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'It's great to have someone to talk to': the ethics and politics of interviewing women

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Janet Finch's chapter explores some of the ethical problems involved in interviewing women. Drawing on her own work in which she interviewed clergy wives, and mothers involved in playgroups, she looks at the development of trust in the interview situation, and the exploitative potential of this. As she points out, if you are a woman sociologist, reasonably skilled in the arts of qualitative research and semi-structured interviewing, it is the easiest thing in the world to get women to talk to you. While traditional methods textbooks encourage the development of rapport (while deploring any 'over-rapport'), there is little discussion, as Oakley (1981) and McRobbie (1982) have indicated, of the difficulties that can arise from this. Daniels (1967) in an illuminating discussion of the low-caste stranger in social research discusses one set of problems which can arise from the social relationships we have with those we interview. Janet Finch, in a rather different situation, discusses another. In the case of clergy wives, she was interviewing her peers; in the case of the playgroup mothers, women more powerless than she - an ambiguous situation as she rightly recognises. The mechanisms that relatively powerful individuals may be able to use when being researched discussed in Scot t's chapter were not open

to Finch's playgroup respondents. As Sjoberg (1967, p. xii) suggests, the ethical dilemmas in this sort of situation cannot be achieved through the repetition of the formula 'I am objective'. What we must do is to examine the impact of ethical and political decisions on social research.

The issues which I discuss in this chapter have been raised by my own experience of doing social research. of a .qualitative variety. In particular, my experience of interviewing .has raised a combination of methodological, personal, political and moral issues, upon which I find it necessary to reflect both as a sociologist and as a feminist. These issues have become focused by considering the extreme ease With which, in my experience, a woman researcher can elicit material from other women. That in turn raises ethical and political questions which I have found some difficulty in resolving. One reason for this difficulty is, I shall argue, that discussions of the 'ethics' of research are commonly conducted within a framework which is drawn from the public domain of men, and which I find at best unhelpful in relation to research with women.

I shall illustrate and discuss these issues by drawing upon two studies in which I was the sole researcher, and did all the interviewing myself. These are firstly, a study of clergy-men's wives and their relationship to their husband's work, which was based on interviews with 95 women; secondly, a study of 48 women (mostly working class) who were running and using preschool playgroups.' In both cases, ~he interviews were arranged in advance. I contacted prospective interviewees initially by a letter which introduced myself and the research, then made an appointment to interview them in their homes at a pre-arranged time. All the inter-views were tape recorded unless the interviewee requested otherwise and were based on a list of questions to be covered during the interview, rather than upon a formal question-naire. In the study of clergymen's wives, the interview was the first occasion on which we met. In the playgroup study, I had met some (but not all) of the interviewees during the

preceding two years, when I had made observational visits to the playgroups themselves.

The woman to woman interview

Both the clergymen's wives and the playgroups studied were concerned entirely with women; in both I used qualitative techniques including in-depth interviewing: and in both I talked to women in their own homes about aspects of their lives which centrally defined their identities as women – marriage, motherhood and childrearing. My consciousness of the special character of a research situation in which women talk to another woman in an informal way, and about these issues, was heightened by recalling Ann Oakley's (1981) discussion of interviewing women. Oakley takes the view that formal, survey-type interviewing is unsuited to the production of good sociological work on women. She prefers less-structured research strategies which avoid creating a hierarchical relationship between interviewer and interviewee. That sort of relationship, she argues, is inappropriate for a feminist doing research on women, because it means that we objectify our sisters.

I share Oakley's preference on both methodological and political grounds, and my own research has all been of the type which she recommends. I have also found, quite simply, that it works very well. Initially I was startled at the readiness with which women talked to me. Like every other researcher brought up on orthodox methodology textbooks, I expected to have to work at establishing something called rapport (Oakley, 1981). In my experience, such efforts are normally unnecessary when interviews are set up in the way I have described. Women are almost always enthusiastic about talking to a woman researcher, even if they have some initial anxieties about the purpose of the research or their own 'performance' in the interview situation. Their intentions are apparent, simply from the hospitality which one characteristically receives – an aspect of the research experience which Oakley (1981) notes is seldom mentioned in reports. In my study of clergymen's wives I was offered tea

or coffee and sometimes meals, in all but two instances; the same happened in the majority of interviews in my playgroup study. One is, therefore, being welcomed into the interviewee's home as a guest, not merely tolerated as an inquisitor. This particular contrast was demonstrated to me in graphic form when I arrived at one interviewee's home during the playgroup study, only to find that she was already being interviewed by someone else. This seemed like the ultimate researcher's nightmare, but in the end proved very much to my advantage. The other interviewer was in fact a local authority housing visitor, who was ploughing her way through a formal questionnaire in a rather unconfident manner, using a format which required the respondent to read some questions from a card ('Do you receive any of the benefits listed on card G?', and so on). My presence during this procedure must have been rather unnerving for the housing visitor, but was most instructive for me. I recorded in my field notes that the stilted and rather grudging answers which she received were in complete contrast with the relaxed discussion of some very private material which the same interviewee offered in her interview with me. My methodological preferences were certainly confirmed by this experience.

I claim no special personal qualities which make it peculiarly easy for me to get people to talk, but women whom I have interviewed often are surprised at the ease with which they do talk in the interview situation. One woman in my playgroup study (who told me that she was so chronically shy that when she had recently started a new job it had taken her a week to pluck up courage to ask how to find the toilet), said after her interview that she had surprised herself – it had not really felt, she said, as if she was talking to a stranger. Another woman in this study said that she found me an easy person to talk to and asked, 'Where did you get your easy manner – did you have to learn it or is it natural?' I quote these instances not to flaunt my imputed skills as an interviewer, but as instances which demonstrate a feeling which was very common among the women I interviewed in both studies – that they (often unexpectedly) had found this kind of interview a welcome experience, in

contrast with the lack of opportunities to talk about themselves in this way in other circumstances. Some variation on the comment 'I've really enjoyed having someone to talk to' was made at the end of many interviews.

How far does this experience simply reflect the effectiveness of in-depth interviewing styles *per se*, and how far is it specific to women? It seems to me that there are grounds for expecting that where a woman researcher is interviewing other women, this is a situation with special characteristics conducive to the easy now of information. Firstly, women mostly are more used than men to accepting intrusions through questioning into the more private parts of their lives including during encounters in their own homes. Through their experience of motherhood they are subject to questioning from doctors, midwives and health visitors: and also from people such as housing visitors, insurance agents and social workers, who deal principally with women as the people with imputed responsibility for home and household. As subjects of research, therefore, women are less likely than men to find questions about their lives unusual and therefore inadmissible. Secondly, in the setting of the interviewee's own home an interview conducted in an informal way by another woman can easily take on the character of an intimate conversation. The interviewee feels quite comfortable with this precisely because the interviewer is acting as a friendly guest, not an official inquisitor; and the model is, in effect, an easy, intimate relationship between two women.

Thirdly, the structural position of women, and in particular their consignment to the privatised, domestic sphere (Stacey, 1981), makes it particularly likely that they will welcome the opportunity to talk to a sympathetic listener. The experience of loneliness was common to women in both my studies. The isolation of women who are full-time housewives has been well documented by Dorothy Hobson, in a study of women whose circumstances were very similar to those in my playgroup study (Hobson, 1978, 1980). The loneliness experienced by clergymen's wives is less obvious at first sight, but in fact it has a very special character. Many of them adopt a rule that they should have no friends in the locality, for fear that they might harm their husband's work

by being seen as partisan (For discussion, see Finch, 1980). The consequences of this were described to me by one Methodist minister's wife as,

I agree if it's going to hurt people, if it's going to harm her husband's ministry, it's better not to have friends nearby. But I think it's terribly difficult, because I think a woman needs a particular friend. I've always tried not to make particular friends but as I say, you can't help being drawn to some people. But as I say, I try not to show it. I never sit beside the same person in a meeting. I never visit one more than anybody else.

The friendly female interviewer, walking into this situation with time to listen and guarantees of confidentiality, not surprisingly finds it easy to get women to talk. In one instance, a clergyman himself thanked me for coming to interview his wife because, he said, *he* felt that she needed someone to talk to. It is not, however, only in the few cases where one is clearly being used as a social worker that women's need to talk is apparent. Almost all the women in my two studies seemed to lack opportunities to engage collectively' with other women in ways which they would find supportive, "and therefore they welcomed the opportunity to try to make sense of some of the contradictions in their lives in the presence of a sympathetic listener. There seems no reason to doubt that most women who similarly lack such opportunities will also find such an interview a welcome experience.

For these three reasons, the woman-to-woman interview (especially when conducted in the settings and in the ways I have described) does seem to me to be a special situation. This is not to say that men can never make good interviewers, although practice in research teams does suggest that research directors often regard women as especially suited to this task, as Scott points out in her chapter. Men, as social workers or as counsellors, for example, can be very effective in getting both women and men to talk about intimate aspects of their lives. But systematic comparisons of men and women interviewers, in a range of research situations, are not possible because we lack sufficient studies or accounts of the research

process which consider the relationship of the gender of the interviewer to the research product. That is an interesting and important methodological issue; but my point about the special character of the woman-to-woman interview is as much political as methodological, and has particular resonance for any sociologist who is also a feminist. However effective a male interviewer might be at getting women interviewees to talk, there is still necessarily all additional dimension when the interviewer is also a woman, because both parties share a subordinate structural position by virtue of their gender. This creates the possibility that a particular kind of identification will develop.

In my own research experience, I have often been aware of such an identification, as women interviewees have begun to talk about key areas of their lives in ways which denote a high level of trust in me, and indicate that they expect me to understand what they mean simply because I am another woman. One example taken from each of my studies - both concerning the interviewees' experience of marriage - should serve to illustrate this. The first extract comes from the interview with the wife of an Anglican clergyman, living in a huge and decaying vicarage, in a mill village in the Yorkshire moors:

One big problem in being a clergy wife I feel is, at the odd time which happens in every marriage - and it happens in clergy marriages as much as it happens outside-- is that when you get the big bang in a marriage, when you get some sort of crisis, and I don't think a marriage ever gels until you've had a crisis in a marriage where do you go for advice? If you're like me, you'll ask your mother because it's an admission of defeat that you have a problem - a big enough problem to seek advice on - in your marriage. You can't ask the vicar or the vicar's wife because you are, by definition, criticising his curate, You cannot ask the bishop or the archdeacon because, again, you are casting some sort of slight on one of his priests who cannot manage his own marriage So who do you ask? -

I was very fortunate in that I knew the widow of a

clergyman who had no sort of direct tie with the church but had sort of been through the lot herself and could help me. I find this sort of person invaluable, but how many people manage to find her? Other than that, just who do you go to?

The second illustration is from the interview with a 24-year-old mother of two daughters under school age, living on a run-down council estate on the edge of an east Lancashire town,

Self: I know that the children are sort of small at the moment, but do you ever have any sort of hopes or dreams about what they might do when they grow up?

Interviewee: Yes, I'm always - Don't get married for a start. (To child) Not to get married, are you not! And have a career, with some money. And don't have a council house. Bet there's no such thing as council houses when they get older. But I don't want them to get married.

Self: No.

Interviewee: No but I don't, because I think once you get married and have kids, that's it. To a lot of women round here - when you see them walking past - big fat women with all their little kids running behind them. And I think, God. That's why I want to go to College and do something. But Fellas don't see it like that, do they? Like, he thinks it's alright for me just going back to work in a factory for the rest of my life, you know. But I don't want that. (To child) You have a career, won't you? Prime Minister, eh?"

Comments of this kind - albeit very differently conceptualised and articulated - would not have been elicited in a formal questionnaire nor if I, as interviewer, had been attempting to maintain an unbiased and objective distance from the interviewees. Nor, I suggest, would they have been made in the same way to a male interviewer. Comments like 'fellas

don't see it that way, do they?' and 'you can't ask your mother because it's all admission of defeat' indicate an identification between interviewer and interviewee which is gender specific.

That identification points to a facet of interviewing which I experienced strongly and consistently throughout these two studies: namely that the ease with which one can get women to talk in the interview situation depends not so much upon one's skills as an interviewer, nor upon one's expertise as a sociologist, but upon one's identity as a woman. In particular, I found that there was some unease in the interview situation if an interviewee was in some doubt about how to place me in relation to the crucial categories of marriage and motherhood. For example, during the three years when I was conducting the observational and interview phases of the playgroup study, I changed both my name and address. With several women who ran the playgroups, I noted some hesitation in their approach to me (comments like, 'You've moved, have you?') until I clarified that this was indeed because my marriage had ended. Other researchers have similarly reported that interviewees wanted to 'place' them as women with whom they could share experiences (Hobson, 1978; Oakley, 1981). Male interviewers of course may also be 'placed' (by their occupational or family status, for example). But again, being 'placed' as a woman has the additional dimension of shared structural position and personal identification which is, in my view, central to the special character of the woman-to-woman interview.

The basis of trust and its exploitative potential

From an entirely instrumental point of view as a researcher, there are of course great advantages to be gained from capitalising upon one's shared experiences as a woman. The consequences of doing so can be quite dramatic, as was illustrated to me in my study of clergymen's wives. As an anxious graduate student, I agonised over the question of whether I should reveal to my interviewees the crucial piece of information that I myself was (at that time)

also married to a clergyman. Wishing to sustain some attempt at the textbook, 'unbiased' style of interviewing (which Ann Oakley, 1981, has so effectively exposed for the sham it always was), I initially merely introduced myself as a researcher. I found however, that before I arrived for the interview, some people had managed to deduce my 'true' identity. The effects of this unmasking so clearly improved the experience for all concerned that I rapidly took a decision to come clean at the beginning of each interview. The consequence was that interviewees who had met me at the front door requesting assurances that I was not going to sell their story to a Sunday newspaper, or write to the bishop about them, became warm and eager to talk to me after the simple discovery that I was one of them. Suspicious questions about why on earth anyone should be interested in doing a study of clergymen's wives were regarded as fully answered by that simple piece of information. My motives, apparently, had been explained. I rapidly found this a much simpler strategy than attempts to explain how intellectually fascinating I found their situation. The result of course was that they talked to me as another clergyman's wife, and often they were implicitly comparing their own situation with mine. The older women especially made remarks such as 'possibly you haven't come across this yet' or 'of course I suppose it's a bit different for you younger ones now'. The general tone of these interviews often made me feel that I was being treated as a trainee clergyman's wife, being offered both candid comment and wise advice for my own future benefit. In several cases, the relationship was reinforced by gifts given to me at the end, and I became quite good at predicting those interviews where the spoils were likely to include a chocolate cake or a home-grown cabbage as well as the tapes and field notes.

One's identity as a woman therefore provides the entree into the interview situation. This obviously was true for me in a rather special way in my study of clergy wives, but that does not mean that only interviewers whose life circumstances are exactly the same as their interviewees can conduct successful interviews. It does mean, however, that the interviewer has to be prepared to expose herself to being 'placed'

as a woman and to establish that she is willing to be treated accordingly. In the case of my playgroup study, my life situation was rather different from my interviewees': I did not have young children, and by the end of the study I was not married either. However, this seemed no real barrier to encouraging women to talk freely in the interviews. In the previous two years, through my visits to the playgroups, I had already established myself as a figure on their social scene, and they had taken the opportunity to make key identifications of me as a woman. Once these identifications are made, it does indeed seem the easiest thing in the world to get women to talk to you.

The moral dilemmas which I have experienced in relation to the use of the data thus created have emerged precisely because the situation of a woman interviewing women is special, and is easy only because my identity as a woman makes it so. I have, in other words, traded on that identity. I have also emerged from interviews with the feeling that my interviewees need to know how to protect themselves from people like me. They have often revealed very private parts of their lives in return for what must be, in the last resort, very flimsy guarantees of confidentiality: my verbal assurances that the material would be seen in full only by me and the person transcribing the tapes, and that I would make any public references to them anonymous and disguised. These assurances were given some apparent weight, I suppose, through my association with the university whose notepaper I used to introduce myself. There were, in fact, quite marked differences in the extent to which my various interviewees requested such guarantees. None of the working-class women in my playgroup study asked for them, although one or two of the women in my middle-class comparison playgroup did so. A number of clergymen's wives asked careful questions before the interview, but I found that they were easily reassured, usually by the revelation that I too was a clergyman's wife, rather than by anything I might have tried to indicate about the professional ethics of a sociologist. With them, as with the women in the playgroup study, it was principally my status and demeanour as a woman, rather than anything to do with the research process, upon which

they based their trust in me. I feel certain that *any* friendly woman could offer these assurances and readily be believed. There is therefore a real exploitative potential in the easily established trust between women, which makes women especially vulnerable as subjects of research. The effectiveness of in-depth interviewing techniques when used by women researchers to study other women is undoubtedly a great asset in creating sociological knowledge which encompasses and expresses the experiences of women (Oakley, 1981). But the very effectiveness of these techniques leaves women open to exploitation of various kinds through the research process. That exploitation is not simply that these techniques can be used by other than *bona fide* researchers: but it is an ever-present possibility for the most serious and morally upright of researchers, feminists included. It seems to me that the crux of this exploitative potential lies in the relationship established between interviewer and interviewee. I would agree with Oakley that the only morally defensible way for a feminist to conduct research with women is through a non-hierarchical relationship in which she is prepared to invest some of her own identity. However, the approach to research - and particularly to interviewing - which this requires can easily be broken down into a set of 'techniques', which can then be divorced from the moral basis in feminism which Oakley adopts. These techniques can be used to great effect to solicit a range of information (some of it very private), which is capable of being used ultimately against the interests of those women who gave it so freely to another woman with whom they found it easy to talk. The prospects for doing that clearly are magnified when (as is so often the case) women interviewers are not themselves the people who will handle and use the data they have created. In those circumstances, women interviewers and research assistants may find that the material which they have created is taken out of their control, and used in ways of which they do not approve and which seem to them to be against the interests of the women whom they interviewed. I have never been in that situation, but I have found that the issues are by no means avoided in research settings such as I have experienced, where I was both interviewer and sole researcher.

Ethics, morals and politics in research
Moral dilemmas of the kind to which I allude are commonly discussed in research textbooks under the heading of 'ethics'. These debates have been well summarised by Barnes (1979). They are formulated in terms of the rights to privacy and protection of those being researched, which are sometimes thought to be assured by adherence to a code of professional ethics. So, are the moral dilemmas raised when women interview women to be resolved by a greater sensitivity to women's right to privacy? Or perhaps a special code of ethics for feminists to adopt if they choose? I think not. I find the terms in which these debates about ethics are constituted unhelpful in relation to women, and it is instructive that the issue of gender is rarely mentioned. Barnes's own discussion conceptualises it as an issue of the rights of the 'citizens' - certainly an advance upon the term 'subject' or even 'respondent' for the people being researched. None the less, 'citizen' is a concept drawn from the public domain of men, in particular from the political arena, from which women have always been excluded (Stacey and Price, 1980), and it implies a framework of formalised rights and obligations, along with procedures of legal redress. Women are unlikely to feel comfortable with such procedures, and do not necessarily have access to them. Barnes is, however, reflecting the essentially male paradigms in which most debates about 'ethics' are conducted. For example, most such discussions tend to focus upon the point of access or of data collection rather than upon the use of the material. These discussions implicitly assume that research 'citizens' *anticipate* potentially harmful uses to which such data can be put, and take action accordingly. Most women are unlikely to be in a position where they can anticipate the outcome of research in this way, since they have little access to the public domain within which the activity of research can be contextualised. When discussing ethical issues about the use of research data, Barnes argues that the tension is between on the one hand the desire of citizens to protect their own interests in the short term, and, on the other hand, the long-term interests of sustaining informed criticism in a democratic society,

which suggest that the results of research should be published whatever they are. Presumably few male research subjects are wildly enthusiastic about having their short-term interests sacrificed to this latter aim, but women are especially vulnerable. The 'democratic society' where this critical discourse is conducted is of course the public domain of men, where the 'debate' is largely conducted *by* men, in their own terms. As Hanmer and Leonard point out in their chapter, the specific interests of women are unlikely to be voiced there and therefore little protection is available to women once the outcome of research has entered the public 'debate' at that level. The sociologist who produces work about women, therefore, has a special responsibility to anticipate whether it could be interpreted and used in ways quite different from her own intentions (an issue discussed in Platt's and Roberts's chapters). '

This highlights a point which is often overlooked in discussions of research ethics, but which is crucial to a feminist doing research on women: namely that collective, not merely individual, interests are at stake. The latter may be relatively easily secured with guarantees of confidentiality, anonymity, codes of ethics and so on. It is far more difficult to devise ways of ensuring that information given so readily in interviews will not be used ultimately against the collective interests of women.

In both my playgroups and clergymen's wives studies, I was very aware that aspects of my data could be discussed in such a way as potentially to undermine the interests of wives and mothers generally, if not necessarily the specific people I had researched in a direct way. For example, many clergy wives expressed satisfaction and contentment in living lives centred around their husband and his work (in which they essentially acted as his unpaid assistants). This could be used to argue that *most* women would be much happier if only they would accept subordinate and supportive positions instead of trying to establish greater independence from their husbands. My developing commitment as a feminist made me very unwilling to see my work used to support such a conclusion. Similarly, in my playgroup study, the character of the playgroups which I was studying certainly was in most cases wildly divergent from bourgeois standards of childcare and

pre-school developmental practice. This evidence, I feared, could be used to reinforce the view that working-class women are inadequate and incompetent child carers. Again, I felt that I was not willing to have my work used to heap further insults upon women whose circumstances were far less privileged than my own, and indeed for a while, I felt quite unable to write *anything* about this aspect of the playgroup study.

In both cases, my commitments as a feminist raised moral questions for my work as a sociologist. In both cases, however, the consequences were that I was pressed into looking more carefully at my data, into thinking through the dilemmas I had raised for myself as intellectual as well as moral issues, and into contextualising my problem in ways which I might not otherwise have done. In both cases, I eventually resolved the moral issues sufficiently to be able to write about these studies. Briefly, in the case of the clergymen's wives, I found that I had to look more closely at the structural position in which these women were placed, and to make a clear distinction between structural position and women's own experience of it. This enabled me to see that evidence of women successfully accommodating to various structural features of their lives in no way alters the essentially exploitative character of the structures in which they are located. In the case of the playgroups study, I eventually saw that I should really be taking more account of the culture and character of the formal educational system, for which the playgroups were intended (in the eyes of the mothers who used them) as a preparation. Rather than focusing on the apparent inadequacies of the groups run by working-class women, I needed to locate the disjuncture between playgroups and schools as part of the continuing cultural imperialism of bourgeois practices within formal schooling itself. In neither case would I claim to have found perfect solutions to the dilemmas which my own work raised for me. But, given that the period covered by these studies was also a period when my own commitment to feminism was developing, partly as a result of radical changes in my own life, they were quite simply the best solutions I could manage at the time. I am also certain that in the pro-

cess, I was producing better sociology.

The dilemmas which I have encountered therefore raise the possibility of betrayal of the trust which women have placed in me when I interviewed them. I do not really mean 'betrayal' in the individual sense, such as selling the story of someone else's life to a Sunday newspaper. I mean, rather, 'betrayal' in an indirect and collective sense, that is, undermining the interests of women in general by my use of the material given to me by my interviewees. It is betrayal none the less, because the basis upon which the information has been given is the trust placed in one woman by another. In such a situation, I find sanitised, intellectual discussions about 'ethics' fairly irrelevant. I have preferred to call my dilemmas 'moral' ones, but in fact they are also, it seems to me, inherently political in character. They raise the 'whose side are we on?' question in a particular form which relates to gender divisions and to the study of women in that context.

It has become commonplace in discussions of research ethics to distinguish between research on powerless social groups (where rights to privacy, protection, and so on, are of great importance) and research on the powerful, where such considerations can be suspended, on the grounds that the groups concerned already have enough privileges or are well able to protect themselves, or have exposed themselves to legitimate scrutiny by standing for public office (Barnes, 1979; Bulmer, 1982). If one takes the view that the powerful are fair game for the researcher, then the issues of gender inevitably must be raised: in a patriarchal society, women are always relatively powerless. Women, therefore, with perhaps very few exceptions, can never be regarded as fair game. Further, precisely because issues of power are central to gender relations, one cannot treat moral questions about research on women as if they were sanitised 'ethical issues', divorced from the context which makes them essentially political questions.

A feminist sociologist of course will be 'on the side' of the women she studies. This stance is entirely consistent with major traditions in sociological *research*, in which - as has been acknowledged from Becker onwards - the sociologist sides with the underdog (sic.) (Becker, 1911 7; Gouldner, 1973;

Barnes, 1979). One essential difference, however, is that a feminist sociologist doing research on women actually shares the powerless position or those she researches, and this is often demonstrated in the research context itself, for example through the under-representation of women in the institution which sponsors the research, and their location often at the most junior levels in her department, her section, or the research team. This experience of shared powerlessness between researchers and researched is seldom paralleled for men unless they are, for example, black sociologists doing work on race, or disabled sociologists researching disability. Siding with the people one researches inevitably means an emotional as well as an intellectual commitment to promoting their interests. How else can one justify having taken from them the very private information which many have given so readily? I find rather unconvincing all argument which says that I should be content with having added to the stock of scientific knowledge. Rather, I would endorse Oakley's position that, as a feminist and a sociologist, one should be creating a sociology *for* women- that is a sociology which articulates women's experiences of their lives - rather than merely creating data for oneself as researcher (Oakley, 1981). How far this has been accomplished is the criterion which I would apply to my own sociological work on women, and to that of other people. This seems to me a more fruitful way to address the moral and political dilemmas I have identified than, for example, writing a separate code of ethics for feminists to follow.

This moral and political position which I (and other feminist researchers) have adopted may provoke the charge that we are not serious sociologists, but merely using our work to promote our politics. Our credibility may be questioned by those who see feminist (and indeed similar) commitments as incompatible with good academic work. Helen Roberts already has produced a convincing answer to such charges, showing that a commitment to taking people's experiences seriously *is* essentially a political activity but is not peculiar to feminist sociologists, nor do we become less professional or rigorous as a result (Roberts, 1981a). Indeed, it seems clear to me that *all* social science knowledge is

intrinsically political in character (Bell and Newby, 1977), and is undertaken from a standpoint which embodies some material interests, whatever the claims of the researcher. As Maureen Cain and I have argued elsewhere, this does not mean that the knowledge we produce cannot be evaluated and appraised by others. Indeed, recognising the intrinsically political nature of both theory and data means that the sociologist has a great responsibility to be open and scholarly about her procedures and her conclusions (Cain and Finch, 1981). It does mean, however, that sociologists who are also feminists need not be defensive about the relationships of our political commitments to our work, nor embarrassed when we resolve the moral dilemmas which it raises by frankly political stances. In so doing, I would argue, not only do we avoid compromising our feminism, but we are likely to produce more scholarly and more incisive sociology.

Notes

1. The study of clergymen's wives was undertaken for my PhD, partly funded by an SSRC studentship. The fieldwork was undertaken mainly in West Yorkshire between 1971 and 1973, and covered wives of ministers in four denominations. An account of the study and my conclusions can be found in Spedding (1975) and Finch (1980). The playgroup study was funded by an SSRC grant, and was conducted in Lancashire between 1978 and 1980. It was a study, based on two years' observation followed by interviews, of five self-help playgroups, four of which were in inner urban areas or on council estates, and the fifth was a comparison group in a middle-class suburb. An account of this study can be found in Finch (1981, 1983b, 1983c).
2. Discussion of the points referred to briefly here can be found in Finch (1980, 1983a, 1983c) for issues arising from the clergymen's wives study; and Finch (1981, 1983b) in relation to working class women and preschool playgroups.