‘Hidden Ethnography’: Crossing Emotional Borders in Qualitative Accounts of Young People’s Lives

Shane J. Blackman
Canterbury Christ Church University

ABSTRACT
In sociology, the movement towards acceptance of emotion within research has been a slow process. Often referred to as the ‘reflexive’ turn (Bourdieu and Wacquant, 1992), this development has enabled qualitative research to put forward more realistic fieldwork accounts. Drawing upon ethnographic studies on young people, this article explores the idea of a ‘hidden ethnography’, that is, empirical data not previously published because it may be considered too controversial. The article examines how explanations of research methods in sociology are reluctant to explore the legitimacy of emotional relations developed between the researcher and the researched, and seeks to add to the literature on emotion as a male researcher studying female participants. I argue that qualitative work in sociology needs to consider the existence of the ‘hidden ethnography’ in order to advance understanding of how studies are carried out and theory constructed.

KEY WORDS
drugs / emotion / ethnography / gender / youth

Introduction: Sociology and the ‘Hidden Ethnography’

Rarely in sociology is the emotional contact between observer and participants made explicit. Barter and Renold (2003: 100) assert that ‘emotion is deemed to be epistemologically irrelevant’. Three factors can be identified as impeding a richer understanding of emotion in sociological research methods. Firstly, there has been a reluctance to describe emotion within fieldwork...
accounts. This hesitancy stems from the fear of losing legitimacy, or being discredited. This unwillingness to give a realistic account of fieldwork may be more keenly felt by younger academics (Coffey, 1999). Secondly, secrecy surrounds fieldwork and the accounts provided, in part because many recognized classic studies of participant observation are linked to deviant ‘outsider’ status and thus retain an aura of mystery (Becker, 1963; Thrasher, 1927; Whyte, 1955[1943]). And thirdly, there is a disciplinary requirement, and an ethical demand, that the storyteller and the narrative should be ‘clean’. This leads to what can be called the ‘hidden ethnography’, empirical data that is not released because it may be considered too controversial. Coffey (1999: 90) talks about ‘a strong oral culture within the ethnographic community’ amongst fieldworkers who offer up ‘tales and anecdotes’. On this basis it can be suggested that within all the sub-groups of the British Sociological Association, there are sociologists who talk about the ‘hidden ethnography’ where emotion played a part, but this data relating to feeling remains unpublished (Aldridge, 1993: 64).

Denzin and Lincoln (1998) maintain that the reflexive turn in qualitative sociology and its consequent challenge to the notion of objectivity has created the space whereby it is now possible to write the researcher into the world they investigate. For Bourdieu, this reflexive turn is about power and risk because the sociologist exposes their origins, biography, locality and ‘intellectual bias’. Over the last 15 years I have undertaken ethnographic studies on young people, looking at their resistance to school, their vulnerability, social exclusion, drug consumption and experience of being homeless. The purpose of selecting different types of ethnographic data is to apply Coffey and Atkinson’s (1996) idea of showing the degree of compatibility between the separate research locations. The rationale for the article is to apply what Marcus (1998) describes as the potential of a ‘multi-sited research imaginary’ i.e. looking across a number of studies. Further, Abu-Lughod (2000: 264) suggests that the use of multi-social sites is to generate new knowledge focused on understanding emotion and locality, which could then engage with the power structures of inequality.

**Discipline, Emotion and Fieldwork**

The sociology of emotions is now becoming an established area within the discipline. It traces its origins back to the founding figures of sociology including Durkheim, Marx, Weber and Simmel (Harre and Parrott, 1996). In recent times the study of emotions within sociology has often been preoccupied with negative concerns rather than asserting the legitimacy of the topic itself (Kemper, 1990). Early key theorists in the USA and Britain sought to place emotion at the centre of sociological thinking in an attempt to breathe life back into the social, so rigorously extracted by structural functionalism (Hochschild, 1979; Jaggar, 1989). However, Bendelow and Williams (1998: xii) think that sociology remains suspicious of emotion. They write that it is seen as the ‘antithesis of the detached scientific mind and its quest for objectivity’. Within British sociology
the study of emotion may apparently have a secure place but, as Barbalet (2002) notes, it may still be ‘rejected’ and seen as ‘inappropriate’. This negativity towards emotions within sociology is derived, according to Shilling (2002), from the long shadow of the Hobbesian problem of order, which places emotion within the realm of subjectivism. Further, Holland (forthcoming) raises the issue of disciplinary border skirmishes between psychology, psychoanalysis and sociology as to who can best understand emotion. At the same time, there have been new movements towards multi-disciplinary approaches; see Hollway and Jefferson (2000), Walkerdine et al. (2001) and Gilbert (2001). What has brought these different approaches together and also allowed space for emotion to move from the margins of sociology has been the growth of reflexivity in qualitative sociology (Taylor, 2002: 3).

Previously in sociology, emotions only entered the discipline from outside the mainstream through ethnographic or feminist work. What made the studies of the Chicago School (Anderson, 1923; Becker, 1963) and the Institute of Community Studies (Young and Willmott, 1957) so popular was their preoccupation with ‘lived experience’, yet emotional relations were rendered largely invisible in the process of writing. Wincup (2001: 19) argues that the rejection of distance and the objectivity in the researcher–researched relationship in feminist work of the 1970s ‘opened up the possibility of focusing on the emotional dimensions of research, which has not been fully exploited’. Lee-Treweek and Linkogle (2000) consider the turn to reflexivity in qualitative sociology as a chance to grasp how understanding emotion can contribute to greater academic insight. Measham and Moore (2007) see emotional reflexivity as a resource rather than a methodological problem, but as Hollway and Jefferson (2000) note, the knowledge of the researcher is bounded by their defended subjectivity. Reflexivity is not unquestionably a positive undertaking and puts incredible strain on the research relationship. In the study of youth, reflexivity has been cautiously advanced by Hodkinson (2005) and more explicitly advocated by Bennett (2003). Denzin and Lincoln (1998) argue that the challenge of reflexivity in ethnography led to a crisis of representation and legitimacy, but it has also created an opportunity for a new responsive focus in ethnographic writing. However, the double-edged side of reflexivity is that, through disclosure, researchers are open to challenge about research ethics, but unless the hidden ethnography is made more transparent a more realistic account of fieldwork will not be forthcoming.

Through the writing process the ethnographer decides how much to reveal. My argument is that to reveal what is usually hidden is to cross emotional borders in fieldwork accounts. The term ‘crossing borders’ emerged from a range of thinkers who have been influenced by the postmodern challenge (Giroux, 1992). In particular, I want to use Homi Bhabha’s (1994: 7) idea of ‘crossing borders’ as ‘an encounter with newness’ to contribute to the discussion on sociological research methods through the idea of a hidden ethnography. Only with the emergence of reflexive sociology has a new priority been given to critical reflection on the writing process to offer a more realistic fieldwork account (Maynard and Purvis, 1994).
Drinking with the Youth ‘Underclass’

After a month of fieldwork in my ethnographic study (Blackman, 1997) on young homeless and unemployed people in Brighton, I received an invitation to go for an evening drink in a public house. I had joined the group on the soup run, on street corners, playing football, on the beach, in cafes and at the park, so it seemed only natural to accept their invitation of a drink. We developed our friendship through an exchange of stories and play. On a gorgeous sunny evening in August we walked off to the pub. Whilst the plan was simple, selecting a pub was more difficult because some young people were known for previous bad behaviour or they did not want to drink at certain pubs because drunken customers had attacked them after closing time.

In the pub I ordered a first round of seven pints, then I bought the second drink because they had very little money. I also bought the third pint. As the young people were finishing their second pint, I noticed that they were becoming quite merry. I realized that they were not used to drinking alcohol in a short period of time. Their experience of alcohol was of making a can last as long as it feasibly could through the day. At the start of the evening they asked questions about the nature of the research and what it might achieve, and talked about how they felt about being studied. They were interested by the fact that I had done previous ethnographic work. The conversation expanded into a series of themes reflecting their own personal disasters and successes. Through the interplay of the discussion I was struck by how lively and coherent their analysis of people and events could be. Inevitably the subject of the future was raised, and here I decided to make a direct comment.

My response was that, from the evidence of everyone talking in the pub, it was clear that each individual had something to offer. I pointed out to them that they had held a series of serious conversations, which demonstrated ability to understand and contribute. I said that in order to discuss these matters as they had, ‘you must be intelligent’. They responded by saying that parents, teachers and other authority figures had labelled them worthless. This self-perceived inadequacy is understood by Shaw (2005: 847) as common, stating ‘participants with problem experiences often feel they do not know anything, that they always mess things up and that they have nothing to feel proud of’.

The following two days were spent at the university but when I returned to the fieldwork I found to my surprise that this small intervention had inspired some of the young people to apply for jobs, seek new accommodation and try to gain access to college courses. This account remained part of the hidden ethnography because I was unsure how drinking with participants would be interpreted in the discipline, yet it had a major impact in establishing rapport. These young people had changed and held a new belief because I was emotionally committed to them. I was prepared to offer them my time to be part of their lives and experiences, and share their loneliness. This emotional investment had a positive impact in that they had begun to act with more agency – for Rager (2005: 425) the key reflexive fieldwork strategy is ‘to be careful not to take the focus off their experiences’.
Young Women and Domestic Violence

In my ethnographic study (Blackman, 1998b) on homeless young families I spent some time visiting young women who lived in sheltered accommodation because of their experience of domestic violence. When doing ethnographic interviews it was necessary for me to be chaperoned by a woman. After speaking with different women over a period of three months, visiting their flats and sitting with them on the grass bank in the sunshine, I was becoming accustomed to their self-narrative and survival strategies, although this did not make the interviews and observation any easier. Dobash and Dobash (1992: 2) state that ‘stories may now be familiar, they still remain both painful and powerful’. I found that when I was in the presence of distress and shown the result of torture, my senses were shaken (Letherby, 2000: 103). One morning standing in the kitchen with Amanda, she said:

What gets to you in the end is all the beatings you have to take. I have new bruises (she shows me her arms) on old bruises. What really gets me though is the way men think they can do the rounds and not be involved in family. I was taught that you need a bloke. A man gives you protection; this is what I wanted. I don’t have a man now. Don’t have rows.

During that late morning there had been problems with children crying and small things not going to plan, then Jenny from the flat next door announced with tears in her eyes ‘it’s not easy to accept that you are a victim of violence’. By lunchtime her friend Gill had joined us. She said:

Yeah, that’s it. It’s wrong Jenny, we’re not victims of domestic violence. That’s what they [social workers] call us; I hate being called a victim. But it’s not, it’s wrong. We’re bloody survivors of it.

During these ethnographic interviews I felt there was an opportunity to support their condemnation of men, in order to show solidarity. But I was reluctant to do this. In discussion, the young women were able to talk about men at a general level then in the same sentence they would refer back to their particular man. Sometimes when they were speaking negatively about men in general, it was possible to detect that they were beginning to feel uncomfortable speaking this way to me as a man. As a result of this I thought it would be best not to join them in their critical assessment of men because they might feel I was speaking badly about their man who I clearly had never met. Being a male sociologist I could not apply Oakley’s (1981) appeal to sisterhood, yet through immersion I did show sympathy (Finch, 1984). But I felt turmoil. I could not be like them; I felt out of my depth to offer advice. They allowed me into their lives to share narratives and reflect and I experienced Cotterill’s (1992: 599) problem of the blur between ‘research friendship and friendship’. I was not and could not be their friend, but we shared ‘friendship moments’ through honesty, a sort of ‘grounded empathy’. In my high-pressured fieldwork, Duncombe and Jessop’s (2002) idea of ‘faking friendships’ was not feasible; it would have been dangerous.
This is not always easy to explain as the next conversation shows. One cold evening in winter I met Sarah, with Mary her social worker. She showed me her new flat and what an opportunity it will offer her. She seemed bright and wanted to speak about the future. Then Sarah said:

He’s knocked me out, knocked me over. Kicking. Kicked me as well a couple of times. I don’t know why he does it. He’s smashing things up. He does really frighten me. It’s the screaming, the threatening gets really quick. I’ve got black eyes and cuts, but I don’t know where it will end.

Sarah was very upset, with tears rolling down her cheeks. She talked further about leaving the other flat and how it had been so traumatic, with terrible feelings of regret. Mary was visibly shaken by Sarah’s account. I was myself devastated by the story as it was further elaborated and found that the experience of weeping brought writing in the field diary to an end. The attempt to hold back the tears made my body posture awkward and Sarah was aware that everyone was upset. Mary gave her a hug and we all decided to move into the kitchen for a cup of tea and change of setting. We were then able to focus on another subject. Sarah added, ‘I don’t like to talk about it, but I have to. It must come out.’

In the evening when I left Sarah’s flat with Mary I said to her that Sarah’s experience had been overwhelming for me as a man to experience, irrespective of being a researcher. In their research on gender violence, Skinner et al. (2005: 16) reveal that ‘we suffered emotional pain, fear, anger and being overwhelmed ... and at some point find ourselves in tears’. During the ethnographic conversation I felt that I wanted to punch Sarah’s ‘bloke’, or smash up some of his things for what he had done, because he had made her life so utterly miserable and driven her close to suicide. Mary maintained that not expressing my feelings was probably good because Sarah was still so distressed by her experience, because she was living it. So she did not need to meet other men who would also feel like being aggressive because that would still be violence.

Sarah hated her violent ex-partner, but I also found myself hating him, alongside other violent men the young women spoke of. Sociologists were not supposed to acknowledge hatred, so I felt reluctant to discuss such emotions and as a result they became part of the ‘hidden ethnography’. Disciplinary expectations and academic training may impose unrealistic demands. For Edwards (1993), research that is sensitive makes the observer ‘self aware’ and at the same time ‘other aware’, throughout the intimate time of dialogue and exchange. During these ethnographic interviews I was conscious of paying very close attention to the words uttered and also the feelings, which made the talk close, so intense, so life affirming. I became aware of the deep silence after each word had been uttered, but in fact there were no pauses. In qualitative work participants can suffer pain, but feminist studies reveal that this real experience is not necessarily damaging and can have a therapeutic effect whereby participants feel empowered (Cotterill, 1992; Gilbert, 2001; Stuhmiller, 2001; Wincup, 2001).
‘I’m Gonna Kill You’

During fieldwork on young homeless families (Blackman, 1998b) I had been brought into a number of difficult circumstances some of which amounted to personal danger. It remained part of the ‘hidden ethnography’. At the time I thought the experience revealed my weakness because few methodological accounts speak of personal failure and they are not positively looked upon by the discipline. It was my second day of fieldwork when Jim, the caretaker of the high-rise tower block, was showing me the launderette facilities. Standing by the washing machines and dryers were a man, Matt, and a woman, Cathy, in their late teens with two small children. Immediately, Matt began to act aggressively towards me saying:

I know you. Yeah, I’ve seen you before, you’re from the Council, coming down here. It was you who refused our application. I recognize you. I’m gonna get you, I’m gonna kill you, you bastard.

Before I could reply, he launched himself at me. I managed to just dodge out of the way, hurriedly telling him that I was not from the Council. In the struggle that ensued, the caretaker physically grasped the young man and eventually reassured him. Jim shouted, ‘He’s from the university’. For a while my heart was in my mouth. This is a clear example of what Lee and Stanko (2003: 3) call sensitive research that ‘poses an intrusive threat’ because it deals with areas of life that are private, stressful and sacred.

After a few minutes Matt began to speak about his housing problem and how both he and Cathy were unable to cope after becoming homeless. She was restless while he was still acting out aggressive behaviour. I informed Matt and Cathy that my role was to speak to various people who were living in the tower block, learn how they find it, and how things could be improved. They seemed okay about this and we began to speak positively, but Matt was still very pent up and unable to concentrate, and Cathy told me he was ‘clucking’. Matt had become a heroin user and was in urgent need of the drug. She suggested that it was impossible to speak with him like this as he could explode again. Matt asked me whether I could drive him to see his drug dealer. Jim said:

The state he’s in at the moment he ain’t no use to anyone. I don’t want him in the flats. I could call the police. But this means further trouble all round. He can’t stay on the premises in his condition.

I decided that a short car journey was probably the best result for everybody; Cathy could return to the flat with the children and the caretaker could resume his normal responsibilities. During the car journey Matt described his drug problem and, after dropping him off, he said I should go back to speak with Cathy and get the whole story. He was very grateful for the lift and I returned to the flat where I found Cathy, played with the children and began one of the many long interviews. Throughout the ethnography, I found the experience of being with Matt and Cathy was electric. They were on the edge
emotion being lived, running with emotion and due to their problems I felt that next morning I might find them gone or dead. Here my emotional relations were shaped by these simple gifts and exchanges, which brought forth open communication. Reflexive sociology raises moral issues, when the danger of collusion or the fear of ‘going native’, which the discipline is critical of, is apparent. Encouraging friendship in order to get data can be seen as exploitation or ‘academic-career’ building. Holland (forthcoming) argues we need to recognize the discomfort of ethnography and work towards drawing into the analysis and writing process the emotions experienced by participants and observer if reflexive sociology is to be honest and rigorous (O’Reilly, 2005).

Sex and Romance in Fieldwork

In 1998 I had a chapter (1998a) published in the book, *Cool Places: Geographies of Youth Culture*, edited by Skelton and Valentine. My study focused on a group of resistant female school pupils called the ‘New Wave Girls’; they were aged 16 to 17 when I undertook the study and I was 22. The article raised the issue of their sexual attractiveness and desirability, but I did not elaborate on this. For male researchers, focusing on young women can present problems, and in general male researchers show a tendency towards conservatism and talk about how they were excluded from female space (Back, 1993; McRobbie and Garber, 1975). In contrast, this study resulted in a male being at the centre of a female space (Griffiths, 1995; Lees, 1986; McDowell, 1992). In one review of *Cool Places*, Seibel (2000: 69) states ‘Shane Blackman gives an interesting and detailed report from her ethnographic research ...’ Furthermore, a number of academic colleagues in universities across the United Kingdom have stated that their sociology final year female undergraduates believe a woman undertook the study on the New Wave Girls. I take this error about my gender as a compliment in terms of acknowledging a valid study on female youth culture. Theoretically, this raises the wider issue of whether a man can adequately undertake a study on young women and also whether a man can be a feminist. Cixous (1981a: 93) suggests: ‘there are men who do not repress their femininity’. My involvement with feminism accords with Cixous’ issue about writing and whether male writing could speak for women. Whilst I have no intention of claiming to speak for women, the study allows the participants to explore their self narrative. This ethnographic representation of the girls by a man is based on Cixous’ (1981b: 256) assertion that as a man I am not ‘afraid of femininity’.

Throughout the fieldwork the new wave girls conducted their close relationship with me in terms of bodily play, desire and physical contact, from kissing, degrees of undress and through public acts of closeness such as walking arm in arm. The ethnographic data I present takes the form of different personal documents. First is a poem written by Debbie, which conveys the feelings of the group during the first month of fieldwork. She handed it to me during an
afternoon lesson. Before it was passed to me, another new wave girl, Sioux, read it and made a written comment.

When you were new  
Everyone wanted to know  
You were the new point of interest  
We became friends, then lovers.  
But I threw it all away  
Because of a stupid rash decision  
That stuck in my mind and grew  
Out of proportion  
Now we have become friends again  
I'm pleased that we can have an  
Easy relationship  
One that suits us both.

Sioux’s comment ‘Hey that’s lovely’.

At the time I thanked Debbie for the poem and put it in my fieldwork file. Hey (1997: 50) calls such personal documents given to the researcher ‘pocket ethnography’. The poem clearly shows how Debbie perceives the fieldwork relationship as a very personal and intimate thing. It expresses changes in feeling and is also a commentary on the development of my relations with the girls. Whilst we did not share a sexual relationship, even though I had slept overnight at her house, there was a powerful romantic attachment with Debbie and other new wave girls due to the fact I was permitted to share a close intimacy in their private space. I never encountered Hey’s (1997) experience of verbal abuse or ‘stare outs’.

I want to suggest here that we need to conceptualize our erotic responses in ethnographic fieldwork, especially in terms of the power relations and forms of exploitation within gender relations. An understanding of sexual contact in fieldwork cannot be limited to penetrative sexual intercourse. Sex in the field needs to be seen in a much broader context, highlighting degrees of physical play and touching, where intimacy, close contact, love, romance and flirtation are part of the ethnographic endeavour, which allows for a more substantial grasp of the true dialogue between participant and observer. In their chapters in the book *Taboo* (1995), Gearing, Willson and Altork assert that sexual fantasies and sensual reveries should be seen as a normal part of fieldwork, which requires explanation and needs to be understood in terms of set limits to prevent temptation. These women are demanding open critical discussion on the forms of, and degree of, sexual contact within fieldwork to enable effective debate to move beyond the ‘hidden ethnography’. This is a challenge to the status quo of the detachment principle, which understands sex in the field as in contravention of the ethnographic code of objectivity (Warren, 1988).

A narrow understanding of sexualized fieldwork fails to reveal the complexities of sexual forms of contact and merely reinforces a masculinist ideology of the erotic. Altork (1995) suggests that Cixous’ assertions have changed
fieldwork understandings because, not being allowed to write from ‘below the hip’, but always from the brain, results in a male objective hegemony that denounces personal reflexivity and ensures that the conservative power of an ‘apparent science’ within the discipline is upheld. The basis of my ethnographic relations with the new wave girls was through emotional commitment and love. Lutz (1986: 290) argues that ethnographic fieldwork and the resultant theoretical descriptions critically derive from the triangulation of the different data sources of the body, emotion and mind. This new combination seeks a more reflexive focus on fieldwork explanations, rather than a retreat to an apparently neutral shield that reinforces the implicit power of the discipline and thus repression (Skeggs, 1994). Below is a conversation, which initially refers to these issues, but then moves on to explore other ‘hidden fieldwork’ issues. The discussion is taken from an audio recording made for me by the new wave girls in bed at a sleepover.

Sally: Don’t you think it’s flattering being studied?
Sioux: Yeah, but like you said I feel inhibited because
Sally: Yeah you do you feel inhibited because
Sioux: Because he’s studying all the time. Even at Cathy’s party he’s still studying people.
Sally: Yeah he always is and you can never be yourself because you know you’re being studied.
Sioux: Yeah exactly.
Lynne: You going to play this tape to him?
Sally: Yeah, sorry Shane.
Sioux: Sorry Shane. I beg your forgiveness.
All girls: – Laughter –
Sally: Oh Shane you look beautiful when you’re asleep.
Sioux: Yeah, you can rub my legs any time.
Sally: Do you remember when he was asleep that Steff went up to him and undid his flies.
Lynne: Oh yeah, oh you better not say the next bit on tape!
Cat: Did she really? I didn’t know anything about that.
Lynne: And someone else, wasn’t it!
All girls: – Laughter –
Sioux: I can’t remember that bit.
Sally: Yes she did it, it was really funny!
Sioux: I remember Christina chucking herself on him. I don’t really think that he wanted her to. Because she had her arms round him.
Sally: Did you Shane?
Lynne: Did you oh.
Sioux: Shane did you know you are a handsome black tape recorder.

When Sally gave me the tape I thanked her and was genuinely surprised to receive it. There are three phases to the girls’ conversation. At the start it is serious. They feel able to speak about the study in my absence. The talk moves from the serious to the game. The girls are enjoying fun: they are ‘having me
on'; it is a playful but complex game where they are challenging the objectivity of the research relationship. They joke, then move on to develop a conversation with sexual overtones, showing how they are conscious of my sexual presence. The girls move out, from their game strategy of ‘putting it on’ to fantasizing a relationship or encounter with the researcher. They play with the theme of seduction and as the sexual theme develops, they conclude with a superb note of irony. It was their idea to record a tape, which they initiated and carried out unprompted. In this sense the cassette is a positive and reflexive gift from the girls, who feel engaged by their participation (Ribbens and Edwards, 1998). In ethnography, gifts as part of small exchanges are an endemic feature of empathy, but rarely does the discipline fully acknowledge their true emotional significance (Oakley, 1981).

‘The Tenth Muse’: Ethnography, Sociologists and Drug Mediations

As with sex in the field, the admission by a sociologist that they have used drugs with their participants is in opposition to the ethical principles of the discipline. In my study of a group of mod boys (Blackman, 1995), one piece of ethnography which I did not use was when I went on holiday with the mods to the Isle of Wight for a scooter rally. The reason for not including this account before was to do with establishing my legitimacy as a researcher. In sociology the rite of passage into the discipline is successful completion of fieldwork and publication of the result. This gives you entry into the discipline, but you have to be judged first before acceptance. There were six mods who had scored a range of drugs from amphetamine powder to pills and cannabis. On arriving on the site the tent was put up and then the holiday began with Keef saying: ‘Well, skin-up then.’ During the day and early evening the mods would consume speed, then after midnight they would go back to their tent and proceed to smoke spliffs. Each mod would ‘spark-up’ a joint: this meant that in the tent approximately four spliffs were being circulated or prepared till the small hours of the morning. As a result the mods themselves were intoxicated and remained so in the morning. This level of consumption was maintained throughout the weekend from Friday to Sunday night. Through participating I became high with them in terms of sharing the experience, but I was not a drug initiator because our close relationship of two years did not demand it. However, I was not in a position to stop them consuming drugs and as an aspiring sociological PhD research student I was not in a secure position to include this content as it might have undermined my legitimacy (Williams, 1990).

Relatively few sociologists have been open about their drug consumption as part of their research methods. Adler (1985) talks about her and her husbands’ use of small amounts of illicit substances in their empirical study on drug dealers and smugglers. Thornton (1995: 89) writes:
We go to the toilets, cram into a cubicle where Kate opens the capsule and divides the contents. I put my share in my glass and drink. I’m not a personal fan of drug use – I worry about my brain cells. But they’re a fact of this youth culture, so I submit myself to the experiment in the name of thorough research (thereby confirming every stereotype of the subcultural sociologists).

Few sociologists directly admit to this experience because of its illegality. Fear of punishment or banishment from the academy is seen as the potential danger. This was the case with Professor Ansley Hamid, at John Jay College, City University of New York, who wrote in his field diary about the experience of using heroin. Smallwood (2002: 4) describes how Hamid was first suspended from his post, then the university began the process of stripping him of his tenure, and finally, in 2003, his university academic career came to an end with retirement. However, if you take a closer look at the drug consumption of sociologists within the discipline, the situation is more complex.

During the 1920s, Walter Benjamin (1979[1929]: 215–22) wrote about his hashish intoxication in Marseilles, combining participant observation and the idea of the flâneur in the city. Benjamin’s (1997: 55) ethnographic interpretation was influenced by Baudelaire’s *Les paradis artificels*, where he becomes a ‘connoisseur of narcotics’ in search of bliss to describe the charm of everyday life as a form of intoxication. Benjamin is not alone in his drug confessions; see, for example, two contemporary philosophers Michel Foucault and Gilles Deleuze. From the 1960s the French philosopher Foucault began using cannabis as one of his leisure pursuits in North Africa. Foucault’s search for pleasure through experimentation led him to consideration of hallucinogens (Macey, 1993: 253). Whilst Foucault seems calm about publicly articulating the connections between drugs and self, Deleuze (1977: 191) appears more apprehensive and adds a footnote to Foucault’s discussion of drugs, stating ‘What will people think of us?’ Deleuze’s apparent reluctance to be open about drugs, according to Plant (1999: 151) is because ‘drugs take all authority away’. She suggests that, Deleuze ‘feared the charge of inauthenticity, artifice, fraudulence, irresponsibility’ against his work.

In contemporary sociological fieldwork few academics have spoken freely about the place of drugs within their qualitative work. Two concepts may be useful here to advance the discussion about the ‘hidden ethnography’: ‘guilty knowledge’ and ‘license to sin’. Polsky (1967: 138) developed the idea of ‘guilty knowledge’ possessed by the ethnographer through their participation with respondents engaged in forms of deviance. Grady (1993: 53) talks about the notion of a ‘license to sin’, where Hollywood film directors are permitted to explore patterns of drug use providing they are set in the past. Using these ideas it is now possible to interpret the ‘hidden ethnography’. For example, today Benjamin and Foucault’s substance consumption is never raised as an issue but remains a thing of the past. Howard Becker, speaking of his early studies into jazz music and cannabis, states ‘I was a musician, and in that capacity came into contact with marihuana and like all my buddies, smoked it as a matter of course. I didn’t smoke it in order to achieve empathy with anyone for research purposes.’ On a different note Paul Willis states ‘it would have been a case of
sticking out if I didn’t. Yes I did smoke weed with the hippies.’ Potter’s (2007) recent ethnographic study of domestic cannabis production is more reflexive, suggesting that within the context of having gained access, drug use results from the setting as part of an everyday exchange. A question for sociology to answer is whether ethics committees and the desire for clean research will hamper ethnographic studies from getting ‘up close’ and result in a failure to gain insight into the lives, motives and experiences of people on the margins, or in situations involving risk.

Becker, Willis, the Adlers, Thornton, Potter and myself suggest that drug consumption is not primarily about empathy, it is an experience which came naturally in the context of the ethnography. For these sociologists their substance use was not a key research strategy, but born from rapport already established. In each of these cases the sociologist is neither a drug initiator, nor are they claiming a ‘deviance halo’ for themselves. Each sociologist is sensitive about admission and the implications about ‘outing’ people. While they can be criticized for their lack of objectivity, their honest interventions allow insight into the unspoken agreements with participants, which usually form part of a ‘hidden ethnography’. These examples cross conventional boundaries of objectivity on the basis of a sensitive and strategic intervention to retain their position in the field.

Conclusions

The different ethnographic episodes show how powerful feelings of emotions from love to hate grip both the researcher and the researched. My fieldwork was always based upon respect and constant negotiation with participants who allow me access to their private and public space. Few male sociologists have discussed their experience of studying women. I found that when studying the new wave girls I was able to deploy my own subjectivity, through biographical similarity and cultural identity, which created bonds of love, desire and solidarity. In contrast, when studying women who experienced domestic violence, I was also able to use my ethnographic experience and training in feminist research to bridge apparent differences as resources for reflexivity.

Mauthner and Doucet (2003: 417) argue that the ‘reflexive turn’ enables sociologists to examine what remains undeveloped in the research relationship. But although increased openness is supported in certain sub-fields of sociology, disclosure is likely to be seen as more risky in other sub-fields. Using unpublished data from qualitative studies, I have tried to make evident the significance of the ‘hidden ethnography’ within research and the need to account for it, which I described as crossing borders. This silence relates to the place of emotion in ethnographic work. The hidden ethnography plays a key role in the development and elaboration of knowledge production, and it also reveals that existing explanations of how qualitative research is undertaken contain an absence. Reflexive accounts, however, can bring problems based around researcher intrusion as a result of exchange, identification, collusion and advocacy.
Revelation of the hidden ethnography raises issues about research ethics. This idea may not necessarily be at odds with ethical codes, providing reflexive sociology demonstrates its rigorous, responsive and systematic approach. Holland (forthcoming) argues that sociologists need to be aware of the danger of being obsessive about consent, because what appears to be good ethical practice can be underpinned by anxiety or compulsion which would disturb participants. To advance more ‘open’ reflexive approaches that explain how research is conducted and written, British sociology needs greater disciplinarily understanding and recognition of the real challenges and opportunities faced by qualitative research, which demands emotion.

Acknowledgements

Parts of the article were first delivered at the International Qualitative Research Conference: Representation and Reflexivity, Universidad de Guanajuato, Mexico, June 2005. I would like to thank Howard Becker, David Downes, Paul Willis, Lisa Maher, Gary Potter, Donna Gaines, Mark Smith, Bob Hollands, Fiona Measham, Ross Coomber, Janet Holland, Tim Rhodes, David Moore and Debbie Cox.

Notes

1 Thackeray, 1999.
2 Private communication, 2005.
3 Private communication, 2005.

References


Shane Blackman

Was an ESRC scholarship PhD (1990) at the Institute of Education, University of London, supervised by Professor Basil Bernstein, and is now Reader in Sociology and Cultural Studies at the Department of Media in Canterbury Christ Church University. He has previously held posts at the University of Surrey, University of Greenwich and at the Institute of Education as a research fellow on a feminist research project. Shane has conducted research into sociological and ethnographic aspects of young people’s culture. His most recent book is Chilling Out: The Cultural Politics of Substance Consumption, Youth and Drug Policy (Open University Press/McGraw Hill, 2004).

Address: Department of Media, Canterbury Christ Church University, Canterbury, Kent CT1 1QU, UK.

E-mail: sjb9@cant.ac.uk