Sociolinguistics

Method and Interpretation

Lesley Milroy and Matthew Gordon

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3

Data Collection

3.1 General Issues

Chapter 2 was concerned with locating research subjects in a manner sensitive to their social characteristics. Once the speaker sample has been identified, the question arises of how to obtain useful data. There are a variety of approaches available to sociolinguists. In choosing from among these – as with all decisions in the study design – the investigator is guided by the aims of the research. What constitutes "good data" depends on the research objectives, as do the methods for collecting such data. Decisions about data collection are crucial because patterns of language use are sensitive to various contextual factors. As a result, researchers must recognize that the manner in which they approach a speaker will affect the data available for analysis.

Traditionally, the data of primary interest to sociolinguists have been those representing the spontaneous, everyday usage of vernacular speakers. However, the status of researchers as community outsiders inevitably challenges their ability to gain access to such data. The investigator is faced with the "observer's paradox": we want to observe how people speak when they are not being observed. The problem is made more acute when tape-recordings of speech are needed for analysis, since many speakers will tend to shift away from their casual usage in situations where they are being recorded by a stranger. Sociolinguists have developed a variety of techniques for overcoming the observer's paradox, or at least for reducing its effects since the problem can never be entirely resolved. Several of these techniques are described below (section 3.3.3).

Preoccupation with the observer's paradox stems from certain beliefs about the speech variety known as "the vernacular." Just what constitutes the vernacular is not always clear (see further Hudson 1996; Milroy and Milroy 1999). Labov has described the vernacular variously as the variety

acquired in pre-adolescent years (1984: 29), and as the variety adopted by speakers when they are monitoring their speech style least closely (1972b: 208). More recently, Eckert refers to the vernacular as "the language of locally based communities" (2000: 17), a definition that focuses on this variety's status in direct opposition to the supralocal standard variety. Labov, Eckert, and others have suggested that the vernacular offers the best database for examining the processes and mechanisms of linguistic change or the structural characteristics of a particular variety. According to this thinking, vernacular speech is seen as more regular because it is removed from the potential influence of high-status varieties. Speakers attempting to adopt a "correct" style of speech often make sporadic and sometimes hypercorrect movements in the direction of the standard.

The difficulty in pursuing the vernacular, however, lies with the impossibility of recognizing the quarry when it is caught. It is a fundamentally abstract object, rather like its counterpart, the standard language. Indeed, sociolinguists' devotion to the vernacular at times bears a striking resemblance to popular conceptions of the standard as the one, true language (see Cameron 1995). Nevertheless, it may be theoretically useful to examine variation in terms of the relative influences of such idealized varieties. As long as we acknowledge the abstractions involved, we will not fall into the trap of attempting to record the vernacular of a given speaker, defining this as his or her most natural and unconstrained linguistic code, for it is clear that *any* speech varies considerably in response to situational context. Hence, the concept of an entirely natural speech event (or an entirely unnatural one) is untenable, as several sociolinguists have pointed out.

Labov's characterization of the vernacular relates to his formulation of stylistic variation in terms of attention paid to speech. However, Labov's (1972a) own work on African American English illustrates some of the difficulties with such an approach since many of the vernacular verbal arts he describes, such as "toasting" or "sounding," inevitably involve rather self-conscious performance and can hardly be described as unmonitored speech styles. In fact, stylistic variation is an area that has garnered a lot of attention in recent years; the whole issue has been greatly problematized. For example, certain studies have argued that shifting between styles can be used strategically by speakers to serve their communicative needs. Such an argument runs counter to an earlier variationist treatment of style as purely reactive to contextual factors such as addressee or topic. These matters are explored further in chapter 8, but here we simply note that stylistic variation seems to be influenced by a range of factors to which investigators must attend when using style as a methodological construct.

As sociolinguists' appreciation of the complexities involved in stylistic variation has grown, so too has their interest in examining data from a range of styles. Data representing spontaneous, everyday conversation can be

useful for examining a number of sociolinguistic questions, but we should not assume that they will work best in every study. The procedures for collecting and analyzing such data are extremely labor and time intensive. Furthermore, free conversation may not provide all the relevant information; for example, conversational data typically pertain only to speech production and not to perception – a deficiency that is especially significant in the study of phonemic mergers (see chapter 6 and Gordon 2001a). Recognizing such problems, researchers frequently turn to other kinds of data that may be used as a complement or even an alternative to conversational data.

Our discussion in this chapter describes several methods for gathering sociolinguistic data of various types. The focus here is on data produced for a particular research project – that is, data initiated in some way by the investigator, as opposed to data produced for some other purpose. Included in this latter category are data from publicly available sources, such as written texts or media broadcasts. Data from public speech can be fruitfully applied to sociolinguistic research; for example, Hay, Jannedy, and Mendoza-Denton (1999) examined phonetic variation in the speech of Oprah Winfrey using broadcasts of her talk show. A less public style was investigated by Kiesling (1997) who used data recorded during group meetings in his study of a college fraternity (see also examples discussed by Johnstone 2000b: 111–12). While investigators may want to take advantage of opportunities to use such data, we will restrict our discussion here to data gathering in contexts that involve more control on the part of the researcher.

3.2 Survey Approaches to Data Collection

3.2.1 Written questionnaires

While an overwhelming majority of variationist studies examine spoken language exclusively, some recent projects have demonstrated the usefulness of written surveys in sociolinguistic research. Collecting data through written questionnaires is an established method in other social scientific fields and has a long history in dialect geography beginning with Wenker's studies of northern German dialects in the nineteenth century (see section 1.2.2; Chambers and Trudgill 1998: 15; Chambers 1998b). The manner in which sociolinguists use questionnaires differs from that of dialect geographers, not so much in the instruments used but in how they are applied. Both types of researcher may ask similar questions, but they typically ask them of rather different types of people. In keeping with their general orientation, sociolinguists strive to survey a sample that is more representative of the

social diversity in a given population than the NORMs (non-mobile, older, rural, males) surveyed by dialectologists (Chambers 1994).

A good illustration of this sociolinguistic focus is found in the Golden Horseshoe project directed by J. K. Chambers. The Golden Horseshoe refers to the region of Canada along Lake Ontario that includes Toronto. This project gathers data using a postal questionnaire that is distributed through various means to respondents who fill it out and mail it back to the researchers. The questionnaire contains 76 questions that investigate the respondent's usage with regard to morpho-syntactic, lexical, and phonological variables. In addition, there are 11 questions covering demographic information including age, sex, occupation, and education (Chambers 1994). With the respondents representing a broad social range, the investigators are able to examine patterns of covariation between linguistic and social variables. In fact, much of the work (e.g., Chambers 1998b) has explored patterns of language change – patterns that would not be apparent without sampling across generations of speakers. The Golden Horseshoe work has established a model that is being emulated throughout the country so that eventually the survey will cover all of Canada. See Chambers's website: [http: //www.chass.utoronto.ca/~chambers/dialect_topography.html].

As Chambers (1994) notes, one major advantage of using written surveys is their efficiency. They allow researchers to gather data from a large number of speakers in a relatively brief amount of time. The Golden Horseshoe project surveyed over 1,000 respondents within a period of about two years (Chambers 1998b). Similarly, by enlisting their students in collecting responses, researchers for the McGill–New Hampshire–Vermont Dialect Survey secured over 1,300 respondents in just two years (Nagy 2001). However, the use of written questionnaires has often raised questions of reliability among dialect geographers and sociolinguists. Chambers (1998a) has responded to these concerns by demonstrating statistically that the data gathered in this way are no less reliable than those gathered through questionnaires administered by field workers. He suggests, in fact, that questionnaire data are *more* reliable when they are gathered through a postal survey than directly by a fieldworker due to the potential for fieldworker bias in recording responses as well as the potential bias introduced by the mere presence of a fieldworker who is unfamiliar to the respondents (the Observer's Paradox). Similar support for questionnaire techniques is offered by Tillery and Bailey (1998) and Bailey, Wikle, and Tillery (1997).

Despite their strengths, written questionnaires (like all data collection methods) have their limitations. They are an efficient instrument for surveying a large number of people, but they do not allow for in-depth examination of language use for any particular speaker or community. One obvious deficiency of questionnaires is that they generally call for categorical responses. They do not, therefore, attempt to examine intraspeaker variation. Thus, as

in the Golden Horseshoe questionnaire, respondents might be asked whether they would use *sneaked* or *snuck* in the frame "The little devil ____ into the theatre" (Chambers 1998b). If one's usage varies, for example sometimes saying *sneaked* and sometimes *snuck*, this information may be difficult to capture. Phrasing questions without suggested responses – for example, "What do you call the upholstered piece of furniture that two or three people sit on in the living room?" (Chambers 1998b: 8) – allows respondents to offer multiple variants. Such responses can be valuable, but unless the respondent offers some explanation, the investigators know nothing about the relative frequency of each variant for that respondent or about any possible situational or semantic distinctions among the variants.

The categorical nature of the questions when this method is used also restricts the types of variable that can be investigated. Respondents must be able to determine their own usage and, when given choices, compare that usage with alternatives presented in the question. In the case of lexical and morpho-syntactic variables, this task is relatively straightforward, but it becomes more difficult when dealing with certain phonological variables, namely those involving subphonemic distinctions. We can illustrate this difficulty by considering two different patterns of variation involving the low back vowels in American English. In many areas of North America, the traditionally rounded back vowel /ɔ/ appears as an unrounded and lowered [ɑ]. In some cases, this development results in the merger of the two phonemes in a change known as the "*cot/caught* merger" (see, e.g., Labov 1994). In other cases, the movement of /ɔ/ is part of a pattern known as the Northern Cities Shift that involves several other changes including the fronting of /ɑ/ (see further chapter 6). An investigator might test for the appearance of the *cot/caught* merger by asking whether pairs of words like *cot* and *caught* or *Don* and *dawn* sound the same or different. While this approach is somewhat crude,[1] it might produce some useful results. On the other hand, it is hard to formulate a question to test for the appearance of the Northern Cities Shift because speakers typically are not consciously aware of vowel-systemic variations like those involved with this change. Asking, for example, whether the vowel in *caught* sounds like the vowel in *cot* is pointless since both innovative and conservative speakers (i.e., those who do and those who don't participate in the shift) are likely to answer in the negative even though phonetically these two groups of speakers sound very different.

When carried out appropriately, written surveys can provide good amounts of useful data in a fairly brief time-frame. As we have suggested in this section, they are better for addressing some matters than others. Fittingly, Chambers refers to his approach as "dialect topography" to indicate that it produces "a description of surface features" (1994: 36). In this sense, such work is seen as a first step that can "be useful in delimiting places where micro-level sociolinguistic studies might profitably be pursued" (1994: 36).

3.2.2 *Fieldworker-administered surveys*

As an alternative to having respondents fill out surveys themselves, prepared questionnaires may also be administered by fieldworkers. This approach was the preferred method for traditional dialect geographers from Gilliéron to Kurath to Orton (Chambers and Trudgill 1998: 15–25), and there were practical reasons for this preference given the generally low levels of literacy among the NORMs who served as their consultants (Chambers 1994: 37). Nevertheless, even in modern societies with mass literacy there may be situations in which the fieldworker-administered surveys have an advantage over other methods.

One clear benefit to having fieldworkers collect data is that it allows for direct observation of language use. This might be particularly useful in the study of pronunciation variants. Returning to the examples discussed in the previous section, both the *cot/caught* merger and the Northern Cities Shift could be investigated fairly simply in this context. A fieldworker might construct questions designed to elicit the pronunciation of diagnostic words (e.g., "I catch the ball today; I ____ the ball yesterday."). More straightforwardly, the respondent might be asked to read a list of words illustrating the variables of interest. Responses could be tape-recorded and analyzed later, or transcribed on the spot.

Still, direct observation of language use is not always what is being done by fieldworkers. As is the case with written questionnaires, fieldworker-administered surveys often gather respondents' self-reports about their usage. They are fundamentally metalinguistic tasks in that they rely on the respondents' ability to consider their own linguistic behavior. Unlike interviews involving free conversation (see section 3.3), it is difficult to disguise – assuming that one wanted to do so – the purpose of a survey in which all the questions ask what the respondents call X or how they would say Y. In general, variationists have been skeptical about the accuracy of self-reported data. There are well-known cases of people who, when asked directly, claim not to use particular forms that they in fact use at less-guarded moments during an interview. Labov's experience of the New York mother and daughter who believed they always pronounced postvocalic /r/ illustrates such discrepancies (1966: 470–2). Incidentally, this case also serves as a cautionary tale for investigators who might seek to disabuse such speakers of their inaccurate perceptions: when Labov played back the tape of their speech, the subjects were "disheartened in a way that was painful to see" (1966: 471).

Despite such discrepancies, we should not assume that all self-reported data are less accurate than those collected through observation of actual usage. Tillery compares self- reports with data gathered through indirect elicitation and through observation in several studies of Texas and Oklahoma and concludes that self-reports are "at least as valid as" the other methods (2000: 65). Direct questioning may, in fact, provide a more accurate usage picture for some variables than would less metalinguistic approaches. Included here would be items that appear infrequently in free conversation and are difficult to elicit through indirect questioning. On these grounds, Bailey, Wikle, and Tillery (1997: 57) argue that their self-reported data on the use of the double modal construction, *might could*, in Texas are a better measure of the extent of this feature that the data from the Linguistic Atlas of the Gulf States, which were collected using indirect elicitation. Indirect approaches may also bring problems of interpretation. Tillery and Bailey (1998) encountered such difficulties in their research on the use of the second person pronoun *yall* with singular reference. When a speaker asks "How are yall doing?" of a single addressee, it is possible that he or she has in mind just the addressee, but it is also possible that *yall* is meant as an "associative plural" covering the addressee's family or colleagues, etc. Tillery and Bailey conclude "The only way to resolve such disagreements, of course, is to ask users what they mean when they use *yall*" (1998: 263). Nevertheless, such direct questioning can be more problematic when it requires subjects to consider less common linguistic phenomena such as is often required in making grammaticality judgments about syntactically unusual sentences (see further section 7.2).

Fieldworker-administered surveys are traditionally very time-consuming. For example, the data collection for the Linguistic Atlas of the United States and Canada began in the 1930s, continued intermittently for decades, and is still incomplete in its coverage. As noted above, this inefficiency is one of the motivations that Chambers (1994) cites for his use of postal questionnaires. Other researchers have used the telephone as an efficient means of gathering data. Contacting subjects on the phone obviously eliminates the time and money costs involved in fieldwork travel. The time savings is demonstrated by Labov's Telsur project which has surveyed all of the US and Canada within a period of about eight years (Labov, Ash, and Boberg forthcoming). This project focuses on pronunciation features, especially on sound changes like the Northern Cities Shift and the *cot/caught* merger. The project utilizes a range of strategies to collect data on the speech production, combining elements of a questionnaire format with those of a traditional sociolinguistic interview. The researchers ask questions to elicit particular forms (e.g., "What's the name for the type of foldable bed often used in the army?") as well as more open-ended questions to capture examples of extended discourse (e.g., "How has your community changed over the years?"). Labov and his colleagues are also interested in speech perception, particularly in whether sounds are judged to be merged. This information is gathered by directly asking respondents whether certain pairs of words (e.g., *cot* and *caught*) sound the same.

Guy Bailey together with Jan Tillery and others have utilized telephone surveys in their work in Texas and Oklahoma (see, e.g., Bailey, Wikle, and Tillery 1997; Tillery and Bailey 1998; Bailey and Dyer 1992). In some cases, this research has been coordinated with general (i.e., non-linguistic) survey projects. In Texas, for example, Bailey and his colleagues received permission to add questions about the usage of certain lexical and grammatical variables to the protocol for the Texas Poll, which is "a quarterly survey of approximately 1,000 randomly selected telephone households" in the state (Bailey and Dyer 1992: 4). They also tape-recorded the interviews to collect phonological data. The fact that such surveys, unlike most strictly linguistic surveys, employ a properly constituted random sample increases the power of the statistical inferences that can be drawn from the results (see section 2.2.1 and Bailey and Dyer 1992). Another advantage of "piggybacking" onto non-linguistic surveys is the reduced potential for interviewer bias. Since the interviewers conducting the survey have no training in linguistics and no knowledge of the issues being studied, the risk of their influencing the results is minimal (Tillery and Bailey 1998: 264). On the other hand, this ignorance of linguistics imposes certain limits on the data to be collected since untrained interviewers could not be expected to record, for example, variable pronunciations reliably.

3.2.3 Rapid and anonymous surveys

A special type of fieldworker-administered survey is done without the awareness of the subjects. In this approach, known as the rapid and anonymous survey, the investigator seeks to elicit a set word or phrase in entirely naturalistic conditions. The prototype for this kind of research is Labov's famous "fourth floor" study in New York City department stores (1972b). In this project, Labov examined the pronunciation of /r/ among employees of three stores seen as catering to different socioeconomic strata. His procedure was to ask various employees in each store for the location of an item already known in advance to be on the fourth floor. He then obtained a repetition by pretending to mishear the response. In this way he elicited four instances of the target feature in two separate phonetic environments (pre-consonantal in *fourth* and word final in *floor*). Pronunciations of the target feature were covertly marked down in a notebook on the spot, using a simple preset schema.

The same principle was used in Philadelphia to investigate the alternation between [str] and [ʃtr] in word initial clusters. In this case, the data were collected in the following way:

> We obtained data on (str) in a wide variety of Philadelphia neighborhoods by asking for directions in the neighborhood of a given street which had a name of a form *X Street*. However, we asked
>
> "Can you tell me how to get to X Avenue?"
>
> In the great majority of cases, the informants would respond "X Street?" with considerable emphasis on street. (Labov 1984: 50)

Another example of this type of survey is described by Rimmer (1982) whose research was carried out in Birmingham, England. Penelope Gardner-Chloros' (1997) study of language choice and code-switching in Strasbourg department stores represents a slight variation on this style of survey. Rather than eliciting responses, she surreptitiously observed interactions between customers and salespeople, recording on notecards the language used.

Plainly, considerable ingenuity is needed to design rapid and anonymous survey questions that will reliably elicit the target feature, and the main advantage of the method is that a clear view of the distribution of a single variant, geographically and sometimes socially, can be obtained quickly. In fact, the [ʃtr] variant appears to represent an innovation in Philadelphia, and Labov was able to track its distribution through various types of residential areas in the city. Nor is the observer's paradox an issue, since speakers are not tape-recorded and are not even aware that they are being observed.

The method is, however, applicable only if the investigator has in mind extremely clear goals that have been well formulated in advance. A major disadvantage is the very limited nature of the data that this method is capable of yielding. Not only is the investigator restricted to a single linguistic feature, but social information on the speakers is likely to be only approximate.

3.3 Sociolinguistic Interviews

3.3.1 The structure and design of the interview

Interviews have traditionally been the most common approach to data collection among sociolinguists. Typically these are one-on-one exchanges conducted in person, though occasionally they are conducted over the phone (e.g., Labov's Telsur project discussed in section 3.2.2 above) and occasionally they involve either multiple fieldworkers or multiple subjects (see section 3.3.3 below). The sociolinguistic interview typically differs from a survey in being relatively less structured. Whereas survey questions are usually asked in a predetermined order and a prescribed form, interview protocols are more flexible. Surveys seek brief responses to fairly direct questions;

interviewers attempt to elicit more extended stretches of unscripted, conversational speech. The basic objective has often been to observe the subject's relaxed, "natural" usage. As we noted earlier, however, the idea of the "vernacular," as this usage is sometimes termed, is problematic. In this section we introduce the interview as a data collection strategy and discuss some examples of its use.

Considering the open-endedness of the interview format (relative to a questionnaire), one question that emerges is: How long should an interview last? Labov has suggested that interviewers should obtain "from one to two hours of speech from each speaker" (1984: 32); but, in fact, it is difficult to be categorical about the appropriate length of an interview. Useful phonological data can often be obtained in a relatively short time – perhaps as short as 20 to 30 minutes, but a very different picture of a speaker's pattern of language use is liable to emerge over a longer period, and it is this pattern that will be of interest to an analyst who wants to get an idea of fluctuations in a speaker's use of key phonological variables. Thus, Douglas-Cowie (1978) suggests that, even when interviewed by a stranger, speakers will settle down to a pattern approximating to their everyday interactional style after about the first hour, and speech produced before this period has elapsed may show radically different patterns. On the other hand, recent research on style-shifting, especially that of Schilling-Estes (1998), suggests that interviewees may move in and out of styles throughout the course of an interview for a variety of reasons (see further section 8.3). Thus, researchers should be careful in assuming that speakers will adopt or maintain a particular style simply based on the fact that some period of time has elapsed in an interview. Furthermore, on the question of interview length, Cheshire (1982) noted in her study of Reading that, for the purposes of *syntactic* analysis much greater quantities of data were required since the relevant structures were not likely to emerge as predictably or as frequently as phonological elements (see further section 7.2). It is for this reason, in fact, that some researchers have adopted more direct approaches to data collection (see section 3.2.2). The question of interview length can therefore, like so many methodological questions, be answered primarily in terms of the goal of the research.

Successful interviewing requires careful planning. While the goal may be to engage the subject in free conversation, the interview situation is very different from the spontaneous discussion that might arise among friends. Most importantly here, the responsibility for keeping the conversation going rests with the interviewer who manages the discussion by asking questions. For this reason, it is essential to have a prepared list of topics that will generate talk in each interview.

Labov's interview techniques for his neighborhood studies in Philadelphia illustrate the preparation involved in successful data collection. These

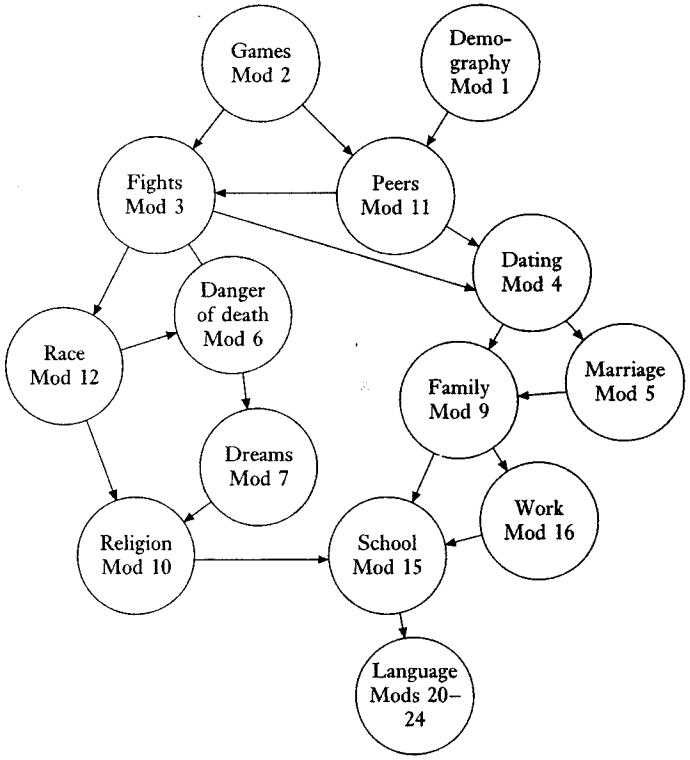

Figure 3.1 Characteristic network of modules for adolescent or young adult speaker (adapted from Labov 1984: 35)

interviews are structured in terms of *modules* or sets of questions organized around specific topics. These modules may then be organized into what Labov (1984) describes as *conversational networks*. The topics of the modules are selected using two criteria. First, previous experience had shown some topics to be successful in engaging speakers in interaction (the danger of death question discussed in section 3.3.3 is of this type). Second, and equally important, is the information that a given topic can yield on neighborhood norms and on general social and background information of value to the researchers. The fieldworker on any given occasion selects modules from a larger set in order to construct a conversational network appropriate to a given speaker; a typical network selected for an adolescent or young adult in Philadelphia can be seen in figure 3.1.

The general idea of this interview schedule is to use interlocking modules to simulate the seamless topic-shift structure of normal conversation. There is no fixed order for working through the modules and the fieldworker is expected to allow the subject's interest in any particular set of topics to guide transition through the network from module to module.

Question design is always given very careful attention in terms of phrasing and ordering within modules: the initial and final questions from each module are designed to facilitate topic shifts to other modules in the system. Module 3, "Fights," ends with the questions:

(1) Do girls fight around here? Did you ever get into a fight with a girl?

This can lead, for example, directly into the module dealing with dating patterns:

(2) What are girls *really* like around here?

All questions, as illustrated by those quoted here, are designed to be brief; Labov notes that questions formulated without preparation can often be lengthy and unclear, containing spontaneous conversation phenomena such as hesitations and false starts. Module questions are also formulated to be as colloquial as possible, avoiding any "bookishness" in syntax and lexicon. While experienced interviewers may adapt the wording of some questions to fit their own style, other questions are to be asked exactly in the prescribed form.

In a large-scale project like Labov's Philadelphia research, the use of a detailed conversational network plan helps to establish consistency across the many interviews conducted by various fieldworkers. In many cases, however, interview preparation need not involve such an elaborate structure as that suggested by figure 3.1, and certainly every question need not be formulated beforehand. For example, Gordon's (2001b) study mentioned earlier (section 2.4) involved interviews with adolescents and adults in two different small towns. Before beginning the fieldwork, he prepared a list of topics to be raised in the interviews. Some items were discussed by all participants while others were relevant only to one or the other age group. All the questions pertained to the general topic of life in the subject's town; they did not cover anything like the range of areas in figure 3.1. Nevertheless, the fairly restricted list of topics generated 60 to 90 minutes of conversation with each interviewee. The key, in addition to locating cooperative speakers, is preparing topics that the participants will eagerly discuss at length. In the small towns investigated by Gordon, the adults spoke fondly of their childhoods and had much to say about how the town had changed since they were young. With the adolescents, the conversation focused on school, their social lives, and their plans for the future.

Clearly, not all topics will work for all speakers. Interviewers must be flexible and willing to adjust their approach to their subjects. For example, in his Michigan research, Gordon interviewed one man who refused to answer any direct questions about his personal history including his educational and employment background. This refusal came at the beginning of the interview and suggested that the interview would not be fruitful. However, the man was happy to talk about more general matters ranging from local history and culture to national politics. The interview thus kept to topics with which he was comfortable. The discussion was as lively as any, and over the course of the interview the man revealed many of the personal details that he had initially refused to discuss. The interview was a success, though it was unlike any other that Gordon conducted.

While interviewers need to be concerned with preparing questions that will generate extended conversational responses, they can use their questions to learn about who their subjects are as well as how they talk. Basic demographic information about the speakers, such as age, sex, and possibly ethnicity and socioeconomic class, must be recorded for later analysis of covariation between social and linguistic variables, and any such personal information that might have some relevance in explaining speech behavior can be pursued. Interviews often produce valuable qualitative data that can complement quantitative analyses. For example, Gordon (2001b) discovered patterned speech differences between pairs of adolescent girls in one of the Michigan communities he examined. These quantitative differences in usage were explained by referring to social differences including the types of friends the girls had and the way they spent their free time. This kind of information was specifically targeted in the interviews because previous research, especially that of Penelope Eckert (e.g., 2000), had demonstrated its relevance in adolescent social structure (see further section 3.4).

3.3.2 *Interviews as speech events*

The preceding discussion of interviews has sketched some of the basic characteristics of this technique. It was noted that the interview has been the most common method of data collection among variationist sociolinguists. In this section, we examine this method further and argue that, despite its popularity, the interview offers a rather problematic solution to the needs of data collection. This is particularly true when the research seeks to elicit natural conversational speech.

Much of the difficulty involved in interviewing stems from the fact that an interview in western society is a clearly defined and quite common speech event to which a formal speech style is appropriate. It generally involves dyadic interaction between strangers, with the roles of the two participants

being quite clearly defined. *Turn-taking rights* are not equally distributed as they are in conversational interaction between peers. Rather, one participant (the interviewer) controls the discourse in the sense of both selecting topics and choosing the form of questions. The interviewee, on the other hand, by agreeing to be interviewed, has contracted to answer these questions cooperatively. Once the interviewer has obtained a response, the obligation rests upon him or her to follow it up with a further question. People are generally quite well aware of the behavior appropriate to these roles, and of their implications in terms of unequal distribution of rights to talk.

The first point that might be made is that individuals who are being questioned will seldom produce large volumes of speech in their replies. This may in part be a consequence of the "cooperative principle", in the sense that they are attempting to comply by responding relevantly and briefly (Levinson 1983: 100). Interviewers may work to 'fudge' the nature of the event in an attempt to encourage the interviewee to relax and produce larger volumes of speech, but the well-defined nature of the interview as a speech event, along with the associated social and discourse roles of the participants can make such efforts very difficult. Interviewers may risk confusing or even angering research subjects if they stray from the expected interview format. For example, during fieldwork in Michigan, Gordon interviewed the owner of a café, asking a series of questions about the operations of the business. This line of questioning was pursued not only because it seemed likely to produce extended replies but also because it provided information about the interviewee's experience and the business environment in that community. The owner, however, became very suspicious, and objected, "You don't care about this stuff; you just want to get me talking." After assurances that the information was important, the interview continued and, in the end, was quite successful. This example indicates, however, the need for interviewers to make clear the relevance of their questions since it is one of the expectations speakers bring to the interview.

Labov has suggested that the basic counter-strategy of the sociolinguistic interview is to acknowledge the "position of the interviewer as a learner, in a position of lower authority than the person he is talking to" (Labov 1984: 40). An experience described by Briggs (1984) illustrates the fruitfulness of this strategy in dealing with some subjects. After a series of disastrously uninformative interviews with the Lopezes, a wood carver and his wife in Cordóva, northern New Mexico, Briggs reports a successful set of interviews with another community elder: "He agreed to both the interviews and their tape-recording. When I returned from my car, Mr Cordóva asked me 'Now what is it that you wanted to know?' I provided him with one of the questions that had fared so badly with the Lopezes. He then proceeded to produce a long, flowing narrative of the local carving industry" (Briggs 1984: 23). Although Briggs attributes his success with Mr Cordóva and his

failure with the Lopezes to different factors (cross-cultural differences in conversational structure), it may be significant that by addressing to Briggs the question "Now what is it that you wanted to know?," Mr Cordóva has, to some extent, reversed the roles of interviewer and interviewee in such a way as to be congruent with the deference due to him as a knowledgable older man. Attempts to downplay the asymmetrical roles characteristic of interviews are, however, not always so fruitful (see Wolfson 1982 for a critique of Labov's recommendation).

A rather different type of challenge posed by interviews relates to the nature of the data they are likely to produce. The basic format of the interview may impose limitations on the structural characteristics of the data. Certain speech phenomena may be difficult or even impossible to study using interviews. One concern relates to the frequency of occurrence of the phenomena under investigation. Many interesting syntactic variables, for example, appear infrequently in the course of everyday conversation (e.g., relative clauses), and their rarity may present challenges to quantitative analysis – an issue that is addressed in more detail in chapter 7. As we mentioned earlier (section 3.2.2), this problem has led some researchers to trust more direct methods of investigation such as surveys to measure usage of certain syntactic features (see, e.g., Bailey, Wikle, and Tillery 1997).

In some cases, the problem relates to the pragmatic constraints of the interview. An obvious example is interrogative constructions, which are plentiful in spontaneous speech in a range of discourse functions, but are likely to be inhibited in the speech of interviewees. One of the variables studied by Cheshire (using a participant-observation method (see section 3.4)) in the speech of Reading adolescents was the *tag question.* Tag questions do not usually function in discourse as requests for information, but rather are conducive forms seeking confirmation of a previously stated proposition:

(3) She's here already, isn't she?

(4) This is your book, right?

Since their main function is to compel a (normally minimal) response from the addressee it is unlikely that they will often be used spontaneously by a speaker whose perceived social role is to respond to questions put by another.

The question-and-answer format of the typical interview may, in fact, inhibit the use of a number of potentially interesting variables. For example, the so-called *hot-news perfect* (McCawley 1971), a feature of Irish English, is unlikely to occur in interview sessions since it indicates an event in the immediate past:

(5) A young man's only after getting shot out there.
[St. E. "A young man has just got shot out there."]

Commenting on these discourse and pragmatic constraints on higher-level syntactic variation, Harris (1984: 316) makes the additional point that tokens of the hot-news perfect are absent from Irish rural data collected by formal interview methods in the Tape-Recorded Survey of Hiberno-English. Conversely, examples such as this turned up in some quantity in the Belfast urban sociolinguistic projects (see section 3.5), which utilized participant-observation techniques.

The formality of the interview may also limit efforts to examine certain variables. The use of overtly stigmatized features (e.g., multiple negation in English) is likely to be affected by the act of answering questions from an interviewer. This is true of syntactic as well as phonological features. For example, in the Belfast project, the researchers encountered severe difficulties in collecting examples of the vernacular pronunciation of words such as *meat, beat, heat, leave* (the MEAT class, as it is commonly known to historians of the language). Among vernacular speakers, the vowel in this class of words approximates to, but is not identical with, the vowel in items like *mate* and *bait* (the MATE class). This feature is of considerable theoretical interest, as it sheds light on a notorious historical problem of an apparent merger in early Modern English and subsequent reseparation between words of MEAT and MATE classes (see Milroy and Harris 1980; Milroy 1992 for details and historical references).[2] The Belfast investigators had heard this pronunciation in casual exchanges but were frustrated in their efforts to capture examples on tape. The vernacular alternates that were recorded nearly all occurred in peer conversations and only very rarely in speech addressed directly to the fieldworker.

A related problem particularly associated with data collected by means of interviews is that the effects of *speaker correction* of socially stigmatized items are often indirect, and can also give a misleading impression of phonological structure. In working-class West Belfast, tokens of /k/ are frequently heavily palatalized, with a palatal glide appearing between the initial consonant and a following front vowel. Pronunciations like [kjap, kjat, kjɪd] ("cap," "cat," "kid") are stigmatized and apparently recessive, being used mainly (but not entirely) by middle-aged and older men. During an interview, a middle-aged, West Belfast woman described a visit to a shop where she stood in a queue [kuː]. One possible explanation of this realization was that the Belfast urban dialect, like many others (such as Norwich, for example), deleted in some contexts the palatal approximant /j/. But only after vernacular pronunciations like [kjap] "cap" had occurred in informal conversations did a clearer picture of the structure of the dialect emerge; the speaker had *hypercorrected* standard [kjuː], classifying it along with non-standard /kj-/ items. Interestingly, this process revealed a misanalysis of phonological structure since the vernacular palatalization rule affects only items with a following front or low back unrounded vowel; a word like *cool*, for example, does not show [k]/[kj] alternation.

These examples demonstrate the limitations of the interview – even the informal interview – as a means of collecting linguistic data. Briggs (1986) offers a more general critique of interviewing as a social-scientific method and discusses alternative approaches of particular relevance to research outside the context of western, industrialized cultures. Sociolinguistic interviewers need at the very least to be alert to possible problems, since, particularly at the level of pragmatic and discourse constraints on syntactic structure, they are not always immediately obvious. Various techniques that have been developed for dealing with such constraints by investigators into *syntactic* variation are discussed in chapter 7. Techniques for dealing with the problem of investigating vernacular forms are discussed in the next section.

3.3.3 *Interview strategies for eliciting casual speech*

As noted in the opening to this chapter, the speech in which sociolinguists have traditionally taken the greatest interest has been the spontaneous, everyday style that is often called the vernacular. For reasons outlined in the previous section, the interview is far from an ideal instrument for gathering data on this speech style. Nevertheless, there are steps that can be taken by interviewers to encourage subjects toward more casual speech. Such strategies are basically of two types: (1) attempts to influence the content of the interview; and (2) modifications to the dynamics of one-on-one interviewing.

When people are emotionally involved (excited, angry, fearful, etc.) in a discussion, they are more concerned with what they say than with how they say it. Following this logic, interviewers can obtain less self-conscious speech by asking questions that bring about such emotional reactions. The best known of these is Labov's "danger of death" question which asks subjects about situations in which they feared for their lives (see Labov 1972b: 93 for details and rationale of the technique). Despite his success with this technique, attempts by others to use it have frequently backfired. For example, Trudgill comments on its lack of success in Norwich, suggesting that perhaps Norwich people have led somewhat less eventful lives than New York City people. But in Belfast it was inappropriate for quite different reasons. During a conversation with a working-class family about the general hardships of life, it emerged that one 19-year-old man had already had a number of narrow escapes from death. First, as a merchant seaman he had almost drowned in the Baltic, his ship having been run down by a Russian vessel; then he had been held up by gunmen in a Belfast alley-way; arrested and beaten by troops as a Republican sympathizer; and two months before the period of the research he had been shot in the legs during an intergroup Republican feud. His response to these alarming events was not at all the

one predicted by Labov, but nevertheless accorded closely with Ulster norms of behavior. It may be described as a rather world-weary cynicism at the duplicity of the authorities combined with a tendency to low-key black humor. On this occasion the danger of death question was not put explicitly; but on the two or three occasions when it was, the characteristic response of Belfast people was a matter-of-fact account of what were often quite unpleasant and dangerous experiences. The question proved equally ineffective for Butters (2000) who used it in a series of interviews with North Carolinians. In contrast to the stoic Belfast reactions, the Americans who had experienced brushes with death were often hesitant to discuss them because they considered them "too terrible or frightening to speak of" (2000: 73). Those who did discuss such experiences tended to use very careful and often philosophical or theological language – hardly their most casual style.

As these difficulties show, the danger of death question does not translate well into every speech community. Nevertheless, many researchers have found other questions that fulfill the function of engaging the speaker's attention in the way Labov describes. Wolfram and his colleagues working in North Carolina have had good success with the strategy of asking interviewees to tell ghost stories (Herman 1999). Even less-structured approaches can be productive; the key is hitting upon a topic that will engage the interviewee. Gordon (2001b) found that questions about childhood experiences had the desired effect for adult speakers, while adolescents often responded well to inquiries about the social structure of their high schools (e.g., Are there cliques in your school?).

Changing the dynamics of the interview away from the one-on-one format can also facilitate the production of casual speech. This may be accomplished by having either two or more interviewers or two or more interviewees. The North Carolina research team headed by Wolfram has taken the former approach (see Wolfram and Schilling-Estes 1996; Wolfram, Hazen, and Schilling-Estes 1999). They use pairs of interviewers to create a three-way conversation which eliminates some of the awkwardness of two strangers having to speak one-on-one. The questioning seems to proceed more smoothly since the fieldworkers can work together to keep the discussion going; and there are fewer lulls in the conversation when one fieldworker needs to look at notes for the new questions. Also, one fieldworker can monitor the recording equipment which is often a distraction in one-on-one interviews.

In their approach to breaking down the interview structure, Labov et al. (1968) studied groups rather than individuals for their Harlem research. As demonstrated in Labov's famous account of an interview with Leon, an African American 8-year-old, and his friend Gregory (Labov 1972a: 210) this has the effect of "outnumbering" the interviewer and decreasing the

likelihood that speakers will simply wait for questions to which they articulate responses. In fact Leon and Gregory tended to talk to each other rather than to the interviewer. Often, fieldworkers can attach themselves to the fringes of the group. Although Labov and his colleagues used both individual recording sessions and group sessions, it was during the group sessions that the richest data were recorded. The speech of each member was recorded on a lavaliere microphone (worn round the neck) on a separate track, and the atmosphere of the sessions was more like that of a party than an interview.

One critical point is that if a speech event can be defined as something other than an interview, it is very likely that group members will talk to each other rather than adopt the role of respondents. The effect of group dynamics seems also to be important, as Nordberg (1980: 7) explains:

> . . . the stylistic level is controlled in quite a different way than in an interview, i.e. the members of the group themselves exercise social constraint on one another's language. It would be quite unacceptable for someone in the group to put on an act during the recording and use a form of language which was not normally used in that speech community or among the individual speakers. The more closed the social network of the discussion group is, the stronger the social pressure will be to speak in accordance with the group norm. But even in the case of discussion groups which must be described as open social networks we are on safer ground when it comes to the authenticity of the language used than we are in the case of an interview.

In support of these remarks, Nordberg cites experimental evidence from Thelander's work in Burtrask, northern Sweden, where the linguistic effects of manipulating group composition in various ways were systematically examined. Other researchers have described episodes which illustrate the kind of control that a close-knit group in particular exercises on the language behavior of its members (Labov 1972a; Milroy 1987).

The point at issue then is not whether or not the presence of the group in some way allows participants to "forget" that they are being observed. This is unlikely since, for example, the groups studied by Labov et al. had been convened specifically for the purpose of recording, and the microphones worn by speakers must have constrained their physical movements considerably. But it does appear that the tendency of outside observation to encourage careful, standardized styles and inhibit the emergence of vernacular structures is to a considerable extent *counteracted* by the operation of the group dynamics described by Nordberg.

For these reasons, a number of researchers have adopted the strategy of collecting data from groups rather than individuals (Reid 1978; Edwards 1986). Hewitt's study of the use made of patois by London adolescents focused on groups (Hewitt 1982), while Edwards finds very great differences in the language of British black adolescents, depending upon whether it is

collected in a group session or in response to the questions of a single interviewer. Cheshire, like Hewitt and Edwards, combined the strategies of studying groups and focusing on adolescent language; she was plainly very successful in obtaining large amounts of quality data. In some cases, researchers have taken the study of groups even further by removing the investigator from the scene. For example, Stuart-Smith (1999) collected samples of Glasgow speech by placing pairs of speakers in a room with a recorder and leaving them to discuss whatever suited them.

In conclusion, it is important to note that although interviews are not ideal instruments for sampling informal speech styles, they can with some "fudging" be fruitful in this regard. The various strategies that we have discussed here need to be used carefully and appropriately in conjunction with each other, and with local conditions very much in mind. In the end, however, investigators who, for whatever reason, need to examine speech styles that are unavailable even in the most relaxed interviews, would do well to consider more ethnographic approaches. In the following section, we consider such an approach as we examine issues related to participant observation.

3.4 Participant Observation

In most cases researchers investigate communities of which they are not members. Their outsider status poses a challenge to their ability to overcome the observer's paradox. In an attempt to change this status, investigators may adopt the role of participant observer. This ethnographic approach entails long-term involvement in a community and is fundamentally a pursuit of local cultural knowledge (Johnstone 2000b: 82). The principal benefits of participant observation are (a) the amount and quality of the data collected, and (b) the familiarity with community practices gained by the investigator.

These benefits can be illustrated by the work of Penelope Eckert (1989a, 2000), whose research in Detroit-area schools was mentioned briefly above (section 2.3). Eckert spent two years studying a suburban high school, and although her research was authorized by school authorities, she intentionally avoided an official role in the school. Any association with the institution might have limited students' willingness to speak frankly with her. Eckert spent her time in the school outside of classrooms, in public areas such as the library and the cafeteria or just wandering the halls. She observed students' behavior and interacted casually with them. She also conducted and tape-recorded interviews with 200 students alone and in groups (2000: 82). Throughout her fieldwork, Eckert took care to circulate among the entire student body and not to limit her investigation to particular social networks.

The linguistic data produced by Eckert's research are difficult to match in terms of quality and quantity. Her interviews resulted in hundreds of hours of informal speech from a variety of students. The comfort the students felt with Eckert is evident in the content of the interviews. They discuss fairly taboo subjects including sex, drugs, and crime, and they share very personal thoughts and stories. Clearly, Eckert's long-term involvement at the school made her a familiar presence, and she gained the trust of students. As she explains, "Students introduced me to their friends, and as my reputation spread, some came and introduced themselves to me on their own" (1989a: 32). Her extended contact with the school also meant that she spoke with students on multiple occasions. In fact, none of the individual interviews represented her first interaction with the student (2000: 79). Knowing the interviewees beforehand helped to eliminate any discomfort in participating in a tape-recorded interview.

Eckert's familiarity with this community gave her not only access to the students' speech but also the insight to make sense of the linguistic (and other) behavior she observed. The depth of this insight is seen most clearly in her treatment of the distinction between two categories of students: the jocks and the burnouts. This division plays a central role in the social structure of the school. It is the "means by which socioeconomic class is constructed in and for the adolescent population" with jocks and burnouts constituting "middle class and working class cultures respectively" (Eckert 2000: 2). While these social categories could be explored in a less ethnographically oriented study, Eckert argues for the broadened perspective that long-term participant observation provides:

> [T]he significance of the jock and burnout categories lies not simply in their existence and membership, but in their day-to-day motion. The two categories are based in practices that unfold in daily and mundane activity, interaction, and movement. And membership is not an either-or matter, but composed of many forms of alliance, participation, comings and goings. Viewing jocks and burnouts as members or representatives of categories would not only gloss over the histories, uncertainties, and multiplicities that constitute social affiliation, but would also freeze the categories and mask the fact that they exist only in practice. (Eckert 2000: 74)

Simply put, the practices associated with being a jock or a burnout can be better understood by a researcher directly observing them than by just hearing about them in interviews.

In order to develop her understanding of the social world she investigated, Eckert spent two years at the school, attending "regularly" for the first year and "occasionally" for the second (1989a: 28). While the rewards of a project such as Eckert's are considerable, few researchers can afford to spend so much time in the field. Fortunately, many of the benefits of

participant observation can be achieved without such a tremendous commitment of time. Patricia Cukor-Avila, for example, utilized participant observation in her study of the small town of Springville, Texas (Cukor-Avila 1997; Cukor-Avila and Bailey 1995). She made daily trips to the town for nearly two months. Rather than circulate through the town conducting individual interviews, Cukor-Avila spent her time in the general store, a focal point for the community and a place frequented by nearly every resident on a daily basis (in part because it also served as the post office). Initially she conducted some interviews with residents visiting the store, but soon these "became closely intertwined with the day-to-day business of the store and began to include a wide range of unsolicited interactions, including teasing, arguments, jokes, business transactions, and the routine conversations that make up much of the community's linguistic activity" (Cukor-Avila and Bailey 1995: 167).

Cukor-Avila's approach proved fruitful in gaining access to everyday speech in fairly natural situations. The fact that her subjects shared town gossip with her and got into heated arguments in her presence suggests the comfort they must have felt with her (and her tape-recorder). The methods represent an innovative approach to overcoming the observer's paradox by allowing the fieldworker to move into different conversational roles. As Cukor-Avila and Bailey (1995) discuss, in most sociolinguistic interviews, the fieldworker remains the addressee. In the Springville corpus, however, she often becomes an overhearer or even an eavesdropper, in the sense of Bell (1984), as conversations among community members go on around her.

There are practical difficulties involved with an approach such as this. The fieldworker sacrifices control over the recording situation which can become somewhat chaotic as several participants interact. Cukor-Avila and Bailey (1995) note the challenges that parallel conversations can pose for intelligibility and recommend that fieldworkers keep notes on who is present, where they are situated, etc. Another potential problem for researchers pursuing quantitative analysis may be ensuring that enough speech is recorded from each speaker. This difficulty can be remedied by seeking follow-up interviews. Cukor-Avila, for example, has returned regularly to Springville.

Participant observation works well in small, well-delineated communities where suspicions about outsiders might inhibit other approaches to data collection. Such is the case with the research directed by Walt Wolfram on Ocracoke Island off the coast of North Carolina (Wolfram, Hazen, and Schilling-Estes 1999; Wolfram and Schilling-Estes 1996; Schilling-Estes 1999). This project differs from those discussed above in that it involved multiple fieldworkers. Wolfram and his team made repeated visits to Ocracoke over the course of several years, staying for periods of up to two weeks (Wolfram and Schilling-Estes 1996: 106). During their visits, the investigators conducted interviews with islanders and engaged in participant observation

of local activities. The interviews were designed to fit with local norms in order to reduce their formality. For this same reason, many were conducted by pairs of fieldworkers. As Wolfram and Schilling-Estes (1996: 107) explain, "[f]or example, the husband and wife team of Walt and Marge Wolfram, or another natural pair of fieldworkers, might make an after-dinner visit to a home for an interview, thus fitting into a fairly natural and recognized type of social occasion."

In the Ocracoke case, as with others, participant observation was crucial to developing a deeper understanding of the community than might otherwise be accessible to outsiders. The objective is "to understand the sociolinguistic dynamics of the community from the perspective of the community itself" (Wolfram and Schilling-Estes 1996: 106). Local knowledge expands researchers' explanatory possibilities; it allows them to move beyond the standard macro-social categories like age, sex, and socioeconomic class. For example, Wolfram and his colleagues encountered the "poker game network," a group of men who meet regularly to play poker. These men come from different educational backgrounds and have varying degrees of contact with non-islanders. Still, they share a "strong belief in the positive value of being true islanders" (Wolfram and Schilling-Estes 1996: 106). These men are also united linguistically in their embrace of traditional vernacular features, especially the backed and rounded pronunciation of the /aj/ diphthong which has become a stereotype of Outer Banks speech (see further section 4.4.3).

Participant observation is particularly valuable as an approach to studying variation in a bilingual context. The observer's paradox can be especially problematic for bilingualism researchers because of the influence of audience in determining language choice. The presence of a community outsider, the researcher, will likely bias the results. For this reason, Sarah Shin chose participant observation in her study of Korean/English bilingual children in New York City, adopting the role of a teacher's assistant in their first grade classroom (Shin 1998; Shin and Milroy 1999, 2000). In this capacity, she was able to casually observe what was apparently characteristic linguistic behavior in the classroom (including language choice and mixing). Her own bilingual abilities permitted Shin to engage the children in either code. Moreover, she recorded peer interactions by having the children wear a small wireless microphone while they engaged in various school activities.

As the examples mentioned here illustrate, participant observation can be an enormously fruitful method for sociolinguistic analysis. It produces a tremendous supply of high-quality data and crucial insight into community dynamics. Nevertheless, there are some important disadvantages associated with this approach. We have already hinted at one disadvantage in noting the amount of time investigators like Eckert spent in the field. Such studies are extremely demanding for the fieldworker not only in time but also in energy, tact, and emotional involvement with community members. Related

to this is the fact that participant observation is rather inefficient as a data collection procedure. It can be quite wasteful since many more speakers and many more hours of speech are recorded than can ultimately be analyzed. A useful rule of thumb is that a minimum of ten hours will be needed to analyze every hour of recorded speech. A more substantive analytical problem relates to the challenge of locating the results of a focused, ethnographic study of a particular community in a wider sociolinguistic context. Examining one high school or one small town alone cannot tell us how that situation at that site fits into the system of sociolinguistic variation in the city or region as a whole. For this reason, investigators engaged in participant observation often supplement their study with forays into similar communities in order to broaden their perspective (e.g., Eckert 2000; Wolfram, Hazen, and Schilling-Estes 1999). Alternatively, researchers might combine participant observation with other approaches, as illustrated by the case study in the following section.

3.5 Balancing Fieldwork Strategies: The Belfast Project

In an ideal world, a researcher interested in sociolinguistic variation in a given community would collect speech samples from every member of the community in every situation of use. Furthermore, the researcher would examine every linguistic variable as they relate to every social variable and present an analysis that accurately describes the local norms and practices shaping sociolinguistic variation. An endeavor of such scope is clearly beyond the reach of even the most-talented, best-funded researchers. Instead every investigator makes choices about the breadth and depth of coverage to be pursued based on limited time and resources.

Breadth and depth generally operate in inverse proportion to each other, and they influence sampling as well as data collection. A comparison of two studies discussed earlier in this chapter – Labov's Telsur project and Eckert's high school study – illustrates this relationship. In surveying all of the US and Canada, Telsur certainly offers geographical breadth. This project can, for example, indicate the range of areas affected by sound changes like the Northern Cities Shift (NCS) (see chapter 6). However, it does not offer depth of coverage for any one location since it surveys only two speakers for most cities and at most six speakers for even the largest urban areas. Thus, Telsur cannot tell us the extent to which a feature like the NCS has penetrated a given community. Eckert also investigated the NCS, but from a very different perspective. Her study examined only a single location but provided in-depth coverage of that location by interviewing some 200 students. Clearly, Eckert can make no claims about the geographical status

of the NCS, but she can offer information about the social distribution of such features.

Decisions about data collection are also influenced by the need to locate investigations on the breadth/depth continuum. Techniques that efficiently gather data from a large number of people are generally limited in terms of the kinds of information they can investigate. For example, written questionnaires can provide broad coverage of a population but cannot tell us much about intraspeaker variation such as how frequently individual speakers use a given feature. On the other hand, approaches that provide a more holistic view of a variety of linguistic variables typically examine fewer speakers. In fact, some studies concentrate on one or two speakers whose usage is examined in detail (e.g., Coupland 1980).

Decisions about how broadly and how deeply one's project will cover a target population and the language of that population are faced by every researcher, and every researcher makes greater or lesser sacrifices in both dimensions. What is critical is that the limitations of the chosen approach be recognized. In many cases, these difficulties can be addressed in follow-up research employing alternative approaches with complementary benefits and limitations. The following discussion considers an example of a project that utilized multiple strategies for data collection. It is presented here as a case study illustrating how various approaches can be integrated. Labov's descriptions (1984, 2001b) of his Project on Linguistic Change and Variation – a series of studies examining sound changes in Philadelphia – serve the same purpose, and readers may wish to consult Labov's account for a comparative perspective on ways of balancing research strategies.

The research discussed here was carried out in 1975–81 in and around the city of Belfast, Northern Ireland, under the direction of James and Lesley Milroy, and many of the key findings have been presented elsewhere (Milroy and Milroy 1978; Milroy 1987, Milroy 1981; Pitts 1985). Our discussion here focuses on the design of the project which included three distinctly different approaches to the study of language variation and change in the city. These are a series of community studies involving participant observation, a doorstep survey based on a random sample of area households, and a study of a rural community in the hinterland of Belfast.

3.5.1 The community studies

A total of five communities within the city were selected for in-depth neighborhood studies. In choosing communities, the investigators attempted to give broad coverage of major geographical, status, and ethnic divisions in the city (see figure 3.2). The first three, Ballymacarrett, the Clonard, and the Hammer, are very low-status inner-city areas. Ballymacarrett is located

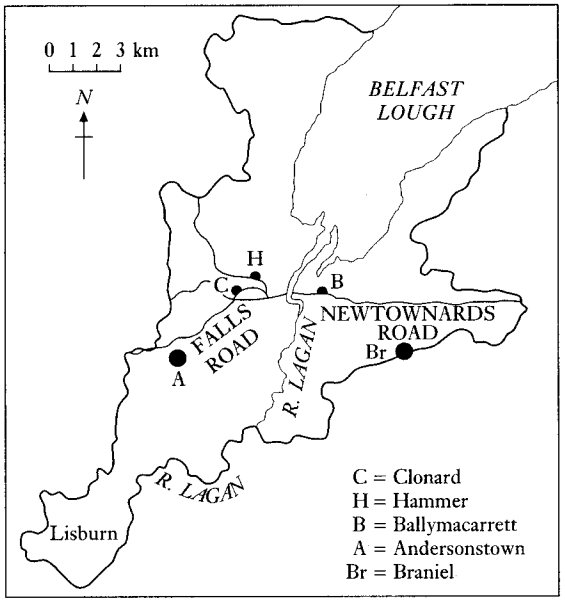

Figure 3.2 Location of five speech communities in Belfast

east and the Clonard and the Hammer west of the River Lagan, which bisects Belfast and constitutes an important socio-geographical boundary. The other two, Braniel and Andersonstown, are both located on the outer edges of the city and might be described approximately as upper-working to lower-middle class. Braniel is east of the river and is exclusively Protestant, while Andersonstown is west of the river and is exclusively Catholic.

Ballymacarrett and the Clonard can be viewed as somewhat lower-status "feeder" areas of Braniel and Andersonstown respectively. Many Andersonstown people originated from or had family ties with the Falls Road, the area where the Clonard is located; the Braniel population on the other hand are in East Belfast, and many have ties with the Newtownards Road where Ballymacarrett is located. The identification of these interrelationships between the areas is well motivated; when Belfast people change their place of residence, for whatever reason, there is a strong likelihood that the new location will be selected in accordance with a highly predictable set of urban sectoral preferences. To some extent, these sectoral preferences are related to ethno-religious lines of demarcation (Boal and Poole 1976); certainly it is reasonable to view inner-city communities east and west of the river as each having a higher-status ethnic counterpart.

The general idea of selecting communities that were interrelated in this way on the dimensions of status and ethnicity was to "match-up" linguistic data from the four areas in order to obtain some fairly detailed information on the linguistic strategies that Belfast people employed as they moved from urban vernacular to slightly higher-status speech patterns. The investigators were also curious as to whether east/west differences in the structure of the vernacular (see Milroy 1987) would be maintained by higher-status speakers. Thus, the choice of communities was constrained quite sharply by the analytical goals of the research.

The fieldworkers were eager to reach central community networks in each area in order to gain access to vernacular speech. Since contacts made through individuals with a clear institutional status – such as teachers, priests and community leaders – can often lead to rather standardized speakers, these contacts were avoided. Instead, communities were always approached initially through persons encountered directly or indirectly in the course of everyday living who had no institutional status in the communities, but were members of them. The Andersonstown fieldworker however, an anthropology graduate, was a local resident and was also one of the subjects. It was in this community that the closest approximation to a traditional participant-observation study was achieved, since the observer had for her entire life been part of the social setting which she was observing.

This guiding principle of participant observation – that the observer should be part of the setting which he or she is studying – was followed as closely as possible in the other communities. It was for this reason that the fieldworkers in the other areas, who were not local residents, adopted the role of "a friend of a friend" which gave them a clear position in the community as noted earlier (section 2.3). Following this strategy, the fieldworker introduced herself initially in each community not in her formal capacity as a researcher but as a "friend of a friend" (see Boissevain 1974 for a discussion of the significance of this relationship) mentioning the name of a person categorized as an insider with whom she had previously made contact and who had given her the names of people who might initially be approached. As a consequence of the reciprocal rights and obligation that members of close-knit groups contract with each other, the mention of the insider's name had the effect of guaranteeing the fieldworker's good faith; moreover, members of the group appeared to feel some obligation to help her in her capacity as a friend of their friend, so that she acquired some of the rights as well as some of the obligations of an insider. In all communities, but most particularly in the poorer inner-city areas (largely as a consequence of the denser, more multiplex structure of local networks), the fieldworkers were passed from one family to another, being received with warmth, friendliness, and trust. The research was presented as an investigation of the way life and language in the community had changed, and general permission

was sought to record interactions at which the fieldworker was present. Once people had agreed to participate, this permission was never refused. As a consequence of the local norm of extended visiting (which was commonest in the low-status inner-city areas and least common in Braniel) many of the recordings were of two, three, or four people talking among themselves, often with minimal fieldworker participation.

The investigators used snowball sampling (section 2.3), recording subjects as described above until a quota sample of 16 people in each community had been filled. The target quota consisted of eight young adults (18–25 years) and eight middle-aged adults (40–55 years), equally divided between males and females. In practice, of course, many speakers fell outside the target quota. The quality and quantity of data collected during these community studies was excellent, including many group sessions with little or no linguistic participation by the fieldworker.

In the Belfast research, no attempt was made to provide an interview schedule such as that developed by Labov (section 3.3.1); fieldworkers were simply briefed on the kind of social information (such as occupation, educational background, family connections, and previous residence of speaker) that they would need to have acquired by the end of the observation period. The topics associated with this information – such as local attitudes and local networks of relationships – generally provided a more than adequate conversational resource.

The Belfast methods in these neighborhood studies were developed primarily for the study of *close-knit* communities – and indeed they are particularly suitable for urban or rural communities of this type. Bortoni-Ricardo's (1985) work in Brasilia provides an example of a sociolinguistic "network" study of a close-knit migrant community from a very different kind of society, using methods similar to those described here.

3.5.2 *The Belfast doorstep survey*

The doorstep survey was designed to complement, by means of a rather wider but shallower study, the detailed information collected in the five communities. This study was developed after intensive work on three inner-city areas had already been completed. Consequently, the basic facts about sociolinguistic patterns in the city were more or less known to the researchers, and the doorstep survey was employed to obtain quite specific and limited linguistic information.

The speakers surveyed were drawn from a random sample of households provided by the Northern Ireland Housing Executive. The sample population used by the project was actually drawn from a larger sample of 500 households which the Executive had used for their own survey of housing

patterns in the city (cf. Labov's subsampling procedure in New York City). In the end, a total of 73 speakers were interviewed, and an analysis was made of data from 60 speakers, 32 males and 28 females. In order to ensure comparability, the main effort in the interviews was focused on recording carefully designed word lists, which sampled realizations of several phonological variables in a range of phonetic environments. Some spontaneous speech was also recorded.

The interviews could be completed on the doorstep in as little as 10 minutes, but most lasted between 15 and 45 minutes and more often than not the fieldworker was invited into the house, offered tea and biscuits – and on one occasion two meat pies!

In view of the brevity of the basic interview, subjects were not contacted in advance; fieldworkers simply knocked on the door and requested interviews. It was hoped in this way to reduce the high refusal rate which is a hazard in sociolinguistic surveys, and indeed the method seemed in general to be successful in this respect; altogether, out of 40 addresses visited, only eight refusals were recorded. As these refusals were mainly from relatively high-status households, interviewers had to be careful to substitute comparable households in order to maintain the social balance of the sample.

The purpose of the research was presented as an investigation of change in Belfast life and language. The rather vague account satisfied and stimulated the interest of most people; fieldworkers began asking interviewees how long they had lived in the area and moved on to discuss their general attitudes and background. The minimum social information required for each speaker was (approximate) age, sex, housing type and (where possible) occupation. Since the word list was considered to be particularly important, it was usually produced early in the proceedings. Where possible, fieldworkers attempted to record a male and a female at each address, but since both were seldom at home at the first visit, a second and even a third visit was often necessary.

The general principle of the Belfast doorstep survey is quite similar to that of other surveys (see section 3.2) as they are not designed to attain any depth of insight into, for example, stylistic variation, or the general structure of a speaker's phonology. Rather, information is sought on broad patterns of variation across a wide social range in the urban community. The major effort of the Belfast research was focused on the community studies; and the main function of the doorstep survey was to provide a context within which to place the findings of those studies. Although the data collected were quite adequate for this purpose, no attempt was made to claim representativeness for the doorstep survey.

It should be emphasized that brief, shallow surveys like that described here are most rewarding when they are designed to solve specifiable and well-understood problems. They are not suitable instruments for exploratory research, which is best carried out by in-depth investigation of a small

number of speakers. For example, in order to prepare instruments like word lists which allow for efficient data collection in such surveys, the researchers must already have a fairly solid understanding of local speech patterns, including information about the linguistic conditioning of the variables to be studied (see further section 6.4).

3.5.3 The rural hinterland study

This part of the Belfast project was motivated by a question of considerable sociolinguistic interest – the relationship between dialects of cities and those of surrounding areas (for other explorations of this relationship see Callary 1975 and Gordon 2001b). Since linguistic change and rapid dialect mixing appear to be a general characteristic of urban dialects (see section 5.3; Labov 1972b: 300; Kerswill and Williams 1999, 2000), it seems reasonable to assume that insight can be gained into the processes of their formation if a set of data from a city is compared with a set from a surrounding area. Belfast, being a relatively young industrial city, is an excellent site for such an investigation (see Milroy and Milroy 1978 for details).

The hinterland area selected for study (see Pitts 1983, 1985 for details) was Lurgan, a small rural town in the Lagan Valley, 17 miles southwest of Belfast and outside the urban overspill area. It had been noted previously that certain phonological features characteristic of west Belfast where both Clonard and Andersonstown were located were also characteristic of the mid-Ulster dialect spoken in Lurgan (as opposed to the Ulster-Scots dialect of Belfast's northern and eastern hinterland). Hence, it seemed likely that information on similarities and differences between the five Belfast communities on the one hand and Lurgan on the other could be used for a variety of theoretical purposes.

The participant-observation techniques in this study were similar to those used in the urban community studies. The "network" method was adopted as a general principle, and several Belfast University students who were Lurgan residents were located as initial contacts. On a number of occasions, the fieldworker arranged for subjects to record themselves and their friends, with excellent results. Altogether, 28 speakers were recorded, 16 men and 12 women. As in the community studies, they fell into two generational cohorts.

Altogether, the six studies that comprised the Belfast project – five from Belfast and one from Lurgan – allowed language variation to be analyzed on the following dimensions: rural versus urban; high-status versus low-status (relatively speaking); east of the city versus west of the city. In addition, the social network approach adopted in the fieldwork proved to be a powerful analytical construct (see further chapter 5).

3.6 Research Ethics

We conclude our discussion of data collection by outlining a range of problems that may be characterized as *ethical*. Some consideration of ethical issues is an important part of any discussion of sociolinguistic fieldwork methods. Our treatment here is necessarily brief, though fuller discussions of many of the issues we outline here can be found elsewhere (e.g., Johnstone 2000b; Neuman 1997; Hammersley and Atkinson 1995).

3.6.1 Informed consent

Most universities and other research institutions have established guidelines for investigators working in any field that utilizes human subjects for research. In our experience, these guidelines are taken very seriously by the institutions since violations can damage much more than reputations. In the US, for example, a violation of federal policies by a single researcher can result in a suspension of funding for all research across the institution. To protect against such problems, institutions typically require investigators to submit research proposals for internal review by an Institutional Review Board (IRB). The human subjects concerns that sociolinguistic research raises are generally much milder than those stemming from, for example, medical research. There are, however, significant issues to be addressed, and we recommend that researchers go through the proper channels since the stakes are so high. The need to gain IRB approval before any data collection can begin necessitates careful planning on the part of the researcher. Fortunately, the procedures, at least in the US, allow for expedited review of studies involving minimal risk to subjects, as is typically the case with linguistic research.

One of the fundamental elements of ethical research using human subjects is the principle of informed consent. Subjects must voluntarily agree to participate in the research and must know what their participation entails. Informed consent is often obtained by having subjects sign written statements, and research involving minor children may require the consent of parents or guardians. The written statements typically contain the following: (a) a short description of the project including its objectives; (b) a description of the procedures used, detailing what the subject can expect and any risks that might be involved; (c) an assurance that the subject will remain anonymous and that all information will be confidential; (d) a confirmation that the subject's participation is voluntary and that he or she can withdraw from the study at any time; and (e) information for contacting the investigators and the sponsoring institution's review board (Neuman

1997: 450). Sample consent forms used for sociolinguistic research are reprinted in Johnstone (2000b: 44–7).

Investigators attempting to record casual speech may be concerned about the potential impact of presenting a formal written document to their subjects. The informed consent statement is an explicit reminder that the investigator's purpose is linguistic research, and thus it may counteract efforts to overcome the observer's paradox. Fortunately, written consent may not be required in every case; it may be acceptable to obtain consent verbally as long as subjects are properly informed in accordance with those elements described in the previous paragraph. Of course, there are some projects for which no informed consent is required. This is likely to be the case for analyses of public language such as published written works or mass-media broadcasts. Also, research that gathers data anonymously through written surveys is typically exempted from the need to obtain informed consent, but investigators usually need to complete the IRB review process to verify that their project is exempt.

The obligation to inform subjects about the nature of the research may also concern sociolinguists. If subjects know that the investigator is studying the use of, for example, multiple negation, /h/-dropping, or some other socially marked feature, this knowledge could easily affect their usage of that feature. Fortunately, statements of informed consent need not be so detailed, and it usually suffices to describe the research as a study of language – though even this general label may put speakers on guard about their usage. For this reason, the investigator may frame the research in even more general terms as examining social changes or life in the community (see Belfast example section 3.5.1). In this context, language can be mentioned as one of many aspects of the study – an accurate description in view of the role that social information plays in sociolinguistic analysis.

3.6.2 Preservation of anonymity and access to recordings

Informed consent is typically given with the understanding that the information provided by participants will remain confidential and that their identities will remain known only to the researcher. Reporting only group data is one means of meeting this obligation. At times, however, it is necessary to refer to individuals. In such instances, sociolinguists may identify people by pseudonyms, initials, or numbers. Some researchers also identify neighborhoods and even towns by pseudonyms. The decision of whether to use pseudonyms for place names is based on the potential for identifying individual research subjects. Gordon (2001b) uses the real names of the two towns he studied, whereas Cukor-Avila (1997) refers to the small-town site of her research by a pseudonym. Population numbers figured into these

choices; the communities on which Gordon reports are towns of over 3,000 residents while Cukor-Avila's site has a population of less than 200. In addition, Cukor-Avila provides transcripts of extended conversations that include personal information and narratives that might facilitate identification of individuals if the location were known. As a general rule, pseudonyms should be used for locations if the researcher has any doubts about preserving the anonymity of subjects. However, sometimes the precise identification of the research site is critical to the study, or difficult to avoid in reporting on the research. In Gordon's case, for example, the research sites were chosen because of their locations relative to a major highway and certain urban areas, and these elements were crucial to the analysis. The need to describe such characteristics of the sites meant that the towns under investigation would have been readily identifiable even if pseudonyms had been adopted, and anyway individual subjects in the study were referred to only by their initials. Nevertheless, if the communities had been much smaller, Gordon would have had to take steps, including using pseudonyms for names of the towns, to avoid jeopardizing the anonymity of subjects.

Concerns about anonymity also necessitate that sociolinguists adopt a firm policy with regard to access to tape-recordings. It is common to restrict access to members of the research group or scholars who are temporarily affiliated for the purpose of carrying out a specified piece of work, and tape-recordings should by no means be freely available as a resource to all. Occasionally material from one investigation may be requested for bona fide research by another scholar or, more commonly, the original investigators may present their material, including excerpts from tapes, in public lectures. Policies regarding these matters should be specified in the informed consent procedures so that the subjects are aware of such possible uses of the material they provide. Some institutions may require researchers to obtain a separate permission for public presentation of recorded material, even in the classroom setting. In all instances where recordings are shared, the tapes involved should be carefully vetted for sensitivity of content or for material that can lead to the identification of speakers.

3.6.3 Surreptitious recording

It may appear that the easiest way of overcoming the observer's paradox is to record speakers covertly. Such deception, however, raises serious legal and ethical concerns. The legal issues are detailed by Murray and Murray (1992, 1996) who review the relevant statutes and case law for the US and Canada. As their discussion makes clear, the question of the legality of surreptitious recording is quite complex with standards varying by jurisdiction. It appears, however, that there are several circumstances in which this

technique is legal. For example, in some states the law allows for conversations to be recorded when only one party is aware of the recording. Thus, a professor could covertly record conversations held in his or her office with students. Other jurisdictions are more restrictive and would not allow for surreptitious recording even in public venues such as parks and street corners (Murray and Murray 1992: 34–45).

For many researchers the fact that surreptitious recording may be legal does not suffice to license its use; they view this deceptive technique as a violation of ethical principles. Labov has been a particularly strong opponent. During his term as president of the Linguistic Society of America, he established an ethics committee that condemned the practice (see Shuy 1993). Nevertheless, linguists not working within Labov's general framework seem less troubled than variationists are by covert recording. For example, Dixon (1984: 80) quite openly states that some interesting material was obtained from Aboriginal speakers by candid recording. Similarly, Harvey (1992) was interested in certain features of "drunken speech" in the Peruvian community she studied. She decided that the only way to gather the data she needed was by surreptitiously recording drinking sessions. Such techniques appear to be less common among researchers working in Western, industrialized societies though Murray (1986) stands as an exception. For a study of St. Louis, Murray used a concealed tape-recorder to gather data from speakers in a variety of public venues, including singles bars, supermarkets, laundromats, churches, and funeral parlors. In some cases, the deception of these speakers was taken further when Murray inquired about their age and whether they were native to the city. Understandably, the informants became suspicious, but Murray pacified them by claiming to be either a government employee who was gathering demographic statistics or a marketing consultant for the business they were patronizing or "a dear friend of someone who bore [the subject] an amazing resemblance, but who lived in another state and was a few years younger or older and on the brink of death" (1986: 8). Fortunately for the reputation of the field, such outright deception of research subjects marks Murray's study as exceptional.

The ethical dilemma associated with covert recording may be lessened if speakers are informed after the fact. It is acceptable in other social scientific fields, especially psychology, for investigators to deceive subjects initially about the nature of the research provided that they are debriefed afterward and given the opportunity to withdraw themselves from the study. Such an approach was taken by Crystal and Davy (1969) who tape-recorded colleagues, friends, and family surreptitiously and subsequently requested permission to use the material. It is important to note, however, that Crystal and Davy are focusing on the language of a few speakers well known to them whereas variationist methods are designed to have more general application to communities not known to the investigator. In the former case the nature of the personal ties between investigator and subject seems to make candid recording a less pressing ethical issue.

Some *practical* disadvantages of candid recording have been noted by Labov (1984). First of all, it can jeopardize the researcher's relationship to the community, and access to subjects is likely to be hindered if people think they are being spied on. Secondly, the quality of recordings is likely to be poor. Imagine, for example, the sound quality of Harvey's tapes that were made by "hiding a Walkman-sized recorder in a pocket or bag" (1992: 78). Shuy (1993) describes a much more elaborate set up used by researchers at the Center for Applied Linguistics in the 1960s. They converted a house into a recreation center and installed microphones in the chandeliers in order to record the speech of the children who came to play there. The technique failed because "[t]he naturalistic conditions produced a great deal of inaudible shouts along with the sounds of ping-pong balls hitting various surfaces, toilets interminably flushing, doors slamming, and unidentifiable fragments of human speech" (1993: 105).

Although we personally endorse Labov's views on the matter of candid recording, it has to be admitted that the issues are not always as clear-cut as they seem. Particularly during successful long-term participant observation, the borderline between overt and covert recording can become blurred and quite difficult problems emerge.

During the community studies carried out in Belfast, for example – and no doubt this experience is quite general – conversations were often interrupted by several people entering the room. Indeed, sometimes the original participants would leave in the course of a long recording session. Although the recording equipment was not concealed, and was monitored quite openly by the fieldworker, it was not always clear whether all participants were aware that they were being tape-recorded. In such situations it was not the researchers' practice to interrupt proceedings in order to renegotiate permission to record; in Belfast such permission was sought at the first contact with each household. There was, moreover, a standing agreement that the equipment would never be concealed and that before leaving the house the fieldworker would erase from the tape any material that subjects or fieldworkers considered sensitive. In fact, more often than not the fieldworker took the initiative in this matter; investigators who build up long-term relationships with communities frequently hear and record material about which they would prefer to remain ignorant.

As the Belfast example indicates, even researchers who reject the intentional use of surreptitious recording such as practiced by Harvey or Murray may face ethical dilemmas related to this issue. While there remains some disagreement on the ethics of candid recording, Labov's general principle seems to offer a good guideline: the researcher should "avoid any act that would be embarrassing to explain if it became a public issue" (Labov 1984: 52).

3.6.4 *The researcher's responsibilities to the community*

Treating research subjects with respect by obtaining informed consent, protecting their anonymity, and so forth, is one dimension of the investigator's responsibility to the community being studied. Still, many linguists have argued that it is not enough simply to do no harm and that we should work to give something back to the communities we investigate. Cameron and colleagues distinguish "ethical research" in which the concern is "to minimise damage and offset inconvenience to the researched" (1992: 14) from "advocacy research" which is conducted not only *on* but also *for* the subjects (1992: 15). The basic idea is that research should benefit the community as well as the investigator.

Labov has been a strong proponent of this advocacy position. In his view, linguists are motivated to take social action by their commitment to certain basic principles:

> *The Principle of Error Correction*
> A scientist who becomes aware of a widespread idea or social practice with important consequences that is invalidated by his own data is obligated to bring this error to the attention of the widest possible audience. (Labov 1982b: 172)
>
> *The Principle of the Debt Incurred*
> An investigator who has obtained linguistic data from members of a speech community has an obligation to use the knowledge based on that data for benefit of the community, when it has need of it. (Labov 1982b: 173)

The principle of error correction motivated linguists in the 1960s to speak out against widespread belief among educators and language pathologists that vernacular speech features were evidence of language deficit. Labov's influential paper "The logic of nonstandard English" (reprinted in Labov 1972a) illustrates these efforts which succeeded in "pushing the definition of linguistic normalcy toward a dialectally-sensitive one" (Wolfram 1993a: 226).

The call for linguists to respond to community needs raises the question of what we have to offer. How can linguists' specialized knowledge be applied to benefit the community? The areas most relevant to sociolinguistic research might include language testing especially as it relates to speakers of non-standard dialects, language policy in multilingual societies, and efforts to preserve moribund languages and dialects (e.g., Nagy 2000; Wolfram and Schilling-Estes 1995). However, by far the greatest attention from sociolinguists seeking to repay some of their incurred debt has been given to issues in education.

Labov (1982b) discusses the "Ann Arbor trial" as an example of the contributions that sociolinguists can make to educational issues. This case

involves a 1977 lawsuit that was brought against school officials in Ann Arbor, Michigan, by the parents of several African American children. The parents alleged that the school system had failed "to take into account the cultural, social, and economic factors that would prevent [the students] from making normal progress in the school" (1982: 168). Language took on a central role in the case as the plaintiffs argued that the children's dialect, a variety of African American Vernacular English (AAVE), posed a barrier to their educational success that the schools had not taken adequate action to overcome. These arguments gained support from the evidence of Geneva Smitherman and other linguists who testified about the structure and history of AAVE. In the end the plaintiffs prevailed, and the judge's ruling led the school district to conduct workshops to raise the teachers' awareness of the nature of sociolinguistic variation (Wolfram and Schilling-Estes 1998: 264).

Many of the issues involved in the Ann Arbor trial were revisited nearly two decades later when the school district in Oakland, California, presented a resolution on Ebonics, the term they chose to refer to the dialect of their urban African American students. A storm of controversy arose surrounding this action due in part to misrepresentations of the district's intent by the media as well as to some problematic phrasing in the resolution itself (see Wolfram 1998a). As they had in Ann Arbor, linguists spoke out about the nature of AAVE and generally offered support for the Oakland proposal. The members of the Linguistic Society of America made clear their support in a resolution that was passed at the annual meeting in 1997. Several linguists stated their case in letters and articles in newspapers and other mass media forums (see especially Rickford, e.g., 1997a). Eventually, the US Senate convened a hearing on the issue during which Labov and other linguists gave testimony. Unlike the Ann Arbor case, there was no definitive resolution of the "Ebonics controversy." Over time the media, and apparently the politicians, simply lost interest in the issue.

The examples discussed so far are cases in which linguists stepped forward to offer their assistance in reaction to situations of perceived need as required by the principle of debt incurred. However, Wolfram has argued that our responsibilities extend beyond such "reactive advocacy" and that researchers should take the initiative in serving the communities they investigate (1993a, 1998b). Following Labov's model, he formulates a principle of linguistic gratuity:

> Investigators who have obtained linguistic data from members of a speech community should actively pursue positive ways in which they can return linguistic favors to the community. (Wolfram 1993a: 227)

Guided by this principle, Wolfram and his research team have developed a series of language awareness programs that are designed to communicate

basic facts about regional and social dialects and, ultimately, to promote greater tolerance and even appreciation of linguistic diversity. Schools offer a useful venue for such programs. Wolfram (1993a) discusses a language awareness program piloted in the schools in Baltimore, Maryland. The curriculum for this program includes a series of exercises that systematically guide students through the discovery of phonological and grammatical patterns in varieties such as AAVE. In this way, the program exposes children to "a type of scientific inquiry into language that is generally untapped in the students' present instruction about language" (1993a: 230). In addition to this intellectual benefit, such programs may offer some measure of emotional benefit. They may promote respect for non-standard varieties by demonstrating that they are as regular as the standard language. Moreover, by exploring the sociohistorical context that gave rise to current varieties, they can encourage students to be proud of their linguistic heritage.

Wolfram's work on Ocracoke, an island community off the coast of North Carolina, provides several models for language awareness programs outside the context of the schools. Because many of the features of the local dialect appear to be fading from use, much of the work on Ocracoke has had a preservationist flavor. For example, Wolfram and his colleagues established a permanent exhibit on the dialect at the local historical museum. They also produced a video documentary describing the dialect and published a book for the general readers (see Wolfram 1998b: 272 for citations). Profits from the sale of these items, as well as from the sale of the dialect-themed T-shirts they designed, are shared with the Ocracoke Preservation Society. As such examples indicate, linguists can be creative in pursuing avenues for returning "linguistic favors" to the community.

The discussion thus far has focused on the positive value of initiatives undertaken in accordance with the principle of linguistic gratuity. In most cases, these efforts seem fairly uncontroversial, at least among linguists. However, it is important to bear in mind some of the problems that might arise from such efforts despite the good intentions of the researcher. Wolfram (1993a, 1998b) details several ethical considerations related to initiatives like his language awareness programs. There is, for example, an element of paternalism involved in linguists performing what they consider to be favors in areas where they perceive a need rather than "responding to the explicitly stated needs of a community" (1993a: 233). In this regard, Wolfram cautions linguists to be aware of their own sociopolitical agendas and how they might influence their work, a concern that may be particularly acute when promoting language awareness programs in the schools. He also raises issues related to the representation of non-standard varieties. The need to describe a dialect in terms that a non-specialist audience can understand may lead to oversimplification of the sociolinguistic facts. Our descriptions tend to include a list of characteristic features that distinguish the dialect from other varieties,

including the standard. Most audiences are not used to thinking about language as variable and thus might assume the use of dialect features to be categorical. The picture that emerges, then, is of a variety more vernacular, more basilectal than any dialect actually is.[3] Nevertheless, while the potential for entering such ethically problematic territory is real, this discussion is not intended to discourage researchers from seeking ways of benefiting the communities they study. The bottom line is that linguists need to be sensitive to community needs and tread lightly as they work to meet those needs.

3.7 Concluding Remarks

These first chapters have attempted to provide a critical overview of variationist approaches to research design and data collection. One conclusion emerging from this discussion is that a wider variety of approaches is being practiced today than ever before in the field's nearly four-decade history. As the field expands, so too do the choices faced by investigators. Thus, while our focus has largely been methodological, we have stressed the need for decisions to be grounded in a defensible framework. In the following chapters theoretical issues become even more central, as our emphasis shifts from largely practical concerns to matters of interpretation.