INT. J. SOCIAL RESEARCH METHODOLOGY, 2000, VOL. 3, NO. 1, 15–34

Collaboration and censorship in the oral history interview

ANNMARIE TURNBULL

(Recieved 9 July 1998; accepted 11 November 1999)

The objective of this article is to highlight seldom considered aspects of the relationship between the researcher and the interviewee in feminist oral history research. As part of a study of the work of British women sociologists the researcher is undertaking a series of oral history interviews with retired academics. The attempts to follow social science ethical injections concerning the protection of human subjects and feminist injunctions to maximize subject collaboration and researcher reflexivity have raised several issues prior to, during and after the interviews. Issues of collaboration and censorship impinge on the research process at every stage of the work and, along with the personal and structural power relations of those involved, determine, in unanticipated ways, the final research product.

Introduction

As part of a research project titled 'Women Professional Sociologists in Britain, 1945–1965' I am undertaking oral history interviews with women who were active as professional sociologists in universities and social research agencies in the two decades after World War Two. My reflections on the use of oral history in my research have raised two recurring and interrelated issues, those of collaboration and censorship. I believe both of these are inevitable in the practice of feminist oral history and that their examination enhances our understanding of the uses of oral history interviews. Here I outline some of my reflections on preparing and undertaking oral history interviews and detail how the search for a more collaborative research practice can alert the feminist researcher to the censoring of information which she and those she interviews practice.

The practice of oral history inevitably involves power relations: it has been used variously to hear the people from below, the working class who previously had no voice in history; to give women a place

Annemarie Turnbull is a feminist sociologist and historian who has worked in educational, social services and oral history research in the statutory and voluntary sectors and taught sociology and history in higher education in four institutions. She currently works at Goldsmiths College, University of London where she is a lecturer in community work; e-mail: a.turnbull@gold.ac.uk.

International Journal of Social Research Methodology
ISSN 1364-5579 print/ISSN 1464-5300 online © 2000 Taylor & Francis Ltd
http://www.tandf.co.uk/journals/tf/13645579.html

in the story of the past; to enable black people to enter history and lesbians and gays to ensure their visibility in future histories. The powerful too have been the subjects of oral history and the closed worlds of elite groups have begun to be made visible. Oral history has long had a political agenda of which its practitioners are justly proud. My own motivations to undertake oral history interviews also have a political dimension. I wished to undertake research that met my own standards of feminist research and was credible to the historical and sociological communities. Sandra Harding has usefully characterized feminist research methodology as including three key features. These are all demanding. The beliefs and behaviour of feminist researchers that will inevitably shape the work they produce probably include, first, a focus on women's experiences of the world, second, a motivation and goal for the research to produce knowledge for women and third, a strong presence of reflexivity within the research process itself (Harding 1987: 9). In interviewing retired women academics about their lives in the 1940s, '50s and '60s I am constantly focusing on describing women's life experiences, with a view to providing useful tools for women to understand and change the world in which we live, in a manner which is as thoughtful and self-critical as I can make it. What I understand to be the injunctions of feminist research are therefore to the forefront of my work.

Joanna Bornat (1994: 8) has noticed how oral historians 'seem to be more interested in what oral history can do, rather than what it is'. I, like her, follow Frisch's goals for oral history to achieve 'a shared authority' to 'redefine and redistribute intellectual authority, so that this might be shared more broadly in historical research and communication' (1994: 25). But this discussion of work in progress explores aspects of the nature of oral history itself and highlights the insights and strengths that I have derived from trying to emulate and improve upon the practice of a specifically feminist oral history (Berger, Gluck and Patai 1991, Scanlon 1993, Chamberlain 1995, Vaz 1997, Summerfield 1998). It is an exploration of what oral history is and the methodological challenges I am facing in using it. I offer this account of my own deliberations and experience as a contribution to the developing consciousness and confidence with the practice of the oral history interview.

The project

The history of academia is a male history despite women's presence. Men have been the great majority of the players in the academic world, but I wanted to know if and, if so, how women had shaped the world of knowledge and learning in one academic discipline – sociology. The four year part-time project examines the lives and work of women professional sociologists in order to explore their contribution to post World War Two social life in Britain. Its focus is the ways in which sociological work related to developments in social policy and the study of society and sociological knowledge, both within and beyond academic

institutions. In the context of a period that has been characterised as recreating 'woman' as a social problem (Wilson 1980) and as 'the nadir of British feminism' (Pugh 1992) the project examines the lives and work of a sample of women who identified themselves as sociologists. In 1965 there were 203 women members of the British Sociological Association; 26% of the total membership. Many of these women would not have called themselves sociologists and others, who we now regard as such, like Ruth Glass and Hannah Gavron, were not members. But in that year the University Teachers' Section of the Association published its first Register of Professional Sociologists in the United Kingdom. Of the 138 entries 23 (17%) were of women. These are my core sample, although other women who worked in post-war sociology and that I have been able to trace, are also included. The sample is therefore inevitably skewed to those who I have been able to learn about through their publications and association with research institutes and universities. The research examines the significance of sociology as a gendered area, both in terms of its knowledge base and in the working practices of women sociologists themselves, in academic and other workplaces.

By the early 1960s women comprised only 10% of the total academic staff in the universities. British university teaching was characterized as 'a traditionally gentlemanly profession informed by the norms of a democratically self-governing guild which is in process of adapting itself to internal and external pressures towards bureaucracy and specialization'. (Halsey and Trow 1971: 170). As Sara Delamont has noted 'what is lacking from the literature is any sense of what being a member of the occupation feels like to its women members' (Delamont 1989: 202). I wanted to know how the 'gentlemanly' world of academic work was experienced by the small group of elite women who had entered it and what bearing their sex had on their occupational lives as sociologists. While there was much documented material to explore, including the women's own published work, the project also uses oral history interviews in order to chart, with participants, the continuities and disjunctures between women's pre World War Two sociological work and careers and those experienced by later generations, including my own.

The project has placed me in a complex power relationship. While it is part of a feminist project to give women in academia a stronger voice in history, and part of oral history's political agenda to help retrieve lost voices, it has also located me firmly in the arena of elite studies. Although now retired, most of the women I wished to interview, as part of my exploration, had had successful careers in university research and teaching. All have published works to their credit. Some are widely acknowledged by their peers as having made significant contributions to knowledge. Others have gained prominence in senior positions within their institutions and on public bodies both locally and nationally.

Discussing her interviews with women members of parliament Puwar concludes:

> If we as feminists, want to use qualitative research methods to show the relevance of a gender analysis to the fields of stratification, politics and elite studies then we have to start aiding each other by reflecting openly on the research process. . .we need reflexive accounts of researching women elites that are just as diverse and rich as the wide range of accounts to be found when the researcher is the privileged one in the relationship. The existence of boundaries of class and race within the woman – woman research relationship have been widely discussed when the researcher is the privileged one but little exists when the situation is reversed (Puwar, 1997: para 11.1).

The reflections of oral historians on the process of their research too rarely reach print and fewer still reflect on the interviewing of elites.[1] To meet my own requirements to be collaborative and reflexive I realized that the research process needed a constant reassessment. I was struck early by the affinity of my research plan with models of action research where the researcher is repeatedly moving through circles of activity with a constant evaluation of methods and meanings as she shifts her methods to meet the goals of the research. Action research methodology has rarely been associated with oral history yet seems far more salient than approaches which regard human participants as a source on a par with a parliamentary paper or even a novel.[2] Kemmis has said of action research 'it is most rationally empowering when undertaken by participants collaboratively' (Blaxter *et al.* 1996: 63) and this emphasis on collaboration, I reasoned, might be one way of linking the epistemological injunctions of a feminist social research agenda with research practice. (Scanlon 1993, Maynard 1994). This article is one point of my four year spiral of action and reflection. Since my early training in social research I have always kept research diaries. The one for this project was started in the autumn of 1996 and, as I expected, became an important tool for the work, not just to record the progress of the work and thoughts and insights on the subject matter, but to serve as an emotional barometer of the project. Indeed this discussion includes quotations from my diary to illustrate my own concerns and reflections as well as my own changing subjectivity as the research progresses. With eight interviews and transcripts completed, writing this paper has enabled me to reflect on and assess progress before I move on towards analysis of the transcripts.

For the purposes of this discussion I have conceptualised the research process so far as falling into two phases. First, issues arising from the preparation for and participation in the interviews are discussed. Secondly, issues that emerged soon after the interview concerning the production and possible use of the transcripts are considered. To suggest such distinct phased staging of the research, however, implies a linearity which does not in fact exist. Looking at the accounts of research that have used interviews it invariably appears that the process has been chronological; each stage being completed before the next is embarked upon. This is far from the case in my work. Maybe it is the reality for some researchers but often in discussions of research methods this pattern is implicitly signalled rather than discussed. Certainly the part-time nature of my work on the project means I am juggling with each aspect of the research process constantly. On the same day I might transcribe part of one interview, arrange another and ponder the transcript from a third. In addition some issues

of concern to me are common at both stages. The narrative that follows should be understood as an attempt to make sequential what must be an iterative process.

Preparing and participating in oral history interviews

In preparing for and undertaking oral history interviews the researcher is enmeshed from the outset, in complex decisions involving censorship and collaboration. The amount and quality of information about the research that will be shared with the interviewees prior to the interviews may vary enormously from project to project. How far knowledge of the project's intellectual origins, or precise details of the methodology is believed to be relevant information to share with participants may have important repercussions. The preparation, content and timing of the interviews themselves raise two further issues: first, how the subject matter of the interviews is negotiated both prior to and during the interview and secondly, the extent to which the interviewer or interviewee orchestrates the interview; for example in its location and timing and in the selection or emphases of the content.

My recent experiences illustrate these complexities. The researcher enters into a relationship with her subjects when the first approach to possible participants is made. I wrote to the women I had been able to trace explaining my aims as clearly, but as broadly as I could, requesting their involvement in the research and explaining 'at present my key concerns are women's contribution to sociological knowledge; their part in shaping the discipline; the sociological workplace and the wider social worlds of women sociologists'. I included a list of the names of women I hoped to include in the project and the oral history part of the work I described thus: 'As part of the project, which I envisage taking three years to complete, I am hoping that you will agree to have at least two interviews with me. These would be semi-structured and last between one and three hours'.

Some women were certainly not going to agree to meet with me with so sketchy an outline of my intentions. As one wrote 'I would like to have a clearer picture of what it is that you are really interested in'. What am I *really* interested in? 'Everything about your life and work as a woman sociologist' would have been a rather self-defeating reply. So to the women who wrote back requesting further information I sent a more detailed four page description of my project. I think in my first letter I had purposely been vague and brief. I didn't want the women to start tailoring their responses in relation to aspects of my intellectual history or politics – so, I censored what I said about the project. This was a mistaken and rather futile attempt at the outset of the project to retain a power of sorts, before I had begun to consider in depth the overall power dimensions of the project and how they might shape the research process. With some women party to a detailed four page account of my intellectual concerns (and implicitly to some of my political and ideological persuasions) and others privy to barely four lines, my attempts at equal and equally high-handed treatment of all, thankfully,

foundered early. Some women ignored my letter and one wrote with misgivings about becoming involved, which I tried, unsuccessfully, to allay in a telephone conversation. My diary records how even this experience, while not eliciting the hoped for meeting, both reinforced my awareness of the elite status of the women and moved on my commitment with the project:

> I ought to record how much I enjoyed my conversation with – It was so stimulating, in less than fifteen minutes many issues were discussed and I felt invigorated and enthused and determined to show my capabilities to pull off a biographical study. Every day shows me how hard it is but I am determined. My conversation was free-wheeling – fact, speculation, past, present all rushing in on me as I sought to impress her with some sense of my credibility as a researcher. I hope I succeeded, the good thing was I persuaded myself of my own credibility. While I have much still to learn about the '40s, '50s and '60s I am beginning to understand it or rather the various it's that are emerging.

From Oakley's seminal discussion on interviewing women to Stacey's call, a decade later, for a more rigorous and self-reflexive research practice there has been mounting concern amongst feminists about the social relations of research (Oakley 1981, Stacey 1991). Oral historians too have called for more involvement between researchers and researched. The Popular Memory Group's critique castigates projects where those whose memories are used 'do not participate, or only indirectly, in the educational work which produces the final account' (Popular Memory Group 1982: 220). The explanatory paper to the women therefore outlined the following goal. 'My intention is to enable the living subjects of the study as much involvement as possible in this reconstruction of the sociological world of three decades; hence the wish to undertake at least two interviews with the sociologists, as the research progresses. Despite this, once again I tried, with perhaps more success, to prevent the possible imposition of what, at that point, I still regarded as the women's power to control my research. I was unsure of the impact of their 'expert knowledge' on me as the 'less powerful' researcher. In retrospect I realize I thought I might be intimidated by their sociological knowledge, and there was a second possibility. In his discussion of the research interview Burgess cites Corbin's observation that 'no individuals think about themselves and their lives in the terms which sociologists use' (1984: 11). But I reasoned that some individuals might, sociologists might, and the women I was hoping to interview were themselves sociologists, or at least had regarded themselves as such in 1965. Their working lives, to a greater or lesser extent, had been immersed in the conceptual frameworks and jargon of sociological theory and practice. In her reflections on interviewing fellow women graduates Sheila Hamilton was concerned that 'certain experience or descriptions will be skimmed over because it is assumed that I should know all about them anyway'. (1982: 61) I was concerned that the assumption by the women that I too held expert knowledge about sociology might limit what they would chose to tell me; they might censor themselves.

My diary shows that while there were other issues of concern at the outset, the negotiating of power in this nominally collaborative exercise was what I feared most:

> Interviews looming. Three issues seem to be worrying me (and I guess there are a lot more that should be) facts and feelings, class and power. I know I'm more interested in feelings, but don't know why this is so important. Second class, I suspect and know from some biographical details already that many of these women come from higher social classes than I do. I feel like the daughter of my working-class father not of my middle-class mother. Why? There is a third one, power – lots of issues here – intellectual authority, senior status – I am frightened.

Approaching the interviews I faced, more honestly, the issue of collaboration. Sociologists working outside a feminist framework sometimes present the interview in a starkly combative manner. Bornat has noted how the terms 'interrogation' and 'questioning' have oppressive connotations (Bornat 1994: 23) and in some sociological literature on interviewing the possibility of encouraging the interviewee to control the interview is not acknowledged as a legitimate role. An example from one of the 'how to interview' texts explicitly formulates a series of assumptions that often seem to underlie the discussions on the principles and practice of the interview. Of Nigel Gilbert's two principles informing research interviews the first is that 'the questioning should be as open-ended as possible, in order to gain spontaneous information about attitudes and actions, rather than a rehearsed position (1993: 138). He alerts researchers to the possibilities of the interviewees attempts at rationalizations, their lack of awareness, 'many people are not used to putting their feelings into words'; their fear of being shown up and the tendency to be overpolite to the interviewer. The tenor of this discussion is essentially combative. A major concern expressed in the literature is that the interviewer must battle with the interviewee to get to 'the truth'. From a feminist perspective the nature of the interview is far more complex and nuanced than this.

In summarizing her own responses to being interviewed about her past, Miriam Glucksmann, a sociologist who has used oral history to explore working-class women's labour, recalled:

> I certainly did not come away feeling good on the occasion when I was on the other side of the microphone being interviewed about my involvement in the student movement of the late 1960s. In fact I felt awful afterwards. I had dredged my memory for what the interviewer wanted to know; what I had thought then about various political arguments and strategies and why. All sorts of memories were churned up and then left up in the air without being resolved. Also I found it very hard to refrain from saying what I now thought in hindsight about what had happened then and only talk as if it were still in the present. And of course I did not know whether the researcher thought it was a good interview (1994: 165).

This was not where I wanted to leave women at the end of an oral history interview. If the interview was to be the start of a collaboration I reasoned, I would have to give the interviewees as much power as possible to work through, in advance, whatever rationalizations they might or might not have for their past actions. I wanted them to have no fear of 'being shown up' and every opportunity to articulate their feelings in the ways they wished, without fear of censor.

So I prepared my interview guides and after telephoning the women to arrange a time and a place to meet with them I sent them the guide. The interview guide was a single sheet of paper that outlined areas of reflection the women might choose to focus upon relating to their memory, their lives before university, their experiences of the war,

university, employment, sociology, life outside employment and to recollections of post-war Britain. Here I moved to an intentional relinquishing of power. In interviewing there seems to be an unspoken and sometimes, as above, an explicit assumption among researchers that it is inappropriate to let the interviewee know the questions you want answered in advance of meeting. Perhaps this is seen as cheating in some way and in somehow invalidating the authenticity of the research interview as a research method that is dynamic and particularly effective in capturing accurate and authentic perceptions. There may be a concern that giving participants time to reflect on the scope of the interview and on specific questions and issues prior to articulating them, will somehow corrupt their answers and make them less authentic. Maybe there is a wish to catch the interviewee unawares, unguarded and vulnerable? In short this holding back can be seen as a way of helping the interviewer retain power. Whatever the taken-for-granted assumptions behind the practice it appears that it does have an impact on researchers' later analyses and interpretations. Penny Summerfield, for example, interprets the anxiety of her interviewees as arising from their awareness that they were required to 'become the narrative subject of the oral history interview' (1998: 22) rather than, more prosaically, from the insecurity of not knowing what would happen next in the interview.

By sending the interview guide in advance and by explaining that I wanted them to select what they wanted to talk about and how much, and not to feel obliged to cover slavishly all my points on the guide, I hoped the women would feel in control. In short I tried to return to Beatrice and Sidney Webb's quite relaxed notion of the interview. 'Bear in mind that it is desirable to make the interview pleasing to the persons interviewed. It should seem to him or her an agreeable form of social intercourse' (Webb and Webb 1932, cited in Burgess 1984: 102).

The oral history interview and the narrative it produces have increasingly come to be regarded as performances (Dunaway 1992, Finnegan 1997, Summerfield 1998). Performances and censorship are intricately linked. All presentations involve a censorship of the self: the conscious selection and ordering of material. The formality of the oral history interview (this is no cosy chat about the past) invites such censorship. The women I interviewed had all made choices about how they would stage their performances. Where we should meet was their decision and venues included both public and private spaces. In their homes what I was permitted to see was clearly signalled. In some cases I was acutely aware of the props for the presentation of self, such as the recent sociology books placed carefully where I would notice them, or conversely of the absence of the predictable paraphernalia of daily life in the prepared room. My diary explains of one interview 'I felt like an ''event'''.

It is difficult to draw many common issues from the interviews themselves. In part this was because the strategies I had devised to ensure the interviewees' control meant that the encounters were enormously varied. In length they ranged from one hour fifteen minutes to well over three hours. Some women seemed to be speaking to me directly, but some appeared to be talking to others (maybe their professional peers) and others

again to history. Some were virtually monologues, while in others the women requested me to follow a more traditional format, where I asked questions and was encouraged to prompt and probe. All this influenced the degree to which I regarded the interview as part of a collaborative project. I suspect such was the case for the women as well.

The simple and uniform interview guide allowed an enormous variety in the overall tone of the meetings; some were formal and serious throughout while others were flippant at times or punctuated throughout by much laughter. The positivist tradition of interviewing I have indicated above places a perhaps unmerited premium on spontaneity in the interview. All the interviews were story-telling but the story-telling where past dialogues were vividly characterized was particularly engrossing to hear. The immediacy of the performance at times masked the telling of what may be well-honed anecdotes. The women who had recently written autobiographical essays or been interviewed by other researchers were certainly less hesitant in the telling of their tales.

Control over time always has to be negotiated when using oral history methods. Time appears to be a significant issue in understanding both how far an oral history interview can be a collaborative event and when and how those involved censor themselves. The women generally controlled the length of the interviews and the length of the anticipated performance emerged early as an issue. Two possible interviewees had expressed surprise at the one to three hours I had suggested for our meeting. I was pleased that they had raised this as an issue to negotiate, after all it was they who were favouring me, not I them, and I reassured them that I would be grateful for any time they could give me, however little. I had read Puwar's discussion of the management of time in her interviews with women MPs and was struck by how understanding of and negotiation around time had become a power game not only in setting up the interviews but during them:

> Although control over time was important, normal day-to-day politeness meant that most interviewees were reluctant to look down at their watches during an interview. I soon noticed that it was easier for the interviewees to draw the interview to a close if they could easily glance at my wrist to see the time, or if they could look up at a clock on a wall that they were facing. This gave me the opportunity to manipulate the situation to my advantage. In order to gain some extra time from them I deliberately chose not to wear my watch during the interview. If the interview room had a clock on the wall I tried to sit facing it (Puwar 1997: Para 6.6).

I had not been as pragmatic as Ken Plummer wisely suggests researchers should be in considering my own time constraints thoroughly. I was to be travelling all over Britain to meet the women, and the risk of six hours of driving to spend less than an hour interviewing was certainly worrying (Plummer 1983). I found however, that when I had given the power over time to the interviewee I was considerably more relaxed and receptive to them. Puwar on the contrary, found time a particularly difficult issue during her interviews with busy women MPs. These elite women were 'anxious to get away' and asked 'how much more is there to cover'. Puwar characterizes herself as a 'feminist researcher. . .trying to negotiate control of the situation for herself, rather than for her interviewees' (para 2.4). Time will nevertheless continue to be an issue of negotiation as the research progresses

and ultimately its control is, and I believe should remain in the hands of the women interviewees if I am to fulfil my expectations of collaboration in the research process. Ironically relatively powerless people have so frequently given their control over time to the researcher that some researchers seem to accept this as the appropriate mode for research with anyone.

The age of the women I would interview initially concerned me. This concern was engendered by my awareness of the therapeutic context of some oral history work, for example in reminiscence groups. I was planning to interview women who would be aged between sixty and ninety and was aware that the partial life history narratives I produced were likely to be part of an ongoing process of reflection on their lives (Coleman 1991). I was also unclear about the frames of reference and expectations that arose from my own views of older people. In approaching each interview issues of age troubled me more than those of class, sex, sexuality, race or religion, although, to a greater or lesser extent, these were also present. My own respect for elderly people was tempered by concerns regarding the participants' health. One woman had been too ill to interview. I could also tell other women were not in the best of health when I spoke to them on the telephone. I thought this sensitivity might make me too careful an interviewer, too keen to interpret hesitancy or slowness as tiredness, or make me unwilling to pursue issues that I imagined might cause mental distress. In the event I could only recall the ethical injunction that the well-being of the interviewee should be paramount. This I tried to ensure by checking, at least once during the interview, whether or not a break would be welcomed.

Thoughts on ageing led me to consider memory. The women I met with had maybe sixty years knowledge including experience of four decades that I had only read about. It was their memories of those decades that I wished to hear, yet 'Ageing affects remembrance in subtle ways: some subjects can't recall certain events; some do not wish to; some do not even pretend to' (Dunaway 1992: 42). Whether or not these outcomes are the result of ageing, my attitude to the memories of the women is also important in clarifying the parameters of power in the research. The criticisms of oral history that emerged from the 1970s often focused on the validity of oral memories. Here for me there is little at issue. Information from the oral history interview is merely a single source of evidence imbued, as are all sources, with subjectivity and therefore to be triangulated against as many others as possible, in order, not to find some positivistic truth but to help build a more complex, richer understanding of human experiences. The more interesting issue to me is that of reliability. Plummer (1983: 105) has argued that 'the oral historians' goal – of recapturing the past – is altogether more ambitious than the sociologists' goal, who is in a sense merely concerned with getting at the way a person sees his or her life history at the moment of the interview'. This is a neat distinction. It holds the subjectivity, the ephemerality and the partisanship of memory easily and is perfectly in tune with the historian's wish to capture a vision of the social world that has shaped the individual's memory, but it also seems to undermine the

power of the individual who is interviewed as an agent in their past. I wanted to get further here, in exploring with the women their ideas on the reliability or consistency of their current perceptions of the past. David Dunaway (1992: 41) defines reliability in the oral history interview as 'whether the same question is answered the same way more than once by the source'. In planning the interviews I tried to ensure the women had the opportunity to consciously consider their constructions of their pasts. If they chose they could prepare for our meeting by reflecting and even checking on their past lives in relation to the interview guide. Some certainly did this as revealed by the retrieved notebooks, papers and curriculum vitaes they mentioned or showed me, by the previously written autobiographical essays they directed me to, and by comments in the interview such as 'I was trying this week to collect some memories of my childhood' or afterwards when, for example, one woman noted of the transcript 'I think there is an entirely disproportionate emphasis on my childhood'. She related this to a reminiscence group she had been involved in. 'We spoke and wrote so much and it is still in my mind'. They therefore had a number of opportunities to establish their own agency in their presentation and perhaps to feel less troubled by their responses than did Glucksmann who 'felt awful afterwards' (Glucksmann 1994).

My first question on the interview schedule was 'How accurate do you think your memory is of these years? I asked this to enable the women who wished to, the chance to reflect before the interview on the subjectivity of their responses to me, not in anyway to provide some measure of reliability. Ironically, as one of the women pointed out to me, this enquiry could have been interpreted as a massive assertion of my power in the interview; in effect, asking the women if they were going to be good enough interviewees. Not all the women chose to answer the question; those who did raised many of the issues that tantalize the oral historian, particularly memory's relation to emotion and the varying viewpoints of participants in an event. For some women the question was ignored or easily answered. 'B' explained, 'Oh very good indeed, yes, yes, probably much, now I'm getting old its probably, much clearer than about more recent events' but 'F' found the process of remembering difficult and its outcomes partial:

> I know they're very, very poor. Its quite difficult to remember except for certain things that obviously had an emotional impact on me, and talking about the effect of things going on, one vivid memory is when we heard over the radio in 1931 that we'd had to go off the gold standard. And I remember clearly the family being in the dining room and listening to this and the tremendous sense of the seriousness and the depression of that situation. And I know my first question to my parents 'Does that mean I'll have to leave boarding school?' which I'd only just started. But it was much more the emotional awareness, than the significance of what had happened, which I have had an awareness of since. I find that is where memory is triggered where it has made an emotional impact.

What I currently see as an important distinction here between these two perceptions of memory was the previous memory work the women had done. The first woman had already placed her past as a woman sociologist in some kind of ordered narrative, she had been interviewed about her past

before, while the second woman may only have started to think about her past systematically as a result of my approach to her. Neither interview is necessarily 'better' or 'worse' but both the interviewee and I can context their current perceptions more easily. 'C' highlighted both the inconsistency of her memory and its subjectivity:

> I think what I think about memory is there are some things that stand out very clearly and you've got a very clear memory and which you think is accurate, which it may or may not be, I completely agree with what you said just now that even if its a single event you are thinking about, and there are six people there, there will be six interpretations of what happened. It will have struck people differently and so on, and so there's that. And in terms of my memory, there are areas that are remarkably foggy and even if prompted it doesn't necessarily come back and others where I'm very clear. So there's that sort of range.

'H' noted not only the subjectivity of memories but, like 'F', their links to emotions:

> Accurate. I would say totally inaccurate. I am always totally impressed by those people who know exactly when it happened, on the one hand, and I'm also ashamed of myself, in the sense of, having lived through rather important bits of history, as it were, realizing just how little, at the time, I actually seemed to take on board. The points I hang memory on to are very personal points. I mean its 'before or after I went there', that sort of thing rather than 'that was when the labour government was elected'. And I know enough from having interviewed people myself to know that – or indeed trying to reconstruct what was said at meetings after they've taken place – that there's quite a lot of, a good deal of, not just forgetfulness, but very personal interpretation. So when you say accurate, accurate in relation to what? . . . I'll do my best, you'll just have to take what comes out of it and check it against the bits of factual things you can check on.

Overall, and perhaps because they are sociologists aware of the challenges of interviews as 'H' indicated, there was an awareness of memory that accords with Thomas's (1990: 204) summary:

> People do not just remember what happened to them. Deep and intricate processes of recall involve selection, formation and reformation of original experiences. Our memories are complicated products of later alteration, structuring, selection and improvement: they can be subtly changed by our later preoccupations. Memories of feelings and opinions are particularly unreliable.

Despite my attempt to ensure the women had power over the content of the interviews I was surprised to find that this was still partly in my hands as the interviewer. There was a sense of collaboration when women's memories failed them and they turned to me for clarification. For example in trying to recall intellectual work she had read and admired one woman commented 'I looked at that [the question] and I found it very, very difficult. In fact I could hardly remember the names of any books . . . I need you to prompt me now' and another checked 'When were the Beatles? Seventies were they?'[3]

Having tried to put both the content and the timing of the interview in the women's hands I had assumed that they would want to close the interviews, but I found a far more complex scenario. One woman completed her reflections on my final question, paused and said warmly 'Thank you for coming to talk to me. It was nice': that was clear. But on the whole the women demurred to me and I ended the interview, never sure that I had negotiated this successfully. On reflection, and no doubt

with my awareness of the potentially therapeutic or disruptive nature of oral history interviews, I discovered that I had wanted each interview to close on a positive note. 'H' ended her reflections in a resigned, reflective mood saying:

> If you'd said to me in the forties, fifties 'what do you want to do? I'd have said 'I want to make this society a better society, that's what I want to do'. So it's a singular failure on that front. I think it is. No need to beat about the bush on that one.

This I found disturbing, but I was silent. I worried about this ending as we said our goodbyes and for some time later. More typically I tried to engineer an upbeat closure and the tapes reveal the following endings:

> 'A': But it was great.
> Me: A good place to stop I think. Thank you.

> 'B': I never heard him [her father] talk about politics.
> Me: Well I'm sure that's probably quite enough for a first interview, we've come full circle, back to your childhood.

Sometimes, greedy for memories, I pushed for more reflections:

> 'D': Right, have I exhausted you?
> Me: No, not at all, but have a look [indicates guide] and see if there's anything you missed.

Only twice did the women close the meeting. For example:

> 'E': Now what else can I do to help? (looks at guide) well that's another afternoon.
> Me: It is really. I think that's been wonderful.

After eight interviews my diary shows despondency setting in because the interviewees rarely appear to present detailed memories of their working lives, but also a growing awareness of the possible difficulties of remembering for the women I am meeting:

> What did I do when I ran undergraduate seminars nearly twenty years ago, and what did I feel? I don't know. I can't remember, it was just a job. What can I seriously expect from women recalling their working lives fifty years ago.

There is also the issue of their intentional censorship, what they are choosing to share and not to share. However sensitive the researcher is in the preparation for interviews and their conduct, there are some issues that interviewees are unlikely to share with us.[4] Jo Stanley has noted of her interviews with political activists that what gets left out are accounts of sexual history, of doubts and pain, of the physical self, of the intensity of friendships and relationships and of detailed consideration of family life (Stanley 1996). These are much of the life of life histories and of course are important in a project such as mine that is centrally concerned with how professional women experience their public and private lives. But as areas to share publicly as the result of research, they raise issues of responsibility for each interviewee: to others as well as to herself, which I consider below in relation to anonymity. But it is not just the private and personal that is circumscribed. Stanley (1996: 64) observes 'Older people in general

maintain a huge corporate loyalty long after their membership of organizations expires'. This is certainly the case for most of the women I have interviewed. In their accounts of universities as employers, for example, the ivory towers of academe remain remarkably spotless.

After the first interview I noted in my diary:

> On the train going home. Hope this strategy is OK, it seems inevitable that I must do a first overview, tape and then return. This takes a long time, but is important re understanding the women and building some rapport. I was pleased with my conduct because the guide meant I spoke only occasionally. I hope I was correct not to take any notes – I enjoyed the eye contact; it kept me involved.

But as the interviews progressed it seemed less simple. Rapport was not enough and I became increasingly aware of the boundaried and partial nature of the content of the interviews. Three months later I noted after an interview:

> I was struck with how she felt rapport with me (returned) and told me so. Will this continue to be important? Check out how far they ascribe me with knowledge or ignorance – or don't they bother at all. Private versus public issues are very important. Huge areas of both are being filtered out – or so it feels.

Alastair Thomson (1994: 78) has written of the very difficult relation of private memories to public events where our painful memories are necessarily repressed or reformulated 'so that they will fit with what is publicly acceptable; or we find safety in smaller publics or peer groups, which may be socially or politically marginal'. It will be interesting to see whether/where the women situate themselves, and/or allow me to situate them, along this supposed insider-outsider divide in their lives.

Producing and using the transcripts

Producing and using the transcripts raises additional concerns around censorship and collaboration. First, the method of transcribing the content of interview tapes must be decided. This includes the degree to which they can be regarded as verbatim, and the inclusion or not of the non-verbal communication of the interview, for example the perceived mood of the interviewee at various points. Secondly, the reactions of the interviewees to the transcript and their power to alter them has to be negotiated. This involves decisions about the transcript as a second stage in the self-presentation of the interviewee. It also raises issues around the visibility or invisibility of other people mentioned in the transcripts. Finally, the responsibility of the interviewer to produce an interpretation which can encompass both her/his perspectives and that of the interviewees needs to be faced.

I had anticipated the time involved and accordingly had warned the participants that they might not hear from me for some months. I had anticipated the tedium of the relentless tape play, type, tape rewind, type, delete, type, tape play, tape rewind, delete, type, routine of the work. But producing the transcripts brought unforeseen issues. My determination to make the transcripts as full and accurate as

possible was a result of being convinced by arguments regarding the possible distortion to meaning that could be imposed by deletions and sophisticated punctuation (Poland 1995). Given the importance of the written narrative to any subsequent analyses there is a paucity of discussion on how decisions are made during its construction. I put reported speech in single quotation marks and used very basic punctuation, only stops and commas, where the words flowed easily, and added dashes for more disconnected and hesitant speech.[5] David Dunaway (1992: 43) has said how 'ideally, oral historians log the mood, candour, cogency, and sociolinguistic interactions of our interviews yet – sometimes neglect to weigh these factors in interpreting the text'. 'How far I wonder will it be possible or useful to chart the changing moods of the women that the recordings of our meetings reveal' I noted in my diary. Considering gender and memory Chamberlain (1995) has highlighted the importance of tonality in understanding the gendered dimensions of language in interviews. I decided, however, to omit verbal mannerisms if they were persistent, for example if a woman punctuated her speech with 'ems' and not to note non-verbal behaviour, like sighs and laughter, unless it clarified the meaning of words. This censorship was instinctive. On reflection I realize that I did not believe the women would welcome such detailed transcripts.

The labour completed I posted the transcripts asking the women to amend or delete as they saw fit. I was ill-prepared for the reactions to the first two transcripts some weeks later. Despite my care to produce a transcript that would be acceptable 'A' wrote 'It comes over as very rambling and with many non-sequitors and makes me sound like an idiot'. She returned a text of carefully constructed fluent autobiography, half the length of her original transcript, to use in its place. 'B' was concerned that I had been too lazy to bother with proper punctuation. 'C' wrote, in what seemed to be unconcerned contrast, 'Thanks for the transcript. I've not read it yet. Should I?' Here I felt I was facing head on the significance of the oft-lauded notion of empowerment in women's oral history. I had underestimated the potential impact of the women receiving their speech transformed into many pages of autobiographical writing. When Samuel and Thompson (1990: 2) say 'When we listen to a life story, the manner of its telling seems to us as important as what is told' they omit to mention the scribe's role in making the manner of telling. Language and the written word are not synonymous. After three interviews I wrote in my diary 'There is a powerful concern in my mind regarding methods. What the hell is going on? What is my interview guide doing? How far does it frame the transcript I produce?' After seven interviews my diary tells of the slide from my initial methodological purity. 'I've definitely decided to move from my complex verbatim accounts, it takes too long and for what. 'A''s response still concerns me, who wants to be seen and heard warts and all? We all dissemble'. Once again I found myself involved in a mixture of collaboration (with the women's expectations of my role as researcher) and censorship (in omitting aspects of the interviews).

In this paper my continual referencing of, 'the women' and the reduction of individuals to letters of the alphabet, will, I hope, have annoyed readers. The effacing of women in history is vividly illustrated in the repeated absence of their names or the omission of a surname, and the practice of unexplained anonymity still remains in much published feminist scholarship.[6] There are sometimes good reasons for anonymizing our subjects but given our history of erasure these need to be explained.[7] My project was envisaged as a direct challenge to the potential of the history of knowledge to obliterate women's contributions. A concern that history should not belong to those who talk or write the most has been a foundation of oral history and a key concern for those exploring autobiography (Stanley 1993, Swindells 1995). From the outset I was determined that, if they so wished, the women in my research would be named. This was hopelessly naive. As soon as I started to listen to the women speaking of their lives, I knew I would be embroiled, just as they were, in an intricate game of naming and not naming and of negotiating both the public celebration of lives and the protection of privacy.

In their consideration of ethical issues in using autobiography Harrison and Lyon (1993: 104) have summarized the ethical principles that have developed over time and which are now common to many research contexts as including 'informed consent, respect for privacy, subject integrity, avoidance of exploitation and betrayal, and protection from harm'.[8] Oral history research should encompass all these. In relation to the individual women whose lives I am researching I believe my task involves protection of those women, those *they* wish to protect *and* those they do not. This has to be done both in the relatively small social world of professional sociology and in wider contexts. The women themselves have and will continue to have opportunities to safeguard their privacy and the privacy of others as they read drafts of my work. When interviewing I was struck by the skill of one woman in never mentioning the names of co-workers. They were all 'colleagues'. Another woman would only speak critically of her long-dead parents off-tape. Yet another woman deleted all evaluative comments on individuals from her transcript, except the complimentary. A fourth woman wrote of her expectations of me, 'There is a big difference between private revelations to one trusted interviewer, and the material one could wish to be publicly displayed. I would not want anything directly critical of my family or colleagues'. Here I have more power than the women and therefore more responsibility. After negotiation one woman's defiant 'He was a pig' may become the muted 'She found him challenging' in my representation of their words. I started from the assumption that anything I wrote or said about the participants and their lives I would be prepared to justify in writing or speech to them directly. This, at points, has certainly halted my pen and shut my mouth. Cruel gossip abounds in the academic world, as elsewhere, and horror stories of historians being sued for libel are salutary.[9] My task therefore must be to maintain the women's confidence that I will always respect their confidences.

My goal would be to do this while meeting the intellectual requirements of my research. What I see as trivial in the transcripts may well be of towering importance to a woman: my interpretation would be crucial. In her chastening discussion of interpretative conflict in oral narrative research Katherine Borland reveals the enormous power the researcher has in reframing her informants' stories. The elderly woman, her grandmother, Beatrice Hanson, whose story she had interpreted felt compelled to engage in lengthy discussion and write a fourteen page letter explaining her viewpoint. Beatrice Hanson's passion (1991: 70) is instructive and that its focus should be an issue of feminism is cruelly ironic. She writes:

> So your interpretation of the story as a female struggle for autonomy within a hostile male environment is entirely YOUR interpretation. You've read into the story what you wished to – what pleases YOU. That it was never – by any wildest stretch of the imagination – the concern of the originator of the story makes such an interpretation a definite and complete distortion, and in this respect I question its authenticity. The story is no longer MY story at all. The skeleton remains, but it has become your story. Right?

If we are to capture the complexity of people's lives in the past and leave them their stories in the present we must from the beginning of our research assume that there will be distinct differences from our own world views with those we interview and find ways to acknowledge and respect these in the analysis. Borland pleads time constraints for not having handled her initial research with Hanson more collaboratively and, after reflecting on the interpretative process the two women engaged in subsequently, proposes that feminist scholars:

> Open up the exchange of ideas so we do not simply gather data on others to fit into our own paradigms once we are safely ensconced in our university libraries ready to do interpretation. By extending the conversation we initiate while collecting oral narratives to the later stage of interpretation, we might more sensitively negotiate issues of interpretive authority in our research (1991: 73).[10]

This is a laudable intention. But while my aim is that the participants in this project will, wherever possible, eventually stand both fully named and interpreted in the research, the process to date has indicated powerful impediments to meeting my initial optimistic expectations of the oral history review.

At the outset of the project, discussions of feminist research methodology and of the theory and practice of oral history provided me with models of what might constitute an acceptable methodology for my work. Yet in reflecting on the progress of my research (as I first formulated it, then prepared for and undertook the interviews and eventually produced transcripts that were acceptable to the women) I discovered that at every stage both conscious and unconscious decisions on the part of myself and my interviewees impinged on the process in unanticipated ways. Some censoring of ideas and emotions, whether in writing, speech or action, was present at every stage of the research and for all the participants. As the ramifications of this censorship were carefully negotiated by us all, the possibility of producing a collaborative research project receded. Giving the women

academics I am studying a place in the history of both sociology and post-war Britain is my overall aim; but while ideals of a collaborative feminist oral history methodology still remain convincing, the hurdles to achieving this are daunting.

Acknowledgements

My thanks to the women sociologists with whom I am working on this project and to Susanne Byrne, Alison Oram, Sally Alexander and Barbara Harrison for their support.

Notes

1. Arlin Mickelson 1994, Thomson 1994 and the contributors to Vaz 1997 are recent exceptions.
2. The links between oral history, action research and community development are apparent in Benmayor's work with the El Barrio Popular Education Project discussed in Vaz 1997.
3. Merton has commented on the difficulty in recalling reading (Riley 1988: 18).
4. This can also sometimes arise from the timidity of the interviewer. Summerfield (1998: 179) illustrates how 'the taboos of the 1940s . . . interacted with the homophobia of the 1990s to render us as interviewers too confused and embarrassed to pursue the discussion of lesbianism in the army'.
5. Compare my decisions with those of Hambrick in a rare discussion of the process of transcribing (Hambrick 1997: 74).
6. Recent examples include essays in Vaz 1997 and articles in auto/biography.
7. Penny Summerfield's consideration of anonymity led her to use pseudonyms, despite her interviewees' wishing to be named. While agreeing with her that anonymity screens interviewees 'from the ultimate expression of the power imbalance in the oral history relationship', it must be said that it also helps screen the researcher from the criticisms of those whose lives she has chosen to interpret (1998: 26).
8. Some of the women whose work I am researching have been centrally involved in issues of professional ethics in social research. The writings of Ann Cartwright, Meg Stacey and Jennifer Platt, for example, have helped my deliberations on ethical issues in oral history and have sensitized me to the need for a more reflective research practice.
9. As were circulating about Australian universities at a recent RHA Oral History Conference, April 1998. See also Dunaway 1992.
10. From a social work perspective Clifford and Cropper have also argued the ethical necessity for a more reflexive consideration of the auto/biographical. (Autobiography, Vols 3:1 and 3:2, 1994).

References

Mickelson, A. R. (1994) A feminist approach to researching the powerful in education. In G. Walford (ed.) *Researching the Powerful in Education* (London: UCL Press), pp. 132–150.

Benmayor R. (1991) Testimony, action research, and empowerment: Puerto Rican women and popular education. In S. Berger Gluck and D. Patai (eds) *Women's Words. The Feminist Practice of Oral History* (New York: Routledge), pp. 159–74.

Berger Gluck, S. and D. Patai (eds) (1991) *Womens's Words. The Feminist Practice of Oral History* (New York: Routledge).

Blaxter, L., Hughes, C. and Tight, M. (1996) *How to Research* (Buckinham: Open University Press).

Borland, K. (1991) 'That's not what I said': interpretive conflict in oral narrative research. In S. Berger Gluck and D. Patai (eds) *Women's Words. The Feminist Practice of Oral History* (New York: Routledge), pp. 63–76.

Bornat, J. (1994) Is oral history auto/biography? *Auto/biography* , 3, 17–30.

Burgess, R. (1984) *In the Field: An Introduction to Field Research* (London: Routledge).

Chamberlain, M. (1995) Gender and memory: oral history and women's history. In V. Shepherd, B. Brereton and B. Bailey (eds) *Engendering History: Caribbean Women in Historical Perspective*, (Kingston, Jamaica: Ian Randle), pp. 94–110.

Clifford, D and Cropper, A. (1994) Applying auto/biography: researching the assessment of life experiences. *Auto/biography* 3, 47–58.

Coleman, P. G. (1991) Ageing and life history: the meaning of reminiscence in late life. In S. Dex (ed.) *Life and Work History Analyses: Qualitative and Quantitative Developments* (London: Routledge), pp. 120 – 143.

Corbin, M. (1971) Problems and procedures of interviewing. In J. M. and R. E. Pahl (eds) *Managers and Their Wives* (London: Allen Lane), pp. 286–306.

Delamont, S. (1989) *Knowledgeable Women: Structuralism and the Reproduction of Elites* (London: Routledge).

Dunaway, D. K. (1992) Method and theory in the oral biography. *Oral History* 20, 40–44.

Finnegan, R. (1997) 'Storying the self': personal narratives and identity. In H. Mackay (ed.) *Consumption and Everyday Life* (London: Sage), pp. 65–104.

Gilbert, N (ed). (1993) *Researching Social Life* (London: Sage).

Glucksmann, M. (1994) The work of knowledge and the knowledge of women's work. In M. Maynard and J. Purvis (eds) *Researching Women's Lives from a Feminist Perspective* (London: Taylor and Francis), pp. 149–65.

Halsey, A.H and M.A.Trow. (1971) The British Academics (London: Faber).

Hambrick, A (1997) You haven't seen anything until you make a black woman mad. In K. M. Vaz (ed.) *Oral Narrative Research with Black Women* (Thousand Oaks, California: Sage).

Hamilton, S. (1992) Interviewing the middle class: women graduates of the Scottish universities c. 1910–1935. *Oral History* 10, 58–67.

Harding, S. (ed) (1987) *Feminism and Methodology. Social Science Issues* (Bloomington, Indiana: Indiana University Press and Open University Press).

Harrison, B. and Lyon, E. S. (1993) A note on ethical issues in the use of autobiography in sociological research. *Sociology* , 27, 101–109.

Kemmiss, B. (1988) Action research. In J. Keeves (ed.) *Educational Research, Methodology and Measurement: An International Handbook* (Oxford: Pergamon).

Maynard, M. (1994) Methods, practice and epistemology: the debate about feminism and research. In M. Maynard and J. Purvis (eds) *Researching Women's Lives from a Feminist Perspective* (London: Taylor and Francis), pp. 10–26.

Merton, R. K. (1998) Some thoughts on the concept of sociological autobiography. In M. W. Riley (ed.) *Sociological Lives* (Newbury Park: Sage), pp. 16–21.

Oakley, A (1981) Interviewing women: a contradiction in terms. In H. Roberts (ed.) *Doing Feminist Research* (London: Routledge and Kegan Paul), pp. 30–61.

Plummer, K. (1983) *Documents of Life: An Introduction to the Problems and Literature of the Humanistic Method* (London: George Allen and Unwin).

Poland, B. D. (1995) Transcription quality as an aspect of rigor in qualitative research. *Qualitative Inquiry* 1, 290–310.

Popular Memory Group. (1982) Popular memory: theory, politics, method. In R. Johnson *et al.* (eds) *Making Histories; Studies of History, Writing and Politics* (London: Hutchinson).

Pugh, M. (1992) Women and the Womens Movement in Britain, 1914 – 1959, (Basingstoke: Macmillan Education).

Puwar, N. (1997) Reflections on interviewing women mps. *Sociological Research Online* 2: 2: http://www.socresonline.org.uk/socresonline/2/1/4.html

Riley, M.W. (ed) (1998) Sociological Lives (Newbury Park: Sage).

Samuel, R. and Thompson, P. (eds) (1990) *The myths We Live By* (London: Routledge)

Scanlon, J. (1993) Challenging the imbalances of power in feminist oral history. *Women's Studies International Forum* 16, 639–45.

Stacey, J. (1991) Can there be a feminist ethnography? In S. Berger Gluck and D. Patai (eds) *Women's Worlds. The Feminist Practice of Oral History* (London: Routledge), pp. 111–19.

Stanley, J. (1996) Including the feelings: personal political testimony and self-disclosure. *Oral History*, 24, 60–67.

Stanley, L. (1993) On auto/biography in sociology. *Sociology*, 27, 41–52.

Summerfield, P. (1998) *Reconstructing Women's Wartime Lives* (Manchester: Manchester University Press).

Swindells, J. (ed.) (1995) *The Uses of Autobiography* (London: Taylor and Francis).

Thomas, R. (1990) Ancient Greek family tradition and democracy. From oral history to myth. In R. Samuel and P. Thompson (eds) *The Myths We Live By* (London: Routledge), pp. 203–15.

Thomson A. (1994) *Anzac Memories. Living with the Legend* (Melbourne: Oxford University Press).

Vaz, K. M. (ed.) (1997) *Oral Narrative Research with Black Women* (Thousand Oaks, California: Sage).

Walford, G. (ed.) (1994) *Researching the Powerful in Education* (London: UCL Press).

Webb, S. and Webb, B. (1932) *Methods of Social Study* (London: Longman Green).

Wilson, E. (1980) *Only Half Way to Paradise. Women in Postwar Britain* (London: Tavistock).