ETHICS, ADVOCACY AND EMPOWERMENT: ISSUES OF METHOD IN RESEARCHING LANGUAGE

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Researching Language, the book-length study on which the following discussion is based, deals with questions about power and method in a range of social science disciplines (anthropology, sociology and sociolinguistics). To put 'power' and 'method' together in such an explicit way, and to foreground them as major concerns, is perhaps an unconventional move. Yet any social researcher who has undertaken fieldwork must at some level be aware that power relations exist in this context as in others; and those power relations are strongly affected by the methods we are constrained to adopt in 'doing research'. That is, they are not entirely determined by pre-existing differences of status imported from other contexts. Something happens within the process of research itself.

Typically, research produces or intensifies an unequal relationship between investigator and informants: authority and control lie with the investigator more often than with the informants, and the whole process benefits the investigator much more than the informants. We want to pose the question, why is this so? What assumptions and practices within social science make it so? Is it inevitable, or can we adopt different assumptions and procedures to produce a different outcome?

Our discussion focuses on linguistic research, though we define that category quite broadly. The disciplines we represent, anthropology and sociology as well as linguistics, are not all concerned with language in the same way or for the same reasons, but they are all, necessarily, concerned with it. For linguistics the point is obvious; but a study of people's religious beliefs or voting intentions must equally be approached by way of their language, through what they tell you, whether the method used is a questionnaire, a highly structured 20 minute interview or several years of participant observation. There is nothing controversial in saying this; what is more controversial is the status of language in such investigations. Social scientists have often regarded language as a neutral medium, a window on social reality; so that when someone tells the investigator 'I plan to vote Labour', this is taken as a direct representation of a reality existing outside the language used to describe it. But as many other contemporary social theorists have pointed out, this view is oversimplified. Language is not a neutral medium but itself a social contruct; it is partly constitutive of social reality. Therefore, social researchers need to take language *qua* language seriously. In this sense—and whether or not it is made explicit—virtually all social research involves researching language.

The research projects we ourselves have carried out, and to which we will refer later on in this discussion, exemplify 'researching language' in both senses; all of us were

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interested in some aspect of people's talk, and all of us used talking as the means of finding out about it. At this point, it is helpful briefly to sketch the projects:

(a) Ben Rampton undertook a sociolinguistic study of adolescent boys in a multiracial peer group, drawing on variationist sociolinguistics and the ethnography of communication. He looked at the use and distribution of syntactic and phonological variables and at their social significance for speakers and educators.

(b) Penelope Harvey undertook an anthropological study of language use among bilinguals in the Peruvian Andes. She examined the role of language in constructing and maintaining social hierarchies within the peasant culture of a post-colonial state.

(c) Elizabeth Frazer's was a qualitative sociological project addressing the construction of gender, race and class identities among British teenage girls from different socioeconomic backgrounds. She was especially concerned with the way the girls' talk about themselves related to their experience of themselves.

(d) Deborah Cameron, a sociolinguist, investigated issues of language and racism with members of a mainly Black youth club in south London, eventually collaborating with them to produce a video on the topic.

These projects will be used in order to illustrate our concerns about power and method in social research. We begin, however, by returning to the more general questions posed above; what assumptions and practices in social science influence relationships between researchers and their subjects when the former go into the 'field' to observe the latter?

Positioning researcher and researched: ethics, advocacy and empowerment

In this section we will identify three frameworks for conceptualizing relations between researchers and subjects: the ethical, the advocacy and the empowerment frameworks. Most social research is conducted within the assumptions of the first framework, that of ethics; some is conducted within the assumptions of the second, advocacy. We are most concerned with the possibilities offered by the third, empowerment.

Ethics

All social researchers are expected to take seriously the ethical questions their activities raise. These questions are discussed during postgraduate training, addressed in the guidelines and codes produced by professional bodies, and posed concretely when ethics committees scrutinize particular research proposals. All this institutional activity testifies to a high level of concern about researchers' responsibility to the people they do research on. The nature and limits of that responsibility may be framed, however, in a number of different ways.

Standard frameworks for discussing what is 'ethical' are fairly narrowly conceived. The question they address is how to strike an acceptable balance between potentially conflicting sets of interests. The researcher has an interest in finding out as much as possible; but it may not be in the interests of research subjects to provide information without limits and conditions. Ethical guidelines set out to make clear what the limits and conditions are.

Within such a framework, certain practices are obviously unethical: coercing subjects to participate or neglecting to get informed consent from them; exploiting or abusing them in the course of research; violating their privacy or breaching confidentiality. On the other hand, it is not considered unethical for the researcher to protect her own interests in various ways. She is permitted, for example, to be less than candid about the ultimate purpose

of her research. Many research designs require that the investigator conceal her goals; if you tell people you want them to talk so you can measure the frequency with which they pause, say, this may affect their behaviour and so vitiate your results. To avoid this problem, you tell them nothing, or invent some plausible alternative rationale.

In sociolinguistic research, this type of problem has of course been agonized over under the heading of 'the observer's paradox' (linguists want to observe how people use language when they are not being observed). Some classical discussions of elicitation technique (e.g. Labov, 1972a) have advised the researcher to minimize the problem by using petty deceptions (like leaving the tape recorder running when the informant thinks you have already switched it off). Typically, the question of whether such a proceeding is ethical is treated as a matter of 'balance'; by tacit consent, some deceptions are innocuous, advancing the interests of the researcher without seriously threatening those of the informant. It is worth noting, though, that the judgement of what constitutes an innocuous deception and what would be an unethical one is left to the researcher. The 'ethical' model of relations between researcher and researched is an asymmetrical one in which the researched play a passive role; the legitimate, knowledge-seeking objectives of researchers can be pursued by any means that do not infringe the fundamental rights of informants.

The underlying conception in the 'ethical' model is one of *research on* social subjects. Because these subjects are human beings, they are entitled to special ethical consideration. But the consideration goes only so far, and the researchers rather than the researched decide its limits (e.g. in judging what counts as 'innocuous deception'). Human subjects no more set the social researcher's agenda than a bottle of sulphuric acid sets the chemist's agenda. Nor is it necessarily assumed that social research should produce any positive benefit to the subjects who partipate. If it does, this is seen as a bonus; but if it does not, so long as no actual harm is done, it can still be accepted as ethical.

Advocacy

For many social researchers, this ethical model is necessary but not sufficient. Over and above the obligation not to harm informants, researchers often feel a more positive desire to help them. This feeling may be present from the outset, perhaps as a political commitment that has guided the researcher in choosing to work on a particular project. Or it may develop later on, as the researcher forms more complex human relationships with her informants. It is not uncommon for informants themselves to ask researchers for advice, support and help. People are aware that the knowledge, expertise and status of academic social scientists may prove helpful in campaigns for better conditions, and they may ask a researcher to participate in such campaigns, perhaps by acting or speaking publicly on the community's behalf. If the option is taken up, it places the researcher in the position of an *advocate*, engaging in research not only *on* social subjects, as in the ethical framework, but also *for* them.

In the last 15 years there has been a classic case in sociolinguistics of research done within the advocacy framework—that is, research *on* and *for* social subjects: the 'Black English' trial in Ann Arbor, Michigan. Here, a lawsuit brought against the school system by a group of African-American parents came to turn on questions about the community's linguistic variety, American Vernacular Black English (AVBE). The plaintiffs argued that their children were being disadvantaged educationally—for instance, wrongly identified as having 'learning disabilities'—by the schools' failure to make provision for the systemic differences

between AVBE and Standard English (SE). Since AVBE is a variety used exclusively by African-Americans, failure to take account of its speakers' needs would constitute racial discrimination. For this argument to succeed, it had to be shown that AVBE was indeed a distinctive variety, highly divergent from SE, specific to African-American communities and reflecting their history of slavery and segregation. In making this case, the advocacy of professional linguists who had studied AVBE was crucial. A number of linguists provided advice, support and eventually expert testimony.

Among those who testified was the sociolinguist William Labov. He subsequently wrote an account of the case whose main title was 'Objectivity and commitment in linguistic science', and this contains a powerful argument about the social responsibilities of sociolinguistic researchers (Labov, 1982). Two principles are laid down for researchers; 'the principle of error correction' (if people believe false and damaging propositions, e.g. 'AVBE' is bad English', or 'AVBE' is hardly any different from SE', researchers who know better are obliged to try and correct the error); and 'the principle of the debt incurred' (if a community has helped researchers by providing access and information, researchers have a corresponding duty to use their knowledge and expertise for the benefit of the community). This amounts to an argument that advocacy—research *for* as well as *on* social subjects—is not just an optional extra, a bonus researchers may look for or not, as they decide; in the right circumstances it is an obligation.

It should be noted that while Labov speaks powerfully in favour of 'commitment', he is equally concerned with 'objectivity'. This, in fact, is a significant concern which the ethical and advocacy frameworks have in common. Both assume that the first duty of researchers is to pursue the objective truth—a concept which is taken to be unproblematic. Presumably it is this assumption of the overriding claims of truth which legitimates certain 'innocuous deceptions' within the ethical framework; presumably, too, someone like Labov might refuse to act as an advocate if it could not be done without compromising the truth. Most of Labov's paper is devoted to showing that in the Ann Arbor case, objectivity and commitment actually *reinforced one another.* For example, he notes that a number of African-American linguists had studied AVBE at least partly out of political commitment; and he argues that the distinctive knowledge and experience they brought to the field resolved a number of disputes and problems, thus producing a more objective and truthful account. In addition, the stringent requirements of preparing a court case sharpened committed linguists' arguments, obliging them to seek the truth all the more assiduously.

This line of argument takes issue with the more familiar idea that commitment must necessarily threaten a researcher's objectivity. In disciplines which accept a positivist philosophy of science (as quantitative sociolinguistics generally does[1]) there is a distinction made between fact and value, and it is seen as important to keep scientific observations from being tainted by value-judgements. Someone who begins from a partisan standpoint cannot observe objectively; and the problem is compounded if researchers permit their opinions to obtrude in a way that might affect the behaviour being observed. (Hence the kinds of instructions traditionally given to interviewers and discussed by the sociologist Ann Oakley (1981); one handbook advised interviewers to answer informants' questions by saying. 'Well right now your opinions are more important than mine'.) Labov does challenge this notion that commitment is totally incompatible with objectivity, but at the same time he appears to accept the positivist fact/value distinction, and the challenge is therefore a limited one. 'Commitment' for Labov seems to lie in the passion with which

a researcher pursues the facts, and in what she does with them once she has established them. It does not seem to enter into the processes whereby researchers construct 'facts'— the design of a project, the field methods, the analysis. These are domains where objectivity must prevail.

Problems with advocacy

This is really where our argument begins. Like many social researchers, including Labov, each of us had sought ways of doing research that would advance our political goals— greater freedom, equality and justice—as well as our intellectual ones. Like Labov we had felt a necessity to go beyond the ethical framework, altering the balance of power between academic researchers and research subjects. Yet the advocacy framework exemplified by Labov's 'Objectivity and commitment' paper also seemed inadequate for two distinct but connected reasons.

The first was that advocacy, as practised by linguists involved in the Ann Arbor case, seemed to beg the following question. If researchers are under an obligation to use their knowledge on behalf of informants, why not go a step further and argue that they should make the knowledge *available to informants* for them to use on their own behalf? For experts to act as advocates is doubtless important (and in present conditions, often vital; we are not suggesting Labov should have refused to testify). But, surely, it would be a positive development if people had the knowledge and skills to act for themselves.

This might seem like an outlandish suggestion, equivalent to saying that everyone should become an academic linguist (and perhaps also, by analogy, a laywer, a psychologist, a forensic scientist . . .). Such an extreme proposal is neither necessary, nor necessarily desirable. But consider the implications of a situation where *only* the expert advocate has access to specialist knowledge about a community's language variety. The expert in such a situation retains some very significant powers.

For example, Labov argues that when linguists act as advocates they serve the community and must bow to the political will of the community. Yet he also acknowledges that the 'will of the community' can be hard for an outsider to locate. This is highly relevant to the current politics of AVBE. Although the community involved in bringing the Ann Arbor case seem to have been in agreement about their interests, the wider community of AVBE speakers in the United States certainly is not.[2] Labov did not speak for all African-Americans; he made, in effect, a choice about whose interests he would support. Clearly, it is inevitable that communities will contain a diversity of interests. But if members of those communities do not possess the information needed to engage in internal debate, there is a danger that external advocates will end up making their choices for them.

The matter of intra-community diversity is problematic in another way in Labov's discussion. He emphasizes the contribution of African-American linguists, implying that if researcher and researched are from the same social group, this automatically reduces the potential for conflicts between them. It may well be true that African-American researchers are regarded by African-American communities as less likely to pose a threat than White researchers; but it is questionable whether a shared racial origin reduces the asymmetry of researcher and researched to the point of insignificance. On many criteria, an African-American researcher is likely to be (and be seen as) an outsider to the community (she may differ from most community members in terms of education, occupation, income, residence). But even beyond such crude measures of likeness and difference, attention has to be paid to the complicated, specific ways in which any research process positions those

who take part in it. If the goal is to alter the balance of power betweeen researcher and researched, it is not enough to make changes in personnel. There have to be changes in the *process* of research and the social relationships it typically involves, with informants being treated by researchers as 'objects of study', and not as co-participants in a form of social interaction.

One major obstacle to changing the processes and relationships of research is the stringent set of methodological requirements imposed by positivism. And this is the second source of our dissatisfaction with the advocacy framework; it stops well short of a critique of positivist research methods in social science. Arguably, it is positivism more than anything else that prevents many researchers going beyond advocacy. They fear that if they make drastic changes in the relationship they have with subjects, their research will no longer be 'valid'.

We think these anxieties are misplaced, since positivist epistemologies are open to serious criticisms on various grounds. But in the light of the foregoing argument about the politics of social research, their most immediate drawback is that they lead almost inevitably to the objectification of informants by researchers. If, as we would claim, people are not objects and should not be treated like objects, this surely entitles them to more than just respectful (ethical) treatment. It means that researcher and researched should interact; researchers should not try to pretend that their subjects can be studied as if the former were outside the social universe that included the latter.

This is the point at which the political critique of objectification connects to a broader epistemological critique of positivism (a philosophy which favours or even prescribes objectification). Positivists would question whether you can avoid objectification and still do good research. We believe the answer is yes: the claims made for positivist methods are overstated, while non-positivist methods can produce research that is valid and insightful. This argument between positivism and anti-positivism may be pursued with reference to the research tradition of sociolinguistics.

As noted above, variationist sociolinguistics has a longstanding anxiety about the observer's paradox. This rests upon the idea that a researcher's presence (and with a tape recorder, to make things worse) is enough to render the environment hostile for the informant, and to produce a form of linguistic camouflage. Assuming that the researcher and her informants do not share the same linguistic background, it is important for the researcher to minimize her own effect on the speech of the researched.

Behind the various strategies suggested for doing this (e.g. the 'innocuous' deceptions mentioned above, the deflection of informants' questions advised by traditional handbooks, etc.) lies the positivist assumption that there is reality independent of the observer's perception, and that *this* is what all science, natural and social, must aspire to discover. From that perspective, interaction between researcher and researched appears as a source of interference or contamination—hence the need to minimize it or even avoid it altogether.

But critics of positivism find this view naïve. The perspective from which we criticize positivists is one which regards all human behaviour as social and interactive by definition. This is not to deny there is a difference between what people do and what they say, between their behaviour and their accounts of their behaviour. But human meanings—for positivists a realm of the subjective—are for us at least partly constitutive of what a given reality *is*. In other words, a researcher who observes some form of behaviour may properly be

interested not only in what the actor appears to be doing, but what the actor herself thinks she is doing. The woman turning a spade in the earth, for instance, may be gardening or preparing to bury a family pet; furthermore, she may be undertaking either task for a variety of reasons which are part of its meaning, and which may only become apparent to an observer it there is interaction between observer and observed.

Returning then to the case of sociolinguistic observation, we can argue along the following lines. If all human behaviour is social behaviour, then interaction between researcher and researched does not produce some anomalous form of communication peculiar to the research situation and misleading as to the nature of 'reality'. Rather such interaction instantiates *normal* communication in one of its forms.

Our own fieldwork convinced us that this is a more insightful way to look at the issue. Talking to us as researchers, informants drew upon their linguistic repertoires as these had been developed in talking to parents, teachers, employers—significant others of various kinds. The roles of 'researcher' and 'informant' are best seen not as pre-given identities which individuals adopt when the situation requires, but as context-dependent identities, negotiated between researcher and researched as part of the process of establishing social relations. The precise content of the 'researcher' role may vary from case to case. On both sides, previous experience informs the way roles are negotiated. So data collected in research situations should not be regarded simply as 'contaminated', a distorted or degenerate version of 'real' interaction; such data provide important insights into the way social relations and identities are constructed through interaction.

Positivists, then, are mistaken in their belief that there is some pristine social reality 'out there' waiting to be discovered by an investigator who is herself neutral and detached from it. And if positivists are thus mistaken, the problem of validity loses its centrality; the scope for introducing a very different kind of research is dramatically widened. If one admits the epistemological and political argument that researchers should interact with the researched instead of trying to remain aloof from them, it becomes possible to do research not just *on* social subjects or *for* social subjects, but also *with* social subjects. The *with* here implies the use of interactive methods, but it also raises two other, connected possibilities. The first is that informants themselves might play a greater role in setting agendas for research than positivist frameworks permit. The second, which goes back to our reservations about Labovian advocacy, is that the knowledge researchers bring to a project or jointly with informants produce in the course of one, might be shared more explicitly with the researched, in an effort to give them a greater measure of control. It is possibilities like these which lead us to label the framework proposed here—research done *on, for* and *with* social subjects—*empowering research*.

Empowerment

It is important to point out that our own focused reflections on 'empowering research' began *after* we had carried out empirical work in situations of evident social inequality. With varying degrees of self-consciousness, we had departed from traditional positivist research methods and introduced into our projects the kinds of concerns we would subsequently relate to the framework of 'empowerment'; the use of interactive methods, the acknowledgement of subjects' own agendas and the sharing of expert knowledge. When we began our discussions, two things quickly emerged. One was that the particular projects we had done would not serve in any simple way as models for empowering research in

general. Like most researchers we had designed those projects to address substantive issues, and not to test particular methods. We therefore treated them as case studies rather than recipes, to be discussed and criticized in the light of the theoretical framework we elaborated after finishing them. How 'empowering' were these projects? What problems did they raise?

The second thing that became clear as we reflected on these questions was this: not only were the projects inadequate as models, the whole notion of a model for empowering research was problematic. The argument we have put forward, analysing social research paradigms in terms of ethics, advocacy and empowerment, is in important ways over simple. Committed though we are to a critique of positivism, we are also aware of a great many crucial questions surrounding the idea of empowerment and complicating any attempt to practise 'empowering research'. Where do we locate 'power'? What are the boundaries of 'research'? Who can define 'subjects' own agendas'? What is the 'knowledge' we propose to share, and how can it be shared?

Every one of the terms we have placed in scare-quotes acquires its meaning within the same set of complex and shifting social relationships we have already referred to in criticizing traditional research methods. If we are not to be as reductive as those we criticize, we cannot produce a *single* account of what would be empowering in every research context. Rather we must point to the kinds of problems a researcher faces whose goal is empowerment, and the questions to which she must pay close attention. In trying to locate various key terms—'power', 'research' and 'knowledge', for example—we will illustrate the problems and questions at issue with reference to our own case studies.

Problems of empowerment

Locating power

An economy of power? Both common-sense discourse and traditional philosophical discussion have a tendency to treat 'power' as the sort of thing individuals and groups can have more or less of. This economic metaphor suggests a fairly simple definition of 'empowerment'; a redistribution which takes power away from some people (the powerful) and gives it others (the powerless). Our own concern about the balance of power between researcher and researched could be addressed in these terms.

But the model of power presupposed here raises a great many problems. The idea of seizing and redistributing power works best if power is taken as a monolith, something with a single point of origin, like ownership of the means of production in classical Marxism, or 'the barrel of a gun' for Maoists. In recent years such monolithic models have been justifiably criticized. There are many simultaneous dimensions of power, interacting with each other in complex ways. It is reductive and inadequate to take one dimension as prior to or more important than all the others—to privilege, say, relations of class over relations of gender and race. Social identity is a fragmented and multiple phenomenon, since social subjects are positioned in many sets of relations, not just one; sometimes the relations are contradictory.

This consideration proved relevant to our case studies. For instance, Elizabeth Frazer studied several groups of girls, among them an upper-class group of public schoolgirls. The privilege these girls enjoyed on one dimension, class, was part of their identity, but not experientially separable from the oppressive gender relations they were also positioned in, and which equally shaped their identity. Privilege and oppression can co-exist; a group

of people can be both oppressing and oppressed. It is therefore difficult in principle and practice to locate groups of unequivocally 'powerful' and 'powerless' people. And it follows that attempts at 'empowerment' cannot be uncritical; it is not just a matter of giving people 'more power', but of recognizing that every group in a community is itself an arena for conflict and struggle.

Power/knowledge. The work of Michel Foucault (cf. Foucault, 1980) represents a major shift away from economic metaphors of more power and less power. It is therefore of interest to us, but it raises another difficulty, this time about our own position as social researchers. Much of Foucault's work derives from the insight that citizens of modern democracies are controlled less by straightforward violence or economic exploitation than by the pronouncements of expert discourse, organized in 'regimes of truth'—sets of understandings which legitimate certain social attitudes and practices. Programmes of social scientific research on such subjects as 'criminality' or 'sexual deviance' or 'teenage motherhood' organize what we 'know' about certain groups of people—'criminals', 'deviants', 'teenage mothers'—and contribute to their becoming targets for social control, as well as helping to shape the forms such control will take. (Of course, Foucault also notes that regimes of truth give rise to discourses of resistance which may become powerful in their turn. The process of 'power/knowledge' which brings into existence 'the criminal classes' also brings into existence the threat they represent to bourgeois society; the classification of certain individuals as 'homosexuals' exposes those people to social control, but also gives them a definite identity which they may use to organize for 'gay rights'.)

What are the implications of this analysis for the project of empowering research? Social science is a major contributor to oppressive regimes of truth; perhaps, then, empowering research is a contradiction in terms. There is certainly no denying the non-neutrality of social research, historically and currently; it is strongly implicated in social control. An enormous proportion of social scientific studies focus on relatively powerless people: factory workers, criminals, juvenile delinquents, *not* bosses and directors, judges and jailers. This is not coincidental, and our own case studies (for instance, the fact that three of them involved White researchers working in non-White communities) instantiate a similar pattern. In the specific case of linguistic research, one can point to many studies that have legitimated questionable attitudes and practices. The study of non-European and creole languages contributed in the past to Western notions of 'primitive' culture; the study of working-class speech has fed into victim-blaming educational theories (though to be fair, sociolinguists have also produced a significant challenge to these); allegedly 'descriptive' enterprises like the Summer Institute of Linguistics have disrupted cultural patterns among the researched, and served colonialism by encouraging indigeneous people to sign away their lands (Mühlhäusler, 1990).

These examples constitute an important critique of social science, and any discussion of empowering research will do well to take them seriously. But returning to the idea of power as a *multiple* phenomenon, we would argue that there is usually more going on in the relationship of researcher and researched than a simple and oppressive 'us/them' opposition. It has been noted already in discussing AVBE that problems of inequality may arise even where researchers are more like 'them', as with Black researchers working in Black communities. But conversely, researchers are not always powerful in an unqualified way. Often, the researched can exert power over researchers by virtue of what they know that researchers do not.

Penelope Harvey found, for instance, that the people of Ocongate in the Peruvian Andes, with whom she lived and worked, very often positioned her not as the omniscient Western expert but as a child needing instruction in the most fundamental ways of behaving. Deborah Cameron, working on a video about language and racism with a group of Black British young people in London, also found her relationship with the researched a shifting one. Sometimes they did treat her as an expert—for example when she was telling them about the history of Caribbean creoles. At other times, however, they consciously enjoyed placing her in different, less powerful positions—when, for example, they cast her as a White racist in sketches performed for the video. In this context, 'White racist' was not a powerful role; it was imposed on Cameron rather than chosen by her, and it placed her, for the moment, outside the locally powerful norms of the group. There are dangers, then, in simply assuming that researchers invariably have absolute power and control while the researched have neither.

Power and representation. This last example—that of the language and racism video— points to a very important form of power, the power to determine how people are represented. And in most research projects, of course, this power does lie with the researcher. At the point when scholars sit down to write their book, thesis, article or report, the complex and shifting interpersonal negotiations that positioned researcher and researched during fieldwork are finally pinned down; fluid and multiple subjectivities become unified and fixed by the writer who must mediate the talk of her subjects for the readers. Furthermore, the interpretations a researcher makes in doing this must inevitably draw on information beyond what was explicitly made available or agreed to by informants.

There is no easy solution for this particular problem of inequality, but it is certainly one argument for the use of interactive methods rather than distanced, 'objective' ones. By talking to the researched and sharing with them, the researcher maximizes their opportunities for defining themselves in advance of being represented. It is also of course possible to use 'feedback' techniques—that is, to present findings to the researched in an effort to get more informed consent to what you will eventually say about them. Ben Rampton used this technique in his work with Asian schoolboys. It is not a novel technique in social science; many researchers have recommended it as a way of checking the validity of results. But while that is one of its functions, it is also a means of continuing dialogue between researcher and researched. The alternative used in Deborah Cameron's project, where the researched represented both themselves and her in a video, is clearly not feasible in every project. Elizabeth Frazer, on the other hand, combined elements of both procedures; she used some feedback techniques, and she also facilitated a further project where two groups of subjects produced their own photo-story magazine, thus representing themselves and the concerns Frazer's research had raised for them.[3]

Locating power. We have argued, then, that 'power' is more difficult to locate than it might seem. We cannot identify some prototypical powerless group to do empowering research with, and we cannot safely assume an unproblematic split between powerful researchers and powerless researched. Power has many dimensions, it is affected by local context and the positions of all involved in fieldwork are shifting and variable rather than static. The more a researcher recognizes the complexities of power that exist both among her informants and in her own relationship with them, the less easy she will find it to formulate a simple agenda for empowerment. That is not, of course, a reason to abandon

the *principle* of doing research on, for and with social subjects. But it is an argument for awareness of complexity and willingness to engage in a constant negotiation.

Locating 'research'

'Research' might seem to be a considerably less difficult notion than 'power'. But one of the problems that arises when you try to do research 'on, for and with' subjects, especially if you propose to address their agendas in addition to your own, is precisely whether it can then remain 'research', or whether it collapses into some other activity, like youth work or education or political activism.

The most familiar definition of 'research' is what might be called 'the 'Ph.D. definition'. In order to receive a Ph.D. degree, a researcher must make a substantial and original contribution to the knowledge available in a particular field. What counts as substantial and original is judged by people who have met the same criteria themselves. In other words, to enter the community of qualified researchers one must satisfy someone who belongs already that one has done something they would define as research.

There is a political critique to be made of this definition, just as there is of positivist ideas about validity and objectivity. The Ph.D. definition clearly has a 'gatekeeping' function, and potentially therefore it might work to exclude certain topics and certain ways of pursuing them from the definition of research, on the groups that some subjects and approaches are more valuable than others. But one could ask, more valuable *to whom*? It should surely be open to all who pursue knowledge to participate in shaping new research agendas and new definitions of what constitutes research. If in fact this is discouraged, that has more to do with institutional constraints like the preferences of Ph.D. examiners, journal editors and research funding bodies than with any obvious criteria of value. The value of any project is a matter for debate; and the debate is not settled by appealing to the criterion of whether certain established scholars would accept it as *bona fide* research. That just returns us to the original question: what is research and who decides?

Apart from this point about the internal politics of academe, there is also a more theoretical point to be made in reply to the critic who is worried about blurring the boundaries of 'research'. We have argued already that the researchers, like the researched, are bearers of complex and multiple social identities; the role of 'researcher' is not, in practice, a distinct entity, but draws on other social roles like teacher, youth worker, parent (and indeed child), friend, etc. If this point is accepted, there is no necessary contradiction in taking up more than one role in a research situation; on the contrary, you are always doing that anyway. What Elizabeth Frazer did, for instance, was both research and youth work; one did not vitiate or interfere with the other. We would argue that research methods chosen within the empowerment framework allow you to exploit the potential of the researcher's multiple role, instead of forcing you to deny it. This step—which is only an acknowledgement of the realities of any fieldwork context—has the further virtue of making it easier to take on the agendas of your subjects.

Taking on subjects' agendas does not imply that researchers must subordinate their own agendas. Rather we are arguing that there should be negotiation, aimed at ensuring that the project meets the needs of all involved. This could mean as little as simply making clear that asking questions is not the sole prerogative of the researcher; or as much as organizing additional activities, like Frazer's photo-story. But we would differentiate ourselves from traditions like that of 'action research', in which the main or only criterion

of value is utility to the researched. Many of the questions we were interested in when we undertook our projects were entirely without interest or utility to our subjects (Ben Rampton, for example, found his informants relatively unmoved by the incidence of retroflex consonants in their speech). This is no argument for shelving the questions that interest the researcher. Moreover, in other cases informants proved deeply interested in matters we had thought would be regarded as abstruse and irrelevant, like arguments for the autonomy of creoles, or the sociological concept of 'reproduction'.

For us, then, it is important that research as a form of knowledge-producing activity continue to be practised and valued. We question, on the one hand, the narrow institutional criteria which validate certain kinds of knowledge and procedures for producing it, while excluding others; and on the other hand, restrictive political criteria which deem research unproductive if it does not address immediate material needs. The desire to analyse the social world you inhabit is to be encouraged, among both professional academics and other members of society—including, of course, research subjects. This is the goal of social research; to carefully observe and interpret human behaviour with a view to improving our understanding of the social world. And the practice of systematic observation and interpretation that broadly defines the term 'research' is not incompatible with other activities. In real-life research projects it is not even always separable from them. The Ph.D. definition, which would exclude some parts of our own case studies—Frazer's photo-story project, for example, and nearly everything Cameron did—is premised on an oversimplification of what researchers and researched actually do.

We have referred to research as 'knowledge producing', and this introduces another complication. What is 'knowledge'? If pressed, a proponent of the Ph.D. definition of research might well say that research is constituted not only by its procedures and protocols but by the kind of knowledge it produces: expert knowledge, which is systematic, formalized in certain ways and preferably original. This question of expert knowledge is one that has so far been put aside, but it must now be taken up in more detail. It is relevant, not only to the question of whether the empowerment framework produces 'real' research, but also to the question of sharing knowledge with research subjects, which is one of our concerns in proposing the framework.

Locating 'knowledge'

In one sense, the selection of 'originality' as a mark of expert knowledge is odd. A great deal of the knowledge researchers produce is constructed out of knowledge their research subjects already possess. Labov's account of the ritual insults used by African-Americans in Harlem (Labov, 1972b) becomes 'original' only when presented to outside, academic audiences; for the people who provided the data, the content of Labov's article would not be 'original' at all (though the form in which it is rendered might be). This suggests there are different kinds of knowledge; and when we talk about the empowering potential of researchers sharing knowledge it is relevant to reflect on that. A crucial question here is whether expert knowledge is normally privileged over lay knowledge for good and necessary reasons (e.g. Labov's account is more systematic than the lay account he based it on) or for merely contingent ones serving narrow sectarian interests.

Although we would want to demystify the category of expert knowledge, by making explicit its relation to already-existing lay knowledge, our research experiences led us to question the strong thesis that there is no significant difference between the two. Some

of us found that sharing expert knowledge can be a valuable mechanism of empowerment.

For example, one characteristic of expert knowledge is the ability to synthesize and relate things, placing them in a broader context. For both Ben Rampton and Deborah Cameron it proved important that the experiences and ideas offered by subjects could be put into a historical context. Rampton related his informants' attitudes concerning aspects of their linguistic repertoires (speaking English, speaking English with a 'Babu' accent, speaking a south Asian language) to the language policies of British imperialists in India, and also to educational theories that were current in Britain during the main immigration period. Cameron, somewhat similarly, approached her informants' ambivalent feelings about patois by giving an account of the history of Caribbean creoles. In both cases, lay knowledge was illuminated by historical contextualization of a kind that had not previously been available to the subjects.

Elizabeth Frazer used a different strategy. On one occasion she asked a group of girls to analyse a transcript of their own previous interaction, with a view to helping them perceive and perhaps resolve certain problems and confusions that had troubled them at the time. The knowledge being shared here was not factual, but processual; Frazer demonstrated the kinds of supervenient reflection, analysis and categorization researchers use in constructing expert knowledge out of informants' talk. These techniques, too, are characteristic of expert discourse, more formalized there than in lay discourse. Frazer's informants reported that they found it empowering to have these ways of analysing made accessible and systematic for them. They felt—as did Rampton's and Cameron's informants—that they were learning something about themselves, something they did not know (or know consciously) before.[4]

We believe, then, that expert knowledge does possess certain specific characteristics that make it worth having; and that if it is worth having, it is worth sharing. And once again, we may note that positivist 'ethical' research methods make this kind and degree of sharing difficult or even impossible; to share knowledge is to intervene actively in the understandings of the researched, whereas positivism enjoins on us a responsibility to leave those understandings undisturbed.

Conclusion

The question we pose in *Researching Language* is whether the balance of power between researchers and research subjects can be altered, with positive results for both groups; and we answer that question in the affirmative. The prevailing 'ethical' framework—with occasional excursions into 'advocacy'—rests its case for the status quo on the idea that reducing the distance between researcher and researched will destroy the enterprise of research; it will bias the results, muddle the scholarly objectives of academic disciplines and lead researchers into conflicting and irrelevant activities.

We hope we have succeeded in showing that these are unfounded fears, based on a questionable epistemology and politics. To do social research 'on, for and with' subjects is certainly not a simple proceeding; it requires enormous attention to the complexities of any actual research context. But these complexities are present whatever kind of research we do. Traditional frameworks do not make them disappear. The less traditional framework we have argued for acknowledges and works with complexity. In our view, this not only benefits the researched, it benefits the researcher too; for although we have rejected the sociolinguist's traditional Holy Grail—speech unaffected by the presence of the observer—

the use of interactive and non-objectifying methods enables us to gain richer insights into subjects' own understandings of their behaviour, and to engage them in dialogue about those understandings. This, we believe, is to our mutual benefit.

Although, like all research paradigms, it must sometimes fall short of the goals it sets itself, empowering research is capable of changing everyone involved in it, providing, not only for researchers but also for their informants, the possibility of constructing new insights and understandings. It is that possibility which should set the standard for all research that concerns itself with language and society.

NOTES

[1] An interesting argument could be made that quantitative sociolinguistics is more realist than positivist. Some versions of sociolinguistics treat observable variation as the effect of a probabilistic component in speakers' grammars, which in turn are held to be 'real' (a claim which connects with the broader debate in linguistics on the psychological reality of grammar). Nevertheless, variationist linguists' methodological assumptions, and especially their definition of what constitutes good or valid data, may with justification be labelled positivist.

[2] There are African-American intellectuals who see any concessions to AVBE as disadvantaging AVBE speakers and reducing their chances of mobility; there are others who feel the Ann Arbor judgement, which made concessions for the specific purpose of teaching SE more effectively, did not sufficiently challenge the dominance of the (White) standard variety. This kind of disagreement should lead social researchers to observe a *caveat* about the uncritical use of the term 'community', which has come to be used in such extended ways (e.g. 'the business community') that it verges on the vacuous, while at the same time its connotations are manipulated for rhetorical effect.

[3] Clearly, the forms of 'self-representation' mentioned here—the video and the photo-story—were not addressed to the conventional audience for academic research and did not use the conventional media for presenting academic research. Furthermore, these non-academic audiences and media are generally accorded lower prestige than, say, a print journal read by academics. But even if 'academic' and 'non-academic' representations are distinct in terms of audience and medium, it would be a mistake for academics to underestimate the potential power and significance of other forms of representation.

[4] It is important to acknowledge here that a linguist, or any expert, who tells a group things about itself, is engaging in a form of interaction which requires sensitive handling. Social identities are at stake and face can be seriously threatened.