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4. Fieldworker feelings as data: 'emotion work' and 'feeling rules' in first person accounts of sociological fieldwork

Elizabeth H. Young and Raymond M. Lee

Introduction

The personal dimension in fieldwork has become an increasingly documented focus in methodological writing (Emerson 1981), and first person accounts of fieldwork have become popular. Such accounts have a variety of forms ranging from the heroic (Van Maanen 1988) to the confessional (Johnson 1975), from the personal and autobiographical (Wilkins 1993) to the more detached and descriptive (Whyte 1981). Accounts often refer, directly or indirectly, to emotions experienced by the fieldworker during fieldwork. In particular, feelings of stress, anxiety, and physical discomfort associated with fieldwork are well-documented in the methodological literature (Hammersley 1984, Sanders 1980, Cannon 1989).

Descriptions of fieldwork have been criticised for being anecdotal and lacking a comparative theoretical base (Dingwall 1980). To try and understand more fully the role of emotions in fieldwork, we suggest that the methodological basis upon which first person accounts are generated needs to be examined. More particularly, this chapter examines how far Hochschild's concepts of 'emotion work' and 'feeling rules' can be used to inform first person accounts of fieldwork. Hochschild describes 'emotion work' as 'the act of trying to change in degree or quality an emotion or feeling' (Hochschild 1979: 561). Emotion work can be accomplished by evocation, the attempt to create a desired feeling, or by suppression, the attempt to diminish an undesired feeling. Hochschild outlines three techniques for carrying out such work. These are cognitive, bodily and expressive. She goes on to suggest that emotion work is directed by 'feeling rules', the social guidelines that influence how we try to feel. These feeling rules are related to particular perspectives or ideologies or, as Hochschild puts it, feeling rules are 'the bottom side of ideology'. We suggest that sets of feeling rules are associated with particular methodological perspectives and we argue that the emotion work involved in fieldwork may be more problematic if the field experience challenges implicit methodological tenets held by the fieldworker. In these situations there are no clear feeling rules by which to operate. The fieldworker is left asking 'How should I feel?' or 'Do my feelings fit?'

In what follows, an account of the first author's fieldwork is presented and then used to illustrate the complex relationship between emotions felt and emotions acknowledged. We argue that the production of first person fieldwork accounts is part of the process of doing research and as such the accounts should be treated as raw data which require further reflective analysis. Their value comes when they are analysed as data in an attempt to gain a sociological understanding of the work carried out by a practising fieldworker, in the same way that any group of people's accounts would be studied by sociologists. The accounts have to be theorised and contextualised. Following from this we go on to work with Hochschild's suggestion that emotion work can be carried out by others on the self. We suggest that reflective work on first person accounts can be carried out by the fieldworker her/himself or as a collaborative exercise.¹ We conclude with a consideration of the potential of providing fieldworkers with a variety of more structured ways, including debriefing sessions, to reflect on their feelings within a sociological context.

We begin by reviewing how the role of emotion has been treated within a number of methodological traditions. We begin with a comment by Hochschild who observes, 'The focus of Mead and Blumer on conscious, active and responsive gestures might have been most fruitful had, not their focus on deeds and thoughts almost entirely obscured the importance of feelings' (1979: 555). This lack of interest in emotions carried over into the methodological writings associated with the 'neo-Chicagoan' tradition, the dominant postwar approach to field research in sociology associated with writers like Everett Hughes, Howard Becker, Herbert Gans and others. Fieldwork accounts in this tradition rarely make explicit the role of emotions in fieldwork. There is the occasional suggestion that fieldworkers may self-select themselves on what might be emotional grounds. Gans (1982: 60) observes for example that 'My hunch is that fieldwork attracts a person who, in Everett Hughes' words, "is alienated from his own background", who is not entirely comfortable in his new roles, or who is otherwise detached from his own society; the individual who is more comfortable as an observer than as a participant'. Generally, however, fieldwork accounts tend to describe 'fieldwork troubles' and methods for their resolution. (See Whyte 1981 for a classic example of this genre.) This is a tradition which encourages the normalisation of feelings. That is, the researcher becomes accepting of the local research setting, and in doing so associated emotional problems are overcome. To facilitate this normalisation as well as to prevent 'overreport' the neo-Chicagoans tended to advocate a marginal role for the researcher (Miller 1952), and the fieldwork accounts they provide tend towards a retrospective and detached description of problems overcome. The emotion work undertaken by the field worker is minimised, perhaps hidden in a semi-embarrassed way in a somewhat sanitised account:

One of my mentors has commented that what typically goes into 'how the study was done' are 'the second worst things that happened'. I am inclined to believe that his generalization is correct. What person with an eye to his future, and who wishes others to think positively of him, is going to relate anything about himself that is morally or professionally discrediting in any important way? This is especially the case since field work tends to be performed by youngish persons who have longer futures to think about and less security about the shape of those futures. We delude ourselves if we expect very many field workers actually to 'tell all' in print (Lofland 1971: 132-3).

By contrast, a later generation of fieldworkers associated with what became known as 'existential sociology' (Adler and Adler 1987) saw emotion as being central to the research process. Johnson (1975) carried out participant observation on child welfare procedures in a social work agency. In an excerpt from his fieldwork diary Johnson describes an epiphany which moved him towards a growing awareness of and greater commitment to bridging the gap between rational self-presentation of fieldwork and the private feelings of the fieldworker. A dramatic and emotional custody case had just finished in which the court had found against Johnson's client. He goes on:

And how did the cool, objective, calculatingly rational social scientist react to all this? Having quickly analyzed all of the formally rational courses of action open to me, and feeling confident that I had controlled for all spurious relations, I also began to cry. As we say in the trade, I presented the appearance of one who had lost all self control. And then, when alone on the grounds of the juvenile facility several minutes later I presented the appearance of a formally rational (explicitly deleted) social scientist beating his (deleted) fist against a tree. Shortly after that, I doubled over and puked my guts out. What am I doing here anyway? To hell with the appearance of sociology and the horse it rode in on! (1975: 158-9).

The emotional experiences of the fieldworker have a central role to play in feminist methodology. In an account of her fieldwork experience with pregnant women, Wilkins describes her deep anxiety about carrying out the fieldwork:

I displayed all the physiological symptoms of acute anxiety: including sweating, mind blitzing and a dry mouth. I resorted to taking a 'panic pack' on my observational outings: this included talcum powder, antiperspirant, changes of clothes, chocolate bars and mints (1993: 95).

However, this emotion work was theoretically contextualised for her when she discovered the feminist literature on methodology. Wilkins felt she

had 'come home' on reading this literature; it made explicit for her a set of feeling rules and provided a theoretical justification for feeling as she did. This literature embraces the emotional sensibility of the fieldworker and the respondent. A distinguishing factor is common identity – woman to woman, sisterhood, common experience and capitalising on 'personhood' (Finch 1984, Oakley 1981, Stanley and Wise 1983). Derived from this is a focus on the personal relationship between the interviewer and the interviewee, rather than on the research setting. The assumption in this is that women, researchers and respondents, have a common experience which is fundamental to the research; difference is counteracted by common experience. The analysis of fieldwork in this tradition is often accomplished through autobiography (Stanley 1992).

There is some tendency for each of the methodological traditions described above to emphasise particular aspects of the fieldwork process. The neo-Chicagoans tended to focus on researcher *involvement* in the research process and with those being studied. Existential sociologists tend to emphasise the *comfort* of the researcher as indexed by feelings of stress, anxiety, the presence of physical ailments and so on. For feminist sociologists, *identification* with those studied is a central concern. We would argue that in so doing, each tradition emphasises different feeling rules and mandates different kinds of emotion work. Of course, categorising fieldwork experiences into neat methodological packages is problematic (Hunt 1989). As Devereux (1967: 134) points out 'The influence of . . . scientist's ideology on his (*sic*) work is particularly difficult to explore, partly because he has little interest in, or awareness of, his own ideology'. In any case, fieldworkers do not always have an allegiance to a particular methodological perspective. Or the experience of fieldwork, and coping with unexpected situations, can challenge previously held methodological allegiances. Nevertheless, we suggest that certain kinds of emotion work may need to be done where the feeling rules associated with various methodological approaches come into conflict, and that the tensions between involvement, comfort and identification need to be managed. As Hochschild states 'Sets of feeling rules contend for a place in people's minds as a governing standard with which to compare the actual lived experience' (1979: 567).

One difficulty in examining the role of emotions in fieldwork relates to the status of first person accounts themselves. In the published accounts there seems to be a tendency for fieldworkers both to overanalyse and to underanalyse their own feelings. Thus, analytic procrastination may be attributed, for example, to deep conflicts over the researcher's sexuality (see *e.g.* Kreiger 1985), rather than being seen as a common aspect of many work situations, the delaying of the inevitable start to a substantial task. Alternatively, anxiety-reducing rituals associated with fieldwork tasks (Wilkins 1993) may be glossed as 'needs', rather than as the 'neu-

rotic' symptoms' likely to be seen by a psychoanalytically-oriented commentator like Hunt (1989). What we have in mind here is that first person accounts often contain statements of causal attribution (I felt X *because of* Y). the difficulty is that alternative accounts or explanations for these processes are rarely offered in the first person account. Alternative explanations have to come from the reader. Usually there is little basis for preferring one interpretation over another. Traditionally in qualitative research of this kind appeal is made to 'ethnographic authority' the argument that the fieldworker was there and has better purchase on what happened than a third party. The point we want to make is that there may be all sorts of reasons why fieldworkers may not be reliable informants on their own actions.² No claim is made that the account given in this chapter escapes problems of over- or underanalysis, but in order to try to understand more fully the role of emotions in fieldwork we suggest the methodology by which first person accounts are generated needs to be examined.

Because of the sensitive nature of the research discussed here, the emotional impact of the study on the fieldworker (and on the respondents) was a central concern. Emotional reactions to the research were charted in a fieldwork diary and in debriefing interviews which the first author held with her supervisor.³ In what follows we reflect on an account distilled from this material. In so doing we treat the fieldwork account as data. However, returning to the account and using it as a source of data to explore the ideas of emotion work and feeling rules in fieldwork involves a complex set of relationships. Reading again the diary and transcripts, this time with a theoretical agenda on the part of the first author, could lead to accusations of selection and reconstruction. However it can be argued that returning to a research account and using that as a source of data makes more explicit the actual processes often undertaken in research writing (Kreiger 1985). Kohli, when considering the process of research account writing, points out that what is salient in the present is used to select and construct in the text relevant elements of the past (Kohli 1981). Working with another author, not involved in the fieldwork or the research management, adds another dimension. It, hopefully, enables a form of secondary analysis to be applied to the fieldwork account.

The research described below in the first person account was concerned with the role of friendship for women who were dying. In this study issues to do with the meaning of death and the boundaries of friendship were explored. This involved interviewing dying women and their nominated friends. Included in these interviews were questions about borrowing and lending money and help with intimate caring tasks. The work was an attempt to get closer to a sociological understanding of the dying experience from the dying person's perspective and the perspective of a friend,

rather than relying on retrospective accounts from family members. It was felt that insights, valuable to the research community and palliative care practitioners, could be obtained by utilising this prospective approach. However, the research team was aware of the sensitive nature of such a direct approach and in order to mitigate any possible adverse effects worked very closely with the hospice staff at each fieldwork site. In the following section the first author's account of this fieldwork is presented. As a direct, account it is written in the first person singular.

The fieldwork account

Writing in the first person for my fieldwork diary feels strange to me. The professional is very obviously becoming the personal. This first person account feels like crossing over boundaries. As well as keeping a diary, I am also having debriefing interviews with my manager and supervisor.⁴ These interviews are tape recorded and transcribed. The debriefing interviews bring out of me things I haven't previously written or consciously thought about. I have had three of these interviews over the fieldwork period. The second interview which took place two-thirds of the way through the fieldwork starts with me saying: 'I don't know what I want to say but I know I need to talk. . . . I have become less self-conscious as the months have gone by. The diary has become a vent for letting off steam and working out what I feel about the fieldwork, and the debriefing sessions are a welcome space to talk about me and my research. So, having begun to get over the self-conscious act of writing the diary and talking about myself, I then begin to exploit that very self-consciousness. In fact, doing this takes me nearer to, and further away, from my feelings about the whole thing. I don't want to overplay the sensitive nature of the research topic, dramatise my experiences, or hide behind the potential sensationalism of the topic. One thing I do know I feel is that first person accounts of fieldwork can leave the reader with the drama of the research experience but without the methodological substance. The more sensitive the research topic the more potential to do this.

The dilemma I mentioned above, about professional forays into my feelings via the fieldwork diary and debriefing interviews, are somewhat resolved for me by thinking, that instead of just 'baring my soul', I am attempting to ground the experiences in a sociological context. For me it is too personal to vent my feelings to a professional audience without some end product. I am also more than aware of presentation of self to peers. I'm not knowingly going to make myself seem foolish. I really want to be able to extrapolate from some of my feelings and experiences of fieldwork to a wider sociological arena – otherwise I feel I am not capitalising on the privileged position of interviewing women who are dying.

I am carrying out a series of depth interviews with dying women, their friends if they nominate a friend, and in a possible third of a series of three interviews, a dual interview with the friend and the dying woman. So I am not 'in the field' in the true ethnographic sense. I often have to travel to conduct interviews. Whether on the train or in the car, the journey itself feels significant. I am away from my normal contacts and routines. I am thinking about the interview. The car particularly becomes like a microcosm, a small world of my own. I carry food and drink, not least because for the majority of my fieldwork time I have been pregnant or breastfeeding. I have the tapes of previous interviews playing and, after the interview, I stop and write in the diary as a way of finishing the interview. So, for me, I am in the field, and this is particularly the case if I am away overnight.

Although the ethnographic literature on fieldwork has a very practical grounded style which makes it is very readable and very absorbing, it is not of direct relevance to me for two main reasons. Firstly, I am not carrying out participant observation. Most of the literature on fieldwork seems to be about participant observation rather than a series of depth or long interviews. Secondly, I cannot live with, and live like, women who are dying in their own homes. In the immediate sense I am not dying. Any attempt at relating on this level would be crass and insensitive. In fact this feeling has been supported many times by the women themselves who do not like people who say 'I know how you feel'. Platitudes like this were referred to with frustration and sometimes anger.

I was finalising details of the research design at the same time as submitting applications to local ethical committees for four fieldwork locations. Local ethical committee approval was required as I was randomly sampling patients' names from hospice home care patient lists. Ethical considerations were uppermost in the research team's mind. As a result, the research design was influenced by our understanding of what would be deemed ethical. After a long question and answer session with one of the ethical committees, the details of the research design and consent form appeared to me to be secondary to the committee. Their decision, after lengthy deliberations, seemed to be based on whether they could trust me as the researcher. My initial reaction was pleasure as they had accepted the proposal. But the emphasis on me and my sensitivities to the interviewees felt quite onerous. I felt I couldn't hide behind procedures as set out in the research design. My judgement was being put on the line. In particular they had wanted reassurances that I would not use the question 'What does your death mean to you?' if I didn't feel it was appropriate. But there were no prescribed indicators for 'inappropriateness' on which I could rely.

This emphasis on the sensitivity of the research begins with introducing the research to the potential interviewee and gaining consent. It became

clear very quickly that while women were consenting at the beginning of the interview, they had no real conception of what was in store. I felt this was not because of any knowing deception on my part but because the women were unaware of what they felt until actually asked. So I developed the technique of obtaining consent from the woman throughout the interview. Every time we moved on to a new topic or the line of questioning became more intense I checked with the woman if she wanted to go on. In the interviews women used my checking whether they wanted to continue as a breathing space in which they composed themselves before continuing. Women who were talking freely seemed to appreciate that, by checking with them, I was acknowledging the effort it required from them to talk. The women who were more reticent caused me to question my interviewing skills. Should I be probing more? Would another interviewer manage to elicit fuller responses? Was I backing off because I was a sensitive interviewer who was taking my lead from the woman, or was I feeling uncomfortable myself and therefore backing off? I began to realise that I had to be more conscious about what made me feel comfortable in the interview setting. Was my feeling comfortable and at ease, a good enough indicator of whether to probe or not? Was my feeling comfortable going to result in an ethical interview? It was only very near the end of the fieldwork when I was having a debriefing interview that it dawned upon me that the relationship between my comfort factor and the interview was complex and significant.

This is illustrated by the interview with Cheryl, one of the respondents who was dying.⁵ Cheryl said, right at the beginning of the interview, that she would not be happy borrowing money from friends and that she had never had any experience of doing so. She went on to say that as far as money was concerned she was very independent. Further into the interview she explained how a friend had paid for her to have an operation which cost £2,000. This piece of information came in the middle of a long description by Cheryl of the onset of her illness. I was immediately aware of the apparent discrepancy in the two pieces of information Cheryl had proffered about money transactions. However, I chose not to follow this up at any time throughout the rest of the interview. I was not secure in that interview situation because I sensed that Cheryl was not completely relaxed. I didn't want to embarrass her, or myself, by drawing attention to the potential conflicting pieces of information. There may well have been a very plausible explanation. But I decided the risk of probing was too great, potentially too exposing.

The second interview was with a dying woman, Katherine. Before the interview started Katherine had said that she was interested in research and had herself carried out some interviews. I felt comfortable that she had an understanding of the research process and this would enable her to consent or not from an informed standpoint. I interviewed her in two

sessions because she had a lot to say and each interview took approximately two hours. Three-quarters of the way through the first interview Katherine had said that her friend Helen had protected her when she had an episode of illness. Helen had done this by not talking about any of her own problems. As Katherine became stronger Helen had talked more about her own life. Very near the end of the first interview Katherine had talked about how she had been irritated by her husband's attempts to protect her, for example, by not even letting her load the washing machine or be involved in decisions about whether their son should take extra French at school. I picked up on this and asked Katherine how she reconciled her different reactions to her husband and to Helen. At the beginning of the second interview I had the opportunity to ask her how she had felt following the first interview. She said that she had felt disturbed by her different reactions to her friend and to her husband and this had troubled her. Later she reported to her MacMillan nurse that she had been quite troubled by this exposure. I had felt comfortable during the interview and so I had followed up on something I felt was interesting. By coincidence, because I was visiting Katherine twice, I had the opportunity of finding out how she had felt about my interviewing. My comfort factor was not a good indicator of interviewee comfort. Was it insensitive to probe with Katherine? Was it bad interviewing not to probe with Cheryl?

The importance and sensitivity of the research topic seemed to make everyone focus on doing a 'good job'. My problem was knowing what that was. I began to think about the 'good' interview. What is a good interview? I knew about the technical references to interviewing skills. I had been on depth interviewing courses. I had experience of interviewing in several settings. But the 'good' interview remained illusory and this became a major source of anxiety for me. In fact it wasn't only me that was aware of this, the women judged themselves as 'good' interviewees, or not, in terms of their perceived ability to express themselves. They were often keen to do justice to the subject matter, which because of its existential nature, was often difficult for them. This was all compounded as both the women and I knew I couldn't go back to them at a later date since many of them would have died.

I am also getting clues about my interviewing technique from the women talking in the interviews about their relationships. This realisation made my reading of the literature on relationships between interviewer and interviewee very focused. I could not relate to the interviewee on the level of our common health status and other characteristics like gender, race and age, felt secondary to the topic. I feel I am the professional researcher come to do a job when I go and interview and I sense that is what the women expect. This 'apartness' from those who are dying has been, and continues to be, a pervading theme in any methodological or

theoretical aspect of my work. The chasm between a dying woman and a healthy woman interviewer makes other commonalities seem, if not banal, secondary. To compensate for this 'separateness', I tend to talk after the more formal interview in a social way by including information about myself which I had purposefully withheld during the actual interview.

The fact that I was obviously pregnant when doing some of the fieldwork provided a point of contact for a lot of the women and myself. Within the research team we had discussed whether this would be an advantage or a disadvantage. Would it be seen as such an obvious sign of new life to a dying woman? In actual fact it seemed to be facilitating. In two cases I revisited the women for a dual interview after the baby was born, having interviewed them first separately when I was 8 months pregnant – and huge. The women had not realised I was pregnant. Understandably, they had been so absorbed in their own agendas they did not notice. However, it proved to be much harder when returning to the field after maternity leave. On more than one occasion, when I talked about my baby as part of the presentation of myself, I was greeted by some quite hostile responses. The women felt I should not be working with a young baby. To them it seemed like a wasted opportunity, something acutely pertinent to dying women. From an interviewing point of view I had not lost any data, as the exchange happened at the end of the meeting. But I felt awful, I had taken so much, and what I had offered had been rejected. I learnt to be very selective when imparting information about myself. So, even at that stage of the interview when I was being more social, I was very circumspect. Everything I talked about was taken from the women. For example one woman had a camper van and had talked about this in the interview. My husband has a camper van. It was a safe topic of conversation. It was not however something I would have chosen as representing me. It placed me in a very peripheral part of my life.

My first interview was problematic. As a social interaction it happened without obvious mishaps. I went through all my question areas and felt comfortable enough to raise or to respond to Cynthia's references to her illness and her death. There were no breakdowns, an agreement that she would approach her friend about an interview, cups of tea and chat about Christmas as it was 2 weeks before Christmas. Three days later I was told that Cynthia had had, a previously booked, counselling session after my interview with her. In this session it became apparent that the research had uncovered a lot of painful areas for her. Her daughters were also angry because their mother was upset. The nurses were extremely supportive of me and did not seem at all concerned by this turn of events. They felt Cynthia's reaction was typical of a counselling response; after exposure, the person withdraws. I am *not* a counsellor or therapist. It wasn't clear to me what in the interview was responsible for sparking off this

response. That was worrying in itself. I was being insensitive and didn't know how. I then, by chance, bumped into Cynthia at the hospice and we talked in a private room. She was quite reticent because she knew her daughters had been on the phone to the hospice and that 'her case' was being discussed.

It transpired that at the end of the research interview I had asked for demographic details, a lot of which I thought I already knew. I thought she had two daughters. So I said as a way of confirmation 'and you have two daughters is that right?' She hesitated momentarily and then said that in fact she had another child. That child had not been mentioned at all during the main body of the interview and I was aware of that, but I did not ask any more questions. We were in the process of winding up the interview and, although this could have been interesting, it was not obviously apparent that it would have any bearing on her friendships, illness and forthcoming death. It transpired that she was not in communication with the third child and this was a cause of great concern and pain to her. It was this simple structured question about her children which left her upset after I had gone. Asking 'simple facts' like age, who you live with, number of living children and last occupation were designed to wind up the interview in a lighter fashion. The obvious is not always the sensitive. The balance of working in this sensitive area and keeping regard to confidentiality is much more complex than on first consideration. I was told in some detail about Cynthia's situation in a debriefing session with the nurses in order to put the interview into perspective for me. They had obviously talked to the counsellor. However, I realised that I couldn't ask about Cynthia too many times during the following weeks. The nurses gave the impression that this incident was par for the course and I needed to be able to deal with it in order to do this type of research. So I quickly stopped referring to the case even though I desperately wanted to know that it was all right, whatever all right was. I had to maintain my professional credibility. This has been repeated in other settings. Nurses have implied that over-concern on my part about emotional responses in patients is inappropriate. Their view is that some response in the patient is natural given their circumstances.

I have been shaken by other people's response to the research topic. People sometimes draw their breathe in when they hear the research topic. I do not have such a clear response. Right from the start I have felt committed to the idea that dying women as a group should be involved in research just as any other group in the population. I have felt privileged to be in the position of interviewing women who are near the end of their life. It allows me to be privy to women's thoughts at a time when these thoughts are not generally exposed. It has been very inspiring. In some cases I have left interviews feeling so very moved by the woman's story, overwhelmed by a general sense of emotion – numb, not positive or

negative, impossible to define except by its pervasiveness. The women's stories are often complex and involve an intermeshed series of life events which have involved their friends. Their forthcoming death is only a part of the story. In a sense I suppose this work is deemed sensitive because so many people do not have access to dying people. But with the privilege of access comes awesome responsibility. I began to feel that I was insensitive, hard, unsympathetic and callous. I was not having physical symptoms as a result of the research. I was not breaking down in tears regularly during or after the interviews. In some cases I didn't even like the women. Interviewers sometimes use first person accounts of fieldwork to 'confess' to not liking some respondents. It is particularly difficult to confess to not liking a dying person. If the majority of people you are interviewing over a period of time are dying, this fact in some senses loses its significance.

Underpinning all these concerns is a sense that whatever I feel *it is data*. This is reassuring, up to a point. Perhaps the research will not be properly finished for me until I am dying. But, in the meantime, I am still left with the sense of risk that this type of research involves. If any of the women regret being involved in the research they have so little time to resolve their concerns. The interviews felt very much like the women rehearsing their biography for perhaps the last time. If it came out wrong, or was misinterpreted by me, they would have very little time to resolve their feelings. Therein lies the peculiar risk of this research.

Joint reflections on the fieldwork account

In this section both authors reflect on the fieldwork account presented above, using it as an established text to be analysed.

The fieldwork account is permeated with a sense of struggle and search. We suggest that the search is for appropriate rules for feelings. Young states that she wants to 'ground her research experience in a sociological context'. By having access to the feeling rules associated with a particular sociological approach her feelings could be more easily managed. The struggle described in the account is with the management of the potentially conflicting sets of feeling rules. The awareness and management of this searching and struggling constitutes the emotion work being carried out. As Hochschild observes, emotion work is an active stance, it requires effort. The effort or work is exacerbated by making it public and attempting to deal with the vulnerability and exposure that writing in the first person in a professional context creates: 'The professional is very obviously becoming the personal. This first person account feels like crossing over boundaries'. There is an immediate sensitivity to the level of involvement required.

The potentially emotive nature of this particular research subject is

referred to in the account. 'In some cases I have left interviews feeling so very moved by the woman's story, overwhelmed by a general sense of emotion – numb, not positive or negative, impossible to define except by its pervasiveness'. But interestingly the sensitivities of talking to dying women and their friends is not the predominant theme in the account and this Young acknowledges when she describes other peoples' reactions to the research topic as being different from her own. 'People sometimes draw their breath in when they hear the research topic. I do not have such a clear response'. There is a sense that physical symptoms, of anxiety for instance, indicate desired feelings which she should evoke. She refers to feeling 'hard, unsympathetic and callous' because she was not experiencing physical symptoms. In a similar way she acknowledges that not liking some dying respondents may be a feeling that she should attempt to suppress. These concerns imply expectations about what should be felt.

A lot of the emotion work described in the account is related to an awareness that her feelings did not necessarily fit with documented expectations of fieldworker feelings. We interpret her reading of the ethnographic literature as a search for appropriate feeling rules. But her reading, instead of making her feel comfortable, or as if she 'had come home', as Wilkins did on discovering a feminist literature, provides only partial insights. She is left to manage sets of potentially conflicting feeling rules. So we see that she particularly rejects as inappropriate the feminist notion of identification when considering her relationship with the dying women. She says 'In the immediate sense I am not dying. Any attempt at relating on this level would be crass and insensitive. I could not relate to the interviewee on the level of our common health status and other characteristics like gender, race and age, felt secondary to the topic'.

In the same way she struggles with placement strategies finding them to be problematic when used in the field. 'On more than one occasion, when I talked about my baby as part of the presentation of myself, I was greeted by some quite hostile responses'. In some cases, she only understands the nature of the feeling rules governing her own emotions when different feeling rules are invoked by others. This is illustrated by the interview with Katherine when Young's feelings of comfort led her to probe further. Katherine expressed unease concerning the emotions generated for her by this probing (access to feelings of interviewees after the interview are relatively rare in research). Significantly this insight about the comfort of the interviewee and probing became apparent to her during a debriefing interview with her supervisor. 'It was only very near the end of the fieldwork when I was having a debriefing interview that it dawned upon me that the relationship between my comfort factor and the interview was complex and significant'. As Hochschild observes, emotion work can be done by others on the self. In this context the debriefing interviews made her aware of conflicting emotions, her own emotional

comfort, the desire to 'do a good interview' in a sensitive situation, and her feelings for the respondents.

One can also see something of the same in the response the nurses made to the interview with Cynthia. The situation of a dying woman reacting with pain to a rehearsal of her biography was familiar to them. They had a response to this situation, based on their own profession's feeling rules, and could do the emotion work required. Young had no such clear rules to guide her. Perhaps significantly, she speaks of needing to maintain professional credibility at that point in the research. Her concern over seeming foolish to peers also implies that there is a proper, sociologically acceptable, way to feel. These examples suggest that professions, including sociology, may be to some extent defined by the feeling rules that underpin them.

A series of questions are posed throughout the account. These are connected with the technical skills required to do 'a good interview', a topic that seems at the root of many of the anxieties expressed. The methods chosen for the interviews did not provide in themselves a safeguard for her feelings. Judgements about probing and exposure of feelings and vulnerabilities are decisions peculiar to each interview. As stated in the account 'I couldn't hide behind procedures as set out in the research design, there were no prescribed indicators for inappropriateness on which I could rely'. She goes on to say that 'the "good" interview remained illusory and this became a major source of anxiety for me'. She is left with a set of unresolved tensions and no allegiance to a particular methodological approach. Hochschild refers to the dynamics of contending sets of rules and the potential for there to be a vacuum as one set become deconventionalised and other sets are not reconventionalised. In this situation the fieldworker is left asking 'How should I feel?' or 'Do my feelings fit?'. This tension may be more productive than an allegiance to a particular set of interviewing rules or techniques as good interviewing practice does not necessarily result in the production of the good interview.

Conclusion

In this chapter we have related Hochschild's concepts of emotion work and feeling rules to the endeavour of sociological fieldwork as documented in first person fieldwork accounts. In particular we have made use of Young's fieldwork experiences when interviewing women who were dying. The sensitivity of this particular research topic may have highlighted the emotion work undertaken during and after fieldwork. However, we feel the association between emotion work and feeling rules applies to fieldwork accounts in general. To an extent, the 'classic'

accounts of fieldwork are ironized by feminist methodologists and by writers like Johnson and the Adlers. Ironically, the cool detached research-as-work orientation typical of the neo-Chicagoan tradition in fact hid a range of emotional responses to the fieldwork situation. We have suggested that this ironization needs to go further. Part of the reflexive process involves understanding the emotional implications of our own methodological preferences, and how they might interact and conflict.

The emotions expressed in fieldwork accounts tend to be negatively cast, or they express difficulties which are finally managed. This is partly because such accounts tend inevitably to describe a process or a journey (James 1984). A rosy picture of comfort and harmony may read as self-satisfied. Although fieldwork accounts are often written by novice fieldworkers who are describing a rite of passage into their profession (Lee 1993), it is not enough to say how difficult fieldwork can be for an individual. The roots of the difficulty have to be explored. The analysis above has made explicit the conscious awareness or sociological introspection which is a necessary part of writing a first person account. It provides a 'way in' to the mass of feelings expressed in these accounts and a means of putting some theoretical perspective on them. For us, the emotion work that is done in research is best seen as an attempt to *manage* the feeling rules implicit in *different* methodological perspectives.

We have argued that first person fieldwork accounts capture the dissonance between what fieldworkers feel they ought to feel and what they admit to feeling. The difficulties or the stress portrayed in these accounts reflect the emotion work undertaken in attempting to manage the tensions between involvement, comfort and identification. We suspect that tensions of this kind are inherent in fieldwork. Our experience suggests that what is important is to provide a variety of forums for grappling with these tensions for example through structured debriefing interviews and the use of first person accounts as data for secondary analysis. Such forums do not ensure that the conflict between comfort, involvement and identification is resolved in a particular way. Indeed, just the opposite. Because the emotion work on this project was partly done collectively, differing approaches to resolving the tensions were always on offer. In fact, there may be something to be said for not fully resolving emotional issues in research. To do so may risk resolving the competing pulls of comfort, involvement and identification in a complacent way which solves some problems but potentially exacerbates others.

Notes

- 1 This paper arose out of a series of discussions between the authors in which an attempt was made to identify theoretical bases for the understanding of the role

of emotions in fieldwork. The starting point for the discussions was the first author's fieldwork account. This was written by the first author at the end of the fieldwork using her fieldwork diary and the transcripts from the debriefing interviews with her supervisor. The intention from the start of the research project was to keep a diary, carry out debriefing interviews and to write a first person account. The ideas for this chapter were developed after the first person account of the fieldwork had been written. Chapter drafts which theorised and contextualised the first person account were passed backwards and forwards between the authors in order to develop the arguments presented. The actual fieldwork account was not altered at this stage. It was treated as an established text to be analysed.

- 2 To be clear, in talking about over or under-analysis we are not referring to the volume of analysis carried out by a particular fieldworker on a particular topic. Nor do we have any view on the 'correct' interpretation of the examples we have given. The point we make is a methodological one about the adequacy of first person accounts.
- 3 When the words supervisor or manager appear in the first person account these do not refer to the second author. He is a collaborator and joint author of this chapter but was not directly involved in the original research.
- 4 Three debriefing interviews took place during the fieldwork period. A topic guide was used by the interviewer and the focus of the interviews was on the emotional aspects of the fieldwork. Particular fieldwork interviews were discussed in some detail.
- 5 All the names used in this account are pseudonyms.

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