

The Compromised Researcher: *Issues in Feminist Research Methodologies**

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Rubrik abstract

This article centres on issues of vulnerability and being compromised in feminist research where the focus has frequently been on researching *the same*. Compromise, here used in its pejorative sense, may for instance occur in terms of one's research topic, the methods one utilizes, or the participants chosen for a study. Drawing on a range of examples including the methodological work of Ann Oakley (1981, 2000) as well as three articles on researching men that appeared in the journal *Signs* in 2005, I argue that feminist researchers, possibly because they work in an identity-based discipline, may be diversely vulnerable when researching the same and/or researching the different, and can be compromised both by how they are treated by those whom they encounter in their research and by their own behaviour in that context. I suggest that these concerns are under-articulated in feminist research and conclude with a series of questions that need to be raised.

Key Words: feminist research, researching the same, difference, interviewing

This is an article about researcher vulnerability. It is about how feminist researchers, among whom I include myself, are affected by and affect the research that we do, about the emotional structures that accompany our work and how we deal with them. Researcher vulnerability is not often talked about (but see Flood-Ryan and Gill 2010), certainly not outside anthropological and specific sociological fields such as criminology. After all, as researchers we are either learning the trade of how to conduct research, or once trained and in possession of our PhD, we are meant to exercise our trade, conduct research, as masters, or should I say mistresses, of what we do. There is therefore not much room for vulnerability, and, in so far as we show it, or it is attributed to us, we make ourselves vulnerable not only within our research but also within our profession.

Don Kulick and Margret Willson (1995) report on this in an interesting way in the preface of their edited volume *Taboo: Sex, Identity and Erotic Subjectivity in Anthropological Fieldwork*. This book centres on erotic relationships between researchers and researched in the field, a practice that potentially renders both researcher and researched vulnerable, not least because it breaches a convention in research etiquette that one should maintain a certain critical distance to that which or whom one research-

* A version of this article was delivered as a keynote address at the international Feminist Methodologies conference at Stockholm University in 2009.

ches. In trying to solicit contributions Kulick and Willson found that ‘The only ones for whom we were repeatedly called on to justify the volume were heterosexual men, many of whom responded with suspicion or even hostility. The only people who actually tried to article us out of doing the book (usually with the ‘think-of-the-damage-this-will-do-to-your-careers’ hex) were all heterosexual males. . . the book nearly went to press without a contribution from a heterosexual male.’ (xiii) Indeed, the only contribution by a heterosexual male in this volume (Killick 1995) is very guarded and refuses in a sense to engage with the question of researcher vulnerability. Nonetheless, it is mainly in discussions of research methods and methodologies, of fieldwork and research ‘close to the heart’ – that researcher vulnerability surfaces, is admitted and discussed. In this article I would like to suggest that feminist research methodologies are accompanied by a variety of vulnerabilities to which the feminist researcher is exposed and exposes herself, and following a discussion of what these vulnerabilities encompass, I shall end with a series of questions that I think are worth raising. I end with questions because whilst I have no answers, or only partial ones, to some of them, I also think that there are debates that remain to be, and need to be, had amongst feminist researchers about researcher vulnerability.

However, I entitled my article ‘The Compromised Researcher’, not ‘The Vulnerable Researcher’, and I did this deliberately because ‘vulnerability’, more strongly than ‘compromised’, suggests a state of being, or an ontology, that one is the object of. However, as my discussion of the various vulnerabilities I want to address will make clear, one is not only not born vulnerable but made vulnerable – to re-write a phrase of Simone de Beauvoir’s (1949) – and the latter, importantly, both by others and by oneself. So, let me begin with an anecdote that suggests one kind of vulnerability, one that is deeply imbricated in the history of how we conceive of feminist research.

In the late 1990s I proposed a book on *Contemporary Black and Asian Women Playwrights in Britain* to Cambridge University Press which they accepted and then published in 2003. The first time I went to see the commissioning editor there, a music specialist with a background in performance but not in Women’s Studies, she said to me, ‘I thought you’d be Black.’ I was somewhat taken aback by this statement, and instantly felt defensive and vulnerable. Did I, as a white woman, have the ‘right’ to write about Black and South Asian women?

My ‘justification’, and I felt that I did have to justify my research topic, was that I had taught and written on Black and South Asian British and North-American women’s writing for years and – especially on the cultural work of *British* Black and Asian women – no book was available at the time I proposed to write mine. But, of course, I was not black and, *pace* Judith Butler, there was no way I could perform blackness in a convincing way. So did this automatically invalidate my proposal to work on this subject? The editor in question did not raise this but I was left to ruminate on the extent to which my integrity as a feminist researcher might be or was compromised by my choice of topic. Very recently, in September 2012, I saw the same problematic enacted at a conference where Line Nyhagen Predelli and Beatrice Hal-saa discussed their *Majority-Minority Relations in Contemporary Women’s Movements*:

Strategic Sisterhood (2012). In my view this volume is in many ways exemplary in its attempt to give equal weight to the perspectives of women from diverse ethnic backgrounds in their reported depictions of relations among them within women's movement political organizing. This is a vexed topic, and there is, of course, a long and long-standing critique of white women by black women around this issue. At the conference, a black woman in the audience offered a sharp critique of Nyhagen Predelli's and Halsaa's summary of their arguments, suggesting that it was not for them to discuss how black and ethnic minority women thought about the issue – even as what they said was based on extensive qualitative data they had collected. The whole situation felt very uncomfortable, reminiscent of the race conflicts that beset feminism in the 1960s, '70 and '80s, and Nyhagen Predelli and Halsaa were vulnerable in this context – and this is not the same as saying that this was justified – to accusations of neo-imperialism and a certain kind of racism.

There have been – as feminist researchers including myself are well aware – extensive and sometimes heated discussions about the propriety or otherwise of women researching women like or unlike themselves, discussions that have dominated feminist debates from the late 1970s onwards. One version (see Hemmings 2005, 2011) of these discussions, the one that succeeded the notion of 'sisterhood being global' in the 1980s, was that since there was and is such a great diversity amongst women, diverse groups of women need to be empowered to 'do it for themselves' in their, and in response to their own, specificities (e.g. hooks 1984). Here the question of who can speak on behalf of whom – and one might of course ask after the extent to which 'speaking on behalf of' is always what is involved in research – is resolved through the notion that there should be congruence between researcher and researched,¹ asserted, in a sense from a quasi-essentialist position of assuming an un?/equivocal identity in relation to which one might produce authentic, if potentially partial, accounts of one's experiences and positions.

The advent of postmodernism, queer, and similar theoretical frameworks moved those debates – at least in theory – away from the notion of an identity-based feminist research politics. I write 'in theory' because in practice, it seems to me, we are still – and possibly for good reasons – very much engaged in an identity-based research politics. The postgraduates in Women's and Gender Studies that I encounter, for instance, routinely continue to conduct research on topics they view as strongly intertwined with their identity or sense of self. This may, of course, be promoted in part by the fact that Women's and Gender Studies as disciplines are identity-category-based – quite unlike other disciplines such as Sociology or History. Typically, for instance, lesbian and queer students, but not students who identify as heterosexual, work on topics related to lesbian or queer identity; a current PhD student of mine who is a stepmother works on step-mothering; female international PhD students from different countries

1 In the context of anthropology, the opposite has in a sense dominated the field: researching those unlike ourselves was at the heart of early anthropology and it was mainly after the disciplinary debates in that field of the 1970s and 1980s that this changed.

predominantly work on aspects of women's experiences in their own countries, often on the experiences of women from their own countries who are less privileged than they themselves are, or on migrant issues etc. These researchers' take on identity may not be essentializing but it is often un-questioning in relation to those whom they research whose identity markers, highlighted in the form of demographic data according to which the research participants are selected, are taken as given and as indexes of certain positionalities that are treated as fixed. In this sense identity continues to rule in feminist research, and it is this, I want to suggest, that puts us, meaning feminist researchers, at least at times, into positions of vulnerability and compromise.

Identity-based research politics impacts on four key aspects of feminist research:

- our choice of research topic;
- our research design;
- the actual conduct of our research/in 'empirical' work (which may be qualitative or quantitative, for example); and
- the analysis of our research data (for instance around the issue of informed consent).

It surfaces most obviously perhaps in feminist texts directly concerned with research methods where the advocacy of an identity-based research politics of a certain kind is variously described and circumscribed by terms such as reflexivity, situated knowledges, historicity etc. (for instance in Hartsock 1999; Harding 1991). Phrases such as 'reflectivity' and 'situated knowledge' have become something of a commonplace in western feminist research where texts routinely feature the 'I' of the researcher and detail the imbrications of the researcher in her work. Personal anecdotes such as the one I referred to above have become a staple and accepted dimension of western feminist researchers' discussions of their work and, I want to suggest, those representations – as indeed did mine - often produce a particular rhetoric, one about the ways in which we compromise, and are compromised by, our work. The title of this article, 'The Compromised Researcher', is therefore both an invitation and a provocation: an invitation to consider a key issue that feminist researchers face, and a provocation in that I use a word – 'compromised' – which can have a pejorative dimension, hence the vulnerability I referred to.

In discussing 'the compromised researcher' I draw on three sources, all fuelled by feminist imperatives:

1. the experiences of both my PhD students and myself as feminist researchers;
2. the demand for reflexivity which has fuelled the articulation of feminist research as such;
3. a series of texts focussing on feminist research methodologies.

The word ‘compromise’ can refer to two things: one is a negotiated position, the other, especially when used as an adjective or adverb, i.e. as ‘compromised’, as an expression of contamination. The *Oxford English Dictionary* defines it as

- a. Agreed by compromise.
- b. Subjected to, arranged or modified by, compromise.
- c. Exposed to risk, danger, or discredit; damaged in reputation.
- d. That has been in contact with infectious disease.

And it is these latter senses of compromise that I am concerned with here since vulnerability is about the possibility of contamination – both of the self and of others. The texts I shall draw on to discuss this further are on the one hand a series of articles on research methodologies by a British feminist sociologist, Ann Oakley, who was a visiting professor at Uppsala at one point. In her methodological work, as she subsequently put it, she ‘began by singing the praises of “qualitative” research, of in-depth interviewing and observation as ultimately more truthful ways of knowing, and . . . ended up advocating the use of “quantitative” and experimental methods as providing what is often a sounder basis for claiming that we know anything’ (2000a:13–14). This shift in method, as I shall discuss below, arose from Oakley’s sense of a certain precariousness in her work and it also simultaneously made her vulnerable in various ways. The other texts I draw on are three articles that appeared under the heading ‘Interviewing men’ in a special issue on methodologies of the journal *Signs* in the summer of 2005 (Vol. 30, No. 4). Here the feminist researchers made themselves vulnerable by whom they chose to research – men – rather than through how they decided to conduct the research. I shall return to both sets of issues.

But before heading towards these texts and their implications let me tell you another anecdote: a PhD student of mine who was working on female friendship networks and lesbian identity in the early 20th century was, as she later told me, asked at a conference where she presented a paper on her research, ‘Are you a lesbian?’ This student, who was in a lesbian relationship at the time, was most indignant about this question and reported it to me with real anger. She felt that she was being asked to account for herself in completely unacceptable ways.

Both her experience and my own around the issue of blackness reveal similar underlying assumptions about the relation between ontology and epistemology, assuming a synonymy between what you are and what you do research on. Here we are straight into an identity politics of research, built on an assumption – notwithstanding all the complex theoretical debates that were had in feminist research and theory in the 1990s about the problematic of essentializing identities (e.g. Fuss 1989, Cornell 1993, Lennon and Whitford 1994) – that you are what you do, and for it to be otherwise might mean, or does mean, that your research is compromised. This position partly comes out of a by now time-honoured feminist tradition of making connections between the personal and the political or professional. At the same time, the expectation that you research what or who you are challenges the notion of a diffe-

rence, that may be hierarchically encoded, between the researcher and the researched, – a difference that has of course underpinned much traditional science research. It also relates to the feminist claim for the democratization of the research process, and a history of reframing research as sources of empowerment for those researched. As such it reacts against certain patronizing histories of research as advocacy and suggests that oppressed groups need to research themselves in order to empower themselves. Aligned both to identity and to difference politics, this is more than the feminist claim that women should conduct research about women for women. It is a claim that the specificities of the researcher's identity should, in very particular ways, determine their research objects.

That claim in a sense informed the assumption about my blackness and the question about my student's lesbianism. Both assumptions proceeded from the notion of identicalness or at least convergence between researcher and researched. In other words, they suggest that feminist research is about researching *the same*. And, indeed, that was the mantra of certain types of feminist research of the 1970s and 1980s, and in modified form this continues to the present. The recognition of the absence of knowledge about and by women, and the recognition of the imbrication of the personal in the political led feminist women to researching issues close to their hearts, issues in which they had emotional as much as intellectual and socio-economic investments, and, I would argue, whilst the latter investments have been quite extensively discussed, the issue of the emotional investment, related not least to researching the same, has been under-discussed in feminist research literature. The reasons for this are complex. In her article on 'Paradigm Wars: Some Thoughts on a Personal and Public Trajectory' Ann Oakley (2000b), for example, discusses how social context impacted on her work. Moving from sociology to health care she found herself the only sociologist in a research environment dominated by medics whose investment was in quantitative research. In response to this context, she began to re-orient her research towards a more quantitative approach. This quantitative approach, Oakley argues, was fuelled by 'evidence that health care practices are more often based on guesswork, personal preference, tradition, professional modelling and fear of litigation than about convincing and reliable data' (1999:249). In other words, quantification was intended to counter the unreliabilities and emotional bases of the data then in use – data that in fact themselves were quantitative in form. In *The Cultural Politics of Emotion* Sara Ahmed (2004) argues that 'Feminists who speak out against established "truths" are often constructed as emotional, as failing the very standards of reason and impartiality that are assumed to form the basis of "good judgment"' (170). Ahmed highlights the affective investment feminist researchers have in their work – researching what is 'close to their heart' – but that very closeness to the heart is also considered its Achilles' heel, the vulnerability of the subject which infects the object, the research. This is of course partly tied up with the residues – or should I call them mainstays? – of certain paradigms around scientific work related to notions of objectivity, disinterested knowledge production, generalizability of findings etc. that we too – by 'we' I mean feminist researchers here – have found difficult to divest ourselves of, even as we criti-

que them, because our scientific credibility is at stake, *and* in a research environment itself infected by uncertainties that render it vulnerable: uncertainties of employment, uncertainties of finance, uncertainties of support etc. (see Pels 2000).

Let me tell you a story in this context which will return us to Oakley by way of the issue of researching the same: in the early 1990s I had a postgraduate student in her late thirties and married, who decided to work on anorexia, and in particular on the relationship between long-term anorexics who in her study were all women, and their partners (Treseder 1993). This student was visibly under-weight and clearly anorexic herself although she would refuse to accept that. Her choice to work on anorexia was thus obviously 'close to her heart', emotionally, physically, socially. One interesting issue about anorexia is that popular perception suggests that either you die young of anorexia, as did the singer Karen Carpenter for example, or you get over it. However, this is not at all the case – there are many long-term anorexics who spend their lives controlling their food intake (see Steinhausen et al 1991; Morley 1996; Collier 2008). It was these women who were the participants of Treseder's research.

One issue that always arises for a doctoral supervisor, especially in the context of feminist research, is the question of whether or not you agree that students do research on topics close to their heart. There are some who are very much against this, arguing that research is not and should not be therapy, that working on topics that closely concern you generates too much bias in your work and that, ultimately, it can, indeed is very likely to, affect the validity and reliability of your research too much – in other words, your work becomes compromised by what you choose to do. I actually agree that this may happen. I had a postgraduate student, for instance, the daughter of Irish migrants to Britain from the 1950s, who decided to work on the experiences of Irish women migrants to Britain during the 1950s, partly because she had ready access to her sample group in the form of her mother and her mother's friends, all of whom belonged to that generation of Irish migrants. Here the issue was not so much the topic choice as the interviewees this woman decided to focus on. For it became clear that one of the areas she had intended to investigate, namely issues of sexuality and partnering among that generation of women, she was unable to collect data on because she did not feel able to ask her mother or her mother's friends about their sexual experiences and proclivities. This was partly because she had certain kinds of close relationships with these women, and partly because of what could be spoken about within these relationships, was determined by her interviewees' Catholicism. This woman's research was thus compromised, though not wholly invalidated, by the fact that she was researching a certain group of women within her own community with whom she found it difficult to discuss certain concerns. Sameness here functioned as a barrier to knowledge rather than as a facilitator for the research process, and indeed what is known as the insider/outsider debate in feminist research has much to say about the vicissitudes of those positions and the ways in which they compromise researchers (e.g. Griffith 1998, Jaschok and Jinjun 2000).

To return to the story about the woman researching long-term anorexic women's relationships with their – in her study all male – partners. What she found was that

the men who chose to have anorexic partners were not only fully aware of their partners' anorexia but also actively supported it by making comments about their weight and appearance. In other words, these men had an investment in maintaining the women in their anorexic state, even if and as that was detrimental to these women's health. The reason for this was that the men turned out to be both controlling and jealous. Their partner's anorexia fed into that disposition because anorexics tend to have very tightly structured lives since the focus is on food intake and calorie expenditure – you eat a quarter of an apple at six in the morning and then you have to go for a two-hour run to get rid of the few calories you have eaten. (This may seem like a caricature but it is not.) Anorexics are highly routinized in this (see Halmi et al. 2003). From the male partner's point of view this means that the anorexic woman is very much predictable in her everyday movements. You can always predict where she will be at a given point in time and you need never worry that she is having an affair or is going to leave you – she is too busy keeping her weight down.

For the student who did this work – middle-aged, married – these findings were very difficult to deal with and they certainly rocked her marriage. Should I have dissuaded her from conducting this research because it was 'too close to her heart'? I think not. In any event, I decided not to, and in fact, I have never asked students not to do specific pieces of research because of the possibility of compromise. But I think one has to be very vigilant when researching the same because such research is vulnerable to the issue of compromise, not least because of the impacts it can have on the researcher herself, and this compromise needs to be addressed if we are to learn from it. Typically, when researchers work on issues close to their heart, especially in the early stages of their careers, they have high expectations that their research findings will support their views of the issue in question. Another doctoral student of mine, for example, found the fact that her adult children were leaving home very difficult to deal with and decided to research this (Green 2010). One of the first women she talked to about this, however, an eminent Norwegian feminist sociologist, told her how happy she was when her children left home and that whilst she was glad to see them for short visits, she certainly did not want them to come and live with her again for any length of time. This came as rather a shock to the student – but it was also a salutary lesson that others who are in a similar position might not think in the same way. Sameness did not guarantee equivalence of response; the same, as it turned out, may turn out to be quite different.

Where the previous examples centred on the ways in which choice of research topic may compromise researchers, Ann Oakley's work was strongly concerned with the question of methods, and it also revealed the compromises feminist researchers may have, or feel compelled, to make in their work. This is the case in relation to a famous article by Oakley, published in 1981 and entitled 'Interviewing Women: A Contradiction in Terms?' In this piece, Oakley distinguishes between the traditional scientific paradigm of conducting research and the feminist paradigm, characterized, she argues, by different attitudes towards the three 'p's: positivism, power and personal values. The title of the article, 'interviewing women', is ambiguous in that it simul-

taneously refers to the process of interviewing women and to women as interviewers. Oakley's view, at the time, was that women could not interview women within the traditional scientific paradigm because their mode of interaction – nurturant, communicative, other-centred – contradicted the behaviour prescribed by the traditional social science paradigm of non-engagement with the interviewee. That view of women as tending to be more nurturant etc. than men has, of course, been critiqued since. Oakley's arguments were based on experiences of research on women's transition to motherhood, involving four interviews each with 55 women during their pregnancy and shortly after their children were born. Oakley argues from the position of doing research *as* a woman who has become a mother, *on* women becoming mothers, in other words, *researching the same*. In this context she also rightly makes the point that the literature on interviewing largely ignores repeat-interviewing and the intimacy built up through that process, in other words the gradual personalization of the interview dynamic. She describes the importance she had for her interviewees as a source of information and support, not least because the medical community was often very uncommunicative towards the women in question, and she points out that some of the women she interviewed became life-long friends. Such forging of affective bonds between researcher and researched, born out of sameness, is still not very much discussed. It also generates issues. One question, for example, one might ask here is how many friends can a woman have? If you collect friends in every research project you undertake, where are the limits of this? How many 'friends' can one seriously give attention to (see also Kirsch 2005 for an interesting take on this)? Indeed, how much can a researcher give back in terms of the needs of those whom she researches? How do we draw boundaries here? And should we?

Oakley was heralded as a champion of qualitative research in the wake of this research which also meant that when she subsequently, as discussed above, became drawn into conducting more quantitatively framed research, and in particular randomized control trials or RCTs, she found herself confronted with the puzzled response of her fellow feminists (see Westmarland 2001). In 'Paradigm Wars' she writes:

In a seminar I gave in a Swedish department of sociology in 1997, the commentator on my article desperately produced evidence from my previous writings of 'the old Oakley' – a persona she much preferred – which she then contrasted with 'the new Oakley', asking me to account for the difference. I have been accused of some sort of strange conversion experience, of being brainwashed by medics, of letting the 'qualitative' and feminist sides down; at the very least, it has been important for people to stress that, whilst my methodological repertoire has seemingly expanded, a primary allegiance to the 'qualitative' tradition must remain. (*The Ann Oakley Reader* 2005:246)

Oakley's history is an interesting one of compromise and being compromised. Her early qualitative work set her apart from traditional social sciences paradigms, making her vulnerable within these, but she became a heroine for feminist researchers. In later years, however, her research changed, and she described how the context in

which she worked – moving from a sociology department to a health care research unit specialising in maternity services research – impacted on that work. This issue – why we research what we do – is rarely discussed in terms of these contextual dimensions. Oakley in fact continued to *research the same* – it became her life's work – but she also found that 'Uncontrolled [as opposed to controlled] experimentation is what usually happens in professional practice.' and that 'Teachers, social workers, criminologists, volunteer "do-gooders", politicians and other promulgators of public policy are all guilty of choosing to do what they believe in, rather than what has been demonstrated to be the best thing to do.' (247) The upshot, in a sense, of her work and her findings was, rather like in the film *Kitchen Stories* (2003), that researchers, and maybe especially feminist researchers, are always compromised and can only work by making compromises – in their work and in their career. As such they are vulnerable.

That vulnerability takes a different turn when we move away from researching the same – which is what much feminist work has been and is about – to researching the different, and here I come – briefly, to the articles I referred to from *Signs*, all published under the subheading 'Interviewing men' in the summer of 2005:

- Lois Presser's 'Negotiating Power and Narrative in Research: Implications for Feminist Methodology',
- Sabine Genz's 'Intersections of Sex and Power in Research on Prostitution: A Female Researcher Interviewing Male Heterosexual Clients', and
- Verta Taylor and Leila J Rupp's 'When the Girls are Men: Negotiating Gender and Sexual Dynamics in a Study of Drag Queens'.

What is interesting about the three pieces, which are all in various ways wedded to heightened self-reflexivity and an interrogation of what went on in the research process, is that they all feature women interviewing men, and not just men in general, but men who operate in particular ways in genderized, indeed sexualized contexts. Presser did research on men in prison and interviewed men who had committed violent sexual acts against women; Grenz worked with heterosexual men visiting prostitutes; and Taylor and Rupp dealt with drag queens. This was about researching difference, and as the titles of their articles suggest, that difference became central to the research conducted.

Disturbingly for me, but also it seems to me to some extent predictably in terms of the topics the researchers were dealing with, all three sets of researchers found themselves subjected to humiliating encounters with the supposed objects of their research in which they were both disempowered and indeed, invited to go along with what were clearly compromising and demeaning situations. Grenz, for example, reports being asked questions by her interviewees such as whether they could take out their penis whilst she interviewed them, or whether they could lick her feet etc. Grenz explains that 'participants would not only talk about their needs but would actually project them onto me. This in turn made me, as a woman and a potential object of their desire, potentially vulnerable.' (2093) The first person she tries to set an interview up

with, asks her 'whether I would be able to tolerate it if he masturbated as when and if [sexual] arousal occurred.' (2093) She states that she 'felt simultaneously powerless and curious, disgusted and adventurous' (2093-4) in response to this. But she agrees to it 'to buy some time and think more about it.' She also states:

However, my 'consent' made me feel extremely weak and powerless. I was trapped in a conflict between sexual permissiveness and resistance against this outrageous request, which would situate me unwillingly in the position of a sexual partner (2094).

Quite. Grenz is compromised by the whole episode. She was in the audience when I gave this talk at the 2009 *Feminist Methodologies* conference and was not happy that I described her as compromised. Her line of argument was that this had happened when she was on the phone. However, being propositioned in this way and – even if only initially – agreeing to go along with it still seems to me like being put into a compromised position. In a later version of her article (2000) she suggests that 'the interview process can be interpreted as an irritation to socially dominant perceptions of masculinity. The "looker" is commonly constructed as being male. Being looked at and/or being investigated may, thus, make a man feel uncomfortable because in the position of the looked-at, one becomes an object...' (58). Here we have the scenario where those who have been the objects of men's violent and sexually transgressive behaviours (= women), become the interlocutors and hence subjects and the male interviewees, reacting to and against this, seek control and dominance through making compromising suggestions to them.

This was also the case with Presser. She was invited to support a prisoner breaking the prison rules and indeed, she allowed this to happen, offering a from my perspective rather lame-duck explanation for why she went along with it. In discussing the details of the research process, Presser suggests that 'chivalry was a popular way of "doing" gender' among all the men she interviewed because 'Such chivalry positions the female other in terms of hegemonic femininity, encompassing vulnerability and heterosexuality' (2079). Quite. She then states that 'the darker reality of chivalry is its assertion of authority' and that for example one of her interviewees, Dwight, 'struggled for control during the interview' and 'instructed [her] on helping him to violate the rule against smoking inside the house' (2079). Presser analyses what follows unambiguously as 'nothing short of a struggle for control' where 'Dwight enlisted me as his accomplice in breaking a house rule.' She interprets her own behaviour as '[giving] in by agreeing not to contest the rule violation' ('I'm gonna let you do what you want to do') while reasserting [her]self as an authority figure: 'I am the one to *let* him do as he wishes' (2080). To me that assertion of authority rings hollow, in particular the notion that 'going along' with someone else's desire is a form of reassertion of self. Presser herself does not discuss how she *felt* about the process. But she is clearly put in a compromised position. In relation to another male prisoner interviewee who threatens to stalk her – it is not clear whether this is meant seriously or not – she writes: 'I did not call Kevin on his sexual remarks' (2085). She justifies this partly by stating:

I may have been concerned that he would terminate our contacts if I challenged him – a risk Andrew Herod (1993: 314) acknowledges with regard to researchers' contesting subjects' sexist attitudes (2085).

The worst in terms of compromise, as far as I am concerned, was the treatment that Taylor and Rupp experienced at the hands of the drag queens they researched. An out lesbian couple, they attended their performances as part of their ethnographic data collection. This is what they wrote:

The drag queens regularly incorporated us into their shows along with other audience members. In a way we became the same kind of prop that any unsuspecting tourist might. They were particularly fond of pulling down Verta's top and bra to expose her breasts onstage. . . Sushi [a drag queen] would also grab our breasts as she went around the cabaret with the tip bucket. Or she might grope our pubic areas. We let them do these things that we as feminists would never allow other men to do, even as we realized that these were, in part, expressions of male dominance. Without quite knowing it, we accepted these actions as part of a levelling process, even though they also made us angry.

In addition, when they introduced us in the shows, they were likely to call us 'pussy lickers' or 'the professors of lesbian love'. They would call us onstage and have us say to the audience, 'I love to lick pussy.' This was in keeping with the politics of vulgarity they practice (2123).

Taylor and Rupp not only went along with this offensive and demeaning treatment; they account for it in terms of a strategy of shifting power which the drag queens employed to level the class difference between themselves as poorly paid performers and these professors. Taylor and Rupp ignore their recognition that they are dealing with dominance and domination, indeed male dominance and domination, but also with a certain kind of violation of themselves, because – as they argue – 'as queer people we are all in the same boat'. I think it is worth noting here that when I have given talks on this topic, especially in front of queer audiences, they read the drag queens' behaviour in terms of irony. This, of course, exonerates the latter's behaviour; it also seems to me a rather easy 'solution' to what is an acutely uncomfortable situation, namely antagonisms or oppressive behaviours and attitudes between groups that consider themselves to be outside the mainstream. This is not a topic that is ever spoken of lightly or easily.

All three sets of researchers, Prosser, Grenz and Taylor and Rupp, offer some explanation for why they went along with how they were treated. Interestingly, none of them discuss how and why they decided to undertake their specific research in the first place, or what their expectations were regarding what would happen. All three, however, find themselves vulnerable and are compromised in their integrity by what happens, and indeed compromise in the interests of their research.

The article by Taylor and Rupp, as much as the other two, for me raise really important questions, to do with vulnerability and with the notion of compromise and being compromised, and it is with these that I want to end and invite discussion. The questions, and they are not exhaustive, are:

1. How far and in what ways should a researcher allow herself to be compromised in the interests of her research?
2. Is all research always compromised, and if it is, how should one deal with this?
3. Do different kinds of research (methods) produce different kinds of compromise?
4. Are issues of compromise a different matter for a feminist researcher compared to other researchers?
5. How do questions of compromise relate to researching the same, and researching the different?

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