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What's Rapport Got To Do With It? The Practical Accomplishment of Fieldwork Relations Between Young Female Researchers and Socially Marginalised Older Men

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Drawing on field notes, interview transcripts and personal reflections, this paper describes an ethnographic research project as a practical accomplishment. The project has employed two young female fieldworkers in negotiating and documenting the social worlds of socially disadvantaged and marginalised older men in inner city Sydney, Australia. We provide a rich description of the various processes involved in this kind of research such as gaining entry, recruiting participants, obtaining consent and conducting interviews. Our analytical and interpretive focus is on the social relationships of fieldwork and the problematic role of rapport as the ideal (or only) basis for such relationships. We show how these relationships and the information they generate have been variably and situationally accomplished in our project. Our findings suggest that communicative relationships in field research can take a variety of forms that produce useful data but that these are not necessarily illustrative of rapport between researcher and researched. We conclude by arguing the need for the methodological literature of ethnography to develop a new analytical vocabulary for describing research practice and a conceptual framework that moves beyond neo-positivist and normative prescriptions for doing 'good' fieldwork.

Introduction

Over recent years the social sciences, including ethnography, have witnessed 'a radical reorientation to the study of lives' (Gubrium & Holstein, 1995, p. 45). In contrast to the 'old' ethnography, which 'cast its subjects as mere components of social worlds, new ethnography treats them as active interpreters who construct their realities through talk and interaction, stories, and narrative' (Gubrium & Holstein, 1995, p. 46). In this framework, social phenomena are understood as human practice undertaken by social agents or actors located within specific relational settings.

Formerly such settings and the potential 'informants' they contained were viewed as sources of information about the phenomenon under investigation. The settings themselves were to be systematically observed and described, and their participants induced to reveal the 'insider' perspective. The task of the ethnographer here was simultaneously to engage in careful observation and to participate in the lifeworld in ways that minimized her or his disruptive 'outsider' presence in the setting. A voluminous methodological 'how to' literature developed around the old ethnography (e.g. Berg, 1998; Bogdan & Taylor, 1975; Lee, 1993; Shaffir &

Stebbins, 1991; Spradley, 1979; Tewksbury & Gagne, 1996). This canon dispensed a broadly similar set of guidelines and instructions for doing 'good' ethnography. While invariably hedged with cautionary disclaimers about the messiness and unpredictability of 'real life' fieldwork (often accompanied by an amusing or embarrassing illustrative anecdote), such texts nonetheless managed a reassuring tone: follow these instructions and you will at least maximize your chances of achieving a successful research outcome.

This literature pays particular attention to the 'management' of relationships between researchers and the people they are studying. Such relationships are viewed simultaneously as the source of many fieldwork difficulties and the key to the successful accomplishment of the research. A pivotal concept here is "rapport" which, according to Spradley (1979), refers to:

a harmonious relationship between ethnographer and informant. It means that a basic sense of trust has developed that allows for the free flow of information. Both the ethnographer and the informant have positive feelings about the interviews, perhaps even enjoy them. (p. 78)

Achieving rapport is accorded particular salience when the research involves oppressed, stigmatized or otherwise disadvantaged groups. According to Tewksbury & Gagne (1996), for example:

The importance of a strong, positive rapport is intensified when the research endeavour involves a sensitive topic or a stigmatised population . . . Among those of discredited character, body, or tribe . . . there commonly exists a trepidation concerning the approach of outsiders . . . The key to successful research with stigmatized groups is their willingness to "embark on a risky course of action" . . . This decision is based on trust and the rapport that precedes it. If and when the stigmatized feel accepted and respected and perceive some degree of similarity with their explorers, a relationship can proceed, and the qualitative researcher can pursue investigation of inhabitants' identities, identity components, and experiences. (p. 128)

Reading much of this literature is to gain an impression of the ethnographic researcher as a director or stage manager as well as an actor in the scene. Indeed, the language of dramaturgy is widely deployed in methodological texts (e.g. Berg, 1998). Most authors acknowledge, at least rhetorically, that research subjects are also interpretively active and that relationships are a two-way affair. For the most part, however, discussion of the activity of participants focuses on those "management strategies" (Tewksbury & Gagne, 1996, p. 129) that the savvy researcher can deploy to stamp it out. The active participant is constructed as an "obstacle" to be overcome; a "hostile" subject who may, nonetheless, warm up to a correctly performing researcher over time (Bogdan & Taylor, 1975); someone who may put up a "front" which careful application of technique can break down (Lee, 1993); or the perpetrator of "evasion tactics" which need to be overcome (Berg, 1998, p. 27). Ultimately the researcher can - and must - control the encounter. If she "gets it right", participants will respond with trust, candour and "quality" data.

Clearly, such a conceptualization of the research process rests uncomfortably within the framework of the new ethnography. In particular, the idea that participants' accounts convey or reflect the 'reality' of their lives, the 'truth' about a setting or situation or an 'authentic' identity has been extensively critiqued (see Atkinson & Silverman, 1997; Gubrium & Holstein, 2001).

Rather, the realities of social settings, and identities themselves, are now conceptualized as situational accomplishments that are narratively assembled through interpretive practices (Gubrium & Holstein, 1995, p. 46). As Somers and Gibson (1994) point out, the concept of narrative identity entails a 'shift in the interpretation of action from an a priori categorization to a focus on contingent narratives of meaning' (Somers & Gibson, 1994, p. 71). From this perspective, the rationale for deploying "research management" strategies in the first place loses much of its (neo-positivist) epistemological grounding: if all talk is situated and *al*l meaning is contingent, there can be no 'good' (accurate, truthful, authentic) or 'bad' data.

What, then, does this mean in relation to the practical accomplishment of the research process itself? In the older ethnography, the desired "output" from one's fieldwork practice - "good quality" data - was linked to a clearly identified (if sometimes problematically realizable) set of "inputs". These latter consisted of such things as the real or assumed characteristics of researchers and the behaviours they deploy in the field. The central purpose of such characteristics and behaviors was (as we have seen) to generate trust and develop rapport with participants. Thus the methodological canon advised fieldworkers to manage their self-presentation in ways that downplay (the appearance of) difference between themselves and the people they are studying. Recommended strategies included: adopting appropriate styles of clothing, language and demeanour to reduce social distance (Bogdan & Taylor, 1975; Fontana & Frey, 1994; Hammersley & Atkinson, 1983); making a show of participating in activities common in the environments that one's participants frequent (Berg, 1998; Bogdan & Taylor, 1975; Lee, 1993; Shaffir & Stebbins, 1991; Spradley, 1979); and establishing common ground by utilising existing skills, knowledge and biographical similarity (Bogdan & Taylor, 1975; Fontana & Frey, 1994; Hammersley & Atkinson, 1983; Lee, 1993; Shaffir & Stebbins, 1991). Indeed, it has sometimes been proposed that interviewers should be selected on the basis of "personal and visible physical or social traits that complement those of the population under study" (Tewksbury & Gagne, 1996, p. 129).

What, if any, is the relevance of such recipes in the new ethnography? To mix our metaphors, if the goal post has shifted - or, indeed, if a whole new game is being played - is the old (conceptual) equipment still appropriate? We have been obliged to confront these questions in the course of conducting an ethnographic investigation into the social worlds of disadvantaged and marginalized older men. For reasons that we explain below, the project has exposed us to a heady mix of epistemological, methodological and practical concerns. As we also explain, neither the old nor new ethnographic literatures adequately address some of these concerns. In the discussion which follows we offer an empirically grounded account of the fieldwork experience. We begin with an overview of the project and identify the key conceptual and methodological issues which it raises in relation to the "conventional scientific attitude" (Gubrium & Wallace, 1990) towards the researcher-researched relationship. We describe how the fieldworkers have negotiated their interactions with the men and document the range of relationships that have emerged. We show how these relationships and the information they generate have been variably and situationally accomplished within a single project and suggest that idealized prescriptions for "rapport" provide an inadequate basis for understanding fieldwork interactions and the information they generate. Our discussion raises more questions than it answers and we conclude by outlining some of the issues which warrant further consideration.

Background

The Ageing Men's Health Project is an ethnographic study of low-income, single men aged 50 years and older living alone in inner urban Sydney, Australia. All of these factors - being male, on a low income, never or previously married, living alone and in insecure housing - are associated with elevated risk of poor health and premature mortality (Mathers, 1994). Such men pose a particular challenge for services systems that depend on older people having adequate housing and families to provide care for them. A key aim of the project is to examine how ageing men outside middle-class suburbs and nuclear families manage their daily lives, particularly in the face of declining health, what supports are available to them, and what their attitudes and experiences are in relation to the receipt of assistance. A second aim is to examine the pathways through which some men enter later life with multiple deficits, and life history materials have been collected for this purpose.

Our study site, the Local Government Area of South Sydney, is a cluster of suburbs on the fringe of the Sydney CBD that contains a substantial concentration of men in our target group, and we chose it for this reason. Despite increasing gentrification, parts of the area retain their historical association with poverty characteristic of the inner areas of large cities everywhere. It affords a range of habitats for low-income single persons, including private rental accommodation in lodging houses and cheap hotels, public housing, aged care facilities, and homeless shelters. Importantly for our methodological purposes, it also contains a number of relatively 'bounded' settings that the men were likely to frequent and which were accessible to us as locations for participant observation. These included charitable drop-in and meals centres, emergency shelters, short-term sobering up facilities, and the streets and parks around them. Other less mobile men were accessed with the assistance of local community service workers.

Two of the authors of this paper, Denise Touchard and Maree Porter, were employed as fieldworkers on the project. They spent time at these services and in public places, participating in volunteer work and generally "hanging out" in order to collect observational data and to recruit men for in-depth life history interviews[1].

From the standpoint of methodological convention, the project had several inherent difficulties. First, the study population contains persons whose "deviant" or "stigmatized" characteristics (homeless, alcoholic, mentally ill, criminal) cast them as "difficult" subjects who are likely to pose particular problems of access and relationship. Secondly, the employment of two young middle class women might be expected to exacerbate such difficulties in impoverished and predominantly all-male settings. Nor can participation in many activities common in the environment, such as going on an alcoholic binge or recovering from the effects of such, be recommended as part of a research strategy. Biographical similarities, one can assume, will be limited at best.

Nonetheless over a 12-month period both fieldworkers have been able to negotiate the field (Grills, 1998, p. 11) effectively enough to record 1200 hours of observation and complete in-depth life history interviews with 67 men. Our discussion draws on these data (fieldnotes and interview transcripts) and on two reflective reports of their experiences written by the field workers some eight months into the project. Like others pursuing research within the framework

of the new ethnography we have premised our study on an understanding that all research accounts are themselves constructions (Atkinson and Hammersley, 1994, p. 252).

The Practical Accomplishment of Fieldwork

Despite our commitment to the perspective of new ethnography we did not entirely dismiss the potential value of conventionally prescribed fieldwork strategies for achieving access and relationship. In fact we had little else to go by, a point we return to in later discussion. So the researchers employed as many of these strategies as were feasible. While in the field, for example, they wore loose fitting casual clothes and went without make-up or jewellery, both to de-sexualize their appearance as part of a conscious safety strategy (Russell, Porter, & Touchard, 2001) and to achieve the accepted ethnographic goal of blending into the setting. They also participated in a variety of ways in their respective settings. As well as sharing casual chat, jokes, meals and smokes, they took art classes with the men, played sport and games, watched TV, went on bus trips and attended chapel services. While biographical similarities with the men were, as expected, minimal, both fieldworkers found areas of common ground with some of the men. Denise, for example, had grown up in the same suburb as two of her participants, while Maree shared, variously, a rural background, Catholic upbringing, and familiarity with military life (through having a relative in the armed forces) with others.

Did they succeed in accomplishing rapport with the men who agreed to participate in the research? Interestingly, the methodological literature pays little specific attention to the question of how researchers are to recognize rapport when they see it. Given the central role accorded to fieldwork relationships in general, and rapport in particular, it is unfortunate that these phenomena are seldom examined empirically. The methodological discourse tends to concern itself with "the environment around the phenomenon rather than with the phenomenon itself" (Silverman, 1992, p. 179) --that is, with what (supposedly) precedes and follows the accomplishment of rapport. However the link between the two remains, as Dingwall (1992) suggests in another context, an empirical "black box"(Dingwall, 1992, p. 162). Some discussions seem to imply that the mere accomplishment of an interview constitutes evidence that rapport has been achieved. Others refer to certain dramaturgical cues in the respondent's demeanour and/or to the disclosure of private or intimate information as indicative of rapport. Sword (1999) for instance reports "participants readily entered into dialogue with me, demonstrated relaxed postures, and shared quite personal information, suggesting that they felt comfortable with me" (Sword, 1999, p. 272). Taking these somewhat equivocal positions as a guide (and noting the circularity of the argument), we offer the following speculative account of rapport in our project as a practical accomplishment.

As far as our recruitment rate is concerned, roughly 60 percent of men approached for an interview agreed to participate. Maree and Denise had almost exactly the same success/failure rate. However there is no clear evidence from the researchers' fieldnotes that "strong, positive rapport" was consistently or systematically linked to consent or refusal to be interviewed. Thus some men whose interaction and demeanour displayed dramaturgical evidence of a "harmonious relationship" (and which the fieldworker had interpreted as such) nonetheless refused to be interviewed. Maree's fieldnotes from a community centre where men in the target group go for

cheap meals describe how her interactions with some men, otherwise eager to talk and apparently comfortable and interested in the exchange, failed to generate an interview:

After a discussion on movies from the 30s and 40s, I asked if he would let me interview him and he said no. My request appeared to have ended our conversation and after a few minutes he politely excused himself and headed home.

Ernest[2] gave anecdotes about his football days and other highlights of his life; many of which he had mentioned when we met previously. He did not mention the project or the pamphlet I gave him, and he diverted the conversation away from the project when either (another patron) or myself mentioned it.

At the same time, fieldnotes record numerous occasions where men had voiced undisguised suspicion of the motivations and value of the project, and even outright hostility towards those involved in it. For example:

Another man joined us at the table . . . He appeared to be quite sharp and witty with his comments. I introduced myself and my purpose for being at the centre and he immediately relayed his opinion of university research and the exploitation of both researcher and subject for political and economic gain.

However, only a few men who were openly critical in this way actually refused to be interviewed. In one case, a 75 year-old subject remained "hostile" throughout the interview. Ralph, like a number of other men, did not consent to audiotaping. Maree's documentation below reveals little that could be construed as "rapport". In spite of this, the reconstructed interview with Ralph looks very much like any other and contains the same kinds of information about his life history and present circumstances.

Ralph dominated the interview and took the opportunity to discuss in detail his experiences of running his own business. He complained about the research project, its usefulness and how it serves those in universities and not the men being interviewed . . . Throughout the interview he made comments about "a woman's place is..." and asked me personal questions about marriage and having children. At times he was quite stern, challenging me and engaging in banter in an attempt to coerce me into arguing with him. I gave neutral and non-committal answers to his questions, some of which I thought were inappropriate and offensive. I felt that he was using the interview as an opportunity to air his grievances and opinions about women in general. He made a comment upon leaving the room after the interview, something like, "What would you do if I said or did such-and-such, would you scream?". Continuing with his style of banter I replied with a smile, "I'd take you out the back and beat the crap out of you." He laughed, mocking me. I can't remember the specific content of his question, but I found it offensive and felt the need to assert myself. I thanked him for participating in the interview and, although he was quite aggressive, I did not feel threatened, and when he left he was in a happy mood.

Overall, then, our data fail to reveal any systematic relationship between "harmony" and "trust" (or their absence) and the achievement of an interview. As we describe below, men's decisions to participate (or not) in the project cannot be attributed in any straightforward fashion to the

characteristics or intentions of the researchers but need to be examined as a situational accomplishment.

Interests, Intentions and Identity

Denise and Maree have had to negotiate a range of understandings of who they are and what they are doing. We have never attempted to conceal the fact that we are conducting research, nor the purpose of that research. This would not have been possible, even had we wished to, since it is a requirement of the University's Institutional Ethics Committee that potential participants be fully informed (via a written Participant Information Sheet) of the study's nature and intent and that they consent (also in writing, wherever possible) to participate in interviews.

Nonetheless, as numerous others have found, full disclosure of one's status and purpose as a researcher-observer is not always appropriate or feasible in the ebb and flow of "street-style" ethnography (Punch, 1994, p. 90). We have addressed the ethical implications of our work on a situationally-specific basis but within an overarching set of guidelines. Central to these are: no active deception in relation to one's research role and purpose; as full a disclosure regarding the study and its aims as any participant seeks; and active disclosure (including documentation) prior to obtaining consent for an interview.

In spite of this the men have assigned a variety of identities to the fieldworkers. In all cases, of course, this process of identity-construction begins with what people can see. In such predominantly male settings, gender is an acutely salient dimension. In interaction with other personal characteristics of the researchers (young, white, not obviously poor), and within the social and political relations of the setting, the men have "placed" Denise and Maree in a variety of ways, and with variable consequences. Such placement, however, can never be understood simply as the result of researcher characteristics. It is also crucially dependent on the social and organizational specificities of the setting. As Grills has pointed out, researcher characteristics are one of many interacting influences on how others understand the "interests, intentions, and identities" of ethnographers (Grills, 1998, p. 11).

In our case, an important setting variable was the extent to which the relevant auspicing organization sought to exercise control over the conduct and access of the fieldworkers. The services and facilities that we used as observational and recruitment sites were operated by several non-profit organizations, mostly Church groups of different (Christian) denomination. Some gave us freer reign in their facilities than others.

Maree was subject to more organizational surveillance and control than Denise. For instance there was a policy at one of Maree's hostel sites forbidding women to approach the men in their rooms. As well, staff in that facility acted as gatekeepers in the recruitment process. As a result staff effectively constructed her identity when they introduced her to potential interviewees. Despite having given an accurate description of her role (as a full-time employee on a research project) to staff beforehand, she was frequently introduced as someone "doing a course at uni", "doing a paper", or "studying at uni".

It is likely that this kind of identity construction affected her potential relationships with the men. Several facilities regularly accept high school students on work experience placements. Denise observed one such visit, noting that the students came in a group and wandered around looking lost, even fearful. In her field notes she described the overt display of an "us" and "them" dynamic which the encounter entailed, and reports comments from some of the men indicating resentment at being subjected to the students' gaze. One older man told her that he always left the premises whenever students visited.

Maree observed that her success rate for obtaining interviews where a staff member acted as intermediary seemed to be related to how the project was explained. Refusals were more likely when she--and the project--were "dumbed down" as some kind of student exercise. At the one facility where a worker (to Maree's surprise) gave the men an accurate account of the project and of what the men would be required to do during an interview, she had a much lower refusal rate.

When not undergoing formal introductions to men, both fieldworkers were most commonly placed as workers, voluntary or paid. While both of them performed a variety of staff-like helping activities, Denise attempted more consciously to avoid being placed as a worker. Yet time and again, she writes, conversations would be initiated with "So, you work here?" followed, often, by a request for help. While on occasion she relayed such requests to staff, mostly she let people know they needed to "speak to one of the guys in the office" and attempted to clear up the mis-placement. She reports that no interviews eventuated from people who first placed her as a worker and asked for help.

Maree's field setting involved her in much closer identification with a work-type role. As well as being regularly accompanied by a service "guide", she was routinely assigned work-type functions by staff while on the premises. Thus the men would nearly always assume that she worked there and, often, that she was a new staff member associated with her particular guide at the moment. If she was seen with the nursing staff, for instance, the men on several occasions began to recount their health histories or requested medication. Being seen with admissions staff or care workers led to approaches involving requests for directions, meal times, or items such as towels, soap or shampoo.

The outcome of such identity construction seems to have varied according to the men's own relationship with the organization and its staff. Those men who remained visibly uninvolved in informal interaction with staff mostly refused to participate. Those who regularly sought out relationships with staff (and were favourably regarded by the latter as compliant and amiable) were more likely to make themselves available for interviews.

Denise on the other hand was more often left to her own devices and was able to approach (or be approached by) potential interviewees directly. As a result, her initial placement by the men and their response to her presence were more varied. Some in fact did not try to place her at all, simply shaking hands and exchanging names. A few confronted her as an unfamiliar face, asking straight out "What are you doing here?" In such cases, she offered a full account of the project and her role in it. This opportunity for self-placement had variable outcomes with no discernible pattern emerging: some men refused outright to be interviewed, others agreed at the time but

subsequently practiced a variety of evasion tactics (Berg, 1998, p. 27), while others were willing participants.

A rather different basis for relationship was also created by her more ambiguous placement in relation to the auspicing service. On occasion she was placed as a "woman in crisis" and offered money, free meals and accommodation. For example:

He rattled around the change in his pocket and asked, "You're not short of a bob, are you?"

He thought I was a "lady in crisis" and was going to listen to my story . . . (later that week) he told me he has been considering things and thinks we should move in together. He is willing to do the work and pay most of the rent.

In another instance, an older man who had spent some time observing her speaking with other service users approached her to start a getting-to-know-you conversation. All three of these service users became participants and interviewees.

Finally, as other research has shown (Russell, 1999), people may participate in research projects with a variety of preformed or emergent purposes. Some of the men were clearly taking advantage of the researchers' presence to pursue agendas of their own. In several cases, participation was used as currency to bargain for favours, information, assistance or attention.

Maree: I realised about half way through the interview that Dan wanted my help in finding available avenues to assist him in gaining a place in a particular public housing block in the area . . . I found it a bit of a challenge maintaining direction during the interview.

Denise: One of the men who interrupted my interview with Wilbur pulled me aside after the interview "because I need a social worker to help me". After explaining my purpose in being there and referring him to one of the other staff he avoided an "interview" but told me his problems anyway.

Just as gaining entry and obtaining consent were variable experiences, so too were the dynamics of subsequent interaction and the "quality" of the data that were generated through it. Again, our process data indicate the inadequacy of attempting to explain such variability through an account of researcher "inputs". All fieldwork encounters, including the production of a formal interview, occur in specific social relational settings of action in which both participants are interpretively active (Holstein & Gubrium, 1994).

Interviews as Practical Accomplishments

To begin with the men brought a range of diverse characteristics of their own to bear on the course of the project and on the way they approached their participation in it. For example, they varied considerably in the extent to which they sought to understand the project and their expected role in it. Some declined to read the information sheet. In some cases this was due to a visual impairment (which the men acknowledged) and/or problems with literacy (which they did not), and the sheet was read to them. Some gave verbal consent to the interview but refused to sign the informed consent sheet. Others read the information carefully, and a small number asked

for further clarification before giving consent. Ernie, for example, "asked if our project was related to the 'one advertised on the television that the Government was running', and I said no, it was...not a direct initiative of the Government."

Quite a high proportion, slightly over 30 per cent, did not consent to the interview being tape-recorded and extensive notes were taken instead. While accounts of their lives could thereby be reconstructed, this meant that certain analytical techniques (e.g., involving analysis of verbatim narrative) could not be applied.

The interviews themselves were also highly diverse productions. They ranged in length from 10 minutes to four hours. One of the shortest, Denise's interview with Charles, ran to seven pages of (single-spaced) transcript. It comprised little more than brief, sometimes monosyllabic, responses to the questions, with the interviewer's prompts and probes failing abysmally to generate the narrative production we sought. For example:

Denise: So have you ever slept out?

Charles: Oh yeah, plenty of times.

Denise: You must have some stories to tell?

Charles: Yeah.

By contrast, Denise's interview with David ran to 28 pages and displays a complex and coherent narrative structure. Several stretches of continuous talk by David required more than a page of transcription and one accounted for three and a half pages.

In at least some cases truncated responses were undoubtedly due, in part at least, to memory problems associated with alcohol-related brain damage. Several residents of crisis accommodation could not remember how long they had lived there, and one wasn't sure how old he was ('Oh I'd be 50, around 60'). Some of these men openly disclosed their impairment, and the reasons for it, and apologised for not being able to provide the kind of life history detail we were looking for. For example, when asked to talk about his early work life John faltered, "It was so long ago. Oh no I can't recall, I can't recall whether I have . . . my memory is a little bit (doesn't finish explanation)". Others did not refer to the issue.

Interviews were also shaped by the degree to which different men sought to collaborate with the study's aims and the ways in which they interpreted their role in it. Some seem to have readily accepted the legitimacy of our agenda, even though they may not fully understand it, and positioned themselves as "informant"[3]. They would seek assurance that they were "getting it right" by giving relevant information. For example:

Kenneth: Is this what, is this what we're getting at? Is this what you wanted? . . . All right Maree, how we goin'?

Luke: You must excuse me, Maree . . . I hope I'm not boring you, I do ramble on . . . Yes, please let me know if I do ramble on.

David (asked to tell his life story): (long pause) From when do you reckon Denise?

Other interviewees seemed quite clear in their own minds as to what the research was about, and constructed their storylines accordingly. Unlike David, Kenneth had no doubt where his life story began, "You'd better start with my health history."

Some participants imposed limits on the timing and duration of the interview:

(Rover) told me I could ask him five questions today and then another time I could ask more.

(Albert said) we could only talk for 30 minutes, as he had to be home by 2pm to watch a favourite television program.

There was also variability in the way men responded to questions that opened up "sensitive" topics. Some men spoke freely about intimate details of their lives (even when these had not been asked for), including (in a couple of cases) issues to do with their sexual activities and performance. For example, asked about his marital status Lionel said:

Never married but I've lived with a couple of women . . . I lost a few days at work . . . She said, "Keep laying down" . . . I'm laying there trying to sleep and she's saying, "You better raise up." I said, "Come on . . . I've already given you two."

Others refused to pursue certain lines of questioning at all. For example, Rover demanded the tape recording stop when he realized he'd wandered into a personal 'no-go' area, "I've got kids and I'm allowed to go home and see my kids . . . cut, cut, cut." One man declined on tape to discuss the reason for his having once been in prison, on the grounds that "it's got nothing to do with the questions you want to ask", but proceeded to volunteer the information once the tape was turned off. Another man reacted angrily when Maree asked him if he got depressed, telling her that "if you was a lady" and "if you're clever" she should know better than to "ask a man" such a question (but he answered it anyway).

One of Denise's participants volunteered to be interviewed before she had asked him, and later reviewed and edited his transcript:

Rover surprised me by wanting to continue talking about the more personal things that he'd warned me off during the formal interview. I have included this information at the end of the transcript. I did so on Rover's instruction . . . (After viewing his interview transcript) He made a few changes where he had missed out a piece of information that he needed to clarify. He also asked that the pseudonym be changed from Henry to Rover.

In addition to these individual differences, the practical accomplishment of interviews was facilitated or constrained within the "specific spatial and temporal horizons of action" (Jessop, 1996, p. 126) within which they occurred. Of the participants who had their own public housing flats three were interviewed in their homes. Their control of a private territory and access to the everyday trappings of hospitality gave these men opportunities and "props" for relationship and identity that were unavailable to others. For example:

Maree: I took a seat on one of the lounges and prepared myself for the interview. Pierce offered me a drink and I accepted the offer of a cold water. Pierce took a seat at his desk and read me a

few poems he had written, one of which was called "The Vagrant" and it was quite good. He took great pride in his art works and poems and I could see that he enjoyed reciting his poetry to me.

Hospitality was not confined to interview situations in the men's place of residence. On occasion Maree was offered food and drinks by men she was talking to at community centres. The men claimed a sense of ownership of these spaces and offered the same pleasantries Maree received from Pierce:

Seth escorted me to his table and set a place for me next to his. He sat me down and fetched a piece of bread and butter and a juice and placed it in front of me . . . Kingsley and Seth fussed over me during lunch; offering me cups of tea; giving me their tomato sauce (sachets) and passing condiments. Throughout the day Kingsley called me "Missy". I thought it was sweet.

(Bob) offered me a cup of coffee to have during the interview, I accepted his offer and he made a cup for himself as well.

Other features of the field sites intruded less positively on possibilities for positive interaction. Men of no fixed abode or residents of cramped rooms lacked even basic social amenities (such as a spare chair). Many interviews had to be conducted under circumstances where privacy was difficult, if not impossible to maintain. Some examples from the fieldnotes:

The interview took place in the sick bay . . . The room had approximately eight beds, there were three other men present during our interview. Privacy was difficult to maintain. One man began listening in to our conversation. I temporarily terminated it at that point. We sat at one of the corner tables in the drop-in centre (in an attempt to create some privacy) . . . There were still quite a few people around and whilst we weren't directly interrupted there was enough noise (including one verbal argument that almost turned into a physical fight) and movement to create constant distractions.

We did not have privacy sitting in the main area of the hall . . . there were approximately fifteen people at the centre . . . When speaking about information that was sensitive Nicholas would lean in towards the tape recorder and lower his voice.

One interview was conducted in a doorway in a lane outside one of the large hostels--and it was raining. Another took place on a bus stop bench. On another occasion a "professional" staff member impinged upon privacy in an interview situation. Maree reports:

(Staff member) interrupted us towards the end of the interview and protested "not to push the men please if they didn't want to answer" and walked off. I was unaware that she had been standing behind me listening to the interview.

The interviewee, Seth, went on to dismiss the worker's concern for him calling her "Big ears!".

Questions of Difference

Of course, the "positionality" (Jaffe & Miller, 1994) of the fieldworkers as young women did influence the research process. However, an important implication of our data is that "questions of difference" (Wong, 1999) did not present themselves in any unidimensional fashion, nor could their effects be generalized as either positive or negative. Naturally occurring social relationships between young women and older men may take a number of different cultural forms, and several of these were represented in our project. One such relationship model with which both participants are potentially comfortable reflects the obvious differentials of age and experience. Maree records several interactions of this kind, in which the men positioned themselves both as men and as experienced persons with authoritative tales to tell. In the course of the interview, they would dispense information and advice to her as a younger, less experienced person. She believes this kind of relationship helped to blur her "higher" status as a researcher and led to a more free-flowing, informal interaction.

Some of these men took a "fatherly" interest in her welfare. For example, at the end of her interview with Peter, a 67 year-old of no fixed abode, she asked if he had any questions. Somewhat to her surprise, he inquired after Maree's future employment prospects. The dialogue went as follows:

Peter: Yes, if you don't mind. What work will you do after this is finished?

Maree: Actually I don't know. I'm employed for the life of this project so when the money runs out I have to look for another job.

Peter: Gee that's a bit tough isn't it?

Maree: A lot of jobs are contract based these days.
Peter: What will you do if you don't get another job?
Maree: I don't know (long pause). I might come here and sit with you.

(We both laughed)

There were other ways for men to take a more active role in destabilizing our implicit claims to expertise or authority and, hence, to make egalitarian identity claims. As we noted earlier, some men were openly dismissive of the project's self-avowed purpose and value, refusing to accept a definition of the situation as something being done in their interests. Open hostility was not usually expressed; rather the value of professional work and status was playfully mocked. Fieldnotes record several occasions where men used banter to 'take the piss':

<blockquote<="" blockquote="">
He asked how my book was going and I told him I needed more interviews. He said, "Interview me!" but he wasn't serious . . . He joked that he would pose for the cover with the other two guys at the table as The Three Stooges.
I have been asked how my "talks" with "us losers" are going and how my book is proceeding (with an offer to pose for the cover shot).

Of course, not all naturally occurring social relationships between older men and young women are potentially positive or even benign. As we mentioned earlier, there were instances (albeit few) of men attempting to sexualize the relationship. One man invited Denise to move in with him. Two others took the opportunity to end the interview, on what for them seemed to be a

positive note, by proclaiming "Don't come back to talk to me unless it's for sex" or asking "Are we going to have any babies?"

However there are other, less objectionable but nonetheless significant ways in which "questions of difference" manifest themselves in research relationships, as Denise's field notes below indicate:

I set up a participant and myself in a dynamic where we both had, from personal experience, competing explanations for one event. I was asked what my Mum and Dad think about me being "in places like this". I could have answered in many ways, but the most ready answer (indicative of my strong feelings) was that I am not in contact with my father. The participant asked for further details and I went on to explain that after my parents divorced, my father disappointed my siblings and I on numerous occasions by making arrangements for contact and failing to show up. During the telling of this I observed the participant's face drop. His competing explanation, as someone close to my father's age, was that as a child you are rarely privy to the full story. I appreciated his explanation as much as he did mine.

Both researchers were at times prepared to disclose details of their personal lives. Such investment of personal identity in the research environment has become a central tenet of the "feminist interview". According to Oakley (1981) "personal involvement is more than dangerous bias--it is the condition under which people come to know each other and to admit others into their lives" (Oakley, 1981, p. 58). In the feminist research literature, such identity work is often constructed as a distinctively, if not exclusively female phenomenon. Ang-Lygate (1996), for example, writes that her decision to share intimate disclosures with her female interviewees was made "in recognition of the preferred styles of communication amongst women"(Ang-Lygate, 1996, p. 378).

Our field experiences however suggest that a sharing of personal information may be equally expected by men. In her early fieldnotes Denise describes one of many occasions when a participant sought such information from her, "He also asked me about my life--this was not inappropriate, rather it was a mutual exchange within the parameters of a 'getting to know you' conversation." Had she not reciprocated, she believes, she would have been given short shrift.

Finally, despite their own "shared location" as young women and self-identification as feminists, Denise and Maree emerged from the fieldwork with very different attitudes towards the men and their own role in the research process. Textbook prescriptions for effective fieldwork often include instructions for researchers to manage the emotional dynamics of the field (Berg & Smith, 1988; Shaffir & Stebbins, 1991), as the impact of one's own feelings about the participants can positively and negatively impact on the rapport building process. In practice, Denise's and Maree's very different feelings highlight the inadequacy of generalized advice along these lines.

For Denise, there is a real dilemma for her in coming to terms with being a feminist who is researching men. She wrote:

The men I have interviewed construct women as both the problem and solution. One spoke of a past partner as a "good breeder". In addition, I am constantly concerned that I am dealing with men who have abused women. There is no doubt that on paper the men who have participated in this research are disadvantaged and marginalized, but I do not feel the compunction to champion their cause in the same way I have for female participants in past projects.

Maree also identifies strongly with feminism, though when in the field she chose for strategic reasons not to wear her "woman symbol". Nonetheless her account documents a shift in "attitude and consciousness" as the project proceeded: "When I became less focussed on my prejudices and the differences between the men and myself, I was able to relax and dissolve myself into the environment". Some eight months into the project she would write:

At this point I have come to understand (without wanting to sound too much like a daytime soap opera) that wealth of experience and knowledge of survival on the streets is admirable, and I have actually come to like some of the men I have interviewed.

Despite these different orientations towards "rapport" with the men, both fieldworkers had identical success/failure rates in recruiting participants for interview.

Discussion

In this paper we have sought to look inside the black box of researcher-researched relationships. We have provided a rich description of such interactions in a project where young women are researching older men. We have set our account in the context of different understandings about how these relationships underpin the conduct of research and the information it generates.

Our analysis has shown that research relationships are situational accomplishments that reflect the specificities of the field--a dynamic interplay of site, researcher and researched. In one sense this is scarcely an original observation. It is a central tenet of the new ethnography, where participants as well as researchers are viewed as interpretively active. But the empirical interest of the new ethnography has largely focussed on the analysis of research outcomes or products, such as accomplished interviews. The implications of the 'radical reorientation to the study of lives' for the actual process of doing research has not been a central concern. The 'rhetoric of training' (Goffman, 1959, p. 55) continues to privilege the epistemological assumptions of the old ethnography. What prevails, in other words, is 'mixed discourse' (Giorgi, 1994) between the literature of 'how to do it' and the literature of 'what it is'.

We have documented our success/failure rate in recruiting participants and displayed the variability in the kinds of data that were generated in interviews. This led us to suggest that the emphasis in much of the research literature on "management strategies" for directing or controlling the course of fieldwork relations is questionable. Again, this simply confirms what others have proposed. Atkinson & Silverman, (1997, p. 311), for example, point out that "interaction is a joint accomplishment by the participants rather than the determined outcome of the researcher's professional agenda". The social order that emerges from a research interaction can never be attributed to researcher "direction". Rather, such order can be seen to be "built through the contingent, embodied, ongoing interpretive work" (Holstein & Gubrium, 1995, p.

264) of both participants, including the "ordinary" members: "In practice, control is interactionally asserted" (Gubrium & Holstein, 1998, p. 10). If this is so, it suggests that much of the practical advice in instructional texts may be redundant or even misleading. If 'management' or 'control' of the research process is neither theoretically justifiable as a goal, nor capable of practical realization in the field, what precisely can we or should we be 'training' novice ethnographers to do?

We have questioned the utility of any normative model of researcher-researched relationship, and specifically one which is predicated on the necessity for rapport. We pointed out that the concept of rapport itself remains vaguely articulated and that its centrality to the research process has been asserted rather than demonstrated. Within the limited framework available, we sought an empirical test of the claimed links between relationship-building inputs and data outputs, and found no systematic relationship. Neither the achievement of an interview nor the amount or "quality" of data could be accounted for in this way: interviews were achieved with hostile and own-agenda participants as well as cooperative ones, and the appearance of "harmonious relationship" did not seem to be a necessary or sufficient condition for the giving of information, including disclosure on intimate or sensitive topics.

In the introduction we warned that we would raise more questions than we could answer. Clearly, findings from a single study cannot be generalized, and we are not claiming to have demonstrated that rapport is a redundant concept. Rather, our analysis suggests that it is useful to work with a conceptual framework that is more inclusive and less normative than one based on rapport as the ideal (or only) type of effective researcher-researched relationship. Our experience shows that communicative relationships in field research can take a variety of forms. Some relationships are characterised by harmony and positive feelings--at least insofar as these were subjectively assessed by the researchers--but we could not show that these consistently delivered particular results. Other encounters failed to generate "good vibes" in the investigators but nonetheless were productive of usable data. If rapport is the answer, perhaps we need to re-frame the question.

Analytical frameworks for treating research practice as an intelligible "order of fact" (Goffman, 1959, p. 232) are, of course, implied in the methods literature and in our own descriptive account. What is needed, as Gubrium & Holstein (1998) have proposed in relation to a different aspect of "methodological self-consciousness", is an "analytic vocabulary" for describing its practical production. Following Goffman (1959), we might identify the main dimensions of analysis as the technical, the political, the structural, the cultural, and the dramaturgical (Goffman, 1959). As Goffman explains, each "can be employed as the end-point of analysis, as a final way of ordering facts' (Goffman, 1959, p. 233), but is simultaneously implicated in all the others. If positivistic assumptions about, and directives for, the practical accomplishment of "good" fieldwork are to be challenged, it is incumbent on those who espouse a "new ethnography" to direct as much critical attention to their processes for gathering data as to techniques for analyzing what has been collected.

In the present context, we can view the dimensions of potential interest in the following way. In Goffman's usage, the "technical" dimension refers to an establishment's "efficiency and inefficiency as an intentionally organized system of activity for the achievement of predefined

objectives" (Goffman, 1959, p. 232). This, of course, is precisely the view of research that directly underpins methods texts and the assessments by others of the "goodness" of particular researchers' work. As we have shown, neo-positivist claims about the technical function of rapport in field research rest on assumptions about the possibility of collecting 'accurate' or 'unbiased' data from and about one's subjects. The new ethnography, by contrast, conceptualizes the research process as a social production and the 'lives' it generates as 'outcomes of joint action' (Atkinson & Silverman, 1997, p. 311). A necessary first step in re-conceptualizing the technical requirements of new ethnographic research is for its practitioners to subject their own fieldwork relations to empirical analysis of the kind we have attempted here. If there is such a thing as 'best practice' in this kind of work, it needs to be grounded in something other than rhetoric.

Politically, an establishment may be viewed "in terms of the actions which each participant (or class of participant) can demand of other participants, the kinds of deprivations and indulgences which can be meted out in order to enforce these demands, and the kinds of social controls which guide this exercise of command and use of sanctions" (Goffman, 1959, pp. 232-233). The exercise (actual or potential) of power in the research relationship has been a central ethical and methodological concern in the new ethnography, especially when the research involves marginalized, stigmatized or otherwise socially vulnerable persons. In the literature the exercise of power is invariably conceptualized as a one-way street: people without power in the social world are confronted by researchers 'studying down' from their 'superordinate and socially distant positions' (Jaffe & Miller, 1994, p. 55).

The unstated assumption here is that 'vulnerability' is an unvarying characteristic of individuals, regardless of the specific social relational setting in which action is (or is likely to be) affected by differentials of power. Elsewhere (Russell, 1999) the first author has shown that vulnerability in the social world cannot uncritically be transferred to an analysis of the research act. In practice, some of the very old and frail people she interviewed were seen to have exercized considerable power over the course of the research and to have participated very much on their own terms as, in varying ways, did some of the men in this project.

In other words, the search for 'a structurally sensitive method' (Jaffe & Miller, 1994, p. 55) of interviewing vulnerable people needs *fully* to acknowledge its interactional character within a theoretically systematic framework. In Goffman's framework, a structural view directs attention to "the horizontal and vertical status divisions and the kinds of social relations which relate these several groupings to one another" while a cultural view does so "in terms of the moral values which influence activity" (Goffman, 1959, p. 233). Certainly, understanding the process and outcomes of research will include an examination both of the wider social context within which both researcher and researched are located and to culturally (or subculturally) derived sets of meanings and expectations. But the experience of such social structural dimensions as class or gender 'relates to more than the ability, in some general way, to mobilize resources' (Bury, 1995, pp. 28-29). The research act itself needs to be analyzed as a practical accomplishment within a local social setting; it is not enough merely to locate researchers and those they research as members of hierarchically related social categories, or to impute to them the 'meaning' of their participation.

Finally, the dramaturgical perspective leads to a focus on the techniques and problems of impression management in a particular setting, and "the identity and inter-relationships of the several performance teams which operate [therein]" (Goffman, 1959, p. 233). What much of the methodological literature neglects is recognition of the fact that it is not only researchers who engage in such practices. As Goffman reminds us, 'All the world is not, of course, a stage, but the crucial ways in which it isn't are not easy to specify' (Goffman, 1959, p. 78). Researchers (and research methods textbook writers) may be better able to articulate their 'stage craft', but 'everyone apparently can do it' (*ibid.*).

Clearly all of these dimensions, and the interconnections between them, are implicated in any consideration of a concept like 'rapport' and its theoretical and practical significance in ethnographic research. If one begins from the assumption that all lives and experiences are 'artful constructions', one is led to viewing rapport not simply as a technical aim but as a topic of inquiry in its own right. It remains to be seen how - or indeed whether - the information generated from such inquiry might inform the practical instruction of novice researchers in the new ethnography.

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Footnotes

[1] A more detailed account of the project and some of its early findings is found in Russell, Touchard, Kendig, & Quine, (2001).

[2] All names of participants used in this article are pseudonyms.

[3] Interestingly, another man's positioning of himself as a potential informant led to his refusal to be interviewed at all:

> He asked what I was doing at the facility and went on to query me about the types of questions I'd ask. He explained that it would be better if he wasn't interviewed as he has information that would "shut the place down".