

## **ETHNOGRAPHY AND ILLICIT DRUG USE: DISPATCHES FROM AN ANTHROPOLOGIST IN THE "FIELD"**

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This article reports on continuing ethnographic research with a social network of young, "recreational", illicit drug users in the inner-city areas of Perth, Western Australia. It describes who they are, how their daily lives are recorded by the researcher (principally through participant observation), and how they react to the research. The means used to establish initial contact with the network and to maintain close social interaction with them for over twelve months are also outlined. The article then discusses some of the issues which this particular ethnographic enterprise raises, seeking to ground the discussion firmly in the reported work while also making some more general observations. The areas covered include the philosophy underpinning ethnography, the related questions of personal morals and ethics, the potential intrusion of personal values into qualitative research findings and some practical comments on ethnographic conduct.

**Keywords:** Illicit drug use; Australia; ethnography; methodology; psychostimulants.

### **INTRODUCTION**

This paper attempts to convey some sense of the nature of ethnography to those in the drug and alcohol field who have little or no prior experience of such an approach or who have ethnographic experience and are interested in discussions of such techniques.<sup>1</sup> After a brief definition of ethnography, the paper moves through four main sections. First, I describe, at some length, the methodology of an ongoing ethnographic study, particularly entering the "field" and maintaining close contact with one's subjects. The second, third and fourth sections all deal with issues raised by this ethnography. The second focuses on philosophy and the question of multidisciplinary research — what we might term the theoretical (or philosophical) issues. The third examines those issues which have personal (or methodological) implications for the ethnographer — ethics, morality and personal values. The fourth focuses on practical considerations — the investment of time, the creation of rapport with subjects and the legal aspects of illicit drug research.<sup>2</sup> Some of these issues are specific to drug and alcohol research, most are relevant to any type of field research. It is difficult to examine more than a few key concepts in a paper and book-length treatments of the topic are common (e.g., Adler and Adler, 1987; Hammersley and Atkinson, 1983; Rabinow, 1977).

As the title suggests, this paper has been written during rather than following ethnographic research into illicit drug use. It therefore reflects my thinking at a certain point in what Spradley (1980) calls "the ethnographic cycle". In such research there is no rigid separation of analysis from data collection but rather a constant feedback

in which interpretations are developed from observed behaviour and then ploughed back into the research process to investigate their explanatory power and to guide the collection of further data. Any theory which arises from the research is thus “grounded” in the collected data (Glaser and Strauss, 1967). Ethnographic analysis is iterative and reflexive, that is, it builds on ideas formulated throughout a study and research problems are transformed, adapted and revised throughout the research process. To paraphrase Willms et al. (1990), ethnography is data-driven whereas quantitative methodologies tend to be construct-driven.

Ethnography requires spending a substantial time period immersed in one’s chosen “field” to record and make sense of a way of life often different to one’s own. The aim is to discover the shared understandings, values and beliefs which inform behaviour by observing and participating in a chosen social context. Participant observation (the main method of ethnography) enables the researcher to take an active role within the group and minimise disruption to the natural flow of social life.

In the drug and alcohol field, ethnography as a research method is still underutilised. This is so despite the important ethnographic contributions made in the last three decades to our understanding of cocaine smuggling (Adler, 1985), the lives of urban heroin addicts (Agar, 1973; Preble and Casey, 1969), the PCP phenomenon in four US cities (Feldman, Agar and Beschner, 1979), the sociolinguistics of drinking (Frake, 1964), qualitative methods for contacting “invisible” populations of illicit drug users (Lambert, 1990), the potential relationship between participant observation and outreach programmes (Power, 1989), drunkenness amongst homeless men (Spradley, 1970) and the world of the “righteous dope fiend” (Sutter, 1966), to name but a few examples.

## AN ONGOING STUDY

The project began in May 1990 when I received a research grant from the Research into Drug Abuse Programme (of the Australian National Campaign Against Drug Abuse). Since then, I have been involved in anthropological research focusing on a social network of young, recreational, illicit drug users. By “recreational”, I mean those individuals for whom drug use is primarily an expressive and leisure-oriented activity but who may, on occasions, experience difficulties of various kinds resulting from, or exacerbated by, their drug use. Employing the term in this way is consistent with the idea that many popular sports and pastimes (e.g., Australian Rules football, cricket, hang-gliding, etc), while being seen as “recreational”, also have the potential for serious injury and even death.

Although the advent of a relatively new “recreational” drug, Ecstasy, provided the initial impetus for the study, it soon became clear that in addition to Ecstasy, many were also using amphetamines (indeed, “speed” was the staple drug), an LSD-like substance called “trips”, and, to a lesser extent, marijuana, cocaine and heroin, as well as the ubiquitous alcohol and tobacco. Although I tried to avoid setting firm parameters on the research, preferring to allow the nature of the “field” to shape the research agenda, uppermost in my mind was a desire to discover and evaluate what anecdotal reports had pointed to — the existence of social mechanisms and controls invented and adapted by drug users to minimise drug-related harm (Zinberg, 1984).

*The sample*

I came into contact with approximately 30 drug-using people who were members of particular, overlapping social networks (and heard accounts of the activities of a score or so more). These drug users were part of larger networks of other drug users as well as of non drug-users. These larger networks numbered in the hundreds. My greatest involvement was with a subset of perhaps twenty young men and women, and within this set, a core of six people, although the identity of the six changed over the period of research. The sorts of activities in which these young people engaged, apart from drug use, included visiting hotels and, more often, nightclubs, watching videos, eating out, visiting friends and, less often, going to parties. To a lesser extent some were, or had been, involved in various levels of drug-dealing.

There were few obvious superficial concessions I had to make to assimilate myself to this network of drug users. They wore a wide range of clothing which mirrored their widely divergent opinions about such things as music, favourite nightclubs and current social issues. Sociodemographically, they came from widely divergent backgrounds — from various social classes (if we define these categories by parents' occupation), residing and/or working in various suburbs (but with a concentration within a 10km radius of the city centre) and almost all were of Australian birth to Anglo-Celtic parents. Their ages varied from the late-teens to early thirties but with a clustering in the eighteen to twenty-two years age range. Some had left secondary school at Years 10 and 11; others had completed secondary school and found work; others still were enrolled in post-secondary vocational or educational courses. Some were unemployed and lived off a combination of unemployment benefits and the profits from intermittent drug dealing (although much of the profit was ploughed back into their own drug use). Employment ranged from positions in the trades and service industries (e.g., the hospitality or retail industry) to semi-professional and professional status. Most were exclusively heterosexual, with a few being either exclusively gay or heterosexual but with experience of gay encounters.

Within the set of thirty people, only two had ever received treatment or hospitalisation for their drug use. Most were injecting drug users most of the time, but occasionally reverted to other modes of ingestion such as oral consumption of Ecstasy and "trips". Several had been using illicit drugs for over a decade and some for between two and five years. For others the consumption of illicit drugs was a relatively recent activity.

*Collecting data*

The research reported here was conducted over a twelve-month period during 1990 and 1991. The data comprise information of three types: (a) participant observation, (b) stories collected during participant observation or in unstructured casual interviews, and (c) tape-recorded interviews. The usual method of recording information was through tape-recorded or typed field notes which were made as soon as possible after arriving home from a period of fieldwork (often at night). Evening fieldwork was most difficult because of the length of time involved, beginning somewhere between 7.30 and 11.30p.m. and ending anywhere between 4 and 9a.m. After such marathon sessions, I would make a page or two of "condensed notes" (Spradley, 1980) — phrases or words which re-evoked segments of social action. The next day (or occasionally within a day or two depending on how hectic things were), I would write an expanded account of the

event with the aid of the condensed notes. I relied largely on memory for recording events and conversations. Interwoven with the “facts” were more personal observations. I also tape-recorded interviews with several key field contacts on a number of occasions to elicit biographical data or to explore potentially interesting themes raised in group conversation.

### *Gaining and maintaining access*

In anticipation of receiving a research grant, I had been in regular contact with a young woman whom I will call Jessie, a friend of a university colleague. Jessie was at this stage a peripheral member of an illicit drug-using network and, through our mutual friend, had offered her assistance to me. I met her on a number of occasions, spelling out what the study would consist of, why I wanted to conduct it and who was funding it. She, in turn, relayed this information to her set of friends and associates. She played the part of “cultural broker” and vouched for my authenticity and legitimacy (at this early stage something I gained largely through my association with our common friend, my university colleague). She considered herself marginal to the core of this particular drug-using set but volunteered to introduce me to them and then, after a settling-in period, leave me to my own devices. She did not particularly want to increase her involvement with this group and possibly increase her own drug use. She was happy being at the periphery with easy access to drugs, but with the option to limit her contact.

At these meetings she was also able to enlighten me about many facets of the drug scene in which she moved. This informal fieldwork highlighted those aspects of this particular social world which she considered important and provided me with considerable background on the personalities and events of recent months. When I began fieldwork several months later I wasn't plunged into situations and a network of relationships which I knew nothing about. With official project approval, my proper involvement began.

Most of the individuals comprising this network had prior knowledge of “this guy who wants to watch us use (drugs)”. In hindsight, this prior knowledge may have been a hindrance. It meant that people were free to create an image of me and of the prospective research into which I had no input (for example, Jessie may have accidentally provided inaccurate information). While this approach was ethically watertight, it meant having to overcome these created images. For example, Lisa (a member of the drug-using network) told me several months after I had started the research that she had thought prior to meeting me, and had told a number of friends, that I would provide her with drugs and then watch her reactions. I also had to overcome the perceived negative connotations implied in the name of my institutional employer, the National Centre for Research into the Prevention of Drug Abuse. Many assumed me to be “anti-drug” and I had to explain, on several occasions, my adherence to a harm reduction paradigm.

During the early part of the research, I stuck close to Jessie. I would phone or call by at her house to find out what was happening. As time wore on, and I had an opportunity to assess the internal dynamics of the network, I consciously and slowly transferred my primary identification away from her and to a couple of the others. I did not want to be too closely identified with any single person. I began to associate more with a number of others in addition to her (and interestingly, she consequently

increased her involvement with this group contrary to her original intention to remain peripheral in order to maintain her own low level of drug use).

It appears difficult to ever fully avoid being identified with particular individuals and sets — it is a consequence of taking a social role within a group. What the ethnographer must do though is attempt to minimise the impact of this identification. For example, one male with whom I was closely identified held ambivalent views about homosexuality. His views were known to a number of bisexual and homosexual men in this network who regarded him as intolerant and “homophobic”. Whenever the opportunity presented itself, I tried to be sociable with these men, discuss issues openly and generally communicate to them that I did not share the views of this man. In this way, I was not stigmatised as “anti-gay” and in the process of visiting scenes where he did not feel comfortable I was able to reinforce contacts with other gay (or bisexual) people within the network.

Naturally, I received many queries about the research (and not all about confidentiality issues). People thought the topic intrinsically interesting and me fortunate to hold such employment. However, the lack of a legal basis for guaranteeing confidentiality meant that, certainly in the initial stages of fieldwork, I was regarded as “suspect” for discussion of certain issues, e.g., the identity of the next person up the dealing pyramid.<sup>3</sup> It was not that they did not trust me personally nor that they doubted the sincerity of the research, merely an accurate and realistic appraisal that without such a legal basis to confidentiality, I could never absolutely guarantee their safety. Protestations on my behalf about a willingness to defy the law to protect anonymity did not gel with cultural notions about typical reactions to police interrogation. The widely-held view was that anybody arrested or even questioned by police would gladly report the illegal activities of even close friends (apparently “dob in five mates” was the going rate for dismissal of charges). In their eyes, there were no grounds for believing me to be any more trustworthy than anyone else.

In addition to these queries, I also had many discussions with individuals and groups about the research. The discussions were less guarded (particularly with respect to comments about other people) if I was speaking with only one other person. I often tested out ideas, sometimes to be met with ridicule; other times there was agreement. For example, on one occasion, I explained to a couple that the US literature suggested that Ecstasy was not so much a sexual drug as a sensual one, and asked for their opinion. They burst out laughing and then proceeded to recount several stories which left me in no doubt that, in their view at least, Ecstasy was considered to be a *very* sexual drug.

## ISSUES OF ETHNOGRAPHIC THEORY OR PHILOSOPHY

### *Objective fact or interpretation?*

One influential paradigm within anthropology and sociology is the “social construction of meaning” (Berger and Luckmann, 1966). That is, “reality” is conceived of as a constant and ongoing negotiation between individuals and groups whereby social meanings are produced, transformed and abandoned. The task of the anthropologist is to describe and analyse the representations that individuals and groups make about these processes, which can only ever be partial perspectives held individually or collectively about the “objective world”. As Rabinow (1977:150) states:

Culture is interpretation. The ‘facts’ of anthropology, the material which the anthropologist has gone to the field to find, are already themselves interpretations.

The baseline data is already culturally mediated by the people whose culture we, as anthropologists, have come to explore.

From this perspective, in contrast to many of the premises underlying quantitative methodologies, one cannot speak of “empirical, objective facts”. As Rabinow (1977:150) further notes, “fact” has its etymological roots in the Latin word *factum*, which means “made”. (One might also note in passing that the word “fiction” derives from a Latin word meaning to “create”. Etymologically then, there appears to be less difference between “fact” and “fiction” than we might like to think.) With this basic principle accepted, Geertz (1975:5) charts a course for a particular type of anthropological investigation:

Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning.

Again, as Rabinow (1977:152) writes:

This (the ‘webs of signification’ Geertz writes of) is the ground of anthropology; there is no privileged position, no absolute perspective, and no valid way to eliminate consciousness from our activities or those of others. This central fact can be avoided by pretending it does not exist. We can pretend that we are neutral scientists collecting unambiguous data and that the people we are studying are living amidst various forces of which they have no clue and to which only we have the key. But it is only pretense.

To allow such close dissection of social representations and to discover how they contribute to the construction of social meaning, ethnographic studies are usually designed to focus on small samples for long periods in “natural” social settings. With respect to the drug and alcohol field, ethnographers might throw new light on some important questions: What is “addiction”? What is the nature of “dependence” outside the clinic? What are the social processes involved in the genesis, maintenance and cessation of drug use? How do drug users themselves define drug “problems”? What is “recreational drug use”? and Why do people use drugs “recreationally”? Alternatively, detailed ethnography may pose a set of new questions.

### *State of mind*

There is also a different attitude involved in ethnography (and I am purposefully exaggerating this difference to make my point). Unlike a research structure in which research assistants, often graduate students working on theses, conduct “data collection” using “objective instruments” designed by more qualified senior staff, ethnography entails a change in one’s “state of mind”, particularly in research involving populations sharing sociodemographic, cultural and geographical characteristics with the researcher (Helms, 1986).

Okely (1984) has also noted the “double vision” effect in fieldwork with populations sharing characteristics with ethnographers. This term refers to the process of learning new meanings and significance for previously familiar places and situations when fieldwork takes place in one’s own locality. Likewise, Davis (1984) comments that it

is common in such fieldwork to experience an event and react to it in two different ways simultaneously — as a researcher and as a member of the fieldwork group. Thus conducting ethnography under these conditions entails “a change of headspace” (Helms, 1986:7), altered modes of personal presentation and a preparedness to interact according to different cultural rules. For me, this double vision was experienced often in the staff meetings of my employer, the National Centre for Research into the Prevention of Drug Abuse. The dominant discourse at such events was of drug-related “problems” and “harm” and how to reduce their incidence. By contrast, my head would be filled with the conversations of drug users noted the previous evening(s) — talk of “big nights” and “speeding” (under the influence of amphetamines); in other words, talk about the perceived benefits of drug use.

### *The importance of small worlds*

A line in Raban’s (1974) *Soft City* seems a good departure point for a discussion about the implications of ethnographic philosophies for health promotion and the reduction of drug-related harm (with apologies for its inherent sexism): “Each man’s city is the sum of all the routes he takes through it.” Raban’s sentence seems to suggest that ultimately the unit of investigation, at least in studying urban life and in applying such findings to interventions, should be the individual. Such specificity is of course impossible (for financial as much as theoretical reasons).

The response of those in public health has been to recognise the implausibility of solely individually-based health promotions and to proceed to the general community level, often with implicit messages about what middle class values would suggest is appropriate and responsible behaviour. But they have missed an intermediate level, that of social contexts, within which all individuals interact and exist. (One could also argue that the “general community” level is a convenient fiction which is made up of various overlapping, competing, collapsing, interacting, evolving and merging “small worlds”).

Ethnography redirects attention to this intermediate level and may be used to inform locally-based, inexpensive health promotions. Unlike public health and its research arm, epidemiology, ethnography provides very detailed, rich material about actors within specific social contexts. To say this is not, of course, to suggest that mass promotion techniques are necessarily redundant. Smaller-scale measures are not intended to replace such initiatives but to complement them. It means mediating the monolithic messages of such broad-based campaigns and translating them into messages (and media) more appropriate to the numerous and overlapping “social worlds” which make up modern pluralistic societies.

### *Multidisciplinary research: possibility or illusion?*

There is increasing acceptance of ethnographic research in the drug and alcohol field within Australia. However, in the eyes of some, ethnography remains a supplement to the “real science” of “objective measurement”. Ethnographic research is considered particularly appropriate to the exploratory study of subjects about which little is known, with its ability to produce hypotheses which might later be tested by “science”. It is still comparatively rare to hear ethnographic research championed in its own right (for a US exception, see Willms et al, 1990). When criticised for holding such a position,

I am informed that drug and alcohol research is now “multidisciplinary” or “interdisciplinary” in its approach. What this usually means is that ethnography is hastily incorporated into an established positivist research paradigm and subjected to criteria which it was never designed to meet and which have received criticism from scholars of research philosophy and method (e.g., Foucault, 1973; Gusfield, 1981; Kuhn, 1970). As long as ethnographers satisfy questions of sample size, representativeness and observer bias, their work is deemed to be valid. Although such positivism has many guises and its proponents adhere to its tenets in differing degrees, the fundamental principles are considered sacrosanct. Can there be true interdisciplinarity when there are different basic notions of what is, and is not, philosophically and theoretically sound? Barthes (Clifford, 1986:1) notes this trend but fails to provide more than a cryptic clue to a future path:

Interdisciplinary work, so much discussed these days, is not about confronting already constituted disciplines (none of which, in fact, is willing to let itself go). To do something interdisciplinary it's not enough to choose a 'subject' (a theme) and gather round it two or three sciences. Interdisciplinarity consists in creating a new object that belongs to no one.

Outside the drug and alcohol area, in social science in general, attempts at making ethnography more widely applicable revolve around the fusion of micro-(cultural) and macro-(structural) analyses (e.g., Marcus, 1986; Willis, 1978) rather than attempts to yield more “rigorous” or “objective” accounts. This of course is not to say that ethnographers are not concerned with issues of theory and methodology (note the vast literature on this subject, see for example Agar (1980), Becker (1958), Cesara (1982), Ellen (1984), Fetterman (1989), Hammersley and Atkinson (1983), Rabinow (1977), Schatzman and Strauss (1973) and Van Maanen (1988)) but that their theories and methodologies pose considerations and concerns of other kinds.

## PERSONAL ETHNOGRAPHIC ISSUES

### *Research integrity or deception?*

There is a general question concerning the ethics of illicit drug ethnography: What does one do with data in relation to law enforcement? Do the police have a right to ethnographic information about trends in drug use that might aid them in arresting people? My answer to this question, from an ethnographer's perspective, would be in the negative. The ultimate responsibility has always got to be to one's research subjects, unless in extremely serious circumstances in which one would probably feel compelled to act as a citizen rather than as a researcher. This issue is noted by Manderson (1990) in her recent paper — not only should researchers be concerned to protect specific individuals from the consequences of their study, but also research populations. Providing information to police may also violate notions of informed consent. If a researcher is intending to advise police in the future about more efficient policing tactics, based on the research he or she is about to conduct, “informed consent” means he or she is bound to inform potential research subjects of this planned future use. I find it hard to believe that many illicit drug users would actively participate in research if so advised. This doubt leads me to question whether or not a study like that of Dobinson and

Polizetti (1989) would be possible if the participating heroin dealers and users were told that the information received would contribute, *inter alia*, to improved methods of apprehension. Did these researchers inform their research subjects that this might be the case?

Ethically, ethnographers must truthfully and fully inform their subjects of the confidentiality measures which will be taken (and whether legal protection is available or not), the ramifications of such research and the overall thrust of the research (i.e., that it is primarily concerned with drug use) despite the repercussions for methodology that such disclosures may entail. Ethical breaches concerning informed consent have been consciously made in the past and will no doubt be made in the future. While such breaches may be “justified” on methodological grounds, they are ethically unjustifiable and seem to reflect a common, paternalistic attitude: because the research subjects are “drug users” and therefore deserve to be “helped”, such unethical behaviour is acceptable. As both Manderson (1990) and Power (1989) observe, the interests of research subjects should always be put before the interests of science. The ethical guidelines concerning research with Australian Aboriginal communities have been much tightened and policed in recent years and it might be beneficial to transfer some of these principles to other stigmatised groups such as drug users.

### *Morality*

Up to this point, I have purposefully excluded the category of moral issues although I recognise the centrality of such considerations in ethnographic fieldwork. As Soloway and Walters (1977:176) state of their work, “It is neither within the scope of this paper nor proper for us to discuss morality, for that is, and must always be, a researcher’s private affair”. There is, however, one area of morality relevant to illicit drug use research which also has implications for methodology and should therefore be discussed. The question of whether or not a researcher should consume illicit drugs during the course of ethnography has no clear answer. It has to be for each researcher to decide on which side of the line he or she falls. What potential ethnographers need to know are the possible ramifications of their decision.

Much depends on the nature of the social group chosen for study. Failure to consume drugs will not necessarily mean ostracism in any group, but this unfortunate outcome might be more likely in one which has heavy drug use as its sole focus. Researchers who choose not to consume drugs will have to remain vigilant as to any effects this might have on the people with whom they move. For example, Jessie said to me, after a few weeks in which she, Lisa and Michael had been using drugs often in the privacy of Michael’s house, that she felt very conscious of my “straight” (non-drug) mood and style of interaction. Although she did not wish to stop me coming to the house, she expressed her misgivings about having someone who was not “speeding” (or “tripping” (LSD) or “Ecking” (Ecstasy)) disrupting the mood of the others. In earlier periods of field research, the group’s activities had been much more externally oriented — nightclubbing and pubbing — where there were other non-drug users with whom drug users had to interact. In these circumstances, the question of me disrupting their mood had not arisen.

Alternatively, the ethnographer may feel that he or she can gain the trust of subjects more quickly, or understand the drug experience better, if the common bond of illicit drug use is forged. This course of action is potentially dangerous as it leaves the

ethnographer in a compromised position. Others in the group may press for greater involvement in illegal activity or may use the knowledge of one's drug consumption as a bargaining point with police if arrested themselves.

There is also the moral question of constantly seeing people buying and then injecting drugs — in other words, engaging in illegal activity. The ethnographer possesses this information and could, at any time, make it available to police. Personally, I do not believe that individuals should be considered “criminals” for choosing to alter their state of mind. Humans do so in a number of ways, some of which are officially condoned and others not. For me, seeing this set of people break the law in this particular manner presents few problems of conscience. I feel my lack of concern is also partly justified by the largely “victimless” nature of the “crime” (Schur, 1965).

### *The intrusion of personal values*

There is an additional methodological question arising from the statement made in the last section which begs discussion. Does not my stated liberalism with respect to illicit drug use and the law inevitably corrupt the objectivity of my account? First of all, I do not think that there can be such a thing as an “objective” account (something I dealt with earlier). Second, the anthropological or ethnographic approach is fundamentally concerned with describing and understanding the “emic” (that is, the insider) view of the world. A central method for getting at such an understanding is through close attention to and recording of what research subjects *say* about their activities, both in interview situations with the researcher and in typical social settings while interacting with other social actors. These social representations can then be contrasted with *observed* behaviour as the ethnographer goes about his or her participant observation. Should there be large discrepancies between the two (and there are inevitably some differences), the task of the ethnographer is then to record how social actors account for such differences. By constantly paying attention to what people say about what they do, and what they say about inconsistencies between what they say and what they do, the ethnographer slowly builds a picture which bears some relation (however partial) to the culture under examination. And in this way, he or she at least minimises, if not totally counteracts, the problem of value clashes between the ethnographer and the subject. Of course, to take the discussion one step further, once a cultural sketch is complete, the practical use one puts such a sketch to is entirely a matter of value, particularly in policy-oriented research areas such as drug and alcohol studies.

An example of a drug and alcohol study in which the (albeit inexperienced) qualitative researchers were negligent in guarding against the intrusion of their own values into the data collection is provided by Homel, Tomsen and Thommeny (1992). Let me say first that their paper is a worthy addition to an area in which little observational research has been done, particularly in Australia. In what must be regarded as a positive trend in studies of alcohol and drug-related harm, researchers are slowly coming to recognise that field studies of the social settings in which drug use takes place are sorely needed. While the Homel et al study has a quantitative component in the investigation of police records, it is to the qualitative element that my comments are directed.

The crux of their paper lies in the identification of factors which contribute to the generation of violence on licensed premises. The authors single out the following variables: patron type, a boring atmosphere with low-quality bands, lack of comfort (defined in terms of crowding, loudness, lack of seating and ventilation), high levels

of drunkenness, and aggressive bouncers. When I read this section I was reminded of a comment on participant observation by Liebow in *Tally's Corner* (1967), a book about Black streetcorner men in Washington D.C. Liebow (1967: 254-55) wrote that:

One Saturday night, with my observer role clearly in mind, I went to a dance at the Capitol Arena where more than a thousand people were jammed in together. I was the only white male, this was my first time at such an event, the music was so foreign to me that I picked out the wrong beat, and I was unable to identify several of the band instruments. I was, willy-nilly, an observer. But here are a few lines excerpted from the field observation:

It was very hot, it was very noisy, it was very smelly, and it was all very exciting. It was impossible to remain simply an observer in a place like this, even for someone as phlegmatic as I. It was only a few minutes after Jackie Wilson started singing that I discovered that the noise wasn't nearly loud enough, the heat wasn't nearly hot enough, and the odour from more than a thousand closely packed people was not really strong enough at all. Like everyone else, I wanted more of everything.

What Liebow portrays in this insightful passage is the very fine, but nevertheless significant, difference between participating in a social context as a group member and passively observing the same context from a vantage point distanced from social interaction. He effectively conveys the value of achieving an "insider" point of view. It is only through attention to the perceptions of participants in such settings and the ways in which such perceptions shape behaviour, that the social meaning of such activity may be understood and the researcher may minimise the biased effect of personal values on results.

Had Homel et al observed the Capitol Arena on the night Liebow attended, they might have drawn the conclusion that any violence they may have witnessed was due, at least in part, to overcrowding, or discomfort in the form of excessive heat, loud noise or unpleasant odours. To Liebow, and his fellow Black participants, these variables would appear to be interpreted in a vastly different way. How can the researchers be sure that, without asking patrons their opinions or experiencing the venue from an insider's perspective, such features as overcrowding or excessive heat are perceived as negative features of such settings? In addition, what constitutes a "boring atmosphere" or a "low-quality band" appears to lie very much in the eyes and ears of the beholder. If researchers are to attempt qualitative methodologies, they must first ensure that they are familiar with such methodology. One excellent example of a study in which the author was careful to record the insider perceptions is provided by Burns (1980) when he describes a series of drinking sprees from the perspective of the young men involved.

## ISSUES OF ETHNOGRAPHIC PRACTICE

### *The investment of time*

A major practical consideration concerning ethnographic endeavours is the amount of time consumed, particularly at the beginning of research. Attempting to minimise the disruption to the natural flow of social life usually entails spending long periods with research subjects apparently doing little in the way of concrete data collection.

Agar (1973), in his book on heroin addicts, comments that he had to convince clinical colleagues within the institution in which he conducted his research that sitting around at lunchtimes with institutional inmates was an essential part of his research methodology, in establishing rapport with his subjects. His lament strikes a resonant chord for those working in so-called "multidisciplinary" settings.

Difficulties directly resulting from this time factor include maintaining non-ethnographic relationships (e.g., with partners or close friends) while heavily involved in the "field" and convincing research supervisors of the methodological necessity of such heavy investment of time. One's immersion in the ethnographic field is very deep and, in some cases, total (Adler and Adler 1987). This is normal ethnographic practice and one of the reasons why doing anything other than intermittent ethnography becomes increasingly difficult as one grows older with the (usually) attendant responsibilities (such as a permanent partner, having children, heavy teaching commitments if one is an academic, and so on).

It is also difficult, with such heavy involvement in field research, to remain a conventional full-time member of a research centre — day-time discussions of drug problems and night-time ethnographic observation do not integrate comfortably. My experience has been of a drastic alteration of sleeping patterns because of the intense nightclubbing activity (usually going to bed anywhere between 2 and 8 a.m. and rising anywhere between 10 a.m. and 4 p.m.), of giving people lifts around town, of lending money, of waiting patiently by the phone for a call arranging the night's activities (sometimes the call did not come) and generally participating in activities not always related to drug use. There is no other way; to become a full member of a social network which prizes sociability, one has to give oneself socially to the people within it.

### *Creating rapport*

A second practical consideration for ethnographers is that they must be willing to "get their hands dirty" and be able to "fit in". For me, this meant occasionally participating in activity frowned upon in health education circles in order to prove that I was not, in the eyes of the heavy-drinking subjects, a "wowser" (I was known by one member of the network, a bar attendant, as "Mr Soda Water" for my practice of following each alcoholic drink with a soft drink).

Several weeks into the research, I found myself accepting a challenge from a key informant, MC, to drink a number of "shooters" (liqueurs served in small, ornate glasses).<sup>4</sup> I had already had a couple of drinks at a nightclub we had visited earlier and I suspected that MC would be "speeding" which meant that consumption of alcohol would hardly affect his perceived sobriety. Still, I accepted his challenge (watched by several onlookers from the social network) and we drank four "shooters" (two of which were twice the normal size) in rapid succession. About half an hour later, the full weight of the alcohol hit me. Luckily, Jessie was leaving the nightclub and offered to give me a lift home. On arriving home, I stumbled into bed and spent a fitful night accompanied by vivid, unpleasant dreams. The following night, having recovered from the morning's minor hangover, I again visited the same nightclub. As I entered, several people including two staff members commented favourably on my previous night's performance and by the time a day or two had passed, several more had also mentioned the incident to me in conversation.

Whatever its health consequences, my drinking exploit brought me closer to the centre of social action. I became, for a night, a participant rather than an observer and the

greater level of acceptance these actions created were important in adding impetus to the process of establishing rapport between myself and the subjects.

### *Witnessing illegal acts*

For researchers who access illicit drug users through snowballing techniques,<sup>5</sup> pre-arranged interviews or community surveys, their subjects remain anonymous. Unlike the typical ethnographer, these types of researchers do not witness the conduct of illegal acts — buying/selling transactions and the use of illicit drugs. They did not visit people's homes; they rarely take down phone numbers nor socialise with people regularly engaging in illegal activities. In other words, their data is unlikely to contain any specific information about particular individuals or particular incidents which might identify them. By contrast, ethnographers possess intimate personal knowledge about individuals and their activities that would be damaging if forcibly released by court demand.

Aside from recourse to any existing legal protection for the preservation of research data confidentiality which might be available, one possible way of defusing any misunderstandings with police is to inform local authorities before commencing a project. I chose not to follow this course. I was concerned that such information would allow police officers (if they deemed fit) to use me as a marker for identifying groups of illicit drug users. Law enforcers and prevention researchers supposedly share a common aim — the reduction of drug-related harm (the degree to which the two share these aims is I think debatable). The ethnographic research role, in which one elicits confidential information from individuals regarding their illegal drug use (and witnesses these actions) amidst a context of trust and rapport, is incompatible with the role of law enforcement officers going about their customary duties.

Researchers contemplating ethnographic research in the illicit drug area should consider carefully such matters. Because their research activities inevitably mean bearing witness to, and being physically present during, the conduct of illegal behaviour, there is a chance, however unlikely, that contact will be made with law enforcement personnel. Legal defense may be necessary and legal advice should be sought and written assurances gained from employers, university or otherwise, prior to the research, to the effect that legal defense will be provided in the event of wrongful arrest. The ethnographer may also be subject to surveillance and/or phone-tapping, even if indirectly through the dialling of another phone number which is tapped. Where legal protection for research confidentiality is not available, the ethnographer has an obligation to inform subjects of this state of affairs. For some sociodemographic groups, assurances that appropriate steps will be taken to ensure confidentiality (code numbers, password-protected computer disks, etc, but not legal measures) will be sufficient. For others, perhaps those involved in drug-dealing or other illegal activities carrying heavy penalties, such assurances may not instil sufficient confidence to participate in the research process.

### CONCLUDING REMARKS

In almost every aspect, ethnography differs greatly from those research methodologies conventionally used in the drug and alcohol field. Philosophically, it progresses from a relativist stance redirecting our attention to the idea that our "objective" world is of our own making; it is socially constructed. The task then becomes to record and interpret the processes by which we, as individuals and as members of social

collectivities, go about these social constructions — What purposes do they serve? How do they compare to similar or different social groups? and What do they mean for these individuals and groups? One unanswered question is: Given the radical differences between the philosophical stances of ethnography and positivism, how do we proceed towards interdisciplinarity?

Methodologically (and this is guided by philosophy and theory and, in turn, develops philosophy and theory), sample sizes tend to be smaller, research periods longer, relationships with field contacts more intense and social interaction between researcher and researched heightened. The issue is also an ethical one — we have no inalienable right to impose upon drug users for their help. If we are to expect people to tell us about their illegal drug use activities then the least we can do is to guarantee that this information will never be used against them, including to improve future law enforcement strategies. There are a whole host of further questions which relate to the actualities of fieldwork. It is difficult to prescribe any rules for conduct in such an area. Experience and trial-and-error are invaluable aids to improving ethnographic research skills. One may prepare for the range of issues and possible scenarios that might eventuate but ultimately the ethnographer is on his or her own.

I hope then that I have not dampened the enthusiasm of any would-be ethnographers keen to embark on research into drug and alcohol issues. Ethnography is fraught with difficulties. It is demanding, emotionally-draining, sometimes dangerous and always time-consuming but the rewards are many for those willing to accept the challenge.

## ACKNOWLEDGEMENTS

The field research on which this paper is based was supported by a grant from the Research into Drug Abuse Programme of the National Campaign Against Drug Abuse. I bear sole responsibility for the final version of this paper but I would like to acknowledge the tolerance and patience of those drug users with whom I spent many hours, and, particularly, the invaluable input and support of Lisa, Jane, Jessie, Mark, MC, Michael and Vinnie. Thanks are also due to Jodie Brown, David Hawks, Alison Marsh, Bill Saunders, Tim Stockwell, Mariella Vallesi and the two anonymous referees for *Addiction Research* for comments on earlier versions of this paper.

## Footnotes

1 The terms 'ethnography', 'fieldwork' and 'participant observation' are used interchangeably in this paper. Although 'participant observation' (or 'fieldwork') is not usually thought of as the sole feature of ethnography (other features include analyses of personal diaries, letters and interviews), for me it is the defining activity.

2 I recognise that these categories are somewhat arbitrary and simplistic and there is much overlap between them.

3 There was also a misunderstanding between the funding body and me concerning the possibility of such legal coverage. Initially, it was thought this legal protection would be granted under the Epidemiological Studies (Confidentiality) Act 1981 — which would guarantee confidentiality for both researcher and data — and this point was conveyed by me to the field contacts. However, later, I was informed that this coverage would not be granted which led to some understandable dissatisfaction on the part of the research subjects.

4 "Shooter" glasses are usually 20mls in size.

5 In snowballing, key informants in specific social networks are first identified and then used to contact other drug users known to them. These second stage informants may then be asked to place researchers in touch with their friends and contacts, comprising a third wave of subjects. In this way, the sample size 'snowballs' as the research progresses and more people are recruited. See, for example, Biernacki and Waldorf (1981).

## References

- Adler, P.A. (1985) *Wheeling and Dealing*. New York: Columbia University Press.  
 Adler, P.A. and Adler, P. (1987) *Membership Roles in Field Research*. Newbury Park: Sage Publications.

- Agar, M. (1973) *Ripping and Running: A Formal Ethnography of Urban Heroin Addicts*. New York: Seminar Press.
- Agar, M. (1980) *The Professional Stranger: An Informal Introduction to Ethnography*. New York: Academic Press.
- Becker, H. (1958) Problems of inference and proof in participant observation. *American Sociological Review*, **23**, 652-660.
- Berger, P. and Luckmann, T. (1966) *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*. New York: Doubleday.
- Biernacki, P. and Waldorf, D. (1981) Snowball sampling: problems and techniques of chain referral sampling. *Sociological Methods and Research*, **10**, 141-163.
- Burns, T.F. (1980) Getting rowdy with the boys. *Journal of Drug Issues*, **10**, 273-286.
- Cesara, M. (1982) *Reflections of a Woman Anthropologist: No Hiding Place*. London: Academic Press.
- Clifford, J. (1986) Introduction: partial truths. In: Clifford, J. and Marcus, G., Editors. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press, 1-26.
- Davis, J. (1984) Fieldwork. *Royal Anthropological Institute News*, **61**, 2-3.
- Dobinson, I. and Polizetti, P. (1989) *Buying and Selling Heroin: A Study of Heroin Users/Dealers*. Sydney: New South Wales Bureau of Crime Statistics and Research.
- Feldman, H.W., Agar, M. and Beschner, G.M. (1979) Editors. *Angel Dust: An Ethnographic Study of PCP Users*. Lexington: D.C. Heath.
- Fetterman, D.M. (1989) *Ethnography: Step by Step*. Newbury Park: Sage.
- Foucault, M. (1973) *The Order of Things: An Archaeology of the Human Sciences*. New York: Vintage Press.
- Frake, C.O. (1964) How to ask for a drink in Subanon. *American Anthropologist*, **66**, 127-132.
- Geertz, C. (1975) *The Interpretation of Cultures*. London: Hutchinson.
- Glaser, B.G. and Strauss, A.L. (1967) *The Discovery of Grounded Theory*. Chicago: Aldine.
- Gusfield, J. (1981) *The Culture of Public Problems: Drink-Driving and the Symbolic Order*. Chicago: University of Chicago Press.
- Hammersley, M. and Atkinson, P. (1983) *Ethnography: Principles and Practice*. London: Tavistock.
- Helms, M.P. (1986) 'Keeping it together': the social processes of community among young urban nomads living in shared houses in Perth, Western Australia. M.A. Thesis: Department of Anthropology, University of Western Australia.
- Homel, R., Tomsen, S. and Thommeny, J. (1992) Public drinking and violence: not just an alcohol problem. *Journal of Drug Issues* (forthcoming).
- Kuhn, T.S. (1970) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Lambert, M.S. (1990) Editor. *The Collection and Interpretation of Data from Hidden Populations*. NIDA Research Monograph 98 Rockville: National Institute on Drug Abuse.
- Liebow, E. (1967) *Tally's Corner: A Study of Negro Streetcorner Men*. Boston: Little, Brown and Company.
- Manderson, L. (1990) Social science and AIDS in the community. Paper presented to the 4th National Conference on AIDS, Canberra, 8-11th August.
- Marcus, G. (1986) Contemporary problems of ethnography in the modern world. In: Clifford, J. and Marcus, G. Editors. *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press, 165-193.
- Okely, J. (1984) Fieldwork in the Home Counties. *Royal Anthropological Institute News*, **61**, 4-6.
- Power, R. (1989) Participant observation and its place in the study of illicit drug abuse. *British Journal of Addiction*, **84**, 43-52.
- Preble, E. and Casey, J. (1969) 'Taking care of business': the heroin user's life on the street. *International Journal of the Addictions*, **4**, 1-24.
- Raban, J. (1974) *Soft City*. London: Collins Harvil.
- Rabinow, P. (1977) *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.
- Schatzman, L. and Strauss, A. (1973) *Field Research: Strategies for a Natural Sociology*. New Jersey: Prentice Hall.
- Schur, E. (1965) *Crimes Without Victims*. New Jersey: Prentice Hall.
- Soloway, I. and Walters, J. (1977) Workin' the corner: the ethics and legality of ethnographic fieldwork among active heroin addicts. In: Weppner, R.S. Editor. *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings*. Beverly Hills: Sage Publications, 159-178.
- Spradley, J.P. (1970) *You Owe Yourself a Drunk: An Ethnography of Urban Nomads*. Boston: Little, Brown and Company.
- Spradley, J.P. (1980) *Participant Observation*. New York: Holt, Rinehart and Winston.
- Sutter, A. (1966) The world of the righteous dope fiend. *Issues in Criminology*, **2**, 177-222.
- Taussig, M. (1980) *The Devil and Commodity Fetishism in South America*. Chapel Hill: University of North Carolina Press.
- Van Maanen, J. (1988) *Tales of the Field: On Writing Ethnography*. Chicago: University of Chicago Press.