



The Art and Politics of Covert Research: Doing 'Situated Ethics' in the Field

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ABSTRACT

This article discusses the covert research relationship. Specifically, it explores the ethical dimensions of fieldwork with reference to a six-month covert ethnography of 'bouncers', in Manchester. Drawing from sociological literatures, the article wishes to raise for scrutiny the management of situated ethics in covert fieldwork which, despite having some increased recognition via debates about risk and danger in fieldwork, remains glossed over. The standard discourse on ethics is abstracted from the actual doing, which is a mediated and contingent set of practices. Traditionally, professional ethics has been centralized around the doctrine of informed consent with covert methodology being frowned upon and effectively marginalized as a type of 'last resort methodology'. What I highlight here is the case for covert research in the face of much conventional opposition. I hope the article will open debate and dialogue about its potential role and possible creative future in the social science community.

KEY WORDS

covert / ethics / ethnography / informed consent / risk

Introduction

This article discusses a covert participant observational study of door supervisors, or 'bouncers' as they are more traditionally called, in the night-time economy (NTE) of Manchester, England. The primary fieldwork was conducted over six months between January and June 1996 with types of secondary data being collected about media reactions and post-fieldwork management of the bouncer role until the present day.

This article is organized into six broad sections. The first section is concerned with outlining the traditional case against covert research, including the governance and regulation of research ethics. The second section critiques informed consent, followed by an examination of the submerged tradition of covert research. The next two sections, based on my case study, explore the 'covert self' and 'situated ethics' in the field. The concluding section proposes a creative and imaginative role for covert research.

The Opposition to Covert Research

The guidance that professional associations and bodies¹ offer their members amounts to a moral and methodological 'frowning upon' covert research. This 'standard position' or 'professional consensus' is strongly tied up with the principle of informed consent and, related to this, the public image of the discipline and the legitimacy of its knowledge.

Homan (1980, 1991), who is regularly quoted in the literature, lists thirteen 'objections' to covert research, including flouting the principle of informed consent; the erosion of personal liberty; betraying trust; pollution of the research environment; producing a negative reputation of social research; discrimination against the defenceless and powerless; damage to the behaviour or interests of subjects; covert methods may become habitual in the everyday life of the person doing the research; the habit of deception may spread to other spheres of human interaction; covert methods are invisibly reactive; covert methods are seldom necessary; covert methods have the effect of confining the scope of research; and that the covert researcher suffers excessive strain in maintaining cover.

Some of the objections raised by Homan (1980, 1991) and the discussion by Bulmer (1982) on the merits of covert research are very useful descriptions of the conditions of doing covert research and are certainly important considerations to be aware of. However, for me, they do not amount to a sustained rejection of covert research.

Within what I call the 'standard view' on covert research, when there is recognition that informed consent is not always achievable in its absolute form, there is often, a call for retrospective explicit debriefing of subjects, so that at least they know that research has been taking place even if they did not know it beforehand. This is an idealized view of the research setting and, again, contributes to covert work being fettered and stifled. The most famous case, which is regularly raised as a clear example of 'bad ethics' in the social sciences, is Laud Humphreys' *Tearoom Trade* (1970). This has received much academic criticism, mainly on the grounds of deception. Alternatively, Mitchell (1993) feels that 'secrecy' is an integral and unacknowledged part of robust fieldwork. More recently, the place of covert research in criminological research has been debated. Interestingly, Wells (2004) argues that covert research should be discussed in terms of research necessity and quality of data, rather than the emotive debates about morality and ethics which have traditionally framed it.

The opposition to covert research is articulated further in the discourse on the governance and regulation of research ethics. In a statement on ethical practice, the British Sociological Association (BSA) state:

...covert methods violate the principles of informed consent and may invade the privacy of those being studied. Participant or non-participant observation in non-public spaces or experimental manipulation of research participants without their knowledge should be resorted to only where it is impossible to use other methods to obtain essential data. (2002: 6)

The ESRC has demonstrated an increased concern about the regulation of ethical conduct in their development of the Research Ethics Framework (REF). It states:

Covert research may be undertaken when it may provide unique forms of evidence or where overt observation might alter the phenomenon being studied. The broad principle should be that covert research must not be undertaken lightly or routinely. It is only justified if important issues are being addressed and if matters of social significance which cannot be uncovered in other ways are likely to be discovered. (ESRC, 2005: 21)

Although the ESRC is supporting some grounds for covert research by asking for additional justification, it is my contention that covert research is effectively stigmatized in the research world.

In 2005 Max Travers, in a short statement within the newsletter of the British Sociological Association, makes some very useful observations about these developments:

The rise of ethical review should be understood as the latest in a series of legislative and institutional measures in which the state has restricted academic freedom and exerted greater control over social scientific research... It will also result in a massive amount of new administrative work, and a whole industry around these institutions. Justified on the entirely spurious grounds that sociological research harms society unless this is properly regulated. (Travers, 2005: 20)

The Critique of the Doctrine of Informed Consent: The Blurred Reality of Fieldwork

One of the issues here is what I refer to as the 'consent to what' problem, in that social research is often contingent and all probabilities cannot be covered by the consent form. Indeed, some research involves a semi-covert element when there is no obvious source of consent, or when some but not all the participants are informed (Burgess, 1985). Concerns have also been raised about the ethicality of ethics committees (Hammersley, 2006) and the ritualism around completing consent forms (Sin, 2005). Richardson and McMullan (2007) argue that overly rigid ethics committees can be counter-productive, although their aim is to suggest procedures to ensure the highest ethical standards for sociology by exploring lessons learnt from the NHS research ethics process. Part of the problem is

the continued adoption of inappropriate medical models of doing social research, yet we still continue to turn to that mentality in terms of research governance (Israel, 2004).

The general problem I have is that there is a sort of codification about the qualitative research journey, which is not always open to risk assessment, review and evaluation. There has been a rise in the bureaucratization around ethics, field research and risk taking and attempts at regulation, most commonly through University Research Ethics Committees, which, if wrongly managed and centre driven from above, can deter, fetter and discourage creative covert research. What these ethics codes articulate is ultimately a species of both protectionism and privileging.

Research is a situated business and not open to rationalistic planning. It is in the particular cases of the here and now with participants that ethics are situationally accomplished. Let me stress that I do not disagree with the concept of meaningful consent (Williams, 2006) in that covert research would definitely not be appropriate for certain settings or groups. It is neither an 'anything goes' nor a 'one size fits all' policy but what is appropriate in that setting. Lugosi (2006), in his ethnographic study of commercial hospitality, argues that concealment and disclosure are negotiated throughout the fieldwork period. He argues that it is the relationships with specific informants that determine overt-ness or covertness in the research. For him, concealment is sometimes necessary and often unavoidable.

In the research setting, even when doing overt research, you can find yourself in a blurred situation with regard to confidentiality and consent. Howard Parker's (1974) ambivalent position of receiving stolen goods from the boys and his selective publication policy mirror my own feelings, with what I eventually published from the fieldwork data. Punch (1986) comments that, in large organizations, it is difficult to get the full and informed consent of everybody involved. Indeed, Punch (1986) stresses that trying to gain absolute informed consent can ironically end up terminating some research projects. What the researcher is trying to capture by investigative methods is often the unofficial view of an organization – what Shulman (1994) describes as 'dirty data'.

What consent forms cover the remit to observe and document all social behaviour and conversations? Anderson and Bissell (2004) have drawn attention to the fact that overt and covert research is a moral continuum, where the boundaries can become blurred in the doing. Bourgois offers some nuanced reflections on doing fieldwork:

...we are taught in our courses preparatory to fieldwork that the gifted researcher must break the boundaries between outsider and insider. We are supposed to 'build rapport' and develop such a level of trust and acceptance in our host societies that we do not distort social interaction. Anything less leads to the collection of skewed or superficial data. How can we reconcile effective participant/observation with truly informed consent? Is rapport building a covert way of saying 'encourage people to forget that you are constantly observing them and registering everything they are saying and doing?' (Bourgois, 2007: 296–7)

Engagement with the ethics of research is not a ritualistic tick box process that once done at the beginning of the project can then be obviated, but runs throughout the lifetime of a project. Although such ethical reflexivity is more pronounced in covert research it applies clearly to overt research contexts as well. A proportion of covert practices are routinely glossed over in sanitized overt accounts. Moreover, it is difficult to plan, or indeed cleanse, research so that ethical dilemmas don't arise. In certain research contexts it is difficult to maintain a strict either/or division between overt and covert hence the messy reality is more akin to a continuum. As Punch states:

...The semi-conscious tactics of the field – eavesdropping, fudging over one's purpose, simulating friendship, surreptitiously reading documents, etc. – make for good data but bad consciences. (Punch, 1986: 73)

The Submerged Tradition of Covert Research

This is clearly not an exhaustive list but a range of work exists from different fields and disciplines that have used covert methods, either deliberately or more emergently, to study a given area. These include Festinger et al.'s (1956) study of an apocalyptic religious cult, Lofland and Lejeune's (1960) study of Alcoholics Anonymous, Rosenhahn's (1973) work on mental health hospitals, Parker's (1974) work on juvenile gangs, Wallis's (1976) study of scientology, Ditton's (1977) work on fiddling and pilfering by bread salesmen, Homan's (1978) work on pentecostals, Fielding's (1982) work on extreme right-wing organizations, Holdaway's (1983) study of the police, Burgess's (1985) work on school culture, Thompson's (1988) work on the Ku Klux Klan, Taylor's (1991) work on mental health wards, Fountain's (1993) study of cannabis dealers, Goode's (1996) use of bogus personal ads to investigate courtship, Hobbs et al. (2003), Monaghan (2002) and Sanders' (2005) work on bouncers, Scheper-Hughes's (2004) study of organ-traffickers, Lugosi's (2006) work on commercial hospitality and Shulman's (2007) work on lying in the workplace.

Partly, what is required is to both recognize and treat this covert tradition in a dedicated manner and recover it from the index status that it has in much of the methodology literature. Despite the growing literature on danger and risk in fieldwork (Bloor et al., 2007; Lee, 1993, 1995, 2000; Lee-Treweek and Linkogle, 2000) and a general move to more reflexive auto ethnography, the covert research experience is often subsumed in ethics chapters and mentioned more momentarily in methodological appendices and disregarded footnotes.

If we look at the methodology of some of the classics, covert research has radically shaped sociology and criminology, if in unintended ways. Goffman undertook a covert ethnographic study of social life in mental hospitals – *Asylums* (1961) – in order to investigate how the inmates were being treated. He justified this approach by claiming that the findings of his research, whilst he was employed as a physical therapist's assistant, would contribute to the

well-being of the inmates. Ultimately, Goffman's study increased our knowledge about the process of institutionalization. Garfinkel's public breaching experiments, which informed his development of ethnomethodology as regards the normative order of ordinary life, were done without seeking informed consent and had a radical influence on generations of sociologists. Melville Dalton was employed as an administrative assistant, wherein he collected data on the informal culture of organizational life in *Men Who Manage* (1959), and thereby influenced many in occupational sociology. Many of the Chicago School studies were done under a broad banner of participant observation, but included elements of covert research. The strong critics of covert research seem to have amnesia over such studies. In more contemporary times, the development of club studies and recreational drug cultures has been driven by accounts seeking insider knowledge, which often employ covert dimensions (Measham and Moore, 2006).

The 'Covert Self': Going Bouncer and Nomadic Ethnography

My ethnography was grounded in the 'lived experiences' of the participants and the commitment was to doing an ethnography, which attempts to provide thick (Geertz, 1973) and faithful (Bittner, 1973) descriptions of the natural and routine setting of door work by engagement with those studied. The context of the study was the night-time economy and in particular the world of bouncers and bouncing, to use the traditional term. There is an emergent literature on bouncers (Calvey, 2000; Hobbs et al., 2003; Monaghan, 2002; Sanders, 2005; Winlow, 2001; Winlow and Hall, 2006). In the UK, the bouncer trade is being professionalized by various agencies which are distancing it from the shadow economy and links to criminality. This project was a very small-scale one, which was partly self-funded and had some departmental assistance. The autonomy of the project didn't produce any problems of bounded censorship or overly policy-driven enquiry, although it did result in some press attention, which compounded the management of the post-fieldwork biography.

The benefits of this covert study were to contribute in some way to a more nuanced understanding of the still under-researched night-time economy and, in particular, the associated theories about the commodification of violence. For many of the commentators in this embryonic field, the standard view is that the world of bouncers is one that is saturated in violence and often equated with forms of masculinity and cultural capital. For me, violence certainly is a typical feature of the door environment and forms part of their 'dirty work'. However, I also feel, from my covert study, that the role and use of violence is overplayed in certain accounts of the work of bouncers. Violence is often an exaggerated part of 'war stories' told amongst bouncers about their work world as part of their occupational worth. Thus, what is appealed to is an exotic account, with the more mundane and routine features of door work being rather submerged. For me, like the other authors in this field, bouncers play the role of a much

maligned private police force. The continual drive is to resist the temptation to paint a picture that fits with one's theoretical framework and becomes a vehicle to tell certain academic stories about society. The goal is about getting a proportionate and authentic picture of that cultural world, which I feel the covert aspect of the study has helped me to do, although my version is one among several.

My interests in studying this area grew out of being a regular consumer of pubs and clubs for many years, where bouncers were classic figures of both fear and fascination. I had trained in martial arts for several years prior to undertaking the study, which gave me the hardness passport (Patrick, 1973) for entry. This was a type of embodied ethnography, displaying the relevant body and cultural capital (Monaghan, 2002; Wacquant, 1995; Winlow, 2001) and interaction rituals (Goffman, 1967) to pass (Garfinkel, 1967) as a doorman. It was the art and politics of 'faking friendship' (Duncombe and Jessop, 2002) as I constructed a door career. After I had completed a local city council door staff training course I had made contact with a door agency that supplied door staff to pubs and clubs in Manchester city centre.

It was the very strong sense of a door community that produced difficult dynamics for me in the post-fieldwork management of self, as many of the door people I had briefly encountered identified me as a doorman for some time afterwards and regularly offered privileges in the shape of free entry to clubs and pubs in the city. After all, to them, I had merely finished doing their door but not necessarily other doors. Consequently, I would regularly be asked which door I was working on. For me, the research was not only about getting on and staying on the door but also staying out. My 'bouncer's story' of who I was still had to be sustained well after the study had finished and I was often, unexpectedly, called upon to 'get back into character', if you will, as I bumped into former bouncers. The usual reply would be that I was in 'early retirement' or 'I couldn't stand the pace any more'. As I was no longer just a punter, but an ex-bouncer with a certain status, I had to distance myself strategically from the field. My partner commented to me, during the fieldwork, 'when you put that jacket on, your bouncer head comes on'. This summed up the duality of the role that I was performing.

This type of embedded ethnography was emotionally demanding. In this sense, it was a form of what Blackman (2007) refers to as 'hidden ethnography', wherein emotional relations develop between the researcher and the researched, but are left unexplained and often glossed over. It was a very open-ended research project with no finishing date. The experience and management of the post-fieldwork self is not new but can be a typical feature of covert work, which is marginalized in most methodological accounts, mainly because it is not a conventional sort of problem.

The particular nomadic role that I adopted arose partly from my comparative desire to understand different door modes but also as a sort of ethnographic risk management. As I developed rapport and closer bonds in doing door work, the covert role became more of an emotional risk in that I felt a sort of 'guilt clause' in the sustained deception that I was involved in. In short, the paradox

was getting close to them without them getting close to me. It was definitely a type of edgework (Lyng, 2005) or what I described as 'sub-aqua ethnography'. In this role, I constantly engineered appropriate exits such as pay or personality conflicts with management, as I moved around various doors. In the event that my cover was blown I had rehearsed certain stories but I was not sure if they would be accepted. Merely stating that I was an academic would offer little or no protection. When asked where I lived I would never give precise locations but had standard responses like 'not far away', 'local' or, as a last resort, the 'city centre'. Also, if I gave a phone number, it would be a mobile one and never my home one. Thus, communication was kept on a first name basis or nickname with payment being done on a cash basis.

I covered 10 door sites, including pubs, clubs and café bars, which exhibit both their door order and status hierarchy. In this sense, I constructed a 'door career' as part of my fieldwork credibility strategy as I was often asked where I had worked and with whom. So when asked if I knew the local 'heads' or people who were 'connected' to criminal fraternities, or at least had aspirations to be, I lied and emphatically said 'Yes, of course' on any occasion that I was asked. This line of work is a type of emotional labour which, although often temporary, develops shifting relationships with door people. Door work typically features a lot of bonding and comradeship as articulated in the practitioner's literature (Thompson, 1994, 1996; Twemlow, 1980).

A journalist who interviewed me after the study stated that when I first sat down he thought I was a bouncer who had become an academic and not the reverse. I take his reaction as a sign of successful 'disguised observation' (Denzin, 1968).

Situated Ethics in the Field

Ethical codes, and various ethnographic accounts, offer a sanitized picture of social research. They offer little or at best limited nuanced understanding of the emotional, biographical and shifting character of fieldwork where ethical decisions are occasioned practices. I am not seeking to abolish professional codes nor add extensive regulations to them. Clearly, they are case contingent.

The move I wish to make is to view ethics as contingent, dynamic, temporal, occasioned and situated affairs. Thus, particularly in my covert case, one is involved in a web of shifting and mixed connections, tactics, identities and motives (Lovatt and Purkis, 1996), which is deeply biographical (Roberts, 2000) and brings a new dimension to 'taking a side' (Becker, 1967). I wanted to resist what I considered to be 'analytic exotica' throughout my project. Hence, I abstained from value judgements about the participants or, put another way, was indifferent to them. The management of situated ethics is not only about adopting a theoretically reflexive attitude but also about a whole series of practical manoeuvres and tactics. Jean Rafferty, an investigative journalist doing a covert study of a homeless hostel in Glasgow, stresses:

The main problem with the use of subterfuge, it seems to me, is not its morality but the practicality of carrying it out. Sustaining a role – that is remembering the details of a fabricated life – is not easy. (2004, 128)

In many ways, it is more of a 'blurred self' that is managed in the covert field setting. Donal MacIntyre, a popular television journalist, has some useful comments to make on his covert role:

The essence of the technique is getting people to tell their stories in their own words, as they would to one of their own. But this can only happen if they believe I am who I say I am ...There is no blueprint, no Scotland Yard course or City University module to prepare you for this kind of work ...The job goes beyond normal health and safety regulations and is outside every EU working directive. (1999: 8)

During the course of my research I experienced, both in the sense of witnessing and participating in, various 'ethical dilemmas' around drug taking, violence towards and from bouncers, withholding information from the police and taking cuts from door money, but these were all occasioned features of the setting, which I 'geared into' as a member of the setting, not an academic zookeeper or moral guardian. When a senior doorman of a famous Manchester nightclub, which has since closed down, told me 'I was in the firm' I simultaneously felt pleased with the credibility of my deception but also troubled about the future consequences of this status. Some would view my role as problematic in terms of collusion, which is only an issue if one retains a traditional conception of fieldworker objectivity. Obviously, these encounters could have put me in an ambiguous legal position, as I had acquired a type of deviant knowledge (Walters, 2003) but fortunately that never happened. In a way, it was a type of 'fingers crossed ethnography' where my luck might have run out.

My role could place me in a 'moral fix' (Van Maanen, 1983). This was made very clear when one former female student recognized me on the door and I had to deny this and assertively accused her of being drunk, which caused her some rather public confusion and distress. I didn't need sociology to tell me to 'turn the tape off' when a bouncer in the field that I had developed trust with began to open up to me about his emotional relationship to his estranged wife and son and the brutalization of door work. In this sense, my ethical conduct is 'self regulated'.

My research dreams were driven by realism and naturalism and, thus, trying to gain naturally occurring data. In some ways, part of the dream is to reduce artificiality and covert research is interesting in obviating that, although it is not a panacea and routinely suffers from the instigation tactics of field researchers. That is to say, researchers can effectively create or alter situations to catalyse certain types of behaviours or responses they are more interested in. In this sense, covert research is not free of active probing, or other methods of getting the story, which is at odds with the picture of a more purist capturing of data that it trades on.

Conclusions: Covert Research as Part of the Sociological Imagination

My aim has not been to offer formulaic advice for ethnographic research but rather to reflect on some particular problems I encountered which led me to reflect more widely on the issue of research ethics and to critique what I consider to be a 'fetish on ethics'. Another researcher with a different intellectual diet and gaze might arrive at a different and even opposing set of conclusions and reflections. For me, the discourse on ethics trades on an exaggerated idea of the role and importance of the academic. In this climate of hyper-sensitivity, covert research is frowned upon and many researchers, apart from a small minority, are effectively deterred from doing it and I do not see a dramatic shift in that. Obviously, we must be sensitized to the complex set of ethical problems in doing covert research, but it is worth pursuing. Ultimately, for me, the analytic understanding gained from the membership that a covert role can bring to the setting outweighs its ethical disadvantages. The decision to do it was driven by neither sensationalism nor romanticism. Erving Goffman concludes his presidential address to the American Sociological Association in 1982 by stressing that sociology must sustain a 'spirit of unfettered, unsponsored inquiry' (Goffman, 1983). For me, covert research clearly demonstrates that spirit.

I see covert research as part of a wider process of disruptive thinking in sociology and the social sciences, where one's normal status and privilege in the setting is removed. Covert research is part of a somewhat submerged tradition that needs to be recovered for future usage in its own right rather than being treated correctively as teaching material for cases of 'failed or bad ethics'. In many cases, covert research is an informed choice of research style rather than an enforced one. Moreover, research in this mould is a tradition that has significantly shaped, often in controversial ways, debates about the research relationship. My deep concern is that, in the present context of governance, we develop forms of 'methodological hypochondria'. This is not a belligerent stance nor a heroic portrayal of the covert researcher as, quite clearly, covert research is not appropriate for certain sensitive topics.

What is interesting is the popular public voyeuristic appetite for covert documentaries while, simultaneously, covert work is being more regulated in the professional social science community. What has been, and still is, routinely used in practitioner fields over the years, for different purposes, has been under utilized in the social science community. For me, covert research has a potentially creative and imaginative part to play and a voice to be heard in the sociological community. Covert research is not to everyone's taste and neither should it be. For me it can be artful, and it does have a place in the history of the research relationship and, I hope, an interesting, if controversial, future as well.

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Note

- 1 There are various ethics codes and policy statements from professional and associations. These include the British Sociological Association's *Statement of Ethical Practice* (2002); the Social Research Association's *Ethical Guidelines* (2003); the Market Research Society's *Code of Conduct* (1999); the National Union of Journalists' *Code of Conduct* (1990); the British Society of Criminology *Code of Research Ethics* (2002); and the American Anthropological Code of Ethics (1997).

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