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Between Overt and Covert Research

Concealment and Disclosure in an Ethnographic Study of Commercial Hospitality

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This article examines the ways in which problems of concealment emerged in an ethnographic study of a suburban bar and considers how disclosure of the research aims, the recruitment of informants, and elicitation of information was negotiated throughout the fieldwork. The case study demonstrates how the social context and the relationships with specific informants determined overtness or covertness in the research. It is argued that the existing literature on covert research and covert methods provides an inappropriate frame of reference with which to understand concealment in fieldwork. The article illustrates why concealment is sometimes necessary, and often unavoidable, and concludes that the criticisms leveled against covert methods should not stop the fieldworker from engaging in research that involves covertness.

Keywords: *covert research; covert methods; concealment; disclosure; ethics*

Covert research and the use of covert methods have always been contentious issues among social scientists. Advocates have argued that covert methods offer researchers access to information that is otherwise denied to them (Calvey, 2000; Lauder, 2003; Miller, 2001), while critics have denounced covert methods as ethically and professionally unsound and vilified all those who engaged in “duplicious” research (Beauchamp, Faden, Wallace, & Walters, 1982; Bulmer, 1982a; Herrera, 1999; Warwick, 1982).

Author's Note: I am deeply indebted to everyone who contributed to this study. Most people will never know or appreciate the extent of their contributions; but I hope, in time, I will be able to share more of my conclusions with them and reciprocate for all their help. I am also grateful to Marcus Stephenson, Steve Shaw, Juliet Solomon, Nikki MacLeod, Norman Denzin, and the two anonymous reviewers for their helpful comments on earlier drafts of this article, and to the Savoy Trust and London Metropolitan University for funding the research.

This article suggests that instead of labeling research “covert” and dismissing it on the grounds of ethical irresponsibility, it is more useful to consider how the nature of the study, the character of the fieldwork context, and the relationships between informants and ethnographer determine overtness or covertness in the field. I argue that prevailing critiques of covert methods instill a sense of ethical hypersensitivity that does not help ethnographers to resolve problems of covertness in their fieldwork. Consequently, this article offers a more nuanced understanding of the processes of concealment and disclosure in fieldwork that helps to reassess the usefulness of existing critiques of covert research and covert methods.

I begin by briefly reviewing the historical treatment of covert research and suggest that this has created a professional climate in which all forms of concealment are treated as inherently transgressive. In the next section, I provide a brief description of my study and the research context and then proceed to discuss the techniques I used to recruit informants and elicit information from them. I focus on the factors that determined when and how details of the research were disclosed and, also, on those factors that determined how my identity as researcher could be presented.

The case study demonstrates why concealment was pervasive throughout the research and why there was constant ambiguity regarding my identity and motivations. Concealment of this kind is often considered ethically problematic, but I maintain that ethnographers in similar situations should not be hampered by all the criticisms leveled against covert research. I am not advocating covert research or covert methods per se; however, I do suggest that examining the contextual nature of fieldwork helps ethnographers and their potential critics to understand why concealment is a necessary and often-inevitable part of research. This article thus seeks to provide an alternative point of reference that informs debates surrounding the emergence of concealment and covertness in research.

The Historical Treatment of Covert Research and Covert Methods

Critics have continually questioned the necessity and the usefulness of researchers disguising their identities and concealing their research agendas (cf. Erikson, 1995; Herrera, 1999; Warwick, 1982). Shils (1982), for example, argued that covert methods were inexcusable forms of civil betrayal that violated the individual’s right not to be studied. He felt it was morally obnoxious of scientists to assume that the search for truth granted them a license to

disregard the rights of those being studied. In contrast to some authors, who argued that the knowledge gained through covert research justified the use of deception (e.g., Denzin & Erikson, 1982; Goode, 2001; Miller, 2001), Shils rejected the claim that the value of such knowledge outweighed the problems caused by the infringement on individual rights.

Bok (1986) pointed to the emotional and psychological stress that covert research causes to those deceived and to those who continually have to deceive. The tensions caused by concealment are often found in the fieldwork accounts of ethnographers (cf. de Laine, 2000; Diamond, 1992; Wong, 1998). The pressure to produce open, reflexive ethnographies has meant that researchers are obliged to account for their indiscretions in the field. On one hand, this provides a richer understanding of how the relationships in the field affected the nature of the data, while on the other hand ethnographers are forced into a perpetual cycle of critical self-analysis in which every aspect of their relationships with informants is problematized.

Evidence of stress among informants caused by concealment is harder to find, although it is generally assumed that they would be offended or troubled by being misled. Kimmel (1996) presented numerous cases where deceptive research, particularly within psychological experiments, had caused stress for participants. However, some authors have noted that the majority of criticisms of covert methods are based on deductive reasoning and that there is little actual evidence to suggest that discovery of the researcher's hidden identity or intention always causes distress among informants (e.g., Mitchell, 1993; Oakes, 2002). Nevertheless, Beauchamp et al., (1982), Bulmer (1982a), and Warwick (1982) concluded that knowledge of concealment in fieldwork would raise self-doubt and suspiciousness among informants and make them reluctant to participate in future research.

This legacy of stigmatization, coupled to today's culture of litigation and ethical hypersensitivity (cf. Nelson, 2004; Wright, 2004), has certainly made the use of covert methods a perilous endeavor. Funding bodies and host organizations, already dismissive of exploratory research that does not use conventional, "safe" methodologies (Lincoln & Tierney, 2004), are thus far more inclined to restrict studies using covert methods. The problem is that the historical criticisms of covert research have established a series of professional benchmarks that are used to evaluate any and all forms of concealment in research. Many contemporary research methods texts continue to use studies such as Milgram's (1974) experiments on obedience and Humphreys's (1970) study of gay men's sexual activities as common reference points in their discussions on covert methods (e.g., Bryman, 2004; Crow, 2000; Esterberg, 2002; Gomm, 2004; May, 2001; Oakley, 2000).¹ The notion of

covert research has come to represent a distinct and reprehensible strategy where researchers consciously obscure their motives, purposively deceive their informants, and in the case of participant observers disguise their identities. In short, covert research is often treated as an antithesis to open and overt research. The danger is that understanding all forms of concealment in fieldwork through this frame of reference means any research program involving covertness becomes vulnerable to censure.

Many authors have recognized that there is not a clear divide between overt and covert research (Agar, 1996; Bulmer, 1982b; Gomm, 2004; Herrera, 1999; Hilbert, 1980). A researcher may be able to maintain the subterfuge and conduct fully covert research, but completely overt research can never be guaranteed. It is certainly a mistake to assume that ethnographic fieldwork can ever be fully open and overt, with all the relevant participants giving their continued support based on a consistent understanding of the research. Consequently, any critique of covertness must question whether all occurrences of concealment should be considered universally unethical.

Most practiced ethnographers concede that fieldwork relationships inevitably involve some covertness (see, e.g., Grills, 1998; Shaffir & Stebbins, 1991; Smith & Kornblum, 1996). Relationships with informants are often contrived, and despite many developing into genuine friendships, the ulterior motives of ethnographers are entangled with all social encounters during the course of the research, however informal or incidental (Coffey, 1999). It is interesting to note that, even for some of the most hardened critics of covert research, certain forms of deception were still an accepted part of fieldwork. Cassell (1982), for example, treated the severity of deception as a semantic issue: Clear transgressions such as Humphreys's were unforgivable, but insincere compliance with social etiquette or exaggerated empathy was deemed to be appropriate when building rapport with informants. According to Cassell, these kinds of "social lies" (p. 18) were often part of everyday interaction and not ethically problematic.

In contrast, many writers have questioned the ethical implications of "rapport management" (Birch & Miller, 2000; Duncombe & Jessop, 2002; Finch, 1984; Luff, 1999). Finch (1984) argued that the carefully chosen gestures that make informants feel at ease and encourage them to be more candid in their responses were not benign actions but instrumental and potentially exploitative tactics. For Duncombe and Jessop (2002) rapport management reflected the power of researchers to control field relationships, and the relative powerlessness of informants to either detect this insincerity or to formulate their own strategies of resistance. Duncombe and Jessop consequently suggested that researchers "should continue to worry about these issues as

they emerge in each piece of research and each individual interview” (p. 121). Such critiques encourage researchers to accentuate the ethical questions surrounding their every action. As a result, all attempts by researchers to engage with informants are potentially treated as a series of lies that (a) mask the researcher’s true intent and (b) distort informants’ ability to make appropriate decisions about the information they divulge.

This highlights the perpetual tension between the moral and professional expectations of academia and the contextual factors that are pervasive in ethnographic research. These critiques instill an intense ethical awareness, but such awareness merely problematizes our relationships in the field, while concealment and covertness may remain a constant part of ethnographic fieldwork. The key issue is how ethnographers and their critics attempt to address this inherent conflict. Institutional Review Boards and ethics committees expect researchers to predict where and when issues of concealment will emerge, assess the implications of such concealment, and construct bureaucratized forms of disclosure and consent that demonstrably negate any potential risk. It is assumed that concealment is thus avoidable because the contractual agreements between researchers and informants clearly define their mutual rights, roles, and obligations, and consensual participation is based on an appropriate understanding of the research.

Advocates of a more participatory approach to research have suggested that the only way to avoid concealment in fieldwork is to develop inquiries with the full collaboration of informants (Christians, 2000; Guba & Lincoln, 1989; Lincoln & Guba, 1989). At the heart of this participative model is an “ethics of care” “rooted in reciprocity, relatedness, and responsiveness” (Noddings, 2003, p. 2), and authors such as Denzin (1997) argued that research should be driven by a moral imperative to empower informants and encourage them to be coauthors of the research. It is presumed that building and nurturing open relationships in fieldwork allows for a constructive dialogue between ethnographers and informants about the development of the study. Consequently, “because the research-subject relation is reciprocal, invasion of privacy, informed consent, and deception are [assumed to be] nonissues” (Christians, 2000, p. 149).

The fundamental problem with the institutional review system and the participatory approach is the assumptions they perpetuate about relationships with informants. The rationalizing tendencies of the institutional review system conceptualize field relationships as coherent, formal processes and, in doing so, seem to ignore or deny the ambiguities intrinsic to relationships in exploratory research. Advocates of participatory research acknowledge the existence of ambiguity and suggest that emerging problems can be

addressed through open dialogue (e.g., Guba & Lincoln, 1989), but the expectations surrounding how relationships are built and maintained are equally unreasonable. In establishing contacts, building relationships with individuals or gaining entry into social networks, concealment is still likely to pose moral dilemmas. Within participant observational studies of fragmented groups, with discontinuous membership, the sporadic and ephemeral nature of encounters make these problems particularly intense (see, e.g., Adler, 1993; Adler & Adler, 2002, 2004). The social context impedes the ethnographer's ability to maintain open, interactive relationships with all the potential informants throughout the research, and certain aspects of the ethnographer's identity and motivations inevitably remain concealed.

These models continue to treat all forms of concealment as inherently corrupt, which in turn has the potential to transform every encounter in the field into a moral quagmire. Therefore, what is needed is a pragmatic examination of why concealment arises in research and how related ethical issues may be addressed. Exposition of case studies such as the one in this article will not resolve the moral and professional crises that surround covert methods, but they will inform debates surrounding the practicalities of concealment in research. My aim is to encourage researchers, academic review boards, and ethics committees to develop a more critical understanding of the research process and the real-world contexts in which it takes place. Funding bodies and academic host organizations can thus avoid treating all forms of concealment as essentially flawed and cease to restrict research involving covert methods.

The Research

Aim

The aim of the current study was to consider how commonality and identification was articulated through hospitality exchange, and I sought to understand how the social aspects of hospitality were entangled with commercial agendas and the business of hospitality. I could not rely on secondary data or on survey methods to explore these issues, and I assumed that the most useful way to understand how commonality may (or may not) operate was by observing social interaction in specific contexts and by discussing people's experiences with them. From the beginning of the research, I attempted to identify potential research sites by making exploratory visits to bars, restaurants, and clubs and examining the nature of interaction between "hosts" and "guests." Consequently, any visit to a commercial hospitality venue became a potential source of information.

Context

The "Freelands" was a small bar located in a peripheral district of "Compton," a suburban town in England.² It was largely patronized by gay and lesbian consumers, although it is misleading to call the Freelands a "gay bar." Heterosexual men and women from the local neighborhood also frequented the venue, and the gay and "straight" clientele continually mixed. Patrons regularly came alone, in pairs, or in small groups of three to five and then formed larger groups that incorporated strangers. The size and location of the Freelands meant the operators could not rely exclusively on gay or straight consumers, and the bar thrived because the managers actively encouraged mixed patronage. My initial experience of the Freelands typified the inclusive culture that the managers sought to create. During my first visit, which was with a female friend, the bar staff were very friendly toward us and invited us to a birthday celebration taking place on the following Saturday. On the night of the party, the bar was officially closed at 11 p.m. but the managers encouraged us to stay for "after hours" drinks, and we eventually left the bar after 4 a.m. Being heterosexual did not seem to impede us from engaging with gay or lesbian patrons or exclude us from the Freelands.

Covertiness in Research

The inclusive culture of the Freelands encouraged me to visit the bar as a consumer, but it quickly became apparent that the character of the social space made it relevant to my studies. During my subsequent visits, the interactions with consumers and service staff were increasingly driven by ethnographic curiosity. My cursory observations highlighted the need for a more intensive examination of who came to the bar, why they consumed there, and how their relationships were negotiated; however, being a consumer limited the time I could spend in the bar to short periods, ranging from 1 to 4 hours per visit. To understand how the consumer profile changed throughout the day, week, and month, it was necessary to spend extended periods in the bar. I, therefore, considered applying for a job at the Freelands so I could gain access to the consumers and staff.

I expected to have to engage in emotionally stressful negotiations with the managers about conducting my research, but gaining access proved to be relatively easy. Only three people worked at the Freelands, and one of the managers, Adam, told me that they needed more staff. I expressed interest in working at the Freelands so when the second manager, Shawn, went on holiday for a week, Adam asked if I wanted to replace him temporarily. After 2 weeks, Shawn phoned and said he had lost his passport so I continued to

work there in his absence. When he eventually returned to Britain, he did not come back to work, so Adam, a third member of staff and I were left to manage the Freelands.

Like many researchers in organizational settings, I found it difficult to be open about my research because I feared that the bar's operators would be reluctant to expose themselves to scrutiny. Diamond (1992), for example, had to pursue the majority of his research on nursing homes covertly because he felt that the doctors and senior staff would feel threatened by his study and deny him access. Similar to Diamond, I anticipated that defining myself as a researcher early in our relationship would be counterproductive, and my sociological curiosity remained veiled in my enthusiasm for working at the Freelands. However, in contrast to him, I did not intend to conduct my research covertly. I assumed that once the managers and I developed a closer relationship, in which I was seen as a sympathetic "active member" (Adler & Adler, 1987) within their social milieu, they would not be as threatened by my ethnographic intent. During my 2nd week, I revealed to the managers my interest in the bar and its patrons. I explained that I wanted to write about the relationship between people's sense of identities and their experiences of drinking venues and, that I wished to interview customers and bar staff. The managers said they would be happy to contribute and from then on they regularly introduced me to customers as the "straight man writing a book about us." I did not gain entry to a group simply because there was no coherent group as such. Nevertheless, working at the Freelands provided me with the opportunity to interact with the staff and customers.

I worked at the bar for 27 months, working nearly every night of the week for the first 8 months, and 1 or 2 nights per week after this period. In addition, I usually came to the Freelands once or twice during the days and on my nights off. Having got to know some of the customers, I also participated in a number of social events with people outside the bar. I regularly went to house parties and to other gay and straight venues in and around London with people from the Freelands. Meeting people outside of the Freelands were sincerely enjoyable social activities, despite the fact that I often commented on these events in one of my diaries. There was an effective collapse of the work-leisure divide, and it was often impossible to separate the moments when I assumed the role of ethnographer and when I resumed being a "civilian."

It is interesting to note that Shils (1982, pp. 131-132) drew a simple divide between situations where observation was part of everyday life, where it was deemed healthy, and situations where observation was academically motivated and made possible through some kind of intentional manipulation on the part of the researcher. However, although the basic principle of Shils's

argument is sound, such criticism does not adequately recognize the duality and multiplicity involved in all social encounters. Awareness of the social and physical surroundings is obviously an essential quality for an ethnographer, and this sense of awareness becomes instinctive. Furthermore, the context blurred the divide between a social encounter and a sociologically useful encounter.

The ethnographic intent was inseparably entangled with my social life, and I continually appropriated idle gossip, conversations, and comments during my visits to the Freelands and other hospitality venues. Patrons constantly provided useful information during momentary encounters, but most of the people I saw and heard did not formally consent to share their experiences. For example, in one incident, two people were dancing in the Freelands—being loud and drawing considerable attention from others. Another (older) patron rolled his eyes and said the words *bloody queens* as a passing comment to some of the others and me in the bar. Publicly broadcast declarations such as this did not necessarily warrant elaborate clarification of my reception or potential interpretation. Writers generally agree that informing people in public places of the researcher's intent is unnecessary (Bulmer, 1982b; Dingwall, 1980; Lee, 2000; Lofland & Lofland, 1995; Punch, 1986, 1994; Roth, 1962). During momentary encounters such as this, stopping the person to explain who I was, what I intended to do, and then asking for him or her to repeat his opinion was certainly impractical. However, as the diary extract below demonstrates, supposedly public space could easily be transformed into private space, which, in turn, radically changed the ethical implications of the encounter.³

A man came in around 8 p.m. . . . I'd never seen him before, and he did not seem like the sociable type. He sat at the bar alone drinking, smoking and staring into empty space. I was really nervous, but I really wanted to find out who he was and what he was doing here. As I was cleaning the bar, I casually mentioned that I had never seen him before. He said he did not come here very often because the last time he came people got a "bit funny" with him. I asked what he meant, but he seemed reluctant to tell me about it. He said he used to come here before [the Freelands started to attract gay and lesbian consumers] and when he read about [the bar's new customer policy in a local newspaper] he came in again. He said he was "surprised at how ordinary it was." I felt the ice was breaking and asked if he went anywhere else. He responded bluntly with "I am not gay!" When I told him that I was not gay either, he seemed to relax a lot more. He told me about his job as well as his identity crisis as a bisexual man. [During the next 15 minutes, Mike talked about his past encounters, sexual preferences, his homophobic family, and his views on bisexuality.] Whenever he mentioned his sexual experiences, he lowered his voice and looked around.

Even here, he was so conscious about what he was saying. I felt terrible because he confided in me, and I knew his comments would make it into the diary. (April 02, 2001)

When customers revealed personal or sensitive information it instantly turned relatively unproblematic conversations into more intimate exchanges. Working at the Freelands facilitated this kind of shift in interaction, and it, therefore, became necessary to continually question the context of the encounter and the personal factors that determined when and how I could provide credible explanations of the research. Within the following sections, I examine the issues that affected my relationship with informants and then proceed to illustrate the techniques I used to communicate my research intentions. I maintain that while these techniques contain elements of covertness, it is wrong to suggest that they are unethical.

Negotiating Covertness

The encounters in the Freelands were often ephemeral, which made it difficult to build rapport, but levels of education and specific issues surrounding sexuality and gender were also critical in determining the relationships I formed with informants. It became evident that those with postsecondary school education, especially those who had been to university, were often much more interested in my research.⁴ Others, usually those without degree-level education, tended to pay less attention to my well-rehearsed explanations and appeared less interested in my work. During these encounters, I was forced to either abridge my accounts or abandon explanations altogether and concentrate on developing informal relationships and building rapport.

Recruiting lesbian consumers to participate in the research was also difficult because male clientele outnumbered females, and lesbian patrons were frequently less integrated into other social networks. Lesbian women tended to drink in couples or small groups and mixed less with other male or female customers. Being a heterosexual male meant I knew less about lesbian women; and, because I had fewer opportunities to interact with them, there were fewer opportunities to find out more about them. Approaching couples or small groups of women was often impossible to do casually, and my attempts to engage members of such groups spontaneously were often fruitless. Only one woman refused outright to participate in the study, but seven other women who initially agreed to take part in interviews did not come to the arranged meetings and never contacted me again.

In contrast, heterosexual women were easier to interact with, although recruitment was still problematic. The vast majority of straight women treated the Freelands as a liberating space where they could interact with men without the danger of heterosexual male objectification (cf. Moran, Skeggs, Tyrer, & Corteen, 2001; Skeggs, 1999). They were often very communicative, although on at least three occasions women misunderstood my motivations and assumed that my conversations about research were part of an elaborate mating ritual. Consequently, I began to emphasize the relationship with my girlfriend early in conversations to avoid misunderstandings.

Abrupt and Incremental Disclosure

Each encounter in the Freelands brought with it different opportunities and tensions, but the strategies and tactics I used to negotiate these encounters became increasingly repeated. In short, the communication of my research occurred either abruptly or incrementally. Within the abrupt method, I approached people unexpectedly, introduced myself, and explained my work before asking a series of questions. It was often necessary to use this approach with lesbian women and other infrequent customers because there were fewer opportunities to obtain their opinions. In many cases, especially in the beginning of my research, people saw this as an intrusion into their leisure time and space and were reluctant to participate. Not everyone reacted negatively, but when this was the outcome of an encounter, withdrawal became the only alternative. I often approached people opportunistically either because they were on their own or because they were part of a larger, boisterous, and friendly looking group with three or more members.⁵ However, as I explain in more detail below, in the later stages of the research my relationship with existing patrons helped to legitimize my work, and emphasizing that other patrons had contributed to the study usually made new informants more comfortable about discussing their opinions with me.

Within the incremental approach, I established an informal contact and developed it into a research relationship. I often neglected to highlight my research intentions during initial meetings and then introduced my work casually during subsequent encounters. For example, I purposively talked to others about books and about how my work was progressing in front of Mike for over a year so he could hear us. I used these opportunities to determine whether it would be reasonable to ask him to participate formally in the research. He did not understand, or seem to want to understand, and in general he was apathetic toward these conversations. Despite this, we regularly talked about relationships, holidays, families, and the Freelands. In certain

situations, our relationship seemed more like friendship, although I found it difficult to ask him to participate in a recorded interview. After much deliberation, I eventually told Mike about my work and that I had written about him. I felt I owed him an explanation, and it certainly felt good to be honest with him. He was unsure about it at first, but after I explained my research in more detail he seemed positive about it. He said he would like to read my work although we were skeptical about giving him extracts in case members of his family found them.

The incremental approach was most effective with the regular male clients, gay and straight, and their female acquaintances. Throughout my research, I tried both variations depending on the situation and my courage at the time, although I tended to use the incremental approach. People were slowly eased into the research relationship and were given time to adjust: The interaction was longitudinal, and mutual understanding of our roles and obligations developed over time. For example, most of my 26 key informants continued to share “gossip” and offer suggestions about who I should interview and what I should ask, without solicitation from me; and in return, I reciprocated with small gifts, drinks, and chauffeured people in my car.

The Language of Concealment and Disclosure

The incremental approach usually involved subtle, often covert methods to elicit information from people in the beginning of the relationship. For example, instead of asking informants to comment on specific people, which implied surveillance, I casually encouraged them to look around the room and comment on what they thought about the customers, the decorations, or the venue in general. Observation no longer focused on the individual but shifted, semantically, to the general, and this transformed a potentially alien activity into a mundane form of voyeurism.

This method was useful during brief encounters, but within longer interactions it was often more practical to offer broader explanations about my work. Being more overt allowed me to ask direct questions not only about people’s opinions of the bar and its consumers but also about their perceptions of other venues and their patrons. However, even in my attempts to be more open, elements of concealment were still present. For example, like many other ethnographers, I usually avoided using terms such as *sociology* or *ethnography* in my initial introductions and explanations of my work (cf. Agar, 1996; Pryce, 1986; Shaffir, 1991; Whyte, 1981). I used terms such as *writing* instead of *researching* because I assumed that the latter would imply invasive surveillance and a separation between the researcher and researched (as the object of study). The term *writing* was deliberately meant to inject

ambiguity to help build rapport between potential informants and me. For the same reason, I highlighted the dominance of male perspectives in my study when trying to recruit lesbian informants. I encouraged them to contribute so they can make their narratives a more overt part of the Freeland's legacy. The explanations of the work I gave to lesbian informants deemphasized my interest in the business of hospitality and, instead, stressed my sociological curiosity about their lives and experiences.

The prevailing critiques of covert methods and concealment led me to question whether such partial and incomplete explanations offered reasonable enough clarification of the study for informants. However, the fieldwork made it apparent that esoteric notions such as identity performance, behavioral patterns, or social networks were alien and meaningless to most of the people in the bar. I reshaped the descriptions of my research to suit the audience, and specific interpretations of my work were purposive attempts to create empathetic relationships between informants and me. The claims that I was writing about the places where people drink, or questions about where else they drank, or what their opinion was of another venue were not meant to mislead informants. To another sociologist these statements and questions may seem like distorted interpretations of my work, but these were attempts to present the research in terms informants found meaningful. My informants' understanding of my work was certainly not as intensive or as nuanced as mine, although it is misleading to suggest that they were deceived. I gave elaborate explanations to individuals who were prepared to listen, but I inevitably abridged my accounts to people for whom clarification appeared superfluous.

Visibility and Change in Concealment and Disclosure

Working at the Freeland's allowed me to be increasingly open about my work: Explanations of my studies were gradually woven into casual conversations, which then filtered through to other clients. Existing informants and patrons who knew about my research began to discuss it with others and, similarly to Mann's study of "Brady's bar," news of my research reached many of the regular clientele through "bar talk" (Mann, 1976; Spradley & Mann, 1975). For example, one of my key informants, Dave, found out about my work after talking to the manager. He approached me and asked about my research, and after a brief discussion we arranged to meet for an informal interview. Following this initial meeting, I regularly talked with him in the bar about my work, and I was often able to turn to others present and ask for their contributions. This ability to discuss the research openly became an effective way to make my work and my dual role in the social setting more

visible. For instance, my first interview with Dave was conducted in the Freelands. Three other customers saw us and then amused each other by walking past our table and talking into the tape recorder. After a brief explanation, they joined us and three more people sat down with us within the next hour. The interview proved to be very productive, and the majority of the initial interviews with other informants were subsequently conducted in the bar.⁶ Visibility thus helped make the research seem more broadly accepted and encouraged new people to take an interest.

The research became increasingly overt during the 27 months, and existing informants supported this progressive disclosure. As I noted previously, informants continued to offer information openly throughout the study, and many of the key informants subsequently helped to recruit new participants. However, the research never became completely overt: Some of the patrons remained unaware of the study, while others continued to have only partial knowledge or understanding of it. Consequently, the problems of concealment persistently surfaced throughout the study, and disclosure was always incomplete.

These undisclosed aspects of the study continue to raise moral and professional dilemmas at the publication stage. Feelings of disillusionment or betrayal are common among informants once they encounter textual representations of their lives (cf. Boelen, 1992; Bosk, 2001; Morgan, 1972; Scheper-Hughes, 2000; Vidich & Bensman, 2000; Whyte, 1981).⁷ The problems may be greater here because many patrons were unaware that they would be included in such accounts. Those championing a more inclusive research agenda have argued that such problems can be avoided by persuading informants to take an active part in the writing and publication process (e.g., Denzin, 1997; Guba & Lincoln, 1989; Lincoln, 1995). This approach, however, rests on the assumption that informants are as interested in or committed to the enterprise as the researcher. It was certainly difficult to expect this sort of commitment or interest from most of the informants in this research context. As I have argued throughout this article, many relationships in the Freelands were ephemeral and tenuous, while others such as the one I built with Mike, though longer and more intensive, still made it largely impossible to engage in meaningful dialogue about my research.

To date, only three informants have seen my written conclusions, and only one, Damien, has read them in detail. He spent several weeks reading drafts of my thesis, and his critical comments were an invaluable help. The vast majority of those who contributed openly to the study no longer patronize the Freelands, and I remain hesitant about distributing my work among the people who continue to consume there. My research arises occasionally in conversations with patrons who know of my study but did not participate for-

mally, although I have only pursued the idea of sharing my conclusions with a small number of them. This is partly because the thesis that emerged from the fieldwork is esoteric and written for an academic audience, but principally because it contains a series of candid observations that I fear would place many people in compromising positions.⁸

The conclusions of my research will most likely appear in heavily edited academic articles. I have agreed to give copies of articles emerging from the study to one of my key informants, and I plan to provide copies to two others. They will undoubtedly pass these articles on to others and, in doing so, will help those unfamiliar with the study to gain a broader understand of it; but I also anticipate that many of my future encounters with patrons will be devoted to mediating between them and the representations of their social milieu that come to light. I suspect that some of these will be tense exchanges, but writing and publishing this article before any others will, I hope, create a clear point of reference that informs any potential dialogue.

Conclusion

Conceptualizing field research as an ethical enterprise reflects the maturity of ethnographic inquiry. However, when we attempt to address the ethical implications of our work, our frames of reference draw on a culture of denigration that treats all our untruths as professional misconduct. My fieldwork experiences demonstrate that our inquiry, and the ethical critiques of our inquiry, can never ignore the context in which the research takes place. By understanding the consequences that specific contextual factors have on ethnographic research, we can accept that the problems of concealment are likely to resurface in our inquiries. It is, therefore, essential that researchers and their critics understand why the relationships between ethnographers and informants are entangled with concealed truths.

Within my research, the social context was critical in determining the level and the nature of concealment. The venue was patronized by a diverse range of consumers who often spent short periods in the bar. The discontinuous nature of the community and the transitory encounters with informants meant these relationships were inherently based on partial knowledge of my intentions. Although this may seem unique to this social context, ethnographers will inevitably encounter fragmented communities in which their roles as researchers remain veiled because they have limited opportunities to disclose information about their work. It is a truism to suggest that certain contexts do not lend themselves to simplistic acts of disclosure or consent; however, I illustrated how the context fundamentally influenced the

social relationships, which in turn were critical in determining the overt or covert status of the study.

The Freelands was part of my own personal "leisure geographies," and this consequently eroded the distinction between work and leisure. It was impossible to disentangle those moments when awareness and interpretation was purely sociological from when it was social, and my academic motivations were, therefore, inherently veiled. Furthermore, I illustrated how such social factors as sexuality, gender, and differences in education influenced the nature of my relationships with informants, which in turn were critical in determining how my work and I could be presented. Certain descriptions of my work, those which abandoned references to esoteric academic concepts, were often more meaningful to informants and, therefore, more useful in encouraging them to participate in the research. It is misleading, however, to suggest that abridged reconstructions of my research were unethical. In contrast, these incomplete explanations of my work and me were used to create and nurture social bonds that could be developed into open relationships in which I could be more explicit about my study.

The earlier stages of my research were characterized by covertness regarding my identity. During the later stages of my fieldwork, when I had begun to develop a network of acquaintances and informants who could legitimize my multiple statuses as consumer, employee, and researcher, I was able to disseminate information about my work more overtly. This did not necessarily mean my explanations were more elaborate and therefore more candid: The problems I outlined above continually made it difficult to form relationships with certain patrons, and many were not interested in my accounts. Nevertheless, I purposefully attempted to make my roles more transparent, and the research became more overt. This demonstrates that instead of focusing on any individual act of concealment, it is important to understand how relationships may change and develop throughout the fieldwork.

However, the most important question to emerge from my research is not simply why concealment was evident or how I negotiated these problems: The key question is how we treat the strategies and tactics I used in the fieldwork. The literature on research ethics discouraged me from conducting my study totally covertly. At the same time, the critiques of covert methods and concealment, particularly those that problematized rapport building and maintenance, made all relationships vulnerable to the charge of being exploitative. In the Freelands, every encounter was tainted with the potential to be sociologically fruitful, which made it subject to these critiques of rapport. Within every encounter, however mundane or trivial, I was acutely aware that the people I encountered were not equipped with comprehensive

information about my work or me. There was constant pressure to compensate for this perpetual sense of dishonesty, but despite my efforts to make my research more overt, the fieldwork context made it impossible to eliminate concealment from the study.

The critiques of covert research and concealment did not eliminate these problems, but they did help to nurture a paralyzing state of reflexive self-criticism where every act was open to perpetual debate. I am not encouraging others to engage in subterfuge, and I do not claim that researchers should feel comfortable about misleading their informants. Nor do I suggest that researchers stop engaging in reflexive criticism of their own actions in the field. However, I do urge ethnographers and their critics to look to this and other accounts of concealment in fieldwork and to reassess the appropriateness of existing critiques of concealment and covert methods (cf. Agar, 1996; Leo, 1995, 1996; Shaffir, 1991). If all encounters in the field are treated as inherently unethical, and the researcher's every act is treated as a potential source of ethical crisis, we risk jeopardizing the future of all ethnographic inquiry. Regardless of whether universities or funding organizations restrict covert or semicovert research because of legal or moral reasons, the fact remains that if all concealment is considered universally unethical, any ethnographic research is potentially at risk of being suppressed because the problems surrounding concealment and disclosure I described here are likely to reemerge.

Therefore, the critical questions for ethnographers do not simply concern how they engage with informants but also how they distinguish between those concealments that are necessary or unavoidable in these relationships and those that represent dangerous or irresponsible moral transgressions. The divide between these different kinds of untruths will always be ambiguous, but understanding the realities of fieldwork can help researchers to avoid agonizing over all their duplicities. Furthermore, reflecting on covert methods without treating them as inherently transgressive can help to avoid polarizing researchers and academic review boards or ethics committees in their interpretations of concealment and disclosure.

Notes

1. Stanley Milgram (1974) conducted a series of experiments in which subjects were asked to administer electric shocks to respondents if they answered questions incorrectly. The electric shocks were not real and the respondents were research confederates pretending to feel pain, but the experience caused considerable emotional stress for the subjects. Laud Humphreys studied the behavior of men who engaged in sex with other men in public toilets. Humphreys observed the men and then recorded their car number plates, which he then used to trace their identities. He

subsequently disguised himself and interviewed a number of the men under the pretence that he was conducting a public health survey.

2. The names of all the people and places have been changed to try to maintain anonymity.

3. Denzin (1997) has argued that the distinction between private and public are no longer appropriate and everything that was once imagined to be individual should instead be thought of as “public and part of the local and moral community” (p. 278). He consequently suggested that all knowledge should be considered equally sacred and that representations of people’s lives had to be constructed through open and honest dialogue with informants. I agree with Denzin that the distinction between public and private is inevitably blurred, and also that the knowledge emerging from our interactions with informants should be treated respectfully; but, as this case study demonstrates, the sort of pluralistic dialogue he championed is not always feasible.

4. Secondary school is the mandatory level of education in the United Kingdom.

5. Approaching couples abruptly often resulted in short, awkward interactions and it was usually better to avoid disturbing two people unless they started the conversation with me or showed active willingness to interact.

6. These public interviews were then followed by private interviews with individual informants away from the bar.

7. I appreciate that offending or upsetting individuals are not the only risks surrounding publication. Revealing the illegal activities that took place in the bar may invite police scrutiny, descriptions of people may undermine their wish to keep their sexuality secret, and my portrayal of individual gay, lesbian, or bisexual consumers may serve various political or commercial interests, but the lack of space prevents me from addressing these issues here.

8. When Damien read drafts of the thesis he easily recognized certain characters in spite of my attempts to disguise their identities, and distributing the whole manuscript in such a parochial social context would compromise many of my informants’ anonymity.

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