: 233). Erman's work shows that it is not necessary to attribute specific meanings to discourse features in order to provide an enlightening analysis of their use.

Studies of discourse variation in terms of social class are comparatively rare, perhaps because in language studies in the USA social class is almost a taboo subject (despite Labov 1966), though not in Europe. The earliest quantitative

study of social class differences in discourse is probably Bernstein (1962), based on small samples (about 2,000 words) taken from group discussions on the topic of the abolition of capital punishment. The subjects were males aged 15–18 in groups identified (on the basis of education) as either working-class or middle-class. Bernstein found that the working-class speakers "used a considerably longer phrase length (3.8 more words to the phrase) and spent much less time pausing (0.06 seconds) than the middle-class group" (Bernstein 1971: 87). He also found that the middle-class speakers used a high proportion of subordination, complex verbal stems, passive voice, uncommon adjectives, adverbs, and conjunctions, and the personal pronoun *I*. The working-class speakers used more personal pronouns (especially *you* and *they*) and what Bernstein called "sympathetic circularity sequences," tag questions and discourse markers, such as *you know* and *I mean* (1971: 115–16). Bernstein's pioneering efforts in discourse analysis have perhaps received less recognition because of the controversy that arose over the implications of his interpretation of his findings into restricted and elaborated codes (Rosen 1972, Trudgill 1975).

Dines (1980) examined the use of terminal tags (*and things like that*, *and that*, *or something*, etc.) in interviews with 18 middle-class and working-class women in Australia. She found that the working-class women used more than three times as many of these tags (58) as the middle-class women (18). Dines does not give any indication of the length of the interviews and we have to assume that they were roughly equal. It would have been helpful, however, if she had given relative frequencies (see below). Dines had earlier discovered that such tags were stigmatized. In looking at the use of the tags in relation to Bernstein's sociolinguistic codes, Dines found "that there is nothing to suggest that the occurrence of set-marking tags marks 'vague and inexplicit speech'" (1980: 30).

Dubois (1993) reports on a careful examination of similar features that she calls "extension particles" in the 1971 Sankoff and Cedergren corpus (Sankoff and Sankoff 1973) and the Montréal corpus of 1984 (Thibault and Vincent 1990).[1] Dubois found that in the overall use of these extension particles "there was no class difference discernible and no difference between the 1971 and 1981 interviews" (Dubois 1993: 185). Younger speakers used the most particles, and only in the 1971 data did women use more particles than men. There were, however, some significant differences in the kinds of extension particles used, showing the effects of time, gender, and class.

Vincent (1993), also using the 1971 and 1984 Montréal corpora, found gender, schooling, and occupational differences in the use of "exemplification particles" (e.g. *par exemple*, *comme*, *genre*, *disons*, etc.). Like Dubois, she found an age difference: "young speakers clearly produce more exemplifying utterances than do older speakers" (Vincent 1993: 160) but the decrease was not gradual over time. Instead, for both corpora, there is a break between those under 48 and older speakers. The similarity of these findings with Dubois' suggests that it might be worth looking to see whether there are qualitative differences be-

tween the interviews with younger and older respondents that might explain the differences in the frequencies with which these features are used. That this is likely follows from the conclusions of Vincent and Sankoff (1993) summarized below.

Vincent and Sankoff (1993) analyzed 13 interviews from the Sankoff-Cedergren corpus for the frequency of what they call "punctors." Punctors are assimilated prosodically to the preceding phrase, almost never preceded by a pause, show a high degree of phonological reduction, and have lost their original meaning or function (1993: 205–6). The punctors include *la* "there," *tu sais*, *vous savez* "you know," *n'est-ce pas* "isn't it so," and others. The number of punctors used by the 12 speakers ranges from 54 to 551. Vincent and Sankoff also give the frequency of punctors per line of transcription, showing that "the rate of punctor use increases with the length of the interview, that is, with loquacity or fluency of speech" (1993: 212). They also show that "punctors are not frequent in simple answers or when utterances are short, objective, and without much speaker involvement" (Vincent and Sankoff 1993: 212). They claim that the use of punctors is linked to fluency and expressivity. They conclude that the distribution of punctors "is conditioned by factors such as prosodic rupture [i.e. a break in the melody of the sentence], context, and genre of discourse; only the choice of individual punctors seems to be conditioned by social class" (1993: 214). This short article is a model for future analysis of discourse variation, not least because it does not depend upon interpretative judgments of variants.

In a small study of social class differences in discourse, Horvath (1987) found that the working-class speakers told almost all of their stories about themselves or members of their families (94.6 percent), whereas in the middle-class, just over half (53.8 percent) told stories about characters that were distant from the story teller, either public figures or strangers (Horvath 1987: 219). It would be interesting to know if this is a general social class difference.

In my own work, I have been interested in social class differences in Scotland (Macaulay 1977), and latterly in social class differences in discourse (Macaulay 1985, 1987b, 1989, 1991, 1995). In Macaulay (1985) I described the narrative skills of a Scottish coal miner, showing his effectiveness as a storyteller, but also providing quantitative information on the use of discourse markers and terminal tags. In Macaulay (1991) this kind of analysis was extended to a sample of 12 speakers with equal numbers of middle-class and lower-class speakers in the town of Ayr in southwest Scotland. I was able to show that the lower-class speakers used more discourse markers and highlighting devices, while the middle-class speakers used more derivative adverbs in *-ly*. This latter point was developed in Macaulay (1995), showing that the middle-class speakers also used more evaluative adjectives.

These works were based on interviews that I carried out myself. A major innovative feature in the study was that I transcribed the interviews in their entirety and thus could present comparisons in terms of the relative frequency of the use of a particular item. This is extremely important when comparing

usage because speech samples are rarely identical in length. I have employed the same procedure in analyzing a set of same-sex conversations recorded in connection with a study of language change in Glasgow (Stuart-Smith 1999). Conversations between friends of approximately half an hour were recorded without the investigator being present. There were two age-groups (14-year-olds and adults over 40) and two social class groups (middle-class and working-class). The sessions were transcribed in their entirety and analyzed for age, social class, and gender differences. The results show that the adults talk more than the adolescents, and females talk more than males. The females tell more narratives than the males and use more quoted dialogue. The working-class women use the most quoted dialogue. One of the more notable global differences in topic is that the females talk more about people than the males. In particular, the girls talk about other girls and the women talk about other women. This was the case both quantitatively and qualitatively. As was the case in the Ayr interviews (Macaulay 1991, 1995), the middle-class speakers, both adults and adolescents, use more derivative adverbs in -*ly*. The women, both middle-class and working-class, use *you know* (8.34 per thousand words) almost twice as frequently as the men (4.48); the adolescents (0.86) use *you know* much less frequently than the adults (6.84). There are other features, such as the use of *well*, that show the adolescents do not make the same use of discourse features as the adults.

6   Prospects for Studying Variation in Discourse

As I said at the beginning, the study of discourse variation is still at an elementary stage. It will be obvious from the preceding review that there are many different approaches to the sociolinguistic investigation of discourse, and it would take a braver person than I am to assert with confidence that we have much solid information on gender, age, or social class differences. What we have are a number of intriguing claims that need to be tested again and again, by the same or different methods, in similar or different settings, with similar or different samples (Campbell and Fiske 1959).

Only when there is a convergence of results from numerous studies will it be possible to make confident claims about discourse differences. There are many known (and unknown) variables that may affect samples of speech. Yet we need not despair. One way forward lies in replication. As more studies are carried out, the influence of accidental factors may be easier to detect. Also, methodologies improve as we learn from the successes and failures of our own and others' work. New methods of analysis, such as those illustrated above, lead to more confidence in the results.

We have, however, reached a point where we can see what is needed in order to make progress in future work on discourse variation. Among them surely are these four aspects:

1 We need more data. The extensive use of the London-Lund corpus (Svartvik and Quirk 1980) shows what a valuable resource it has been. Yet it presents a very limited sample of speakers. The Corpus of Spoken American English (Chafe et al. 1991) will no doubt prove equally useful. But we need data from different groups in a variety of settings. Probably more use could be made of evidence from media archives (Dougherty and Strassel 1998, Elliott 2000a, 2000b, Franken 1983 [cited in Holmes 1995: 33–7], Holmes 1997b, Macaulay 1987b).

2 It would help if investigators, regardless of their own particular interest, would report as fully as possible on the frequency in their data of features that other scholars have studied, so that a store of comparative data could be amassed.

3 It is important that the relative frequency of features should be reported in similar terms. My own preference is the frequency per thousand words. Some investigators report frequencies in terms of lines in the transcript but this is less informative because the length of the lines may vary. Since computers provide a word count, it is easy to calculate the frequency per thousand words. It is much less informative to report the proportion of a variant used with no indication of the total sample of speech from which the figures have been derived. If percentages are given, the raw figures should also be provided.

4 As Sinclair (1992) points out, the impact of computers on the study of language is likely to be immense in the next few years. It may not be necessary to follow his guideline – "Analysis should be restricted to what the machine can do without human checking, or intervention" (1992: 381) – but it would be helpful to identify a set of discourse features that can be easily collected by mechanical means.

NOTE

1 The Sankoff–Cedergren corpus consists of 60 interviews with French speakers in Montréal in 1971; in 1984 these speakers were interviewed again, with the addition of 12 younger speakers.

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