

Interviewing: Fear and Liking in the Field

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You do not have to say anything. But it may harm your defence if you do not mention when questioned something you later rely on in court. Anything you do say may be given in evidence.

Police caution when arrested in the UK

WHAT MATTERS?

Words, stories, narratives matter. It is how we explain ourselves to others, how we justify our actions (or inaction), how we present ourselves to others. But interviews, like the classic police interview that starts with the verbal warning above, are also more than a straightforward or simple exchange of words. Relations of power and authority affect the nature of exchanges, most clearly in the police interview but in almost all social interactions. And body 'language' matters too: a suspect may sweat or twitch, and avoid eye contact with the interrogator. In job interviews, where we are a supplicant rather than a suspect, we tend to 'dress to impress'. In a recent UK guide to interviews for university candidates, for example, it was suggested that young women interviewees should avoid short skirts or large earrings which may distract the interviewer but in interviews

that I have undertaken with women working in merchant bankers, some of the more senior and successful women suggested that dressing to catch the eye, to stand out, is a better strategy when dealing with the banks' clients. Clearly assumptions about the gender of the people to be impressed are also embedded in this contradictory advice to women.

So language, bodies, clothes, gender, clearly matter in the sorts of exchanges that take place in interviews. Commonsense, I hear you muttering, everybody knows that. Yet in human geography and the social sciences more generally, the significance of the personal nature of the interaction and the consequent impact of embodied social characteristics was not widely accepted until relatively recently. Now, however, these questions of embodiment as well as the affects of language and questions about interpretation and representation have become central to philosophical discussions in the social sciences including human geography, as well as to the nature and practice of talking to people in a wide range of circumstances and locations (see for example Pryke *et al.*, 2003). For human geographers interested in the difference that place makes to the public and private lives of individuals, households, social groups, communities, gangs, outcasts,

hermits, wanderers, nomads, vagrants, professionals, workers, government officials, children, patients – the list is long – talking to people, some form of personal interaction between researchers and the people we are interested in is the most obvious method of collecting ‘data’. But, as I have already intimated, this relatively simple statement hides a multitude of complex issues which are the focus of this chapter and of two related chapters – those by Jacquie Burgess on focus groups and Peter Jackson and Polly Russell on collecting life histories. With interviewing, these methods are now amongst the most common of the qualitative approaches utilised by human geographers in the collection of information about past and present lives and the circumstances that influence the course that they take. And because interviewing does involve personal contacts and interactions, it is perhaps the most exciting and the most challenging of the methods currently in wide use.

I have been interviewing people in the UK during my entire academic career so far – from steel workers in Corby New Town, local authority officials in Brighton, bankers in the City of London, young men in Sheffield and Cambridge, parents in London and Manchester, Latvian migrants in Leicestershire and elsewhere and, most recently, new migrants from the European Union and elsewhere working in service sector industries in Greater London. And yet each time, before I go to talk to the people I have identified as important to the aims of my work, my heart thumps, my palms sweat and I wonder whether I have the energy, confidence and the sheer cheek required to persuade them to share with me the sometimes intimate and occasionally painful details of their lives for what might seem to them to be very little return. The returns for me, perhaps unfairly, are much greater. I get to meet a range of interesting people often in circumstances that are new to me: people who tell me the most interesting and important things about their lives without necessarily expecting, or indeed wanting, reciprocal disclosures on my part. To add to this inequity, what they tell me

eventually appears as a scholarly paper in an academic journal or as a book that other people then ask me to talk about, raising more new and interesting questions for me to consider. In this process as I have outlined it, the emphasis shifts from an initial focus on the lives and stories of the people interviewed to a focus on the researcher who becomes responsible for both the interpretation and the reception of other people’s lives.

Fieldwork, then, is often transformative for researchers but probably is much less often as exciting for those who are interviewed, although here too in recent years, we spend rather more time than previously thinking about the impact of an interview on the people involved than perhaps we used to (see for example Eyles and Smith, 1988; Limb and Dwyer, 2001). In this chapter I want to try and capture something of the fear and the delight of interviewing, as well as address the philosophical and ethical issues raised rather than provide a detailed technical guide to informant selection, question order or analysis and interpretation, although these questions will not be entirely ignored. There is already a whole series of useful articles, books, pamphlets, guides and on-line resources to turn too, that are easily available and accessible to novices, whether new researchers experimenting with different approaches or the lecturer who is asked to teach qualitative methods to beginners (Flowerdew and Martin, 1997; Kitchen and Tate, 2000; Mason, 2002; Silverman, 2000). Furthermore, many questions about data, confidentiality, analysis and interpretation that apply to interviews in general are addressed by Jackson and Russell in the succeeding chapter. So, this chapter is in a way an introductory exercise before their more detailed exposition.

WHY DO INTERVIEWS?

Perhaps the first question to think about is why interviews are a useful technique. As with all qualitative methods, the aim is to

probe an issue in depth: the purpose is to explore and understand actions within specific settings, to examine human relationships and discover as much as possible about why people feel or act in the ways they do. In comparison to large-scale quantitative techniques, interview methodologies typically aim for depth and detailed understanding rather than breadth and coverage. Interviews are often associated with case study approaches rather than attempts to include a large sample, although short postal schedules or telephone interviews are a common way of collecting a limited range of material from a large population. As the interviewer and interviewee are not co-present in these encounters, as the scale of the work usually is large and the questions straight forward (to avoid misunderstandings), this type of interview methodology might more accurately be placed on the quantitative side of the methodological divide.

What distinguishes most interviews, however, is the scope they provide for probing meanings and emotions: interviewing is an *interpretative* methodology. It is this claim that has been at the heart of a fascinating philosophical debate about interviews as social encounters for the last three decades or so. I want to capture the main arguments in what follows. But first a brief reminder: interviews are often used in association with other methods – both qualitative and quantitative – in part as all social scientists who undertake interviews to explore what is going on in particular places and circumstance draw on a range of other methods to check our interpretations of what people tell them. Newspaper articles, census data, films, diaries may all be useful in providing both context and validation of the material collected through interviewing people.

WHERE TO START?

Undertaking interviews of any kind involves both a number of chronological decisions

and stages as well as a set of philosophical, political and ethical issues that underpin both the initial decision to adopt interviews as part of or the main methodological strategy and the practical issues that are important at different stages of the research. Thus, questions about identification, contacts, interactions, interpretation and representation are important at different stages in the process whereas issues of ethics, responsibility, equity, status and power underlie the whole process. It is these underlying issues about power and equality that I want to focus on in more detail, as well as arguments about the nature of knowledge constructed through fieldwork. It is here too that the debates in the social sciences about interviewing in particular and qualitative methods in general have been transformed in recent years.

When I first started interviewing migrant men working in the steel industry as part of an undergraduate project in the early 1970s, there was a widespread and strongly-held belief that such an approach should and must conform to what were then the key standards of scientific method – that is rigour, objectivity and replication. The social characteristics of the researcher about to go out into the city streets or the villages of an unknown rural area were completely ignored, seen as irrelevant to the whole process and talk revolved around the nature of the ‘survey instrument’ that was to be utilised in the proposed exchange. The extract below from Ann Oakley’s book *Becoming a Mother* Martin (1979) lays out the advice she received as a social researcher planning to undertake interviews and the dilemmas it raised when she started interviewing pregnant women in the early 1970s for her doctoral research. At the time she was a mother of a small child and she became pregnant herself half-way through her project.

In the passage below Oakley (1979: 209) sets out the advice that was common at the time she began her research.

Regarded as an information-gathering tool, the interview is designed to minimise the local, concrete, immediate circumstances of the particular

encounter – including the respective personalities of the participants – and to emphasize only those aspects that can be kept general and demonstrable enough to be counted. As an encounter between two particular people the typical interview has no meaning; it is conceived in a framework of other comparable meetings between other couples ... (Denzin, 1970: 196)

But, as Oakley found, an interview is in practice a local and immediate encounter and the people involved in the exchange matter. ‘Contrary to what the text-books say, researching and being researched are parts of *human* interaction’ (Oakley, 1979: 310). The pregnant women whom she interviewed asked questions about her circumstances, about being pregnant and wanted advice when they discovered that she was already a mother. She answered as best she could and, as she found, ‘there were times in the research when I began to confuse roles – researcher, pregnant woman, mother, feminist, participant observer and so on’ (Oakley, 1979: 4). ‘The point is’, she concluded, ‘that academic research projects bear an intimate relationship to the researcher’s life, however “scientific” a sociologist pretends to be’ (p. 4). Interviews are not and can never be ‘typical’ – one instance among comparable others, as the methods textbooks then insisted, but instead capture the variety of meanings and experiences. Thus, through interviews, difference rather than similarity in experiences is explored and so generalisations may be challenged, as well as allowing into both the encounter and the resulting text the emotions and feelings of the subjects, and more recently the researcher.

In the years since Oakley began her research, feminist arguments about power and responsibility, about rapport and positionality (Gluck and Patai, 1991; Bell *et al.*, 1993; Professional Geographer, 1994), work by radical anthropologists about dialogic writing and the politics of texts (Clifford and Marcus, 1986), and in the 1990s the wholesale impact of post-modern work in the humanities and the social sciences (Roseneau, 1992; Smart, 1993; Yeatman, 1994; Butler, 2002) have revolutionised understandings of

what is involved in interviewing, transforming the relationship between thought and materiality. Those older ideas that the exchange of information involved in an interview was independent of the social characteristics of those involved or of the place, time of day, the topics involved or whether payment was involved or not have now been overturned, although it is important not to exaggerate the extent of the shift nor its immediacy. Indeed, recent debates about affect in human geography seem unaware of the long debate within feminist theory about the place of emotions in social encounters.

At the same time as Oakley was struggling with the advice to be neutral and scientific in the 1970s, other social researchers had begun to recognise that personalities and opinions mattered, although their emphasis was on how structures of power and inequality influence research encounters rather than on issues of inter-personal interactions, emotions and feelings. Thus in a book published in the late 1970s based on ten personal accounts of doing research, it was argued that ‘social research is political because the researcher has interests which may coincide with or contradict the interests of the researched. All social research has an end: the formulation of policy, the conservation, reform or radical transformation of the social situation being studied’ (Cass *et al.*, 1978: 143) so social science is a *political* endeavour. Here we see links between methodological debates and some of the claims for a radical or critical practice of human geography that were evident in the 1970s and 1980s. I shall return to arguments about political purpose and transformative possibilities of qualitative research at the end of the chapter.

LANGUAGE MATTERS

From the late 1970s onwards, then, there has been a wholesale challenge to the notions of scientific objectivity in interviewing and to

the transparency of the exchange of information through careful consideration of what James Clifford and George Marcus (1986) termed the poetics and politics of all cultural exchanges. Language, it is now recognised, is not a neutral instrument of communication but instead it produces, as Judith Butler (1993) has noted, 'the effects that it names' (p. 2). 'Reality' is no longer assumed to be 'out there', waiting to be discovered, named and described by social researchers but is itself constituted in and by discourse, and embodied interactions, as are the representations that we chose to construct from fieldwork and interviewing. Thus we construct questions to ask and make knowledge claims on the basis of what we discover through the already existing conventions of language and discourse within which we are working.

Here the work of Michel Foucault, the French historian and philosopher who was writing on the history of sexuality, the penal system, modern medicine and systems of classification in the second half of the twentieth century, has had a significant influence on geographical practice and scholarship. He argued that all knowledge is constructed within interrelated sets of statements or discourses that establish the ways in which it is possible to make statements about things. These discursive practices, he argues, are based in systems of power, in which it is possible only to make certain claims. Discursive practices thus establish what it is possible to speak about, to define and research. Such practices thus, Foucault (1977) argued, construct 'a delimitation of a field of objects, the definition of a legitimate perspective for the agent of knowledge and the fixing of norms for the elaboration of concepts and theories' (p. 23). It might help to think through this claim with an example.

Let's go back to Ann Oakley's work. Before she wrote her book on becoming a mother, she was responsible, almost single-handedly, for putting the issue of housework on the social sciences agenda. Asking questions about and collecting information on who did what within the home had, before

the publication of her book *The Sociology of Housework* in 1974, barely appeared at all on the agenda of sociology or geography. Because work was defined solely as waged labour in the labour market, what went on in private (and here is a second contested term – you might like to think about why the home was seen as a private arena despite its penetration by all sort of capitalist goods and services and its regulation by state officials such as planners, health workers, and so forth), in the private sphere of the home was disregarded as a suitable subject for either theorisation or empirical investigation. In establishing how to explain this absence, views and opinions about the significance of women's place in the contemporary world then become important. Individualist explanations based on women's choices or their natural aptitude for certain types of repetitive work leave little to explain, whereas different philosophical positions – perhaps based on the necessity of housework for the functioning of a capitalist economy or in beliefs about male power and the oppression of women – open the possibility of empirical investigation based in part on interviews with the key actors that feature in the different explanatory frameworks. Nicky Gregson and Michelle Lowe's (1994) excellent book on commodified domestic work was one result of this re-conceptualisation of the significance of housework and has been followed by a long series of investigations of what goes on in the 'private' sphere of the home in which interviewing has been a key methodological strategy (for just two more recent examples of work by geographers see Blunt, 2005; Pratt, 2003).

It is clear then that starting a research project involving interviewing is a complex matter. The very issues that we decide interest us as well as the detailed questions that we plan to ask our interviewees are all set in the frame of already existing discourses, in our engagement in existing work and our commitment to a broader philosophical position that underpins our decision to do a certain type of work. These questions about

power and poetics, about language, theory and context have become particularly significant for geographers as one of the effects of the post-structural and post-modern turn in the 1990s has been a growing interest in the theoretical significance of place, in diversity, difference and particularity, and its significance in the explanation of patterns of inequality. Some of these changes are captured in Doreen Massey's book *For Space* (2005) where she explores some of the complex philosophical origins in her commitment to theorising the significance of spatial diversity.

Rather than searching for law-like regularities in the lab or the library, the dominant purpose of those years of spatial science, many human geographers have (re)turned to fieldwork to explore difference and have adopted the interview as their main approach. However, theory, context and structures still matter. Like many theorists of the particularity of place, I see it as constituted in the coincidence of social process and flows/interconnections across different spatial scales. As always, for social researchers, the relationships between structures and agency, about constraints and choice, and the adoption of different theoretical positions to explain these relationships influence our initial questions, our methodological strategies and how we interpret our findings. Although through interviews we often are searching for the difference that place makes, in interpretation we test these particularities against broader or more general theoretical explanations of how we think the world works. Thus although Denzin's advice to minimize 'the local, concrete and immediate circumstances' is now generally disregarded (and he has changed his own mind), for geographers the difference made by the local and concrete is often the very thing of interest. This local particularity or difference, however, typically is interpreted within a broader structural context. Indeed as I have argued elsewhere (McDowell, 2004), I believe that one of the key issues in contemporary theorising in human geography is how to combine a

discursive relational approach to difference and particularity with the continuing significance of categorical inequalities.

TALKING TO WHICH OTHERS ABOUT WHAT?

I want now to assume that detailed reading and the development of an acceptable philosophical framework and a set of beliefs about what needs to be done has resulted in the identification not only of a research topic but a potential set or sets of people to interview. Actually identifying, contacting and arranging to meet interviewees often raises complicated logistical and ethical questions. And here too the philosophical notions that lie behind such encounters have also changed in recent years. Rather than being a transparent, straightforward exchange of information, the interview is a complex and contested social encounter riven with power relations. To a large degree, the social researcher is a supplicant, dependent upon the cooperation of interviewees, who must both agree to participate and feel willing and able to share with the interviewer the sorts of information on which the success of the work will depend. When interviewing the powerful – perhaps politicians, government officials, media 'stars' or whomsoever – the interviewer is frequently the less powerful party in the encounter but might also have in common social or educational background, class position or accent with the interviewees, leading to a sense of ease in the exchanges.

In other cases, however, interviewees are less obviously supplicants when the interviewees are much younger, less educated, or less socially skilled than they are and here care has to be taken not to intimidate or overwhelm potential participants. Even so, growing numbers of people have at least some experience of being interviewed: at school, for college entry, by their doctor, in applications for loans, as part of opinion polls and talk shows. Indeed, the interview is now such

a ubiquitous feature of everyday life that some scholars have characterised the USA as 'the interview society' (Silverman, 1993; Atkinson and Silverman, 1997). The police interview that I began with or the research interview at issue here are now the norm rather than exceptions. As the contemporary media have become more and more intrusive it seems as if almost any member of the population is fair game for the most intrusive type of questioning. Indeed in their chapter on interviewing in a huge and comprehensive handbook on qualitative methods (interestingly co-edited by the same Denzin discussed by Ann Oakley: see Denzin and Lincoln, 2005), Andre Fontana and James Fey (2005) have suggested, drawing on Jaber Gubrium and James Holstein (1998), that 'the interview has become a contemporary means of storytelling in which persons divulge life accounts in response to interview enquiries' (p. 699).

Interviewers, nevertheless, often have to work hard to secure agreement and involvement and even harder to construct an encounter in which the exchange is both sufficiently collaborative to make the 'respondents' feel comfortable and that their participation is highly valued while at the same time not being overly intrusive or too focused on the interviewer's own life, values and beliefs. It has been suggested that revealing something of yourself, your own circumstances and feelings is a way to persuade interviewees of your good faith. However, getting personal should be more than just a way of squeezing more information out of people, but rather a way of creating both greater empathy and attempting to reduce the power differentials in the actual encounter, even if this is wishful thinking at the broader social scale. The idea that the interview exchange is more of a collaboration than an interrogation has now permeated geographical research and in common with anthropologists, geographers are now much more aware of the ways in which an interview is and should be an interactive and reflexive exchange wherever possible.

In establishing contact and arranging interviews, researchers must always follow the ethical code of their own institution and professional association. As a general rule, children and young adults under the age of 18 must not be interviewed alone and parental permission must be sought. In the UK, a police check is also necessary for those hoping to work with children. The ethical and practical issues that arise working with children and young people are discussed in more detail in papers by *inter alia* Stuart Aitken (see Aitken and Thomas, 1997; Aitken, 2001), Gill Valentine (1999) and Linda McDowell (2001). Similar issues are raised and codes of conduct apply in interactions with groups in the population who might be regarded as 'vulnerable' – the very elderly perhaps, people who are confused or ill, or those whose lives and livelihood are in other ways insecure.

A good guide for making the initial contact and deciding where to meet, assuming the interview involves face-to-face interaction rather than being either an e-survey or based on a telephone interview is to use empathy and try and imagine yourself in the pace of the interviewees. Will they be scared by your approach? How much will they know or understand about your work? Do you want to see them in their place of work or study, at home (theirs or yours?) or on neutral ground and why? What difference will these decisions make? How will the interviewees react to you? Will they take you seriously? Are you too different to be able to establish a connection? Here I often reflect on my naivety when as a young white undergraduate student I interviewed steel workers in Corby New Town. These were men from the former Yugoslavia, Latvia and Ukraine who had fled Soviet repression and, after a period in displaced persons camps in Germany, had become workers in heavy industry in the UK. What possible point of contact could I have with them? And yet my unthinking confidence and their courtesy made the encounter both possible and interesting (at least for me) as

I collected narratives of displacement that much later led to a new research project with women who had come to England as ‘volunteer workers’ at the same time (McDowell, 2005). In the 1970s, however, we did not use the word ‘narrative’ and much of the personal information these men told me about their hopes and fears for their lives in the UK disappeared in the eventual product – a dissertation based on a factorial ecology.

Although in this case my initial focus and aims and the eventual result coincided, partly I think because I was too inexperienced to realise the value of what I had actually collected, in the initial encounter with their research ‘subjects’, interviewees must be aware of the provisional nature of their intended research focus. The range and scope of the research may be open to re-definition or renegotiation once contact is made with the participants in the research endeavour. Here’s Philippe Bourgois (1995), whose wonderful study of crack dealers in New York City is an inspiration for all qualitative researchers, talking about how his research focus identified itself:

I was forced into crack against my will. When I first moved to East Harlem – ‘El Barrio’ – as a newlywed in the Spring of 1985, I was looking for an inexpensive New York City apartment from which I could write a book on the experience of poverty and ethnic segregation in the heart of one of the most expensive cities in the world. On the level of theory, I was interested in the political economy of inner-city street culture. From a personal, political perspective, I wanted to probe the Achilles heel of the richest industrialised nation of the world by documenting how it imposes racial segregation and economic marginalisation on so many of its Latino/a and African-American citizens. (p. 1)

So here, Bourgois makes clear both the theoretical assumptions that underlie his choice of subject and location as well as his own political position, illustrating my earlier arguments about how theory and politics structure the choice of what to study. When he and his wife arrived in NYC, however, he found rather than exploring a range of street activities, selling and smoking crack imposed

itself as the focus because of its growing significance in the area.

I had never even heard of crack when I first arrived in the neighbourhood – no-one knew about this particular substance yet ... by the end of the year, however, most of my friends, neighbours and acquaintances had been swept into the multibillion-dollar crack cyclone: selling it, smoking it, fretting over it. (p. 1)

So crack took over both as Bourgois’ focus and, to a large extent, his life, as he talked to his interviewees in a range of places including on the street and in crack houses and in his own apartment where their behaviour often repulsed his family. Furthermore, his field-work raised difficult ethical issues about, for example, his possible involvement in illegal activities and in the inevitable encounters with law enforcement agencies that were part of his time on the streets. His class, accent and skin colour all had to be negotiated not only with his informants but also in encounters with the police:

I was almost never harassed by the street sellers; at worst they simply fled from me or ignored me. On the other hand, I was repeatedly stopped, searched, cursed and humiliated by New York City police officers on the beat. From their perspective there was no reason for a white boy to be in the neighbourhood unless he was an undercover cop or a drug addict, and because I am skinny they instantly assumed the latter. (Bourgois, 1995: 30)

While Bourgois’ work raises questions of access and the performance of identity in perhaps extreme forms in its focus on illegal street activities, all researchers must address similar issues of how to negotiate their identities, especially in the initial contact. In my own work, I have often made the wrong decision in, for example, how I contacted people or in the methods I used to elicit opinions. In the work in Corby New Town that I have already mentioned, I appeared unannounced on the doorsteps of eastern European migrants, wielding an officious looking clipboard and so appearing as a rather threatening ‘official’ despite my (then) youth. And to my surprise now, nobody challenged me or even asked how I had acquired their name, nor did they ask about the purpose of the

research. More recently, in selecting and interviewing young men with little education, I over-estimated their literacy and embarrassed one or two by giving them a too-complicated newspaper cutting as a stimulus to talk about sex.

And like Bourgois, I too have had the experience of my research focus being changed by circumstances. In my case, it was in the study of migrant women's lives in post-war Britain (see McDowell, 2005). My initial aim was to critically explore the hegemonic image of domestic femininity that was established in post-war Britain and so I decided to explore the lives of a group of women who were clearly non-conformists, that is women in the labour market during the late 1940s and throughout the 1950s. I chose as possible interviewees a group of Latvian women who had come to the UK between 1946 and 1949 as migrant workers, recruited by the British Government to work in female-employing sectors that were short of labour – textiles, hospitals and various forms of institutional domestic service. I worked hard to produce an unstructured questionnaire schedule to explore the intersections and contradictions between their home and 'working' lives, but these women, when I began to talk to them, had a clear set of ideas about what they wanted me to know: about Soviet aggression in Latvia (one of the three Baltic states), about the German and Soviet occupations, about the years they spent in displaced persons' camps in post-war Germany. Only then would they turn to what was to them the relatively unimportant topic of their lives in the UK. And so my research changed and the eventual book is as much about the Second World War and about national memory as it is about divisions of labour in post-war Britain.

Times have changed, of course, and in the 'interview society', people are now more cautious and more savvy about being interviewed. In the work with bankers for example, I was subjected to intense cross-questioning about the research aims. Here is a further ethical dilemma: how much should/

must a researcher reveal about the purpose of the work to facilitate access? In general, I believe in honesty and openness but this principle might conflict with access and with the quality of the interview encounter. Was it reasonable of me to present myself to the gatekeepers in the merchant banks where I interviewed workers in the mid-1990s as interested in human capital and personnel policies or should I have admitted to a theoretical (and practical) interest in women's oppression, discrimination against women and members of minority groups, in sexualised workplace cultures and their effects? In all these examples, too, whether with male workers in heavy industry, with bourgeois bankers in the City of London, with working class young men and with elderly women of my mother's age, issues of class, gender, accent, previous experiences and shared (or not) knowledge make a difference to the encounter. There are no easy guidelines about establishing contact and rapport, although practice certainly helps and courtesy, a certain degree of persistence and open-mindedness are essential.

HOW TO DO IT?

One of the difficult issues of what the focus of research should be and who to talk to in order to construct a detailed picture of what is going on, the more mundane details of what sort of interviewing to undertake tend to pale into insignificance. But here too questions about empathy, power and control, and the degree of mutual respect and collaboration that is achievable influence decisions. Interviewing ranges from the more to the less formal through the use of structured to unstructured sets of questions (sometimes grandly called the survey instrument) and what to choose depends both on the interviewees and on the skill and confidence of the interviewers. It is easier to administer a formal questionnaire survey than a less structured one but only if the respondents

(and the interviewer) are prepared to stick to the themes. The best advice I was ever given at an early stage in my career was not to talk so much. Although it is hard advice to follow, allowing silences is often productive.

Doing interviews on your own is often harder than with a colleague, but two of you may seem intimidating. But then one of you can write notes while the other does the questioning, unless you decide to record the interview. Recording raises questions about unease and about confidentiality, as well as operating the machine and making sure it is still recording, as well as needing to transcribe the material afterwards. In the next chapter on life histories, Jackson and Russell provide an excellent guide to doing and analysing interviews so I shall not repeat their advice here. Instead, I want to return to the questions about power, language and representation I began this chapter with and conclude with some comments about multiple voices and the 'crisis' of representation.

WHOSE VOICES ARE/SHOULD BE HEARD? WHO'S LISTENING? WHO ARE YOU WRITING FOR?

As I argued in the introduction, doing interviews involves a set of political acts and negotiations of power differentials. This argument applies just as strongly to the analysis and interpretation of interviews and to deciding how and what to 'write up'. Representations and writing are also political acts. The researcher has to decide whose voices will be heard in the text (the majority of human geographers rely on textual representation although more recently visual and other forms of representation are becoming more common – see Crang, in this volume; Rose, 2001). Indeed, it might be that the interviewees themselves could/should be involved both in interpretation and writing, helping to select which parts of the interviews are included and which are excluded,

although to my knowledge such a method has never been used by geographers. Many of us, however, give our respondents draft texts to read, whether returning their own interview after transcription or involving them in reading drafts of future articles or policy documents. This opens up the prospect of producing a range of alternative discourses, which are (should be?) based in and on a collaborative encounter between researchers and their interviewees rather than on the straightforward transcription of transparent stories for an, usually academic, audience. But it also runs the risk of different degrees of censorship, from an interviewee perhaps refusing permission for a sensitive part of the interview to be used to outright refusal, perhaps by the more powerful, for the interviews to be used at all.

Even if less dialogic methods are possible or desirable, all interviewers are faced with the decision of whose voices and what claims should be heard in the text that eventually supersedes the interviews. In some cases, perhaps especially, although by no means always, in work with deprived or extremely disadvantaged groups, ethical questions about whether to include information about, for example, involvement in illegal activities, in tax fiddles, in working off the books for example, become important. It is also essential to avoid a 'warts and all' voyeuristic description, even a celebration, of the lives of the poor and desperation, ensuring that vulnerable participants are not exploited. Other groups – the policemen, for example, interviewed in East London by Michael Keith (1992), the young men in Cambridge and Sheffield to whom I talked and the crack dealers whose lives Bourgois portrayed so vividly – are often racist and sexist in their attitudes and speech. Interviewers may both find these views offensive and off-putting and may also want to avoid reinforcing popular stereotypes of young men and yet want to accurately portray the texture of people's lives. What sort of judgements face interviewers in these cases? In the extract below I repeat some of the questions that I found

myself addressing as I interviewed young school leavers.

Is it appropriate, for example, to discuss semi-criminal and illegal activities at a time when young men are being demonised in the press as feckless or troublesome? What should be done with information about different ways of making a living if they involve tax or benefit fraud? How should the connections between troubling individual behaviours and attitudes (racist, sexist and homophobic acts for example) and macrosocial changes be represented when the participants of research expressly do not make these connections? What about naming acts as abuse if the informants do not call it this? Is it ethical, as Fine and Weiss (1996) ask to 'display the voyeuristic dirty laundry that litters our data base?' and further, 'how can we risk romanticizing or denying the devastating assault on poor and working class families launched by the state, the economy, neighbours and sometimes kin?' (pp. 258–9) (McDowell, 2001: 96–7).

To these questions, I now add another as well as suggest an appropriate response. If there is a possibility that interviewees might be prosecuted because of something they revealed, is the researcher obliged based on some abstract notion of truth to fully and accurately record, transcribe and publish the entire interview? These judgments are particularly hard in case where interviewees reveal that they have been victims of, say, bullying or other forms of verbal or bodily abuse. Here, recommended practice is to advise interviewees of appropriate sources of advice and help or to ask their permission to speak to someone on their behalf. At a more general level, however, all interviewers are faced with complex decisions about representation.

As well as deciding whom to include and what parts of their interview – or indeed as part of this decision – the intended audience and the purpose of the written piece (or less usually a video, an exhibition, or another form of performance) affects the nature of the argument and the ways in which it is

presented. As Bourgois (1995) argued in his book, 'in the US there are few nuances in the popular understanding of the relationship between structural constraints and individual failure' (p. 15). As a consequence, he suggests that intellectuals typically have evaded their responsibilities, by either not addressing the devastating urban poverty that is still current in wealthy societies or by producing 'positive representations of the oppressed that those who have been poor, or who have lived among the poor, know to be completely unrealistic' (p. 15). Further, Bourgois argued that he often received a hostile reception among scholars when he presented the results of this work: his academic peers either reacted in outrage or suggested that his findings would be used against the poor.

In my work with white working class boys, I have been accused of denying the effects of racism on school achievement by not including young men of colour and, in a much earlier period, when feminist arguments about interpretative and contextual research were not acceptable within geography, my work has been dismissed as either lacking objectivity or being 'political'. While the world of scholarship has changed almost immeasurably, it is clear that careful thought about the audiences for and about the reception of different forms of text is remains significant. Most social researchers (most geographers) undertaking interviews tend to have a particular audience or audiences in mind, and write in light of their concerns, although sometimes it is hard to identify the multiple audiences who might be interested. The answer to the question 'For whom am I writing?' may include for, with and about the informants (which are not at all the same thing), for the funding body, for academic peers, for the next research assessment exercise, to improve one's own status, to gain promotion or more nobly to influence policy makers, even to change the world. Sometimes it is difficult to disentangle these audiences and motives and to address their implications.

REPRESENTING THE 'OTHER': ISSUES OF DISSEMINATION AND ADVOCACY

For student interviewers, the decision about audience and reception typically is more straight forward. The dissertation is usually the main or the most immediate output and so the audience is the examination panel. Here a set of conventions usually constrains the form of writing and presentation. Similar conventions operate when the final product is an academic article. But other outputs and different forms of presentation are also significant. The research may have been funded by a sponsor who expects a set of policy proposals. Alternatively, the researcher may be writing as an advocate of a particular group or point of view, with the aim of influencing political debate or decisions.

Many human geographers assert their belief in 'critical' research – research which, drawing on Nancy Fraser's (1989) definition, 'frames its research program and its conceptual framework with an eye to the aims and activities of those oppositional social movements with which it has a partisan, although not uncritical, identification' (p. 113). The usual aim of critical social research is to traverse the boundaries between research, policy, activism and theory construction (see for example Kobayashi, 2001), which raises the issues about advocacy but also whether and what extent interview-based research may have an impact not only in challenging the representation of social groups but also in the amelioration of inequality and injustice. In the case of my own work with under-educated young men, which I hoped fell into the category of critical research, I had four interconnected aims, all of which influenced the ways in which I designed and undertook the research, as well as how to write about it. This is what I argued at a time when I was just at the end of the second round of interviews:

First, I want to contribute to challenging the increasingly dominant and stereotypical designation of young working class men as 'jobs and thugs'¹ that has become common in Government

pronouncements on youth issues. Through both personal experiences and theoretical reading, I was aware that the ways in which young men behave and perform their masculinity were more varied than this singular view. This knowledge linked directly to my second aim: to contribute to the growing work in the broad area of scholarship in gender studies that attempts to combine a structural understanding of inequality and limited opportunities and discursive, post-structural analyses of identity (see for example Segal, 1999). Thirdly, although the young men with whom I talked were clearly at a transition in their lives, in the sense that they completed their compulsory schooling and moved into new forms of work or study during the year in which I interviewed them, I also want to contribute to recent youth debates that emphasise the variety and longevity in such transitions [rather than a singular view of a single successful transition] Finally, and perhaps of greatest importance, as the work was funded by the Joseph Rowntree Foundation which is a UK-based institution funding policy research, I hope to contribute to current policy debates about youth services, about raising the level of the minimum income and, indeed including presently-excluded 16 and 17 year old employees, about improving income support for the low paid and the provision of flexible forms of further education provision. In common with other critical studies of the attitudes and actions of less privileged groups, I hope to show that actions that are often seen as irrational or illegal are, in fact, economically rational given the ways in which the tax and benefit systems, for example, systematically discriminate against or exclude certain individuals, including young people. I wanted to demonstrate the huge efforts being made by these young men, in relatively adverse circumstances, to construct what appears to them to be a respectable life. (McDowell, 2001: 95)

Looking back, and after the publication of a book based on this work (McDowell, 2003), I am not sure that I have been equally successful in achieving all these aims. I hope I achieved the academic aims, but I am much less certain that I have had any impact on policy formulation, although I have presented my work to a wide range of teachers in different types of schools, many, but not all of them dealing with 'difficult' boys. Complex questions about dissemination and influence are important as well as the willingness and opportunity to contribute in significant policy arenas and I do not think I tried hard enough

to get politicians and other policy innovators to read my work. In part, my relative failure lies in not thinking hard enough about different forms of writing and multiple dissemination strategies. Sending an academic book to policy makers is seldom the best way to influence their debates.

A final issue that I grappled with in this and more recent work with women migrants (McDowell, 2005) is both how to involve the people to whom I talked in the process of representing their lives and to return the text to them. Informants are active participants in the initial stages of the research process and are able to (re)-direct the course of the conversations in which they are participants but they tend to have much less power in the later stages. Practices differ: some interviewers return transcripts to their interviewees, others send drafts of their text but this demands considerable time and resources from interviewees – much more than a simple commitment to take part in an interview.

Once the work is complete, it seems only courteous to send copies of papers and books to interviewees, as well as to the funding body and academic outlets. Yet, academic prose is often inaccessible and furthermore, re-presenting the lives of informants in the context of structural inequalities, especially if they are the victim of these forces, may seem brutal. Although many of the young men to whom I spoke recognised the poverty of the opportunities that faced them, I still shied away from presenting such an interpretation of their lives to them. I chose to talk to them because they were ‘low achievers’ at school and had little chance of a ‘career’ but it is a different matter to make this brutally plain in a written version of their lives. I did in fact send a short (four page) summary of my findings to each of them and although I spoke to them all again a year or so later, none of them commented on what I had written.

When I finished the book based on long life history interviews with women migrants from Latvia to the UK, I sent a copy to each woman as a gesture towards repayment of

my huge debt to them. I also presented some of the ‘findings’ to an audience at the Latvian Embassy, finding it difficult to represent women’s lives to an audience that included many of the women whom I had interviewed. But the response was generally good: many women appreciated having a concrete record of what they suggested was a largely ‘forgotten history’ and I was moved by the woman who told me she had given the book to her grandson: ‘now he understands’. But some reactions surprised me – one woman hated the title of the book *Hard Labour*, suggesting it implied that she had been a prisoner in a Soviet labour camp. And at the Embassy, I found myself facing hostility from many of the men present who resented what they saw as the absence of their lives. Fine words about women’s history, the perspectives of the Other and so on had limited effect.

Despite Bourgois’s (1995) cynicism about contemporary practices and his belief in the need for committed academics, he ultimately left the judgement of his work to the reader of his book: ‘I do not know if it is possible for me to present the story of my three and a half years of residence in El Barrio without falling prey to a pornography of violence, or a racist voyeurism – ultimately the problem and the responsibility is also in the eyes of the beholder’ (p. 18). Is this a sufficient response? I value the comments from the women whose lives are at the centre of my book (our book?) perhaps more than the academic reviews but still want favourable responses from my academic peers.

A CRISIS/LIMITS OF REPRESENTATION?

In this final section I want to conclude by pursuing the question of the relationships between politically-inspired forms of work based on a notion of advocacy and issues of writing and representation in a little more detail. Bourgois included in his book a critique of the sort of work advocated by the

anthropologists Clifford and Marcus. Clifford and Marcus in their championing of both 'poetics and politics' have embraced of a type of work that is complex, multiple and polyphonic – that includes not only multiple voices but different points of view. Bourgois (1995) believes that the form of writing that has characterised ethnographic and interview-based work in the last decade or so has been profoundly elitist and so relatively inaccessible to policy makers and almost entirely so to the people represented within these texts. Thus, Bourgois suggests that:

Although postmodern ethnographers often claim to be subversive, their contestation of authority focuses on hyperliterate critiques of form through evocative vocabularies, playful syntaxes, and polyphonous voices, rather than engaging with tangible daily struggles. Postmodern debates titillate alienated suburbanised intellectuals; they are completely out of touch with the urgent social crises of the inner-city unemployed. (1995: 14)

While there may be force in this argument, I think Bourgois evades the question of multiple audiences for social research and the importance of different forms of writing. However, in the decade since his critique of postmodern playfulness, an even more serious criticism has been levelled at social research and specifically at the (im)possibility of representing the lives of others. In geography this critique has been summed up in a turn to non-representational theory. While Bourgois argued for a return to a simpler form of writing and what he saw as a more accurate representation of the lives of others, in the last decade or so, a number of influential critical theorists in the discipline of geography have developed a forceful analysis of the limits to representation. Nigel Thrift is a key figure in this critique (1996, 2003). In a useful (critical) summary of this non-representational turn, Noel Castree (2004) outlines the three charges that Thrift has levelled at work which relies on representational theory.

First, representation is about distance – a scholastic disposition that divorces putative 'observers' from 'objects'. Second, representation is about codification – it seeks to 'fix' or capture the represented as

if it or they possess(es) some stable identity. Third . . . representation is about cognition, speech and vision, as if these were the only or privileged way of knowing things and, thus, of doing things. (p. 472)

Now, it is clear from what I have argued so far that these criticisms have all concerned qualitative researchers for many years. Ways of reducing distance, of emphasising interconnection and the significance of the relationship between 'observer' and 'object', and of recognising the fluidity, complexity and context dependence of social identity have all had a significant impact on practices. Ideas about more fluid, provisional encounters between subjects and analysts are important. Furthermore, as Steve Hinchliffe (2001; 2003) has argued, contingent and incomplete encounters in the world involve not only relations between human subjects but the involvement of numerous corporal non-human and inorganic entities as we enter a 'cyborg' world (Haraway, 1991). This means that a wider range of 'actants' may have to be considered in any research encounter, including the many non-human species and objects with which we have daily interactions and which even make life possible.

It is almost impossible, however, to imagine how any researcher might avoid representation entirely, as well, I would suggest, impossible to evade the political implications of our work (although this is not what the non-representational theorists are arguing, but rather for a new form of less hierarchical politics). But however complex, fluid, multiple and contingent social relations are, the very act of naming something, perhaps even thinking about it, always, however temporarily, constitutes an ordering or a representation of a relationship. Despite this 'fixing', Thrift's (2003) advice to remain open to multiple possibilities in our research encounters is useful. He advises researchers to work in 'a spirit of generosity towards the world' acknowledging 'people's increasingly extended and unexpected capacities' (p. 74), as new technologies reconfigure and make possible different types of encounters.

Even so, it still seems unavoidable that, as critical academics committed to engaged scholarship, we have to intervene in the sphere of and through representations of the world, which may extend beyond the text to various forms of artistic representations, including art and dance, writing about and representing the diversity of human and non-human interaction in place in ways that contribute to a politically-progressive agenda.

As interviewers, we cannot and should not evade the academic and political responsibility of speaking for/on behalf of others through interpretations of the world that start, if not end, with the personal interactions that take place in interviews and the ways in which we interpret these through the lens of our philosophical, theoretical and political frameworks. What may seem like a simple methodological approach has significant implications, as I hope I have made clear in this chapter. In the shift from accepting interviews as a supposedly objective method to the acceptance of their interpretative status, the responsibilities of researchers for their work has greatly increased, as has the significance of their own positionality. While we are no longer able to hide behind a veil of invisibility and objectivity, we are now able to assert and take responsibility for claims for change and greater social justice. This transformation in the status of interviews and interviewers has been both the initiator and effect of that range of new questions that has dominated human geography over the last three decades, from the position of women in the 1970s to the significance of a multiple others, including non-humans, in the new millennium. It is a far more exciting discipline to be working within than it was thirty years ago.

NOTE

1. In a speech on 3 July 2000 Tony Blair referred to the shame of Britain's 'yobs', to 'drunken louts' and 'thug bars' and the need for 'zero tolerance on yobbery' (Coward, 2000). In the six years since then

there has been a constant reiteration of this discursive construction. In January 2006, the British Government published a 'Respect Action Plan' to tackle the anti-social behaviour of young people (www.respect.gov.uk).

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