National Institute on Drug Abuse

Research 98

MONOGRAPH SERIES

The Collection and Interpretation)ata n Hidden ulations

U S. DEPARTMENT OF HEALTH AND HUMAN SERVICES • Public Health Service • Alcohol, Drug Abuse, and Mental Health Administration

The Collection and Interpretation of Data from Hidden Populations

Editor:

Elizabeth Y. Lambert, M.S.
Division of Epidemiology and
Prevention Research
National Institute on Drug Abuse

NIDA Research Monograph 98
1990

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Public Health Service
Alcohol, Drug Abuse, and Mental Health Administration

National Institute on Drug Abuse
5600 Fishers Lane
Rockville, MD 20857

For sale by the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402

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ACKNOWLEDGMENT

This monograph is based upon papers and discussion from a technical review on the collection and interpretation of data from hidden populations that took place on July 13 and 14, 1988, in Rockville, MD. The review meeting was sponsored by the Division of Epidemiology and Prevention Research, National Institute on Drug Abuse.

DHHS publication number (ADM)90-1678
Printed 1990

NIDA Research Monographs are indexed in the *Index Medicus*. They are selectively included in the coverage of *American Statistics Index, BioSciences Information Service, Chemical Abstracts, Current Contents, Psychological Abstracts,* and *Psychopharmacology Abstracts.*

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Introduction

Elizabeth Y. Lambert and W. Wayne Wiebel

The relative merits of qualitative versus quantitative research designs have long been debated in scientific literature. Each brings its own strengths and weaknesses to the process of analyzing and understanding complex human behavior. The National Institute on Drug Abuse publishes numerous documents, reports, and monographs about quantitative studies of drug abuse and drug-related problems. This particular monograph is different because it addresses qualitative approaches to the study of drug abuse, namely, ethnography and ethnographic research designs. The intent here is to examine unique attributes of ethnographic research and review how these attributes support efforts to understand issues often obscured by more quantitative methods. Specific focus is on ethnographic methods and techniques designed to access the so-called "hidden populations."

"Hidden populations" euphemistically refers to those who are disadvantaged and disenfranchised: the homeless and transient, chronically mentally ill, high school dropouts, criminal offenders, prostitutes, juvenile delinquents, gang members, runaways, and other "street people"—those we are all aware of to one degree or another, yet know so little about. These populations are often omitted from nationally representative surveys, largely because they have no fixed address or because they are less likely to be found at home or to agree to an interview. Ironically, those who belong to hidden populations are often at greater risk of drug abuse and drug-related morbidity than the general population. That is, the very individuals who might benefit the most from drug abuse treatment and prevention efforts are the least studied, the least understood, and the most elusive to epidemiologists, clinicians, researchers, and others concerned with understanding and improving the public health of these populations.

Ethnographic research methods are appropriate for topics about which little is known, primarily because ethnography is by its nature fundamental and exploratory, preparing the way for more rigorous studies that strive for precision and quantification. An example is the advent of acquired immunodeficiency syndrome (AIDS) in the early 1980s, a mysterious disease that challenged medical research and epidemiology throughout the world. Ethnography served to fill knowledge gaps and set the groundwork for further scientific inquiry. Such efforts included, for example, studies of needle-

sharing behaviors and sexual practices, particularly among prostitutes. It is at this exploratory, descriptive stage of research that ethnographic and qualitative methods can make significant contributions to the knowledge and understanding of problems and to the formulation of subsequent questions for quantitative research, including clinical studies, laboratory experiments, and population-based surveys.

This monograph reviews ethnographic methods used to study drug abuse and drug-related issues, then presents case studies that illustrate practical applications of ethnography. The first paper, by Dr. Wayne Wiebel, sets the tone for the entire monograph. It provides the rationale for the technical review meeting and gives an overview of issues related to ethnography and qualitative science, then describes sources of drug abuse indicator data and several well-known studies of drug abuse that were conducted using ethnographic research methods. Dr. Harvey Feldman, author of the next paper, provides an historical perspective on the use of ethnography in substance abuse research by describing one of the first events ever recorded (back in the 5th Century BC), moving through time to current applications like the study of the AIDS epidemic, and projecting future uses of ethnographic methods in efforts to prevent and treat drug abuse and AIDS.

The next set of papers focuses on methodological issues in ethnography. Dr. Karl van Meter's paper addresses snowball sampling, while Dr. Peter Adler reviews the history, application, strengths, weaknesses, and current status of the method of sociologic ethnography known as inductive analysis. The paper by Dr. Richard Fritz is devoted to a thorough review of the analytic needs of ethnographic researchers, including a survey of current word-search and text-retrieval programs that can support the analysis of qualitative data. Dr. Paul Goldstein's contribution focuses on the ethnographic field station and includes descriptions of real-life situations and experiences common to the conduct of substance abuse research from such settings in New York City.

The next set of papers merges theory with actual case studies. The first, by Dr. Patricia Adler, presents a candid review of practical considerations, constraints, and experiences encountered while conducting ethnographic research in the world of drug distributors and smugglers. The next, by Dr. Patrick Biernacki, describes recovery from opiate addiction without treatment and reviews findings from a study that used the chain referral method to identify its sample and used the retrospective approach for data collection. Dr. Marsha Rosenbaum and Ms. Sheigla Murphy discuss the issue of addicted women and heroin abuse from the perspective of ethnographic interviews with female drug abusers in California. Dr. Reyes Ramos provides unique insights into the world of heroin abuse among Chicano intravenous drug users, demonstrating the value of ethnography as an approach for the discovery and exploration of relatively unknown or poorly understood hidden populations. The final paper in this monograph,

by Dr. Hans Verbraeck, presents an ethnographic view of heterosexual prostitution, drug use practices, and AIDS in Amsterdam.

We are grateful to the authors for their valuable contributions to the technical review meeting and to this monograph. It is our expectation that this monograph will serve important educative functions in the years to come, not only as a methodological guide for ethnographic research, but also as a sourcebook for examining case studies and practical applications of ethnography in substance abuse research among hidden populations.

AUTHORS

Elizabeth Lambert, M.S.
Division of Epidemiology and
Statistical Analysis
National Institute on Drug Abuse
Parklawn Building, Room 11A-55
5600 Fishers Lane
Rockville, MD 20857

W. Wayne Wiebel, Ph.D.
AIDS Outreach Intervention Project
Epidemiology-Biometry Program
School of Public Health, MC925
University of Illinois at Chicago
2121 West Taylor Street, Room 555
Chicago, IL 60612

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Identifying and Gaining Access to Hidden Populations

W. Wayne Wiebel

INTRODUCTION

A number of convergent factors were instrumental in bringing about this Technical Review at the National Institute on Drug Abuse (NIDA). First and perhaps foremost, the spotlight of applied research interests at the Institute has once again focused on the types of drug users and patterns of drug use that often prove elusive as targets of inquiry for more rigorous survey research and experimental design methodologies. Second, since NIDA's last publication on ethnographic research methods (Akins and Beschner 1980), there have been a number of technical advances in the field, particularly the computer analysis of qualitative data, that deserve review and discussion. Finally, in recognizing the more recent contributions of ethnographic research in the substance abuse field, there have been a number of studies noteworthy for their methodological as well as substantive advancements.

It is fortunate for the qualitatively oriented social scientist that the substance abuse field offers such a fertile testing ground for qualitative methods and research designs. This reality, however, seems to be quite independent of developments or advances within the broader discipline of qualitative research methods. Instead, the nature of substance abuse in our society, together with applied research priorities, appears to be of greatest significance in maintaining a rich field of inquiry for exploratory and descriptive scientific investigations.

Generally speaking, there appear to be two factors that contribute to the importance of qualitative methodologies in the field of substance abuse research. First, continually evolving patterns and trends of substance abuse within our society foster a fluid situation in which emergent and novel phenomena are integral facets of today's drug scene. Whether it is the appearance of a new psychoactive substance (as in the case of designer drugs), the evolution of new combined drug-using patterns (as in the case of "T's & blues" (Talwin and pyrabenzamine)), or the emergence of new problems associated with the abuse of controlled substances, deviations from the status quo are an ever-present feature of drug abuse.

What is true for researching drug abuse patterns in general becomes particularly problematic for the study of novel and emergent trends. That is, as a relatively rare and isolated phenomenon, the abuse of illicit substances poses a host of methodological constraints on the process of scientific inquiry. The options available for case identification and the construction of practical sampling frames can rarely satisfy the requirements of true representative sampling. Moreover, in attempting to further the understanding of emergent phenomena, we must exercise caution in the development of research questions and hypotheses. Inaccurate assumptions about factors influencing a phenomenon or the relationships between variables are likely to result in misleading findings. When attempting to construct meaningful data collection instruments for drug-related research, the researcher must gain sufficient a priori familiarity with the topic to frame appropriate, meaningful questions. Such knowledge is the province and product of qualitative methodology.

The second factor confirming the value of qualitative methods in the substance abuse field relates more to the types of information required of research. That is, the need for substance abuse research, as well as the resources and priorities allocated to it, is in large part driven by the practical requirements of policymakers and professional interest groups. Quite simply, policymakers who want to make good decisions concerning complex social issues must have accurate, reliable information. As a consequence, the availability of funds for specific subjects of inquiry are most often compatible with applied research vs. basic science orientations. The "fundable value" of a research design is less likely to be based on its potential for advancing a particular scientific paradigm than it is on its promise for generating pragmatic evidence to inform decisionmaking. Clearly, qualitative research is often the only appropriate means available for gathering sensitive and valid data from otherwise elusive populations of substance abusers.

Given the recognized strengths of qualitative methods in exploratory and descriptive research designs, it would appear that applied substance abuse research will continue to value the potential contributions of this methodological orientation. The remainder of this chapter will address the issues of identifying and gaining access to hidden populations for purposes of research and/or intervention.

IDENTIFYING HIDDEN POPULATIONS

Because the use of illicit intoxicants is largely a covert activity in our society, it is not possible to enumerate all individuals who engage in such behaviors. Representative sampling, irrespective of scientific merit, is quite simply not possible in relation to the numerous varieties of phenomena at issue. Accepted strategies to accommodate these realities differ among methodological orientations. Survey researchers tend to focus on the larger,

more meaningful subpopulations that can be enumerated for the purpose of random sampling. Qualitative researchers, on the other hand, tend to focus their inquiry more narrowly to a smaller social unit of analysis for indepth investigation. This discussion will address scientific issues in studying "hidden populations" from a qualitative research orientation.

The term "hidden population" refers here to a subset of the general population whose membership is not readily distinguished or enumerated based on existing knowledge and/or sampling capabilities. As previously noted, this is often the case when research interests are directed toward emergent drug trends and novel patterns of use. For the researcher schooled in ethnographic methods, the task of selecting an appropriate sampling frame first requires the identification of meaningful units of analysis. This often, though not always, centers on distinguishing natural social units of analysis that share a common set of attributes dictated by the topics of investigation.

Research questions to be addressed are of paramount importance in determining available sources of information. For questions dealing with specific varieties of drug users or types of drug use, a number of resources are available to the researcher for exploration. First are secondary indicator data sources; second are institutional and professional authorities; third are indigenous informants.

Secondary indicator data offer a wealth of information to the substance-abuse researcher for defining an area of inquiry and identifying meaningful analytic frames. Among the more common sources of secondary indicator data are:

(1) Drug Abuse Warning Network (DAWN) reports on substance abuse-related emergency room episodes and coroner reports;

(2) law enforcement data on arrests, seizures, and laboratory analysis of controlled substances;

(3) treatment admissions for primary problems of drug dependence;

(4) toxicology reports including urinalysis results for drugs among admittees to treatment and arrestees, as in the case of the Drug Use Forecasting (DUF) system of the National Institute of Justice;

(5) prescription tracking systems for psychoactive pharmaceuticals; and

(6) street drug analysis programs.

While by no means exhaustive, this list of potentially useful secondary indicator data sources should offer the researcher fruitful territory for preliminary investigation. In addition to secondary indicator resources,

existing research, including NIDA's household survey and annual high school senior survey, can yield valuable information to the researcher. The nature of the problem to be studied will help determine the promise of any given informational source.

In dealing with particularly toxic drugs or drug combinations, emergency room mentions and coroner reports are likely to be more helpful than in the case of less toxic substances. Likewise, drugs leading to the more rapid development of problem dependence are likely to appear in treatment admission statistics before drugs less often associated with chronic patterns of abuse. In any case, secondary indicators offer only an indirect reflection of actual drug-using patterns. Also, there are inherent time lags between the actual change in a pattern of substance abuse and its appearance in these reporting systems. Researchers can therefore rarely rely exclusively on secondary data sources.

Institutional and professional information sources are likely to offer the researcher an opportunity to approach research interests on a more familiar basis. Sometimes these sources may supply the investigator with a meaningful and appropriate population from which to sample. Such was the case in an earlier study that required an ethnographic analysis of a PCP-using group in Chicago (Wiebel 1979).

As part of a planned four-city comparative ethnographic study of PCP users (Feldman et al. 1979), I was assigned to identify and select a local PCP-using social network as part of the larger study. While my previous research with multiple drug-using adolescents brought me into contact with numerous individuals who used this intoxicant, I did not know of any groups that had adopted PCP as a primary drug of abuse. Through a number of inquiries to local treatment facilities, I found that Northwest Youth Outreach (NYO) had regular contact with a number of high school drug-using networks that favored this particular drug. Through the introductions of NYO outreach staff, I was able to evaluate these potential research groups and select one that met the criteria established for inclusion in the larger study.

In another instance, I was solicited to conduct an exploratory study of new heroin users in Chicago (Wiebel, in press). Operationally defined as individuals who had begun using heroin for the first time in the preceding year, a major intent of the research on new heroin users was to determine whether recent inductees to the drug might be different from earlier cohorts of users. Given the apparent difficulty of identifying a social network of recent heroin initiates, a different sampling frame was pursued in this study. Institutional contacts and professional acquaintances were requested to identify qualifying candidates. Over a period of a few weeks, I was able to identify 20 independent subjects representing a remarkably diverse demographic and drug-use history profile. Specific sources of sampled

subjects included methadone detoxification programs, drug abuse counselors, the county jail, a criminal justice diversion program (TASC), and indigenous street informants.

Indigenous informants are a final and most valuable source of information to the researcher attempting to identify difficult-to-reach populations. Without question, street contacts cultivated over years of ethnographic investigations among Chicago's drug users comprise my greatest resource for identifying emergent phenomena and the otherwise hidden populations associated with relatively rare or novel behaviors. Of course, the ability to tap such resources is dependent upon developing networks of established relations among active drug users. Time and dedication are necessary prerequisites to the effective use of indigenous informants but—as many ethnographers recently introduced to the field of street drug research can attest—the task is rarely as imposing as appears initially.

GAINING ACCESS TO HIDDEN POPULATIONS

Having assessed available sampling frames and meaningful units of analysis, the investigator is left with the task of gaining access to these subjects for purposes of research and/or intervention. Assuming a qualitative research orientation and a focus of inquiry on social networks, an independent ethnographer has few options other than immersion in the social scene in order to develop rapport with members of the target population. For a successful research relationship to evolve, ethnographers must establish legitimacy both for their presence and for their intentions.

In previous work with drug-using groups in the natural setting (Hughes 1977; Shick et al. 1978; Wiebel 1979; Shick and Wiebel 1981; Wiebel 1988), research activities focused on the congregation areas frequented by drug users during the course of their daily activities. Most often such sites have included drug-copping areas, shooting galleries, pool halls, bars, restaurants, prostitution strolls, and public parks. When preliminary fieldwork is initiated in such an area, introductions through known and trusted confidants are solicited whenever possible. In those instances in which introductions are not possible, "hanging out" at public congregation sites proceeds in a friendly yet nonintrusive manner. Frequently, it is important to find something to do that legitimizes the researcher's presence (playing pool at a pool hall, ordering a meal at a restaurant).

Preliminary field observations should concentrate on defining the social structure of the target group as well as the roles played by various group members. As researchers prepare to establish more direct rapport with the target population, it is important to have some general sense of the status attributed to the individuals they have begun to recognize. While relations generally should be friendly with all subjects, it is the higher status network members who should be singled out for development of more close

alliances. Securing an approving relationship with high-status individuals is likely to facilitate rapport with all group members. By way of contrast, establishing close preliminary ties to a marginal, low-status group member, such as a police informant, may irreparably compromise the researcher's ability to develop a meaningful research relationship with the core group.

Much of our research in the substance abuse field in Chicago over the past two decades has utilized a multimethod design combining medical epidemiology and community ethnography. Originally conceived as an innovation both to further understanding and to intervene in neighborhood outbreaks of heroin addiction (Hughes 1977), what has become known as the "Chicago Model" has more recently shifted its focus to address AIDS research and intervention (Wiebel 1988). In both our earlier and current work, the employment of indigenous field staff has been a key component of our research and intervention programming.

Specifically, individuals indigenous to and intimately familiar with targeted populations are hired to assist in data collection or intervention service delivery. General qualifications for such positions include high status among targeted networks, superior communication skills, reliability, and expressed interest in performing job responsibilities. The experience to date of those who developed the Chicago Model and those working on the AIDS Intervention Project in utilizing indigenous field staff covering a wide array of different projects would highly recommend their consideration for research studies focusing upon hidden populations. Among the more noteworthy advantages to be realized through the employment of indigenous staff are the facilitation of entree and establishment of rapport with target groups, enhancement of project legitimacy, as well as improved capabilities to both gather and validate fieldwork evidence.

One of the more impressive demonstrations of the effectiveness of indigenous staff in gaining access to otherwise difficult-to-reach populations comes from our current work with groups at high risk for contracting AIDS. One such population, the sexual partners of active intravenous drug users (IVDUs), presented the greatest challenge in attempts to both effectively identify and gain access to appropriate research/intervention subjects. In the beginning months of the project, substantial links with networks of active IVDUs were established. At the same time, there was little initial success in trying to encourage these individuals to identify or bring in their non-IVDU sex partners. As time went by, however, and the trust between outreach workers and subjects solidified, this situation changed considerably. By the second year of the project, we were readily able to fulfill sampling requirements for our non-IVDU sex partner cohort.

CONCLUSION

As a final note in discussing means for enhancing access to hidden populations, two additional factors may be particularly helpful. First, the establishment of community-based field stations has proven to be extremely valuable in both providing convenient access to and maintaining regular contact with otherwise difficult-to-reach research populations. Further, like the hiring of indigenous staff, the rental of a storefront facility in a neighborhood frequented by target groups is often interpreted as an investment in the community. By headquartering research operations in the community and employing staff from the community, targeted populations are much less likely to define the project as a fly-by-night or fair-weather-friend operation.

Second, the payment of subject fees is a near-imperative prerequisite to obtaining cooperation and meaningful data from community-based research samples. Payment of subject fees both indicates the value of the data being collected and acknowledges the worth of the time subjects are expected to dedicate in cooperating with the research protocol. I have little doubt that the combined influence of the trusting relationships established between fieldworkers and subjects and the convenience of community-based field stations, together with the provision of subject fees for data collection, has helped us approach an 80 percent followup rate for 6-month followup interviews in our prospective study of active IVDUs and their sex partners in the community setting.

REFERENCES

Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Monograph. DHHS Pub. No. (ADM)80-946. Washington, D.C., Supt. of Docs., Govt. Print Off., 1980.

Feldman, H.; Agar, M.; and Beschner, G., eds. *Angel Dust: An Ethnographic Study of PCP Users.* Lexington, MA: D.C. Heath & Co., 1979.

Hughes, P. *Behind the Wall of Respect: Community Experiments in Heroin Addiction Control.* Chicago: University of Chicago Press, 1977.

Shick, J.; Dorus, W.; and Hughes, P. Adolescent drug using groups in Chicago parks. *Drug Alcohol Depend* 3:199-210, 1978.

Shick, J., and Wiebel, W. Congregation sites for youthful multiple drug abusers: Locations for epidemiological research and intervention. *Drug Alcohol Depend* 7:63-79, 1981.

Wiebel, W.W. Burning out on the Northwest side: PCP use in Chicago. In: Feldman, H.; Agar, M.; and Beschner, G., eds. *Angel Dust: An Ethnographic Study of PCP Users.* Lexington, MA: D.C. Heath, 1979.

Wiebel, W.W. Combining ethnographic and epidemiologic methods for targeted AIDS interventions: The Chicago model. In: Battjes, R.J., and Pickens, R.W., eds. *Needle-Sharing Among Intravenous Drug Abusers: National and International Perspectives.* National Institute on Drug Abuse Research Monograph 80. DHHS Pub. No. (ADM)88-1567. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1988.

Wiebel, W.W. Factors influencing personal decisions to use heroin. In: Forster, B., and Salloway, J., eds. *The Socio-Cultural Matrix of Alcohol and Drug Use: A Sourcebook of Patterns.* Lewiston, NY: Edwin Mellen Press, in press.

AUTHOR

W. Wayne Wiebel, Ph.D.
Principal Investigator
AIDS Outreach Intervention Project
Epidemiology-Biometry Program
School of Public Health, MC925
University of Illinois at Chicago
2121 West Taylor Street, Room 555
Chicago, IL 60612

128611

The Role of Ethnography in Substance Abuse Research and Public Policy: Historical Precedent and Future Prospects

Harvey W. Feldman and Michael R. Aldrich

INTRODUCTION

Perhaps the best way to begin is to answer the question: What is ethnography? One of the earliest definitions of ethnography was provided by A.H. Beatty in 1896, who wrote, "Ethnography is purely descriptive, dealing with characteristic social and political conditions of people, irrespective of their possible physical relations or affinities" (Wiseman and Aron 1970, p. 238). The definition sounds old fashioned, formal, and somewhat unclear, but it serves as a starting point.

James Spradley, who died in 1982, was an ethnographer affiliated with the University of Washington in Seattle and the author of *You Owe Yourself a Drunk: An Ethnography of Urban Nomads* (Spradley 1970), a landmark study of the way alcoholics move through the criminal justice system. He called ethnography "the science of cultural description" and claimed that "through a painstaking process of participant observation one slowly comes to apprehend the insider's view of life" (Spradley 1977, p. 13).

Jennifer James defined ethnography as "the study of cultures from within" (James 1977, p. 180). James Walters, who worked the streets of Philadelphia, called ethnography "an analytic description of behaviors that characterize and distinguish cultures or sociocultural groups" (Walters 1980, p. 17). Jacqueline Wiseman and Marcia Aron, in their book *Field Projects for Sociological Students* (1970), said, "Ethnographic research is an investigator's application of his sociological methods and skills to a specific scene in order to learn enough about its people, situations and human relations to be able to present the reader with a slice of life" (Wiseman and Aron 1970, p. 237). Finally, George Beschner, an adapted ethnographer affiliated with the National Institute on Drug Abuse (NIDA), once described ethnography as "the study of a culture or social group from the perspective of its members" (Feldman et al. 1979, p. 4).

To sum up these definitions, there are perhaps three essential features they touch on. First, even though ethnography is a scientific method, it requires an artist's touch or even better, an artist's soul, rather than being the simple application of a technical approach to analyze human behavior.

Second, clearly the aim is to get an insider's view of a culture or social group, with emphasis on being authentic and on being accurate. To achieve that, the ethnographer has to go where members of the culture or social group carry out their activities and observe members by watching them carefully, asking the right questions, and by listening.

Finally, like many others in scientific fields, it is the ethnographer's task to assemble data in a way that makes sense and that reflects actual phenomena. How detailed the final product will be is subject to debate. The old view of ethnography as cultural description required that the product be absolutely thorough. To achieve this completeness may have required several years of living among the populations under study, so that each piece of the culture was understood in its relationship to all the others. As ethnography moved more and more into the arena of applied sciences, where its influence may have been determined by its ability to respond with data in a timely fashion, completeness may have been sacrificed to significance.

The more recent view, because of some developments in ethnography and its use in government's management of social and health problems, is that the ethnographer must gain a quick but accurate understanding of a phenomenon in order to act on it. The use of ethnography for understanding the spread of the human immunodeficiency virus (HIV) within intravenous (IV) drug-using populations is a recent example of the application of ethnographic research methods to a health problem with complex social implications; it will be discussed later in this chapter. Ethnography is a useful device for understanding a phenomenon quickly, so that findings can be converted into public health policies and control measures. As ethnography grows in importance as a tool for obtaining information quickly for policymakers, it risks losing its reputation for completeness. Ronald Cohen, an anthropologist, said in "The Logic of Generalization" that "scientific findings and explanations are always partial, and they always leave something out" (Cohen 1970, p. 31).

Generally, ethnography is the term used to describe almost all qualitative research, from strictly interviewing, which is its most limited form, to extensive observation from some selected social role that allows an insider's view. Many ethnographers prefer fieldwork, which means spending time where members of a group spend their time, seeing what goes on, and then asking questions about the observations.

HERODOTUS: THE BEGINNINGS OF DRUG ETHNOGRAPHY

Where and when did ethnography's connection to substance abuse begin? Drug ethnography as a research method is a fairly recent development. The first recorded example in Western literature is found in Herodotus in his description of the Scythians, who were nomadic horsemen of Central Asia somewhere around the fifth century BC. Briefly, Herodotus claimed that, after the burial of a king, the Scythians purified themselves by fixing three sticks in the ground, tied together at the top and tightly covered with felt. Inside this little tent they put a dish with red-hot stones in it. Then they took hemp seed, crept under the tent, and threw it on the red-hot stones. And, to quote Herodotus:

> It smolders at once and sends up such billows of smoke that no Greek vapor-bath could surpass it. The Scythians howl with joy in these vapor-baths, which serve them instead of bathing, for they never wash their bodies with water (Aldrich translation).

Herodotus is often accused of making up wild tales, and, in his *Histories*, (De Selincourt 1954) he often says he does not believe them himself. But this account of bathing in marijuana vapors was verified in 1948 when a Russian archaeologist (Rudenko 1970) dug up a Scythian tomb barrow in Siberia and found, next to well-preserved corpses, six poles about 18 inches high, a felt blanket, a copper cauldron with stones in it, and a leather medicine bag containing hemp seeds, quickfrozen in ice since about the fifth century BC. So Herodotus was verified in every particular, except that there were six sticks instead of three. This demonstrates another point: ethnographers have difficulty counting.

COLUMBUS AND PANE: THE DISCOVERY OF DIMETHYLTRYPTAMINE (DMT)

The first ethnography in America came from Christopher Columbus and a friar he hired on his second voyage (1493-1496). The friar's name was Ramon Pane, and his special task was to record native customs. Columbus noticed the Tainos of Haiti sniffing a dust that made them "become like drunken men" (Wassen 1967, p. 235). Pane investigated and found that this powder, called *cohoba* by the natives, was snuffed by chiefs and shamans to communicate with the spirits. Pane noted a bizarre outcome, which today would be considered psychedelic: "Consider what a state their brains are in, because they say the cabins seem to them to be turned upside down and the men are walking with their feet in the air" (Wassen 1967, p. 238).

At first the Europeans thought *cohoba* was tobacco and dismissed the visions Pane reported as heathen nonsense. Pane commented, "They speak many things incoherently" (Wassen 1967, p. 238). The Tainos were

exterminated about 50 years later, and not until 1916 was a botanist able to identify cohoba as a hallucinogenic drug made from the beans of a tree, *Anadenanthera peregrina* (Safford 1916). The active ingredient was identified in the 1950s as dimethyltryptamine or DMT. Not until 1972, when Altschul reviewed ethnographic reports of surviving tribes who used this snuff, did the meaning of the visions reported by Columbus and Pane become clear:

> the person, who is seated with his head turning to one side and his arms around his knees, becomes at first unconscious, then wakens and, with his face turned toward the ceiling, speaks unintelligibly; the Indians say that the house in which the powder is taken then appears to be upside down and they themselves seem to be walking on air (Altschul 1972, p. 13).

So it took from 1496 to 1972 for the discovery of DMT to make sense. The lesson from this for ethnography and ethnographers, as well as for all scientists, is to keep pursuing analysis of the data; sooner or later, it will begin to make sense.

VESPUCCI, CIEZA, AND ACOSTA: BUT WHY DO THEY DO IT?

In 1504, Amerigo Vespucci gave the first ethnographic report of coca chewing, as he found it in 1499 on a little island off South America. Watching the natives put coca and powdered lime in their mouths so "their cheeks bulged with the leaves . . . which they chewed like cattle," he wondered why: "They did this frequently, a little at a time; and the thing seemed wonderful, for we could not understand the secret, or with what object they did it." After asking for drinking water and being offered coca instead, Vespucci guessed that "they kept this herb in their mouths to stave off thirst" (Aldrich and Barker 1976, p. 3).

Fifty years later, Cieza de Leon, an ethnographer in South America during the Spanish Conquest, asked directly: "When I asked some of these Indians why they carried these leaves in their mouth . . . they replied that it prevents them from feeling hungry, and gives them great vigor and strength" (Aldrich and Barker 1976, p. 3). At first this notion was dismissed as pure imagination, but when the Spaniards discovered that the natives would not work the silver mines of Potosi without coca, they allowed them to chew it. By 1570, Jose de Acosta said that the traffic in coca at Potosi had exceeded a half million pesos a year (von Hagen 1959).

From these very early ethnographic reports of drug use, a lesson can be learned that has modern application. Ethnography takes place in a cultural context, affecting both the observer and the observed. Drug ethnography, in particular, is a matter of one group with a certain viewpoint studying another group with quite a different viewpoint. A value and a limitation of

ethnography is that it occurs in a specific place and time. This should always be remembered while collecting and interpreting data from "hidden populations" or populations whose behavior we might not like or find socially undesirable.

DE QUINCEY: THE INDUSTRIAL CONTEXT

Depending on how strictly we want to hold to a definition of ethnography, the modern era for drug ethnography probably began with Thomas De Quincey in his 1822 *Confessions of An English Opium-Eater*. De Quincey was an ethnographer in many ways and was the first to consider opiate use in the modern industrial context. At an early age, he ran away from home and lived in the streets of London, mingling with thieves, prostitutes, and other social outcasts. He wrote:

> At no time in my life have I ever been a person to hold myself polluted by the touch or approach of any creature that wore a human shape. On the contrary, it has been my pride to converse familiarly . . . with all human beings . . . that chance might fling my way, a practice which is friendly to the knowledge of human nature (Ward 1966, p. 42).

This is a great quote for all ethnographers to remember. Having disdain for social classes, particularly if those classes are the very subjects of study, is the basis for bias that will distort and probably destroy objectivity. De Quincey's message is timely: ethnographers cannot present an accurate portrait of a population whom they dislike, distrust, or look down on.

De Quincey began his study of opium users by observing men he noted were distinguished for talent or eminent station. He preserved the anonymity of his contacts by leaving blanks for the names of famous addicts. Later, in an 1856 revision of his *Confessions*, he revealed that they included Samuel Taylor Coleridge, the famed poet and critic and author of "The Rime of the Ancient Mariner"; Sir William Wilberforce, the man most responsible for abolishing slavery throughout the English Empire; and many other prominent British figures. Following this, he began exploring opium use among the lower classes, and, as a participant observer, he moved into the real world of the opium system of the time—users, suppliers, their bosses, and working conditions. Here he noted, among other things, that one immediate reason for working-class opium abuse was "the lowness of wages, which, at that time, would not allow them to indulge in ale or spirits" (Ward 1966, p. 24-25). The result was an enlightening description of opiate abuse in the context of the onrushing Industrial Revolution. De Quincey's conclusion was that, in this context, opiate use cut across all classes of society, which was also noted in America by Fitz Hugh Ludlow (1867).

THE ETHNOBOTANISTS: PERILS OF PARTICIPANT OBSERVATION

In the 19th century, many botanists were drug ethnographers. Richard Spruce, who spent much of his life collecting plants in the Amazon, was the "great granddaddy" of them all. In 1851 through 1852, he found himself in Brazil among a head-hunting tribe of the Rio Vaupes, who had never seen a white man before. He asked about a drink called *caapi* that their sorcerers used, and they urged him to try some. He managed to down a cupful, and the tribal leaders, anxious to show him all their drugs, plied him with manioc beer, a two-foot-long cigar (Spruce had never smoked before) and a large bowl of palm wine. Overcome with a strong inclination to vomit, he retired to a hammock, drank coffee, and passed out (Wallace 1908). Despite this negative experience, he was sufficiently impressed to gather specimens of the vine from which the drink was made. He correctly identified it as *Banisteriopsis caapi*, also called yage, which is now known to contain the hallucinogen harmine. More than a century later, Schultes subjected Spruce's specimens to chemical analysis and found them "comparable with freshly collected plant material" (Schultes and Hofmann 1980, p. 168). Another lesson for qualitative research: if possible, with the permission of the respondents, collect evidence to verify observations. More importantly, Spruce reminds us that "participant observation" can be a contradiction in terms. The question is, how much does one participate in order to be allowed to observe? In extreme cases, one's life may depend on the answer.

Other ethnobotanists who have conducted drug research in this century include La Barre, whose book *The Peyote Cult* (La Barre 1938) is a key reference on the Native American Church; Schultes (1937), whose early work on peyotism among the Kiowa (Vestal and Schultes 1939) led to 40 years of studying Amazon drug plants; Wasson (1980), who discovered the modern use of psilocybe mushrooms in Mexico, which led to a complete revision of our understanding of native uses of such plants; and, more recently, Davis (1985), whose fieldwork in Haiti revealed that the drug used in voodoo to make men into zombies is from a West Indian puffer fish. Understanding the use of each of these drugs within their respective cultures was the contribution of ethnographers, in these cases, ethnobotanists.

In our urban milieu, ethnographers themselves must answer the question of how deeply to participate in the activities of a social group or a culture. Field researchers have always been warned about the dangers of "going native." Arthur Vidich (1955) stated that, in doing so, researchers lose their objectivity. Vidich's advice is sound: the development of mutually beneficial research relationships should not depend on demonstrating to respondents the researcher's trustworthiness by becoming involved in activities that are either personally repugnant, ethically questionable, physically dangerous, or outright illegal. It is not necessary to share

needles with IV drug users to study needle sharing and the way that activity may affect the acquired immunodeficiency syndrome (AIDS) epidemic; researchers' credibility is at risk both within the scientific community and with respondents if researchers do not clearly identify themselves as nonparticipants in illegal activities. As often as not, an invitation to share drugs or commit a crime is a test of whether researchers are who they say they are—researchers rather than agents of social control. These are, of course, individual decisions that should always be made on the basis of pursuing information, not from insecurity or for thrill seeking.

THE DEVELOPMENT OF MODERN DRUG ETHNOGRAPHY

Probably the first important drug ethnography for modern times in America was carried out, appropriately enough, in Chicago, the home base for field research in America. Being within the famed "Chicago School" of social research, it was an extensive one-man study by Bingham Dai titled simply *Opium Addiction in Chicago* (Dai 1937). Dai's study was a combination of field research and the psychoanalytic approach. It is important because it captured the evolving opiate scene between the latter nineteenth century, when drugs were freely available in the United States, and the changes brought about by the Harrison Act of 1914.

Dai divided his respondents, who were predominantly white, into two categories: those addicted to morphine through medical treatment, and those addicted primarily due to the influences of other drug addicts and "peddlers," through what Dai called "pleasure parties," or association with pimps, prostitutes, pool rooms, gambling houses, and homosexuals. Dai's method was the indepth life history interview. Given his psychoanalytical background, it was not surprising to find his observations cast in terms like "infantile" personalities, excessive dependence on others, and a tendency to withdraw or escape from social responsibility (Dai 1937). The fact that he drew many of his cases from the Psychopathic Hospital in Chicago may account for his tendency to identify psychopathology as the underpinning of all drug abuse.

Alfred Lindesmith, who had helped Dai gather respondents, used qualitative interviewing techniques to develop definitions of addiction—probably the first in the world derived from ethnographic research—and published them in 1947 in the landmark book called *Opiate Addiction* (Lindesmith 1947). His work in the 1940s and 1950s changed the theoretical perspective toward addicts. Based on his findings and conclusions, he argued against what he believed were the severe limitations of the public policy of prohibition that grew out of the Harrison Act. Lindesmith criticized and contradicted the enforcement policy of the Federal Bureau of Narcotics at the time, which he described in *The Addict and the Law* (Lindesmith 1965).

Lindesmith's ethnographic work contributed to a scientific understanding of the meaning of addiction and was used to juxtapose two views of the phenomenon: drug addiction as crime and drug addiction as disease. That juxtaposition was, in fact, the title of a report, *Drug Addiction: Crime or Disease?*, issued by a Joint Committee of the American Bar Association and the American Medical Association (1961). The Federal Bureau of Narcotics argued against the report before it was published, which resulted in the publication of what had originally been only an internal report, and Lindesmith's research became the basis of public discussion about the management of drug addiction in America for the next 30 years.

From the 1940s to the 1960s, most of the ethnographic studies in the drug field shifted the emphasis from asking why people used drugs to asking how they went about getting involved in drug use and how they remained involved. Rather than looking for underlying causes, ethnographers began to find their search for etiological influences in the social world rather than the internal world of experimenters. This period constituted the first major shift away from psychoanalytic theory and a medical model of addiction to a more sociological perspective. The concept of drug use as a "career" became very useful. Building on the work of Everett Hughes (1984) who introduced the study of occupations into sociology, Howard Becker (1963) took the career model and applied it to deviant careers in general and drug users specifically. This conceptual shift had broad implications and was the basis for what came to be viewed as a new sociological movement called "labeling theory." When applied to the field of addiction, it provided a different perspective that included an analysis of individual drug users and of public policy, as well as a legal framework within which such activities took place. In this way, Becker's concepts helped structure the way drug ethnographers began to collect and analyze data. This shift in concept was evident in his own study, "Becoming a Marijuana User" (Becker 1953).

At approximately the same time, other small ethnographic studies of drug users were in progress. One of these became most influential, namely Harold Finestone's (1957) study of black heroin addicts in Chicago entitled "Cats, Kicks, and Color." Many in the field of drug ethnography, while impressed with the influence of this single study, were critical of it, because it seemed less a description of heroin addicts than a description of low-income street blacks in Chicago in general, whether or not they were involved in heroin use. In carrying out interviews in an office rather than spending time among heroin users in their natural settings, Finestone apparently mistook a general black perspective as something peculiar to heroin users. In noting that they employed a special argot of slang terms, for example, the word "bread" for money, or because they had a preference for "cool jazz," he attributed these characteristics to addicts when, in fact, they were common among street blacks almost everywhere during the 1950s. Finestone's fame will rest not so much on having carried out top-level ethnography as it will on the use of his work by later sociologists

who, in scouring the literature for descriptions of drug users, found the drug ethnography library of the period relatively barren except for Finestone's study.

In an attempt to develop a broad theory of delinquency, Richard Cloward and Lloyd Ohlin (1960) aimed to unite the previously disparate theories of structural functionalism and anomie as developed by Robert Merton (1957) and Emile Durkheim (1951) with the tradition of differential association as developed by Shaw and McKay (1942) and Sutherland (1947). In their book *Delinquency and Opportunity: A Theory of Delinquent Gangs*, Cloward and Ohlin attempted to portray several delinquent subcultures, one of which was a "retreatist" subculture of addicts, which was based on a theoretic category developed by Merton, Cloward's mentor. The only empirical study that provided actual descriptions of addict behavior and supported this perspective was Finestone's short description in "Cats, Kicks, and Color." It became the sole basis of support for a theory that was soon to achieve national prominence. Had the work of Cloward and Ohlin occurred a few years later, they would have had available to them numerous studies to draw from, but, at the time, Finestone's was one of few descriptive studies of heroin addicts available. Because of the importance of the Cloward and Ohlin opportunity theory and its use as the underpinning of the Mobilization for Youth delinquency project in New York City, the "trial balloon" for the 1960s antipoverty program, their minitheory of addicts as double failures within a retreatist subculture became a persistent theme.

Along with the explosion of drug use in the 1960s, primarily among youth (Mandel and Feldman 1988) and various populations throughout the United States, there was also a rise in the funding for ethnographic research that came principally from NIDA and its predecessor, the Center for Drug Studies, under the National Institute of Mental Health, through a woman most contemporary drug ethnographers look upon with great respect, Eleanor Carroll. She was a strong advocate on behalf of ethnography and field research and did much to advance the field.

The 1960s produced what has been referred to as the "era of iconoclasts" in the drug field. This involved a shift away from the classic question that Lindesmith posed about addiction as crime or disease. Several ethnographers, operating independently in different parts of the country, asked instead, "How about neither?" Building on the career model and going directly into the hangouts of drug users, these ethnographers moved away from trying to understand drug use as a consequence of the physical and/or psychological effects of drugs to exploring how the activity of drug use itself was viewed within the community contexts where it took place. A consistent conclusion was that drug use should be viewed not as a pathology so much as a status-enhancing activity within certain communities where street action and high-risk behavior were highly valued.

The *Add Center Project Final Report: The World of Youthful Drug Use* by Herbert Blumer (1967), one of the old-timers of the Chicago School, was a key document in the move toward a different view of drug use. Much of the report was written by Alan Sutter, who mixed easily with street drug users and was probably the first to note that drug use seemed less an issue of pathology and more a means to acquire social status and reputation within an environment that valued risk-taking behavior. This view was supported by concurrent studies by Edward Preble (Preble and Casey 1969) and Feldman (1968).

Almost all these developments in the 1960s were snapshots of special populations: Sutter (1966; Sutter 1969; Sutter 1972) with blacks in Oakland; Polsky (1967) with pool hustlers; Feldman with Italians in Boston (Feldman 1973; Feldman 1974, Feldman 1977); Carey and Mandel (1968) with amphetamine use in the San Francisco Bay Area; and Preble and Casey (1969; Preble 1980a) in New York City with Puerto Ricans and mixed white ethnic heroin addicts. The rise in the number of ethnographic studies continued into the 1970s: Sherri Cavan (1972) with hippies in the Haight; Irving Soloway's 1974 study of methadone clients (Soloway 1974); major studies of marijuana use in Jamaica (Rubin and Comitas 1975) and Costa Rica (Carter 1980); Patricia Cleckner's work on cocaine users in Miami (Cleckner 1976a; Cleckner 1976b; Cleckner 1977); Jennifer James's ethnography of prostitutes in Seattle (James 1976; James 1977); Joan Moore's now classic study of Mexican-American "homeboys" in Los Angeles (Moore 1974); and the Feldman et al. (1979) study of phencyclidine (PCP) users. The ethnographers in the PCP study worked in four cities: Wayne Wiebel in Chicago, Jennifer James in Seattle, Patricia Cleckner in Miami, and James Walters in Philadelphia. As ethnographers became interested in special populations, Marsha Rosenbaum and Sheigla Murphy conducted research to improve the understanding of females who injected heroin (Rosenbaum 1981), of women in methadone maintenance programs (Rosenbaum and Murphy 1987; Murphy and Rosenbaum 1988), and, more recently, of professionals who use methylenedioxymethamphetamine (Rosenbaum and Morgan 1988).

Coming into the 1980s, there has been a dual research emphasis on street dealing and drug trafficking and on the out-of-treatment drug user. Three studies of drug dealing have added greatly to our understanding of that illegitimate business: Allen Fields's (1984; Fields 1986) study of young street "weedslingers," Bruce Johnson's (1985) study of heroin dealers, and Patricia Adler's (1985) study of cocaine traffickers.

Two ethnographic works focused on addicts who have never been in treatment: *Life With Heroin* (Hanson et al. 1985), and—important for understanding how some addicts can move away from heroin use without entering formal treatment programs—Patrick Biernacki's *Pathways from*

Heroin Addiction (Biernacki 1986), which won the Charles Cooley award offered by the Society for the Study of Symbolic Interactionism last year.

The tendency of 1970s ethnographic work was to shift away from a focus on ethnicity and culture to studies that were largely drug specific. During this period, there was a growing concern over the emergence of new drugs. It was widely believed that each drug had its own special subculture; and it was the contribution of ethnographers that revealed the clear existence of polydrug users and the way they sifted and sorted through a variety of drug options. What came out of the 1970s ethnographic research, however, was a clearer picture of social subgroups, not by drug of preference necessarily, but in demographic configurations based on age, sex, race, ethnicity, and neighborhood affiliation. Collectively, many of these studies challenged the notion of the existence of "drug subcultures" and showed that a better understanding of drug-using behavior might be achieved by studying drug users within particular community contexts where such behaviors occur.

FUTURE ETHNOGRAPHY IN DRUG/AIDS INTERVENTION AND PREVENTION

What emerged from these studies was an accumulation of information that supplemented and improved national data collections on drug abuse. For example, in the PCP study (Feldman et al. 1979), surveys that the Federal government sponsored were examined and compared to data collected from young people in natural social groups in four cities. It was possible to identify in those four cities certain patterns of PCP use. For example, there was an interest in identifying as precisely as possible when the fad of PCP began and when momentum developed such that youth in a wide variety of social settings in different parts of the United States began to experiment with PCP. It was possible to establish the year 1973 as the date of entry in all four cities, with a clear rise in use in 1974 and a leveling off in 1975. When the national survey data were examined, especially the surveys of high school seniors, it was found that, for those peak years of experimentation in 1973 and 1975, there was no mention of PCP use among young drug users. In other words, the PCP phenomenon entered the world of youth and diminished without the national data system ever identifying it. What became obvious to many researchers in the 1970s was that ethnography was a useful supplement and, in many ways, a far sharper instrument for collecting information about emerging drug patterns and the ways drug users adapted to them than had previously been thought.

The discovery that ethnography might well be useful as a method for identifying new drug use trends gave rise to serious discussions about employing ethnographic field stations (Feldman 1980; Preble 1980b). There were two important conferences held to develop this idea, which resulted in two publications: the book, *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings* (Weppner 1977), which has Ed Preble's pro-

phetic article, "Methadone, Wine and Welfare," and the NIDA monograph, *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields* (Akins and Beschner 1980).

These publications evolved from two meetings sponsored by NIDA, which were held to discuss the utility of funding ethnographic field stations: the first, in 1976, in Key Biscayne, and the second, in Chicago, on ethnography as the basis for State and local planning in the drug and alcohol fields. The first meeting was cosponsored by the Drug Enforcement Administration (DEA). There was general agreement that ethnographic methods would be extremely useful in identifying new drug trends. There was similar agreement that the connection of ethnography with the DEA might be problematic, and many of the ethnographers in attendance were reluctant to have a close association with that agency while collecting data from populations involved in illegal activities. As a result, several ethnographers in attendance opposed common research undertakings; and the idea of ethnographic field stations was shelved for the time.

At the Chicago meeting of 1979, which was sponsored solely by NIDA, the concept of ethnographic field stations was reexamined and endorsed as a unique method for collecting data to be utilized for planning and policymaking purposes. It was not supported by government funding sources through the 1980s, however, until the emergence of the AIDS epidemic.

From the exchange of ideas between ethnographers and policymakers during these national meetings and from ethnographic studies themselves, some new prevention and education strategies began to emerge. For example, most of the publicity on the dangers of PCP use came through the national media, with such television news shows as "60 Minutes" and others, which emphasized the connection between PCP use and violence and/or psychotic behavior. However, research on the violence associated with PCP use indicated that there were few such incidents of violence, other than panic reactions to hospital personnel or police attempting to employ physical constraints. Instead, it appeared that any drug-associated violence was not precipitated by ingesting the drug necessarily but by medical personnel and police who were ill-informed on how to manage adverse reactions. What emerged quite clearly from this study was that PCP was a powerful drug and that youth knew it to be a powerful drug. In fact, its very reputation, reinforced by the national media, was the motivation that prompted users to take it in the first place. The drug did have adverse effects; but these effects, according to the users themselves, were not the ones the media publicized. Rather than the dramatic incidents of violence, the condition young people identified as most problematic was called "burning out," a state of cloudiness and forgetfulness resulting from prolonged consumption of the drug, which was basically an anesthetic. It was this condition of "burning out" that eventually led to a lessening of interest in PCP use among most youth throughout the country.

Probably the most important use of ethnography in the drug field is now in progress. With the development of the AIDS epidemic among IV drug users, there is an urgency to mobilize our finest scientific resources to address what has the potential of becoming a national crisis and has become one already in New York City and northern New Jersey. If the Surgeon General is correct in saying that he does not anticipate a cure for AIDS and that a vaccine is in the distant future, then the urgency must be to keep new infections to a bare minimum. In San Francisco and Chicago, where ethnography has been something of a tradition, it has been found that the most effective approach to carrying the prevention-education message to IV drug users and their sexual partners has been through an aggressive strategy of outreach based on ethnography.

Right now, IV drug users are truly members of a hidden population whose behaviors must be understood by those responsible for managing the HIV epidemic in order to mount an effective intervention. One response has been to support ethnographers to develop studies that can quickly grasp relationships between needle sharing and the epidemic in ways that traditional research methods cannot (Feldman et al. 1984; Feldman and Biernacki 1988; Wiebel 1988).

In addition to providing street-based intervention with a conceptual basis for managing the AIDS epidemic among IV drug users and their sexual partners, ethnography can make continuing contributions to drug research in general. One area of interest is that of the heroin addict and his or her resistance to change. Ethnography and the experience of street-based interventions have accumulated evidence that the notion that IV drug users are hard to reach and resistant to change may be in serious error. The experience of street-based efforts in San Francisco, Chicago, and now several other cities has demonstrated that addicts are receptive and responsive to educational campaigns and will change certain behaviors as long as the necessary changes do not require alterations of customs, values, and lifestyles that are essential to their identities and cultural associations. While this may run counter to the conventional wisdom regarding addict behavior, it suggests a wider universality that can be applied to other social groups and cultures. Through ethnography, it is possible to apply this finding to IV drug users and the HIV epidemic. In San Francisco, Chicago, and the other cities where an ethnographic approach has been employed, there has been success in changing needle-sharing and needle-cleaning behaviors in a relatively short period of time (Watters 1987; Watters et al. 1988). The strategy of employing ethnography as the forerunner to aggressive outreach to IV drug users at risk for HIV infection has now been expanded to 27 major American cities.

As these ethnographically based projects develop, one result may be the creation of a structure for the delivery of health care, early intervention, and social services to inner-city communities that is new and more closely

tailored to the needs of urban populations. Ethnographically oriented outreach AIDS prevention projects may be a model for a variety of health and social services in the future.

CONCLUSION

In summary, ethnography has made significant contributions to the substance abuse field. History has shown that ethnography has successfully penetrated a wide variety of drug-using populations from the time of Herodotus to our present day. Invariably, ethnographers have presented "different" views of drug users, some which have contradicted popular conceptions. These analytic descriptions may be closer to the way drug users are seen and responded to within their own sociocultural context than to the portrait painted by some health professionals. In this sense, ethnography has challenged theories of etiology and provided new concepts that are practical and utilitarian. Similarly, ethnography has identified problems in maintaining accurate updates on patterns of drug use and the emergence of drug fads by noting limitations of current national data systems; it has suggested ways to modify and augment the present arrangements. Finally, ethnography has demonstrated and continues to demonstrate its usefulness in applied research. With the emergence of the HIV epidemic as a problem of potentially catastrophic proportions, ethnography has the flexibility for use in short-term exploratory research efforts and in day-to-day outreach intervention by trained health educators, among populations that have been viewed as difficult, if not impossible, to access and change.

The challenge for drug ethnographers will be to fulfill the promise of their potential by addressing the major social and health problems of the day. For now, management of the AIDS epidemic should hold the highest priority. If success in that effort is achieved, thousands of drug users, their families, and their neighbors may never experience the epidemic, helped by a delivery of health services that will benefit nondrug users as well as the "hidden populations" who are the subjects of present concern.

REFERENCES

Adler, P.A. *Wheeling and Dealing: An Ethnography of an Upper-Level Drug Dealing and Smuggling Community.* New York: Columbia University Press, 1985.

Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Services Research Branch. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980.

Aldrich, M.R., and Barker, R.W. Historical aspects of cocaine use and abuse. In: Mule', S.J., ed. *Cocaine: Chemical, Biological, Clinical, Social and Treatment Aspects.* Cleveland: CRC Press, 1976. pp. 3-11.

Altschul, S.v.R. *The Genus Anadenanthera in Amerindian Cultures.* Cambridge: Botanical Museum, Harvard University, 1972.
Becker, H.S. Becoming a marijuana user. *Am J Sociol* 59:235-242, 1953.
Becker, H.S. *Outsiders: Studies in the Sociology of Deviance.* Glencoe, IL: Free Press, 1963.
Biernacki, P. *Pathways from Heroin Addiction: Recovery Without Treatment.* Philadelphia: Temple University, 1986.
Blumer, H.; Sutter, A.; Ahmed, S.; and Smith, R. *Add Center Project Final Report: The World of Youthful Drug Use.* Berkeley, CA: University of California, 1967.
Carter, W.E., ed. *Cannabis in Costa Rica: A Study of Chronic Marihuana Use.* Philadelphia: Institute for the Study of Human Issues, 1980.
Carey, J.T., and Mandel, J. A San Francisco Bay Area speed scene. *J Health Soc Behav* 9:164-174, 1968.
Cavan, S. *Hippies of the Haight.* St. Louis: New Critics Press, 1972.
Cleckner, P.J. Blowing some lines: Intracultural variations among Miami cocaine users. *J Psychedelic Drugs* 8:37-42, 1976a.
Cleckner, P.J. Dope is to get high: A preliminary analysis of intracultural variation in drug categories among heavy users and dealers. *Addict Dis* 2:537-552, 1976b.
Cleckner, P.J. Cognitive and ritual aspects of drug use among young black urban males. In: DuToit, B.M., ed. *Drugs, Rituals and Altered States of Consciousness.* Rotterdam: A.A. Balkema, 1977. pp. 149-168.
Cloward, R.A., and Ohlin, L.E. *Delinquency and Opportunity: A Theory of Delinquent Gangs.* Glencoe, IL: Free Press, 1960.
Cohen, R. The logic of generalization: Generalizations in ethnology. In: Naroll, R., and Cohen, R., eds. *A Handbook of Method in Cultural Anthropology.* New York: Columbia University Press, 1970. pp. 31-50.
Dai, B. *Opium Addiction in Chicago.* Shanghai: Commercial Press, 1937. (Reprint, Montclair, NJ: Patterson Smith, 1970.)
Davis, W. *The Serpent and the Rainbow.* New York: Simon and Schuster, 1985.
De Selincourt, A. *Herodotus: The Histories.* Harmondsworth, England: Penguin, 1954.
Durkheim, E. *Suicide: A Study of Sociology.* Glencoe, IL: Free Press, 1951.
Feldman, H.W. Ideological supports to becoming and remaining a heroin addict. *J Health Soc Behav* 9(2):131-139, 1968.
Feldman, H.W. Street status and drug users. *Trans/Soc* 10(4):32-38, 1973.
Feldman, H.W. Street status and the drug researcher: Issues in participant observation. Washington, DC: Drug Abuse Council, 1974.
Feldman, H.W. A neighborhood history of drug switching. In: Weppner, R.S., ed. *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings.* Beverly Hills: Sage, 1977. pp. 249-278.

Feldman, H.W. Introduction: Background and purpose of the ethnographers' policymakers' symposium. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Services Research Branch. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980. pp. 1-7.

Feldman, H.W.; Agar, M.H.; and Beschner, G. *Angel Dust: An Ethnographic Study of PCP Users.* Lexington, MA: Lexington Books, 1979.

Feldman, H.W., and Biernacki, P. The ethnography of needle sharing among intravenous drug users and implications for public policies and intervention strategies. In: Battjes, R.J., and Pickens, R.W., eds. *Needle-Sharing Among Intravenous Drug Abusers: National and International Perspectives.* National Institute on Drug Abuse Research Monograph 80. DHHS Pub. No. (ADM)88-1567. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1988. pp. 28-39.

Feldman, H.W.; Mandel, J.; and Fields, A. In the neighborhood: A strategy for delivering early intervention services to young drug users in their natural environments. In: Friedman, A.S., and Beschner, G.M., eds. *Treatment Services for Adolescent Substance Abusers.* National Institute on Drug Abuse Research Monograph Series. DHHS Pub. No. (ADM)84-1342. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1984. pp. 112-128.

Fields, A.B. "Slinging weed": The social organization of street-corner marijuana sales. *Urban Life* 13(2-3):274-280, 1984.

Fields, A.B. Weedslingers: Young black marijuana dealers. In: Beschner, G., and Friedman, A.S., eds. *Teen Drug Use.* Lexington, MA: Lexington Books, 1986. pp. 85-104.

Finestone, H. Cats, kicks, and color. *Soc Prob* 5(7):3-13, 1957.

Hanson, B.; Beschner, G.M.; Walters, J.M.; and Bovelle, E. *Life with Heroin: Voices From the Inner City.* Lexington, MA: Lexington Books, 1985.

Hughes, E.C. The study of occupations (1959). *The Sociological Eye: Selected Papers.* New Brunswick, NJ: Transaction Books, 1984. pp. 283-297.

James, J. Prostitution and addiction: An interdisciplinary approach. *Addict Dis* 2:601-618, 1976.

James, J. Ethnography and social problems. In: Weppner, R.S., ed. *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings.* Beverly Hills: Sage, 1977. pp. 179-200.

Johnson, B.D.; Goldstein, P.J.; Preble, E.; Schmeidler, J.; Lipton, D.S.; Spunt, B.; and Miller, T. *Taking Care of Business: The Economics of Crime by Heroin Abusers.* Lexington, MA: Lexington Books, 1985.

Joint Committee of the American Bar Association and the American Medical Association on Narcotic Drugs. *Drug Addiction: Crime or Disease?* Bloomington, IN: Indiana University, 1961.

La Barre, W. *The Peyote Cult.* New Haven: Yale, 1938. (2nd ed. New York: Schocken, 1969.)

Lindesmith, A.R. *Opiate Addiction.* Bloomington, IN: Principia Press, 1947. (2nd ed. *Addiction and Opiates.* Chicago, IL: Aldine, 1968.)
Lindesmith, A.R. *The Addict and the Law.* Bloomington, IN: Indiana University, 1965.
Ludlow, F.H. What shall they do to be saved? *Harper's Magazine* 35:377-387, 1867.
Mandel, J., and Feldman, H.W. The social history of teenage drug use. In: Beschner, G., and Friedmann, A.S., eds. *Teen Drug Use.* Lexington, MA: Lexington Books, 1986. pp. 19-42.
Merton, R.K. *Social Theory and Social Structure.* Glencoe, IL: Free Press, 1957.
Moore, J.; Cerea, L.; Garcia, C.; Garcia, R.; and Valencia, F. *Homeboys.* Philadelphia: Temple University Press, 1974.
Murphy, S., and Rosenbaum, M. Money for methadone. II: Unintended consequences of limited-duration methadone maintenance. *J Psychoactive Drugs* 20(4):397-402, 1988.
Polsky, N. *Hustlers, Beats, and Others.* Chicago: Aldine, 1967.
Preble, E. Problems utilizing ethnography in a single state agency. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Services Research Report. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980a. pp. 90-100.
Preble, E. What an ethnographic field station looks like. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Services Research Report. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980b. pp. 64-71.
Preble, E., and Casey, J.J. Taking care of business: The heroin user's life on the streets. *Int J Addict* 4(1):1-24, 1969.
Rosenbaum, M. *Women on Heroin.* New Brunswick, NJ: Rutgers University Press, 1981.
Rosenbaum, M. *Surrender to Control: Women on Methadone.* Final report to the National Institute on Drug Abuse, unpublished manuscript.
Rosenbaum, M., and Morgan, P. Ecstasy use among professionals: An ethnography. Presented at the meeting of the American Society of Criminology, November 1988.
Rosenbaum, M., and Murphy, S. Not the picture of health: Women on methadone. *J Psychoactive Drugs* 19(2):217-226, 1987.
Rubin, V., and Comitas, L. *Ganja in Jamaica: A Medical Anthropological Study of Chronic Marihuana Use.* Scotch Plains, NJ: Mouton/ MacFarland, 1975.
Rudenko, S.I. *Frozen Tombs of Siberia: The Pazyryk Burials of Iron-Age Horsemen.* Berkeley: University of California, 1970.
Safford, W.E. Identity of cohoba, the narcotic snuff of ancient Haiti. *J Wash Acad Sci* 6:548-562, 1916.

Schultes, R.E. Peyote and the American Indian. *Nature* 30(3):155-157, 1937.
Schultes, R.E., and Hofmann, A. *The Botany and Chemistry of Hallucinogens.* 2nd ed. Springfield, IL: Charles C Thomas, 1980.
Shaw, C.R., and McKay, H.D. *Juvenile Delinquency and Urban Areas.* Chicago: University of Chicago, 1942.
Soloway, I.H. Methadone and the culture of addiction. *J Psychedelic Drugs* 6:1-99, 1974.
Spradley, J.P. *You Owe Yourself A Drunk: An Ethnography of Urban Nomads.* Boston: Little, Brown and Co., 1970.
Spradley, J.P. Foreword. In: Weppner, R.S., ed. *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings.* Beverly Hills: Sage, 1977. pp/ 13-16.
Sutherland, E.H. *Principles of Criminology.* 4th ed. Philadelphia: J.P. Lippincott, 1947.
Sutter, A.G. The world of the righteous dope fiend. *Issues in Criminology* 2(2):177-222, 1966.
Sutter, A.G. Worlds of drug use on the street scene. In: Cressey, D., and Ward, D.A., eds. *Delinquency, Crime and Social Process.* New York: Harper and Row, 1969.
Sutter, A.G. Playing a cold game: Phases of a ghetto career. *Urban Life and Culture* 1(1):77-91, 1972.
Vestal, P.A., and Schultes, R.E. *The Economic Botany of the Kiowa Indians as It Relates to the History of the Tribe.* Cambridge, MA: Botanical Museum, 1939.
Vidich, A.J. Participant observation and the collection and interpretation of data. *Am J Sociol* 60(4):354-360, 1955.
Von Hagen, V.W. *The Incas of Pedro de Cieza de Leon.* Norman, OK: University of Oklahoma, 1959.
Wallace, A.R. *Richard Spruce: Notes of an Ethnobotanist on the Amazon and Andes.* London: Macmillan, 1908. (Reprint, New York: Johnson Reprint, 1970.)
Walters, J. What is ethnography? In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Services Research Branch. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980. pp. 15-20.
Ward, A. *Confessions of An English Opium-Eater, by Thomas De Quincey.* New York: Signet, 1966.
Wassen, S.H. Anthropological survey of the use of South American snuffs. In: Efron, D.H.; Holmstedt, B.; and Kline, N.S., eds. *Ethnopharmacologic Search for Psychoactive Drugs.* National Institute of Mental Health. PHS Pub. No. 1645. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1967. pp. 233-289.
Wasson, R.G. *The Wondrous Mushroom: Mycolatry in Mesoamerica.* New York: McGraw-Hill, 1980.

Watters, J.K. Preventing human immunodeficiency virus contagion among intravenous drug users: The impact of street-based education on risk behavior. Presented at the Third International Conference on AIDS, Washington, DC, 1987.

Watters, J.K.; Cheng, Y.-T.; Lewis, D.; Jang, M.; and Carlson, J. Drug-use profile, risk participation, and HIV exposure among intravenous drug users in San Francisco. Presented at the Fourth International Conference on AIDS, Stockholm, June 1988.

Weppner, R.S., ed. *Street Ethnography: Selected Studies of Crime and Drug Use in Natural Settings.* Beverly Hills: Sage, 1977.

Wiebel, W.W. Combining ethnographic and epidemiologic methods in targeted AIDS interventions: The Chicago model. In: Battjes, R.J., and Pickens, R.W., eds. *Needle-Sharing Among Intravenous Drug Abusers: National and International Perspectives.* National Institute on Drug Abuse Research Monograph 80. DHHS Pub. No. (ADM)88-1567. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1988. pp. 137-150.

Wiseman, J., and Aron, M. *Field Projects for Sociological Students.* Cambridge, MA: Schenkman, 1970.

ACKNOWLEDGMENTS

This work was supported in part by U.S. Public Health Service grants 1 R18 DA 05879-01 and 1 R01 DA 05015-01 from the National Institute on Drug Abuse. Patrick Biernacki and Jerry Mandel reviewed various portions of this paper.

AUTHORS

Harvey W. Feldman, Ph.D.
Michael R. Aldrich, Ph.D.

Youth Environment Study, Inc., and
The MidCity Consortium to Combat AIDS
1779 Haight Street
San Francisco, CA 94117

128612

Methodological and Design Issues: Techniques for Assessing the Representatives of Snowball Samples

Karl M. van Meter

INTRODUCTION

The difficulty of studying hidden populations with traditional sociological research methods reveals a distinction between ascending methodologies adapted to the study of small or local populations and descending methodologies adapted to general populations. Snowball sampling and hierarchically ascending (HA) classification analysis and its more advanced form of cross-classification analysis together form a coherent and rigorous ascending methodology for studying hidden populations. This methodology's problems with the calculation of general population estimates and the explanation of variance have been the subject of recent research. Through the combined application of ascending and descending methodologies, recent work has largely resolved these problems and provided both a formal and operational tie between the two methodologies.

ASCENDING AND DESCENDING METHODOLOGIES

Explicit to the theme of this chapter is an association between the problem of studying social groups or types of social behavior that are not accessible by established means of sociological research ("hidden populations"), on the one hand, and, on the other, the use of research methodologies based on intensive, detailed data-collection strategies and their associated methods of analysis ("qualitative research designs"). Implicitly, this identifies established means of sociological research with large-scale representative surveys ("quantitative research") and opposes them to "qualitative" counterparts. Though the problem of studying hidden populations is real, as is the adequacy of intensive data collection strategies, such as snowball sampling, for studying hidden populations, the opposition of qualitative vs. quantitative is far more a social product of institutional conflict than a formalized mathematical reality (Combessie 1985; Wilson 1986). Indeed, most social researchers have abandoned this opposition, while acknowledging and

adapting the complementary nature of these different methodologies (Combessie 1986).

The difficulty of studying hidden populations does reveal an opposition between extensive survey methodologies and intensive data collection methodologies. This opposition distinguishes between what has been called descending methodology and ascending methodology (Van Meter 1985; Department of Social Psychiatry 1986; Kaplan et al. 1987). This opposition can be found in both data collection and methods of statistical analysis. Descending methodology involves strategies elaborated and executed at the level of general populations. It, therefore, necessitates highly standardized questionnaires and rigorous population samples and, for historic and economic reasons more than methodological considerations, usually involves traditional statistical analysis. This methodology typically has been used by national governments to make statistical inferences and to decide future social policy. The strict scientific rigor of this methodology, even in its most exemplary use, can be easily criticized (Guttman 1984), but this does not reduce the usefulness of its results. However, there are problems with descending methodologies, particularly when data are needed on or from hidden populations.

Ascending methodologies involve research strategies elaborated at a community or local level and specifically adapted to the study of selected social groups; for example, a hidden population. To be efficient, the means of data collection are usually selective and intensive. Snowball sampling, life histories, and ethnographic monographs are typical forms of data collection in ascending methodology. Methods of analysis in this type of methodology must also be adapted to the specific form of data furnished and also to the specific objectives of the research. Typical forms of ascending data analysis are social network analysis and classification analysis, often called "cluster" analysis.

However, the specific adaptation of ascending methodology is not obtained without the loss of easy generalizability. In exchange, descending methodology cannot reach hidden populations without specific adaptations. Indeed, that is exactly what the President's Commission on Organized Crime (1986) was proposing when it recommended oversampling high school dropouts and people without residence in the National Institute on Drug Abuse High School Senior Survey and Household Survey on Drug Abuse. Similarly, ascending methodology, such as network analysis, can be employed in the study of large populations, though at great material cost and rigorous standardization. The best example is Joel H. Levine's "Atlas of Corporate Interlocks" that covers the entire world population of corporations (Levine 1984).

CLASSIFICATION ANALYSIS

In the case of classification analysis, there is a very clear distinction between ascending and descending methods. The latter construct classes (or clusters) by starting with the entire population and successively dividing it in order to obtain a descending hierarchy of classifications: the first level will have the entire population included in one single class; the second level will have the population divided into two (or possibly more) classes; and the third and following levels will each have at least one new division resulting in a new class. This process continues until it reaches an optimum classification or the zero level, with each individual in its own unique class. Inversely, HA classification starts at the zero level (each individual in its own unique class) and constructs successive levels of classification by grouping similar individuals into the same class (Lerman 1981).

The classes obtained by HA classification are called polythetic classes, as opposed to monothetic classes (Lerman 1981). A class of individuals is defined as monothetic if it is characterized by one and only one characteristic. This means that an individual belongs to the class if and only if that individual has the given characteristic. A class G is defined as polythetic if and only if each individual belonging to G has an important (but not fixed) proportion of a certain subset B of characteristics, and if each characteristic belonging to B is present in an important (but not fixed) proportion of the individuals in G. A corollary to this definition is that an individual with many characteristics in B is not necessarily a member of G, and there may be members of G that have an important proportion of characteristics not in B. This formal definition of a polythetic class is the generalization of a Weberian ideal type and that given by the World Health Organization for a syndrome in the context of drug and alcohol abuse (Van Meter et al. 1987; World Health Organization 1981).

Those methods of classification analysis that are HA formally construct polythetic classes. HA, in this case, means that resemblance between individuals is used to form classes. Therefore, the first classes formed are composed of those individuals who resemble each other the most. The later the class is formed, that is, the higher in the hierarchy, the less its members will resemble each other. This also means that for HA methods, each level in a tree of classifications corresponds to a particular and distinct classification of the entire population under investigation with successively fewer but larger classes of individuals. The final classification or level in the hierarchy, of course, is composed of one unique class including the entire population.

A specific type of HA classification analysis called automatic classification (Lerman 1981) allows the application of minimal criteria, which provide a formal systematic means of reducing or summarizing the often considerable

list of variables or dimensions involved in typical ascending strategies of data collection. In automatic classification analysis, proximity between individuals or entities is measured by a similarity index calculated over all variables taken into consideration (Prod'homme et al. 1983). It allows us to construct classes based on the "maximum likelihood of association" ("vraisemblance de lien") between two individuals that emphasizes the resemblance between two classes more than the distance that separates each of them from nearby large classes of individuals (SAS Institute 1985).

The classifications based on interindividual resemblance are then arranged in an ordered hierarchy or tree by this method that also establishes, for each level of the hierarchy, the statistical probability or likelihood of the occurrence of that particular classification, given the original null hypothesis that all characteristics are normally distributed. The evolution of this local statistic, associated with each level of the hierarchy of classifications or classification tree, permits the identification of local minima where improbable, and, therefore, significant classifications are situated. By this means, one can identify within successive classifications the more significant groups of subjects or nodes that by their construction form polythetic classes. Moreover, this same process reveals the development and evolution of such significant groups throughout the hierarchical tree of classifications, starting with the first and most detailed classification (each individual constitutes his or her own class) and extending all the way to the final and roughest classification (everyone in one single class).

Cross-classification analysis is the Cartesian crossing of two automatic classification analyses of the same data. It reorganizes a two-dimensional data set (individuals by variables) by projecting onto the set the results of an automatic classification analysis of the individuals (or rows of figures) and of the descriptive variables (or columns of figures). In this manner, all initial data can be directly presented along with the organization of that data due to the cross-classification analysis, thus permitting comparative or critical analysis of the same data. This method was employed to find the significant nodes or polythetic classes mentioned above and to extend, in an unlimited fashion, the application of classification analysis to any finite population, no matter how large (Faugeron and Van Meter 1987).

This latter result is significant due to the fact that classification analysis of large data sets has been inhibited until now by its large requirements of computer time and memory capacity. Furthermore, the groups of units of individuals and variables found by cross-classification analysis meet the same criteria as the blocks obtained by block-modeling methods in social network analysis (Van Meter 1986). Cross-classification analysis was used, for example, as the methodological basis for a sociological survey of cocaine use in Europe (Kaplan et al. 1985).

APPLICATIONS OF CLASSIFICATION ANALYSIS

These groups of individuals and variables are of course polythetic classes and are rather stable units of analysis. This stability allows the construction of classification grids that characterize the most pertinent subset of variables for each group. This stability also permits the use of these groups in further cross-classification analyses, where the initial results concerning a "state" of behavior (the initial blocks of individuals and variables) are then submitted to a cross-classification analysis over a certain period in time. Thus, one can construct a polythetic class corresponding to a "syndrome," which would be a stable block generated by the cross-classification analysis of individual states crossed in chronological time. In turn, the individual states will have resulted from an initial cross-classification analysis of individuals crossed with descriptive variables. This particular adaptation of cross-classification analysis, along with the experience sampling technique of gathering repeated measurements of behavior and mental state at randomly selected moments in the daily life of an individual, form the ascending methodology of a current research project on anxiety and drug abuse at the University of Maastricht in Holland (Van Meter et al. 1987).

As with all methods of analysis, care must be taken in the application of classification analysis. Very evocative counter-examples exist of unclear results obtained by classification analysis, but they can become much more comprehensible when compared with results of another method of analysis of the same data. In this context, the complementariness of descending methods of analysis is quite useful. Often such classification results are the artifact of the algorithm of class construction, such as the chain effect due to the use of the lexicographic algorithm (Lerman 1981). In other cases, this algorithm is particularly well adapted to distinguishing the different classes in a data set, particularly when they have complex nonlinear forms (SAS Institute 1985).

The simultaneous use of classification analysis and a descending method, factorial correspondence analysis, has clarified certain problems of proximity in both sociological research (Le Guen and Jaffeux 1988) and in anthropology (Menk 1980). In the latter case, problems concerning differing proximities between various human populations were resolved by superposing an HA classification analysis upon a two-dimensional factorial diagram.

In terms of ascending methodology, not only are cross-classification and snowball sampling compatible, they are also theoretically integrated: one collects intensive data in a context of social contacts and seeks to establish the different types of individuals or behavior that are represented, and the other requires extensive descriptive data to calculate similarity, according to all variables, and to establish pertinent blocks of individuals and variables.

Kaplan et al. (1987) presented this clearly, while tying it to the historical development of snowball sampling and network analysis.

DRUG ABUSE AND THE ANALYSIS OF HIDDEN POPULATIONS

In an important review article on the analysis of drug abuse, Kozel and Adams (1986) present a critique of traditional medical epidemiology models, showing the unadapted nature of such descending methodology, though the authors do not use the term. Similarly, without using the terms polythetic or monothetic classes, they criticized the "attempt to classify drug-using behavior into one of two apparently distinct categories," (Kozel and Adams 1986, p. 970) while noting that current research tends to study "patterns of abuse." Although the usefulness of surveys for monitoring trends and prevalence of drug use is clearly indicated, they concluded with the statement that "drug epidemics often are localized and involve specific subpopulations that make surveillance based on national data systems difficult" (Kozel and Adams 1986, p. 970). This is an explicit indication that descending methodologies have encountered serious problems that ascending methodologies may be able to resolve.

Although these authors present the NIDA National Household Survey on Drug Abuse as the "single most important measure of drug abuse in our general population" (Kozel and Adams 1986, p. 971), they openly admit its limitations due to sample bias: transient and nonresidential populations are not included. The PCOC has noted this as well, stating that the Household Survey and the High School Senior Survey "have been criticized because they not include information from those populations that are frequently involved with drugs, high school dropouts and people without residence" (President's Commission on Organized Crime 1986, p. 340). As we mentioned above, the PCOC also proposed a solution, "oversampling," which implies an ascending methodology. The PCOC recognized that "surveys of cocaine users demonstrated there is no 'typical' cocaine user" (President's Commission on Organized Crime 1986, p. 25). They have also recognized that the necessary level of study, prevention, and treatment is the community (President's Commission on Organized Crime 1986).

The PCOC's specific criticisms of descending methodology include a lack of data on the prices and amounts of drugs purchased and on funding and resources for acquiring such information and the unreliable nature of "analysis by negotiation," with final estimates of drug use "resulting from a bargaining process among member agencies" involved in estimations (President's Commission on Organized Crime 1986, p. 343). As described by Hall (1988), these are the very lacunae that community epidemiology attempts to fill by focusing on the consequences of drug abuse rather than on the prevalence of use.

> For local purposes, it is often more useful to determine answers to the questions, "who" and "where," rather than "how many" so that limited resources may be most effectively applied to provide the greatest benefit. (President's Commission on Organized Crime 1986, p. 2)

"The snowball methodology for tracking drug abusers is ideal because of its intensive results and the ability to present findings rapidly" (President's Commission on Organized Crime 1986, p. 3). This may also be the specific methodology needed for monitoring the heterosexual spread of AIDS in the general population, since it is now recognized that AIDS as an "epidemic will be long and drawn out as it spreads through the different at-risk groups and in different localities over the coming decades" (Anderson 1987, p. 570).

SNOWBALL SAMPLING AND DRUG ABUSE IN ROTTERDAM AND MUNICH

In a recent study (Avico et al. 1988), snowball sampling was found to be economical, efficient, and effective in the study of cocaine incidence in three European cities: Rotterdam, Munich, and Rome. In the context of this study, Emile Durkheim's concept of social "milieu," instead of "social strata," imposed itself as the means of describing other drug users (Avico et al. 1988, p. III-7). Plotting the incidence curves of cocaine use (year of first use) in Munich and Rotterdam for the last 15 years indicated that cocaine is not at all a new drug in these two cities. Further, the Munich data suggested that the supposed threat of a European cocaine epidemic, in this case, is the result of discrepancies between endemic cocaine use and changes in police strategies of seizure and arrest data. Similar discrepancies can be found in some of the official U.S. Government data.

Incidence curves of Rotterdam and Munich further suggest a certain convergence of the cocaine consumption situation in these two cities, with current levels tending toward a level of 15 to 20 percent of the total user population. However, the snowball-sampling data show that the history of cocaine consumption is rather different. The first real cocaine epidemic in Rotterdam took place in the early 1970s when Munich had no noticeable consumption. As the epidemic in Rotterdam subsided, an epidemic in Munich began. Another such cycle followed before arriving at the current situation.

This study produced clear estimates of the prevalence and distribution of certain patterns of drug use. Analyzed according to job milieu, the data revealed some interesting similarities and differences. Although Munich is recognized as a cocaine "glamour" city, the prevalence among artists and actors in Rotterdam at 29 percent is slightly higher than in Munich at 25 percent, though this difference is not likely to be statistically significant.

Nonetheless, a quarter of cocaine users are artists, even in a workers' city such as Rotterdam, an interesting phenomenon for further research. Common patterns were also found in the pimp and prostitute milieu (about 5 percent), the campus milieu (about 15 to 18 percent), and the blue-collar-worker milieu (about 20 percent). The latter is particularly interesting, in that it proves that cocaine is not only a luxury, high-status drug. As in the United States, cocaine use has spread to youth and working and lower classes, becoming more evenly distributed throughout the entire society in these two cities.

The differences between the two cities are also of interest. Munich, being an entertainment and "yuppie" city, has a much higher prevalence of drug abuse in the restaurant and cafe milieu (15 percent) and the white-collar milieu (20 percent) than Rotterdam (4 and 7 percent, respectively). This is further supported by the high prevalence of unemployed users in Rotterdam (18 percent), a category that does not even appear in the Munich data. In Rotterdam, use among working and lower classes is indeed at least as prevalent as among those in the "glamour" class. This is consistent with the image of Rotterdam as a hard-working city.

One of the specific omissions mentioned by the President's Commission on Organized Crime in general survey data is filled by "cost per gram" information (given here in Dutch guilders). In Rotterdam, 75 percent of the sample pay less than DFL 200, while in Munich the proportion is merely 2 percent. Most Munich users pay between DFL 200 and DFL 250 per gram, while only 11 percent of Rotterdam users pay that equivalent price. This pattern continues beyond the DFL 250 level. The explanation of this price differential is complex, but it probably has to do with the different "character" of each city's work force and the relatively lower demand in Rotterdam in terms of lower absolute numbers of active cocaine users.

Both cities have a pattern of problems engendered by cocaine use. The majority of users in both cities report some kind of problem. Most of these were restricted to private difficulties in social life and at work. However, the prevalence of nonproblematic use is higher in Rotterdam (39 percent) than in Munich (25 percent). An interesting phenomenon to explore would be whether problem usage is related to job milieu and cost factors.

PROBLEMS OF EXTRAPOLATION AND CURRENT RESEARCH

Questions of bias are always present in any data collection strategy and are particularly acute in the study of hidden populations. Random error or sampling error, sample bias, and response bias are major sources of inaccuracy in data collection. The first is usually dealt with by increasing the sample size: in accordance with well-known statistical formulas, the larger the sample, the smaller the random-sampling error. But this may vary with types of data collection. Response bias is also dealt with through

traditional means such as split samples, rearrangements of items within the same questionnaire, or indepth interviews. Sample bias is often considered a more serious problem. Indeed, much of the criticism mentioned above concerning the High School Senior Survey and the Household Survey concerns sample bias. Descending strategies of data collection can often miss whole hidden populations, rendering accurate estimations of drug use extremely difficult.

If ascending strategies, particularly snowball sampling, offer a solution to the problem of data collection among hidden populations, the formalization of their sample biases seems to be beyond the reach of current statistics, although admirable attempts have been made (Rapoport 1979; Frank 1979; Frank 1981). But these formal treatments of the question usually require further general information on the population such as the mean number of ties between individuals and the distribution of ties throughout the population, including the number of triads and the number of diads. Once again, the solution being proposed is to revert to better descending strategies of data collection instead of addressing directly what has been called "the limitations of this technique [snowball sampling] involve an uncontrollable selection bias that limits the external validity of the sample" (Kaplan et al. 1985, p. 3).

In snowball sampling, selection can be modeled as a stochastic process that permits the calculation of sample weights that are inversely proportional to the probabilities of selection and can generate unbiased estimates (Avico et al. 1988). Sample selection bias formulated in this manner offers a reliable sampling frame, particularly when data from initial respondents, other than selections of other respondents, are not recorded and subsequent respondents are selected at random (Kaplan et al. 1987). Another complementary manner of selection is to select respondents according to a quota system of sampling, thus assuring an unbiased coverage of a hidden population according to the structure of the parent or general population. This strategy was used in recent research on the spread of AIDS in the male homosexual population in France (Pollak and Schiltz 1988). Moreover, this snowball sample (n=300) was used as a control for a general survey of male homosexuals (n=1,557) who read gay periodicals. The snowball sample respected standard quotas for age, class, and size of town of residence corresponding to the French male population from 18 to 55 years of age. The only significant difference found was that readers of homosexual literature changed their sexual behavior due to the threat of AIDS earlier than nonreaders by an average of 6 months. The same authors have published other works in this field using combinations of descending and ascending methodologies (Pollak and Schiltz 1987a; Pollak and Schiltz 1987b); in this specific case, a general typology of male homosexuals and their attitudes toward AIDS, a typology based on HA classification analysis and factorial correspondence analysis, was complemented by numerous indepth, extensive, semidirective interviews.

Another problem often associated with ascending methodologies is calculating variance. Biernacki and Waldorf (1981) have addressed the problems of basic procedures of snowball sampling and called for further research on such questions as variance adequacy, explicitly stating that contributions from both qualitative and quantitative methodologies are necessary (Avico et al. 1988). If these qualifiers are changed to ascending and descending methodologies, there is thorough agreement between these researchers (Biernacki and Waldorf 1981; Avico et all. 1988) and some indications of recent research developments in the very direction they have indicated can be offered. Le Guen and Jaffeux (1988) have completed a survey of small and medium-size French businesses quoted on the stock market. They used not only automatic classification analysis to establish distinct classes of businesses, but also confirmed this analysis through the use of factorial correspondence analysis. Using these classes, they then plotted the means for each class, permitting a decomposition of the means and variance according to specific classes that had been constructed in an ascending manner based on similarity. This is indeed quite different from the traditional descending manner of decomposing variance over an entire population. Moreover, this complementary association of ascending and descending methodologies offers a reliable way of situating specific portions of the variance in distinct classes or hidden populations, while permitting the calculation of general population estimates. This seems to be a particularly happy marriage of ascending and descending methodologies.

CONCLUSION

Although the distinction between qualitative and quantitative methodologies is not as clear as it once was, the difficulty of studying hidden populations reveals a distinction between ascending and descending methodologies. If the former are well adapted to the study of hidden populations, as is the case with snowball sampling and cross-classification analysis, problems have been found with their handling of sample bias, of estimates at the level of the general population, and of variance explanation. Examples of recent research employing complementary combinations of ascending and descending methodologies show ways of resolving these problems and suggest interesting new perspectives in an integrated methodological approach to social science research.

REFERENCES

Anderson, A. President's new AIDS commission in turmoil. *Nature* 329:6140, 1987.

Avico, U.; Kaplan, C.D.; Korczak, D.; and Van Meter, K.M. Cocaine epidemiology in three European community cities: A pilot study using a snowball sampling methodology. Research Report of the Cocaine Steering Group, Health Directorate Commission of the European Communities, Brussels, Belgium, February 1988. 110 pp.

Biernacki, P., and Waldorf, D. Snowball sampling: Problems and techniques of chain referral sampling. *Soc Methods Res* 2:141-163, 1981.
Combessie, J.-C. Oubli et paradoxes. *Bull Methodologie Sociol* 6:29-38, 1985.
Combessie, J.-C. A propos de méthodes: Effects d'optique, heuristiques et objectivation. *Bull Methodologie Sociol* 10:4-24, 1986.
Department of Social Psychiatry. Cross-classification analysis. Presented at the Experience Sampling Symposium, University of Maastricht, 1986. 13 pp.
Faugeron, C., and Van Meter, K.M. Analysis of deviance and of social classes: The impact of methodological research. Presented at the Annual Meeting of the American Sociological Association, Chicago, IL, August 17-20, 1987. 15 pp.
Frank, O. Estimation of population totals by use of snowball samples. In: Holland, P.W., and Leinhardt, S.L., eds. *Perspectives on Social Network Research*. New York: Academic, 1979. 319-347 pp.
Frank, O. A survey of statistical methods for graph analysis. In: Leinhardt, S.L., ed. *Sociological Methodology*. London: Jossey-Boss, 1981. 110-155 pp.
Guttman, L. What is not what in statistics. Statistical inference revisited—1984. *Bull Methodologie Sociol* 4:3-35, 1984.
Hall, J.N. The community-based drug epidemiology network. Brussels, Belgium: Health Directorate of the Commission of the European Communities, 1988. 23 pp.
Kaplan, C.D.; Korf, D.; and Sterk, C. Temporal and social contexts of heroin-using populations: An illustration of the snowball sampling technique. *J Nerv Ment Dis* 175(9):566-574, 1987.
Kaplan, C.D.; Van Meter, K.M.; and Korczak, D. Estimating cocaine prevalence and incidence in three European community cities. Luxembourg: Commission of the European Communities, 1985. 12 pp.
Kozel, N.J., and Adams, E. H. Epidemiology of drug abuse: An overview. *Science* 234:970-974, 1986.
Le Guen, M., and Jaffeux, C. Le conjunction analyse de données et statistique inferentielle pour conduire à une meilleure perception visuelle. France: University of Orléans, 1988. 26 pp.
Lerman, I.C. Classification et Analyse Ordinale des Données. Paris: Dunod, 1981. 350 pp.
Levine, J.H. *Atlas of Corporate Interlock*. Hanover, NH: Worldnet, 1984.
Levine, J.H. The methodology of the Atlas of Corporate Interlocks. *Bull Methodologie Sociol* 17:20-58, 1988.
Menk, R. L'analyse des clusters en anthropologie physique. Une nouvelle methode: Les dendogrammes tridimensionnels. *Archives Suisses d'Anthropologie Generale* 44(1):51-59, 1980.
Pollak, M., and Schiltz, M.-A. *Une Épidémie Autogérée: Les Homosexuelles Face au SIDA*. Tome 1. Paris: EHESS, 1987a. 119 pp.
Pollak, M., and Schiltz, M.-A. *Annexes Techniques et Statistiques*. Tome 2. Paris: EHESS, 1987b. 152 pp.

Pollak, M., and Schiltz, M.-A. Does voluntary testing matter? How it influences homosexual safer sex. Presented at the Fourth International Conference on AIDS, Stockholm, Sweden, June 13, 1988. 14 pp.

Prod'homme, A.; Breton, L.; and Guenneguez, L. Note méthodologique sur l'utilisation de l'algorithme de vraisemblance des liens en sciences humaines. In: *Actes des Journées de Classification.* Brussels: Société francophone de Classification, 1983. 367-383 pp.

President's Commission on Organized Crime. *Report to the President and the Attorney General. America's Drug Habit: Drug Abuse, Drug Trafficking, and Organized Crime.* Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1986.

Rapoport, A. A probabilistic approach to networks. *Soc Netw* 2(1):1-18, 1979.

SAS Institute. *User's Guide: Statistics.* Version 5 ed. Cary: SAS Institute, 1985.

Van Meter, K.M. Block-modelling and cross-classification techniques for estimating population parameters from network data. Workshop on the Methodology of Applied Drug Research, European Community Health Directorate, Luxembourg, 1985.

Van Meter, K.M. Basic typology and multimethod analysis in the social sciences. Presented at the Eleventh World Congress of the International Sociological Association, New Delhi, August 18-22, 1986.

Van Meter, K.M.; De Vries, M.W.; Kaplan, C.D.; and Dijkman, C.I.M. States, syndromes, and polythetic classes: The operationalization of cross-classification analysis in behavioral science research. *Bull Methodologie Sociol* 15:22-38, 1987.

World Health Organization. Nomenclature and classification of drug and alcohol related problems: A WHO memorandum. *Bull WHO* 59(2):225-242, 1981.

Wilson, T.P. Qualitative 'versus' quantitative methods in social research. *Bull Methodologie Sociol* 10:25-51, 1986.

ACKNOWLEDGMENTS

Dr. Marc Morival, Chairman of the Cocaine Steering Group of the Health Directorate of the Commission of the European Communities is acknowledged for initiating and directing many of the research projects mentioned in this chapter. Professor Charles D. Kaplan of the Erasmus University of Rotterdam is acknowledged for recognizing the contribution that new methodologies have to offer in the study of drug use and for getting the author directly involved in research in this field.

AUTHOR

Karl M. van Meter
Secretary of the Research Committee on
Logic and Methodology
International Sociology Association
54 boulevard Raspail
75006 Paris
FRANCE

128613

Requirements for Inductive Analysis

Peter Adler

INTRODUCTION

Sociological ethnography is currently experiencing a renaissance. For the first time in decades, epistemological and methodological issues are being addressed that will shape the way ethnography is practiced and applied. This chapter will look at the concept of "induction," tracing its history and use in the social sciences, outlining some of the major tenets and presuppositions that an inductive methodology must employ, assessing how the concept of induction has been either changed or misunderstood over the last several decades, and exploring how induction will shape social scientific methodology in coming decades.

Analytic induction as an epistemological underpinning of sociological methodology can be credited to students from the University of Chicago, where the method of participant observation first took hold and the theory of symbolic interaction was formulated. The midtwentieth century saw a great number of masters' and doctoral theses generated by students of Everett Hughes and Herbert Blumer, two of the major proponents of naturalistic inquiry. These men helped to create the richly textured "portrait of the people" that good ethnography represents. Covering a wide variety of subjects, mainly community, race and ethnic relations, deviance and criminology, sociology of work, and indepth looks at the growing subcultures in American society, these researchers helped to define early American empirical sociology. As per Blumer's (1969) edict, students entered the everyday world and found out about the subjective meanings, perspectives, understandings, and salient issues in urban Chicago.

Trying to find their niche in a discipline dominated by Columbia, Harvard, and Wisconsin (Vidich and Lyman 1985), these midcentury ethnographers battled against the tide of positivism and quantification. It is not surprising that induction, a philosophy and technique rooted in a "scientistic" nomenclature and procedure, was wedded to the emerging, but largely unaccepted, method of participant observation. To gain any acceptance, practitioners of participant observation needed to speak the language of their positivistic colleagues, while following a subjective, naturalistic methodology.

Most scholars today emphasize induction's subjective, impressionistic, and "grounded" features. Although it was formulated in direct opposition to the objective, positivistic, deductive methodologies, it adhered strongly to the canons and principles of the scientific method. Not until recently has induction come to represent the subjective, dynamic process most people now associate with it.

DEFINITIONS

There is no easy, simply stated definition of analytic induction. Perhaps because it is rarely practiced and is associated with philosophical, rather than methodological issues, several definitions have emerged.

Manning stated that "analytical induction is a nonexperimental qualitative sociological method that employs an exhaustive examination of cases in order to prove universal, causal generalization" (Manning 1982, p. 280).

Katz suggested:

> . . . the fundamentals of analytic induction can be stated simply. The researcher is committed to form a perfect relation between data and explanation. When encountering a "negative case"—evidence contradicting the current explanation—the researcher must transform it into a confirming case by revising the definition of either the explaining or the explained phenomenon. The researcher is enjoined to seek negative cases and the resulting opportunity to modify the explanation. (Katz 1983, p. 130)

Strauss claimed that:

> . . . induction refers to the actions that lead to discovery of an hypothesis—that is, having a hunch or an idea, then converting it into an hypothesis and assessing whether it might provisionally work as at least a partial condition for a type of event, act, relationship, strategy, etc. (Strauss 1987, pp. 11-12)

Emerson, in probably the clearest summary statement, stated that proponents of analytic induction:

> . . . begin with a rough formulation of the phenomenon to be explained and an initial hypothesis explaining the phenomenon, then go to a small number of cases (even a single case) to see if the hypothesis fits that case. If not, either the hypothesis or the phenomenon to be explained is reformulated so that the case is accounted for. The procedure then continues, with the researcher examining cases and producing reformulations of ...

> these sorts whenever negative cases are encountered, until *all* cases can be explained. (Emerson 1983, p. 97)

In every definition, the researcher is asked to create generalizations that apply to *all* instances of the problem. Unlike multivariate analysis, in which generalizations apply to most of the cases (statistical significance), induction requires that all instances of the phenomenon be explained. It is assumed that any scientifically grounded proposition must be stated as a universal (Denzin 1970). Necessarily, then, it is critical that every single case be examined and fit into the developing theory. If not, the theory must be revised to incorporate even the single negative case. Thus, analytic induction promises perfect correlations and universal explanations, instead of probabilistic findings (Katz 1983).

Induction begins with a hypothesis to be tested. Much qualitative research that passes as induction claims that research should not start with a proposition or hypothesis to test. According to its early proponents, however, analytic induction requires an originating hypothesis, which can be revised as data are analyzed, providing induction with an openness that other methods do not have. "In practice, one does not begin with an hypothesis and then encounter exceptions one by one. Instead, one begins with multiple hypotheses and is confronted with a mass of hostile evidence" (Katz 1983, p. 132). The strength of the method is its ability to consider many alternatives and then evolve dynamically as opposing or corroborating evidence appears.

HISTORY

In the social sciences, most methods textbooks or discussions of epistemology devote little coverage to analytic induction. It is either completely ignored or briefly addressed before it is discarded in favor of hypothetico-deductive models of reasoning. Justification for an inductive form of analysis dates back to Hume, and it has held considerable sway in philosophical debates on epistemology for many centuries (Kaplan 1964). It was not until Florian Znaniecki's work, however, first in his article, "Social Research in Criminology" (1928), and later in the more recognized source, *Method of Sociology* (1934), that the first systematic statement of analytic induction in the social sciences appeared. By this time, the sociology department at the University of Chicago was committed to a study of human life that emphasized subjective elements either by studying people *in situ*, or by closely examining human documents (i.e., diaries, letters, autobiographies, newspaper files). In line with the research done by Znaniecki and his collaborator, W.I. Thomas, in their five-volumes *The Polish Peasant in Europe and America* and in Thomas' work, *The Unadjusted Girl* (1923), analytic induction sought to correct failures of

earlier methods in dealing with subjective, concrete, and experimental dimensions of personal and group life (Manning 1982).

Others at the University of Chicago debated a case history/human documents approach vs. a statistical/enumerative approach. In the late twenties and early thirties, several innovators attempted to combine the approaches, using case history and personal documents to give life to the antiseptic statistics they were collecting. In the area of delinquency, Frederick Thrasher's (1927) study of delinquent gangs, Clifford Shaw's (1930; Shaw 1931; Shaw 1938) series of works on delinquent boys, and Walter Reckless' (1929) *Six Boys in Trouble* used both forms of analysis. Ernest Burgess, a key figure at Chicago, used both in his study of marriage (Burgess and Cottrell 1939; Burgess and Locke 1945), although he clearly preferred the case history. Samuel Stouffer, later to become a proponent of statistical sociology, asked students to write autobiographies, which he used in his dissertation. Stouffer found, however, that the analysis of life histories was a time-consuming task and, in his opinion, did not contribute much to the statistical sampling he was developing. Given its advantages in time and energy and the perception that life histories were tedious and relatively unproductive, the statistical/enumerative method took stronger hold on American social scientists, while the case history method continued, but without the force of the twenties (Manning 1982).

From the time Znaniecki's book appeared in 1934 until the early 1950s, induction received little attention in the sociological literature on methodology. The only major study to use this technique was Robert Cooley Angell's *The Family Encounters the Depression* (1936), until three important, now classic, works appeared: Alfred Lindesmith's *Opiate Addiction* (1947), Donald Cressey's *Other People's Money* (1953), and Howard Becker's "Becoming a Marijuana User" (1953). At about the same time, several sociologists began to debate the relative strengths and weaknesses of induction. Spurred by W.S. Robinson's (1951) article, "The Logical Structure of Analytic Induction," arguments debating the utility and limitations of this method followed as comments and rejoinders to Robinson's propositions (Turner 1953; Lindesmith et al. 1952; Angell and Turner 1954). Despite their publication in a major sociology journal, not much came of these debates. Recently, Martin Bulmer (1979) mentioned induction, Peter Manning (1982) wrote a comprehensive statement on the subject, and Jack Katz (1982) attempted a revision of induction in his book, *Poor People's Lawyers in Transition*, and, in an amended version, in Robert Emerson's *Contemporary Field Research* (1983). Still, as Katz stated, "Since Turner's (1953) review of the handful of studies then recognized as examples of induction, there has been virtually no explicit discussion of the method" (Katz 1983, p. 132).

Grounded theory, first proposed in 1967 by Barney Glaser and Anselm Strauss, claims many similarities with analytical induction; the two are often

considered the same. Reacting to overemphasis on statistical measures, Glaser and Strauss developed a theory of methods that allowed the researcher to continuously modify propositions as they fit the data. Both induction and grounded theory use the logic of discovery rather than the logic of justification, proceed from the data collected to provide a social theory, and believe in studying people in their natural environments as much as possible. Differences exist:

> Analytic induction seeks to produce a total explanation of the phenomena observed in a way that responds to the requirements of verification, [w]hereas grounded theory separates the processes of explanation and verification in theory in order to pursue the former more intensely. (Emerson 1983, p. 99).

Strauss recently stated:

> Many people mistakenly refer to grounded theory as "inductive theory" in order to contrast it with the theories of Parsons or Blau. But as we have indicated, all three aspects of inquiry (induction, deduction, and verification) are absolutely essential. (Strauss 1987, p. 12)

Thus, grounded theory has been misunderstood and equated with induction, although grounded theory originally viewed induction as only a subset of a larger methodological strategy.

The history of induction shows that it has been underused, misunderstood, confused with other qualitative approaches, and unfavorably compared with more positivistic, deductive methods. Most sociologists who teach methods courses incorrectly use the term induction for all forms of qualitative, naturalistic, and/or participant-observation research. In its original form, the tenets and suppositions of induction are more rigorous and encompassing than acknowledged today. In fact, its goals are stricter than those of most statistical measurements.

STEPS REQUIRED TO PERFORM ANALYTIC INDUCTION

The requirements of analytic induction follow the procedures used by Cressey (1953).

(1) A rough definition of the phenomenon to be explained is formulated.

(2) A hypothetical explanation of that phenomenon is formulated.

(3) One case is studied in light of the hypothesis, with the object of determining whether or not the hypothesis fits the facts in that case.

(4) If the hypothesis does not fit the facts, either the hypothesis is reformulated or the phenomenon to be explained is redefined so that the case is excluded.

(5) Practical certainty may be attained after a small number of cases have been examined, but the discovery of negative cases disproves the explanation and requires a reformulation.

(6) The process of examining cases, then redefining and reformulating hypotheses is continued until a universal relationship is established, each negative case calling for redefinition or reformulation.

Lindesmith's (1947) study of opiate addiction illustrates these points. Lindesmith began with the hypothesis that if an individual did not know what drug he or she was receiving, he or she would not become addicted. When Lindesmith found a case in which a doctor received morphine for several weeks in full cognizance, but remained unaddicted, the initial hypothesis had to be reformulated. The subsequent hypothesis stated:

> Persons become addicts when they recognize or perceive the significance of withdrawal symptoms which they are experiencing, and if they do not recognize withdrawal distress they do not become addicts regardless of any other considerations. (Lindesmith 1947, p. 8)

This reformulation, while more salient than the original, also proved to be inadequate to explain all cases. More cases appeared in which people experienced mild withdrawal and understood it to be withdrawal distress, but did not use the drug to alleviate it and never became addicts. Finally, Lindesmith's hypothesis was changed to include people who had knowingly taken the drug, recognized the withdrawal symptoms as a product of the drug's absence, and defined the drug as the means of alleviating the distress. In this example, each reformulation of the hypothesis was predicated on the earlier ones, redefining the previous attempts until the theory fit all of the cases Lindesmith observed. By searching for negative cases, these procedures offer us a theory that is capable of universal applicability and an accuracy that even the most sophisticated statistical test cannot match.

ADVANTAGES AND DISADVANTAGES

Many authors have recognized that analytic induction represents a unique process for testing and generating theory. While it has not yet become a dominant form of inquiry in the social sciences, it provides many advantages over other approaches.

Advantages

(1) A broad advantage is that analytic induction makes it possible to test theories by juxtaposing one against another. For example, while attempting to test for the sociological significance of causality, one might also test other theories from other disciplines that are currently in vogue.

(2) Analytic induction provides a method by which established theories can be revised and incorporated into new theories as negative evidence is presented. The refinement of theory from one hypothesis to the next allows the researcher greater latitude than do deductive methods.

(3) Since the method emphasizes the negative case, there must be a close relationship between observation and theory. To find and test all cases, the researcher must be intimately familiar with case characteristics and conditions or events from which cases can be identified. While many other methods stress a "double-blind" type of relationship between the researcher and subjects (i.e., the researcher is deliberately not informed as to whether a subject is considered a "case" or "not a case"), analytic induction requires that the researcher be thoroughly immersed in case characteristics and subjects of study, thus, as Cicourel (1964) would argue, eliminating the potential for artificial constraints between the researcher and study subjects.

(4) Analytic induction allows the researcher to move from substantive, or middle-range theories, to formal theories. For example, given the close attention to negative cases, generalizations can more easily be made from the substantive area under study to other similar areas (i.e., from drugs to alcohol or from embezzlers to other kinds of criminals).

(5) Analytic induction leads to dynamic theories, as opposed to the static theories generated by most hypothetico-deductive methodology. Sequencing, temporality, and logical order are typical of a theory of process rather than a simple cause-effect model. For example, Cressey discovered a process that one must go through to legitimize embezzling, a sequence of events that would not have been discovered otherwise.

Manning has summarized the advantages as follows:

> Many of the strengths of qualitative methods characterize analytic induction as well—they both seek to capture the nature of the empirical world firsthand, often through the subject's perspective, and seek to minimize the significance of

preconceived stances toward the world in the form of previously sanctioned concepts, models, axiomatic approaches, and hypotheses. (Manning 1982, p. 290)

Disadvantages

Opponents of analytic induction have pointed to several major deficiencies:

(1) Analytic induction is unable to predict. Ralph Turner (1953), in the most important critique of this method, has argued that induction does not allow us to know which individuals will engage in a particular behavior. In assessing both Lindesmith's and Cressey's work, Turner claimed that there is no way to predict who will become an addict or a trust violator. It is nearly impossible, using inductive techniques, to establish causality.

(2) Analytic induction is too simplistic. By focusing heavily on negative examples and universals, it eliminates the complexity and gray areas of human social life. There are no partial answers, there are no degrees, since the theory must fit all cases. Common sense tells of exceptions to the rules, minor perturbations, and other deviant cases, which do not necessarily mean that the general theory is not applicable. By speaking in general and probabilistic terms, statistical tests of significance allow for a greater diversity and plurality of behaviors.

(3) Analytic induction is a somewhat inefficient method. Like most qualitative data collection and analysis, the level of involvement and difficulty and the time required by firsthand immersion in the social worlds of the subjects are consuming and exhausting. The search for negative cases and the involvement this requires are incompatible with a publish or perish society. Analytic induction is not a quick and easy method; therefore, many people are discouraged from using it. Despite the relatively few cases usually studied by inductionists, the time it takes to analyze the data is greater than that needed for an experiment or survey.

(4) Analytic induction makes generalizations from relatively small samples. Unlike survey research, in which thousands of cases can be studied, only a few (i.e., Lindesmith studied 50 to 60 people, Cressey studied 133, and Becker about 50) are used in inductive analysis. Those methodologists who are not comfortable with sample sizes of this order claim that the theories generated are spurious, because universal statements cannot be based on a small number of cases.

Several features of analytic induction, therefore, make it unattractive to many. Given the time it takes and the problems associated with any study

based on a few dozen people, the inability to produce definitive causal definitions or propositions makes it risky. In a discipline dominated by positivists who favor close adherence to scientific canons, these risks may not be worth the effort, since one can expect to encounter more difficulty publishing this sort of research than more traditional forms.

ANALYTIC INDUCTION TODAY

Despite the paucity of studies that have used analytic induction as it was originally proposed, most qualitatively oriented social scientists still give lip service to its primary tenets. As mentioned, it is often used synonymously with the terms qualitative methodology, grounded theory, symbolic interactionism, ethnography, and participant observation. Since they do not begin with preconceived notions or hypotheses, many proponents of naturalistic inquiry err when they claim to use induction as the epistemological underpinning of their research. Nevertheless, although they have stretched the premises proposed by Znaniecki, Robinson, Cressey, and Lindesmith, researchers who refer to the new form have much in common with their ancestors.

As Katz has pointed out, there are few, if any, perfect explanations of anything.

> The test is not whether a final state of perfect explanation has been achieved but the *distance* that has been traveled over negative cases and through consequent qualifications from an initial state of knowledge. Analytic induction's quest for perfect explanation, or "universals," should be understood as a strategy for research rather than as an ultimate measure of the method. (Katz 1983, p. 133)

In other words, if we can borrow some of the rigor of Znaniecki's approach, adhering to his doctrine of close attention to personal documents and naturally occurring events, without being fanatically concerned with all negative cases and perfect adherence to theory, a more humanistic, reliable, and valid social science will emerge.

Most teachers of field methods today suggest that their students remain open and sensitive to members' perspectives. Rather than recommending the procedures of orthodox analytic induction, these professors ask their students to remain open to concepts, before forming any hypotheses. The researcher is instructed to go out into the field, responsive but without preconceived notions beyond their own cultural biases and understandings (Bogdan and Taylor 1975; Hammersley and Atkinson 1983; Lofland and Lofland 1984; Schatzman and Strauss 1973). This form of "retroduction," a more descriptive term than induction, portrays field method epistemology as a "double-fitting" or alternating shaping of observation and explanation

rather than an *ex post facto* discovery of explanatory ideas (Baldamus 1972). Instead of beginning with a hypothesis or research problem, field-workers retroactively develop ideas, concepts, or hypotheses after spending time in the setting. As researchers are immersed in the setting, they develop ideas. Insights, hunches, and generative questions come from both observations and the researcher's personal experiences and knowledge of the social world (Douglas 1976). From personal experiences come the ideas that eventually become hypotheses. Modern-day "retroductionists" have accepted subjective experiences, biases, personal history, and close involvement with subjects as beneficial to the process of research (Adler and Adler 1987). Theories are generated from these personal involvements and personal histories, which inform who is studied, how they are studied, and what is seen. This is probably true of all social scientists, although more objectivist practitioners may be reluctant to admit it.

Given the subjective nature of the data collected, how is the credibility of the data assessed? First, it is the very closeness to the data, the intimate knowledge of the scene, that gives the ethnographer an ability to make judgments about the people, behavior, and events observed (Bittner 1983). Second, both Douglas (1976) and Wiseman (1970) have suggested various forms of "interaction effectiveness," whereby the researcher's accounts can be verified by others trying to pass as members of the group under study. Third, as Rochford (1985) did in his study of Hare Krishnas, some researchers take their findings to the members for verification. This "member validation" (Gould et al. 1974) serves as a check on the researcher's bias. Ultimately, however, given the requirements of this type of research, in which the researcher attempts to get as close as possible to those studied, the ultimate assessment comes from the researcher's conveying accurately what was observed and allowing the social scientific community to judge the work on those grounds.

Recently, new forms of ethnography have emphasized the personal and subjective aspects of the research process, placing less emphasis on scientific constructs or on objective data. Termed "opportunistic research" (Riemer 1977), "auto-ethnography" (Hayano 1979), "systematic introspection" (Ellis 1987), or "post-modern" ethnography (Denzin 1989), all of the new forms of retroduction emphasize feelings, emotions, personal insights, and reflective analysis.

What are the requirements for this sort of research? Close personal experience and involvement with the subject population are important. These guided the work of the early inductionists, and it is no coincidence that most of their studies were of "hidden populations." No other methodology could examine a group that is primarily unknown, with considerable risk attached to the researcher-researched relationship, small numbers of available people, and members that are, more often than not, highly suspicious of outsiders. To do this research, we must approximate, as much as possible,

the natural attitude and position of the members. Studies that do not, but remain distanced from the people being studied, will not yield the dynamic data characteristic of an inductive analysis. Rich data of this sort, as Emerson (1983) has argued, are the product of long periods of time in the setting, a multiplicity of observations and analyses, and many different procedures. Such data have been subjected to literally thousands of everyday life tests, as the fieldworker is immersed in the setting.

As I have argued elsewhere (Adler and Adler 1987), the subjective nature of our methodology should be the primary requirement for doing retroductive analysis. With the help of new social researchers (Douglas 1976; Johnson 1975; Wax 1971), ethnographers are coming of age and revealing more than ever before. As Robert Emerson so forcefully and astutely argued in the introduction to his *Contemporary Field Research* in 1983, ethnographers must become more self-conscious for us to grow and learn. In the last 5 years, many researchers have allowed us to feel, emote, and reflect with them, giving a deeper and more realistic image of life in the field. Krieger's (1985) admission of her lesbianism, Forrest's (1986) sensitive analysis of studying psychics, Horowitz's (1986) experiences with Chicano gang members, Peshkin's (1984) abhorrence for the fundamentalists he studied, and Rochford's (1985) techniques for studying Hare Krishnas, are examples of this new self-honesty. If this trend continues, field research will continue to grow; future ethnographers will be sensitive to their positions in the field; and fieldwork accounts and natural histories will reflect the truly subjective research produced. Rather than being embarrassed by this, we can extol the uniqueness of our social scientific method and know that, while others pay lip service to objectivity, our version of objectivity can only come from honestly appraising ourselves. Long ago, C. Wright Mills implored us to exploit our own biographies, to reflect upon ourselves, to be better social scientists, stating:

> . . . you must learn to use your life experiences in your intellectual work: continually to examine and interpret it. In this sense craftsmanship is the center of yourself and you are personally involved in every intellectual product upon which you may work. (Mills 1959, p. 196)

CONCLUSION

The ethnographer of the 1980s has begun to attain the goals of the "sociological imagination" that our colleagues have ignored. We are on the verge of being creative and imaginative enough to go beyond speaking merely of data, subjects, and other distant scientific constructs and talk about the ethnographic voice, the ego of the author, and the way that we relate to both our audiences and the members of the worlds we study (Van Maanen 1988; Becker 1985; Clifford and Marcus 1986).

Norman Denzin has recently argued that recent trends indicate a new type of ethnography, a *postmodern* one. It is a form of ethnography that goes beyond the scientific method, qualitative data collection and analysis, and the search for generic principles. Its concern is with experience; the observer is no longer a passive, objective recorder of experience, but is part of the experience. Denzin states:

> Terms like observer role, role relations, going native, role disengagement, cultural shock, gaining acceptance, coding, problem selection, domain of analysis, and data gathering are no longer operative. These are modernist, positivist, structuralist concepts. They belie a commitment to the grand narratives of the past which held that objective truths about human societies could be written. The postmodernist abandons this project . . . The texts that are now brought before the reader are interpreted as accounts of experience, and, as such, are seen as having the qualities of fiction, native literature, autobiography, and folklore. These personal experiences and self stories from and of the field are judged to be meaningful within the narratives that contain them . . . The ethnographic experience can no longer be separated from the life of the author. (Denzin 1989, p. 92)

To reach this new plane, ethnography must become seriously existential. We must be able to be, feel, sense, hear, and emote with the researchers. To do anything less would be tantamount to presenting falsehoods. To try to cover up would only hurt the entire research enterprise. The more reflexive, existential, and conscious we can be, the more our readers will learn about, experience, and relate to the worlds we study.

Induction in its present form allows us to call upon our experiences, utilize our feelings, and be a part of the research instrument while evolving and formulating theory that is grounded in the experiences of ourselves and the people we study. By remaining open and flexible, with a close eye and ear to behavior in natural settings, induction (or retroduction), is the only epistemology that addresses the subjective nature of human life, while generating theories that respect the everyday realities of its members.

REFERENCES

Adler, P.A., and Adler, P. *Membership Roles in Field Research.* Newbury Park, CA: Sage Publications, Inc., 1987.

Angell, R.C. *The Family Encounters the Depression.* New York: Scribner's, 1936.

Angell, R.C., and Turner, R.H. Comment and reply on discussions of the analytic induction method. *Am Soc Rev* 19:476-478, 1954.

Baldamus, W. The role of discoveries in social science. In: Shanin, T., ed. The Rules of the Game: *Cross-Disciplinary Essays on Models in Scholarly Thought.* London: Tavistock, 1972. pp. 276-302.
Becker, H.S. Becoming a marijuana user. *Am J Soc* 59:235-242, 1953.
Becker, H.S. *Writing for Social Scientists.* Chicago: University of Chicago Press, 1985.
Bittner, E. Realism in field research. In: Emerson, R.M., ed. *Contemporary Field Research.* Boston: Little, Brown, 1983. pp. 149-155.
Blumer, H. *Symbolic Interactionism.* Englewood Cliffs, NJ: Prentice-Hall, 1969.
Bogdan, R., and Taylor, S. *Introduction to Qualitative Research Methods.* New York: Wiley, 1975. 266 pp.
Bulmer, M. Concepts in the analysis of qualitative data: A symposium. *Soc Rev* 27:651-677, 1979.
Burgess, E.W., and Cottrell, L.S. *Predicting Success or Failure in Marriage.* Englewood Cliffs, NJ: Prentice-Hall, 1939.
Burgess, E.W., and Locke, H. *The Family: From Institution to Companionship.* New York: American Book Co., 1945.
Cicourel, A. *Method and Measurement in Sociology.* New York: Free Press, 1964. 247 pp.
Clifford, J., and Marcus, G., eds. *Writing Culture.* Berkeley, CA: University of California Press, 1986.
Cressey, D. *Other People's Money.* New York: Free Press, 1953.
Denzin, N.K. *The Research Act.* Chicago: Aldine, 1970. 368 pp.
Denzin, N.K. Review symposium on field methods. *J Cont Ethnography,* 18:89-100, 1989.
Douglas, J.D. *Investigative Social Research.* Beverly Hills, CA: Sage, 1976.
Ellis, C.S. Systematic sociological introspection. Paper presented at the American Sociological Association annual meeting, 1987.
Emerson, R.M., ed. *Contemporary Field Research.* Boston: Little, Brown, 1983. 335 pp.
Forrest, B. Apprentice-participation: Methodology and the study of subjective reality. *Urban Life* 14:431-453, 1986.
Glaser, B., and Strauss, A. *The Discovery of Grounded Theory.* Chicago: Aldine, 1967.
Gould, L.; Walker, A.L.; Crane, L.E.; and Lidz, C.W. *Connections: Notes from the Heroin World.* New Haven, CT: Yale University Press, 1974.
Hammersley, M., and Atkinson, P. *Ethnography: Principles in Practice.* New York: Tavistock, 1983. 273 pp.
Hayano, D.M. Auto-ethnography: Paradigms, problems, and prospects. *Human Org* 38:99-104, 1979.
Horowitz, R. Remaining an outsider: Membership as a threat to research rapport. *Urban Life* 14:409-430, 1986.
Johnson, J.M. *Doing Field Research.* New York: Free Press, 1975.

Kaplan, A. *The Conduct of Inquiry.* Scranton, PA: Chandler, 1964. 428 pp.
Katz, J. *Poor People's Lawyers in Transition.* New Brunswick, NJ: Rutgers University Press, 1982.
Katz, J. A theory of qualitative methodology: The social system of analytic fieldwork. In: Emerson, R.M., ed. *Contemporary Field Research.* Boston: Little, Brown, 1983. pp. 127-148.
Krieger, S. Beyond subjectivity: The use of the self in social science. *Qual Soc* 8:309-324, 1985.
Lindesmith, A. *Opiate Addiction.* Bloomington, IN: Principia Press, 1947.
Lindesmith, A.; Weinberg, S.K.; and Robinson, W.S. Two comments and rejoinder to 'The Logic of Analytic Induction.' *Am Soc Rev* 17:492-494, 1952.
Lofland, J., and Lofland, L.H. *Analyzing Social Settings.* 2nd ed. Belmont, CA: Wadsworth, 1984. 186 pp.
Manning, P.K. Analytic induction. In: Smith, R.B., and Manning, P.K., eds. *A Handbook of Social Science Methods: Qualitative Methods.* Vol. II. Cambridge, MA: Ballinger, 1982. pp. 273-302.
Mills, C.W. *The Sociological Imagination.* New York: Oxford University Press, 1959.
Peshkin, A. Odd man out: The participant observer in an absolutist setting. *Soc Ed* 57:254-264, 1984.
Reckless, W.C. *Six Boys in Trouble.* Ann Arbor, MI: University of Michigan Press, 1929.
Riemer, J.W. Varieties of opportunistic research. *Urban Life* 5(4):467-477, 1977.
Robinson, W.S. The logical structure of analytic induction. *Am Soc Rev* 16:812-818, 1951.
Rochford, E.B. *Hare Krishna in America.* New Brunswick, NJ: Rutgers University Press, 1985.
Schatzman, L., and Strauss, A. *Field Research: Strategies for a Natural Sociology.* Englewood Cliffs, NJ: Prentice-Hall, 1973.
Shaw, C.R. *The Jack Roller.* Chicago: University of Chicago Press, 1930.
Shaw, C.R. *The Natural History of a Delinquent Career.* Chicago: University of Chicago Press, 1931.
Shaw, C.R. *Brothers in Crime.* Chicago: University of Chicago Press, 1938.
Strauss, A. *Qualitative Analysis for Social Scientists.* New York: Cambridge University Press, 1987. 319 pp.
Thomas, W.I. *The Unadjusted Girl.* Boston: Little, Brown, 1923.
Thomas, W.I., and Znaniecki, F. *The Polish Peasant in Europe and America* (5 vols.). Chicago: University of Chicago Press, 1918-1920.
Thrasher, F. *The Gang.* Chicago: University of Chicago Press, 1927.
Turner, R.H. The quest for universals in sociological research. *Am Soc Rev* 18:604-611, 1953.
Van Maanen, J. *Tales of the Field.* Chicago: University of Chicago Press, 1988.

Vidich, A., and Lyman, S.L. *American Sociology*. New Haven, CT: Yale University Press, 1985. 380 pp.
Wax, R.M. *Doing Fieldwork*. Chicago: University of Chicago Press, 1971.
Wiseman, J.P. *Stations of the Lost*. Englewood Cliffs, NJ: Prentice-Hall, 1970.
Znaniecki, F. Social research in criminology. *Sociology and Soc Res* 12:307-322, 1928.
Znaniecki, F. *The Method of Sociology*. New York: Farrar and Rinehart, 1934.

AUTHOR

Peter Adler, Ph.D.
Department of Sociology
University of Denver
Denver, Co 80208-0209

128614

Computer Analysis of Qualitative Data

Richard B. Fritz

INTRODUCTION

The advent of the personal computer affords new opportunities to compile and manage massive quantities of textual data. Given sufficient administrative and secretarial support for word processing, transcription, and file management, an experienced researcher can now assemble virtually limitless volumes of field notes, personal observations, indepth interviews, research reports, letters and formal correspondences, archival documents, organizational records, interdepartmental memoranda, and other communications. In the past, the task of analyzing and interpreting such mountainous files may have intimidated even the most seasoned researcher. However, recent developments in text search-and-retrieval programs have lent a much needed boost to the analysis of large bodies of qualitative data. The researcher with access to these programs can now explore hundreds or even thousands of documents within moments.

The purpose of this chapter is to examine some global objectives of qualitative data analysis and to survey several word-search and text-retrieval programs that can assist in the data analysis process. Although persons engaging in other forms of qualitative research and data analysis may also find this presentation useful, the primary focus is on the analytic needs of the ethnographic researcher.

Ethnography, among other things, provides a powerful means for explaining and understanding normative structures, belief systems, values, and forms of social organization. These insights are gained, however, only through exhaustive effort, great patience, and personal participation in field situations. As many ethnographers have observed, there is no replacement for hard work, perseverance, and experience. But all scientific endeavors, including ethnography, may benefit from technological advances that speed the research process and expand productive capacities. Most social scientists are familiar, for example, with recent well-publicized improvements in survey methodologies and computerized statistical programs. These advances have not necessarily made "quantitative" social scientists "better" researchers. They have, however, provided new tools to forge a

better understanding of social structures, social dynamics, population parameters, sample estimates, and public opinion. Although there have been few comparable advancements in the "tools" or technologies exclusively devoted to ethnographic research, there are still many computer applications that can lighten the ethnographer's workload.

Before proceeding further, it should be noted that this chapter does not intend to catalog all contemporary programs that show promise as aids to ethnographic analysis. Neither will it detail the specific features of all useful software packages. Instead, it will review some of the core objectives and strategies of ethnographic analysis and will discuss how the current generation of text search-and-retrieval programs can help the ethnographer achieve these objectives.

Rather than rehash formal definitions of ethnography, this chapter will discuss four basic ethnographic activities that contemporary computer software packages may enhance or facilitate: exploring topics, developing ethnographic lexicons, conducting inductive and deductive analysis, and checking the reliability and validity of qualitative data. If there is an overarching point to be made, it is this: the selection of research tools should be guided by analytic needs rather than by rigid attachments to the traditional "tried and true" methods of the past or, conversely, by fleeting attractions to technological fads.

EXPLORATION OF TOPICS

One of the more fundamental objectives of qualitative data analysis is to explore new topics and issue areas. A current concern in the field of drug abuse research, for example, is the relationship between intravenous drug user (IVDU) needle-sharing rituals and the spread of the human immunodeficiency virus (HIV), the virus that causes the acquired immunodeficiency syndrome (AIDS). This single topic touches on numerous related issues and raises a variety of questions requiring exploration in the field and during analysis: What are the behavioral and normative components of needle-sharing rituals? Do IVDUs share their works with friends, with acquaintances, or with strangers? How many people do they share with and how often? Do they disinfect their needles and syringes before reusing them or passing them on to another user, and if so, what are their sterilization procedures? What is the current level of knowledge about AIDS among IVDUs, and is their information accurate? What are their beliefs or myths about AIDS prevention programs? What behavioral changes are IVDUs willing to make to reduce their risk of HIV infection?

Researchers new to the world of intravenous (IV) drug use (or any other internally coherent social reality for that matter) must adopt an open-minded exploratory approach to data collection if they hope to understand the meanings and motives of the groups and individuals under study.

Consequently, field exploration of new topics is often a free-wheeling, wide-ranging, and sometimes even serendipitous process. An off-the-cuff remark in the street may set off a chain of inquiry that generates new ideas and opens fresh areas of study. Ethnographic exploration may also take a variety of forms. At different stages of activity, the ethnographer may look for new ideas to study, investigate new variations on old themes (for example, the idiosyncracies of different groups using the same drugs), probe the nuances of well-known behaviors (such as the specific details of needle-sharing rituals), or examine changes in familiar field situations. At all stages of exploration, the ethnographer must remain open and receptive to new phenomena. An awareness of emergent patterns in the data in turn often suggests working hypotheses that guide and focus future field explorations.

The exploration of topics is thus intrinsically tied to the process of data analysis. Whether sitting at a computer terminal or talking to a "street source," the ethnographer is always "working with the data"; that is, thinking and wondering about meanings, relationships, and explanations. By continually constructing and testing working hypotheses, the ethnographic analyst maintains an intimate familiarity with the data, generates new interpretations of field evidence, and plots new directions for further field exploration. As Schwartz and Jacobs observe, "the judicious combination of facts with the restless mind of an analyst, suitably fertilized, . . . yields patterns and problems" that often serve as leads for new areas of inquiry (Schwartz and Jacobs 1979, p. 291).

Throughout the entire exploration process, then, the ethnographer engages in activities requiring insight, intellect, interpretive training, and a thorough grounding in the research scene. But, as most ethnographers readily admit, the bulk of their time is often spent in routine, laborious data management tasks. Although it is unfair and misleading to label any part of the research process as "time lost," it is nevertheless sometimes frustrating and even disheartening to return from a stimulating day in the field only to be confronted with mundane tasks of transcribing interviews, organizing and managing piles of documents, thumbing through volumes of notes and papers to look for new ideas or confirm old ones, or plowing through reams of paper to track down that one "perfect quote" to illustrate a key point.

Fortunately, there are a number of widely available software programs that can make exploring and analyzing computerized ethnographic data easier, faster, and even more fun. Probably the simplest and most readily available "computer aids" to qualitative analysis are standard word processing programs. WordPerfect, Word Star, Microsoft Word, MultiMate, PFS.Write, and others share a number of basic features that make text management a much more manageable task. More important, the advanced versions of many word processors offer basic text search-and-retrieval facilities. Such capabilities help boost the speed of data exploration and analysis. Although

processing and interpreting qualitative data will always require trained analysts, the ability to instantaneously locate a specific word or "string" of text accelerates the "mundane" operations that have historically hindered the more creative aspects of qualitative analysis. Thus, access to an inexpensive personal computer with basic word processing capabilities can facilitate the analytic stages even for an ethnographer working alone on unfunded research.

Despite their efficiency at managing text, word processors have limited utility as analytic tools. Although they allow the researcher to search for particular words or strings of words, most do not permit Boolean operators (for example, "and," "or," "near," or "within") that make relational searches possible. Moreover, most also restrict their searches to the single document that is being processed—a limitation that is extremely problematic and inhibiting for the researcher with thousands of pages of field notes and interview transcripts scattered across different directories. In today's research office, it is not uncommon for personal computers to sport 20- or 30-megabyte hard disks. Computer manufacturers have recently begun offering models with hard disks ranging from 50 to 120 megabytes, thus making it increasingly difficult to locate specific individual bits of data without software assistance. Yet finding a particular word or locating all occurrences of a phrase may be exactly what is needed to open up a new line of inquiry or to finish a paper. Clearly, the researcher desiring to explore and analyze research topics thoroughly requires a more powerful program that can reach across documents and files, and can even read from one subdirectory to another.

A thorough, but relatively slow search-and-retrieval facility is currently offered by Norton Utilities, a general utilities package. Among other things, Norton Utilities allows the user to "unerase" documents, optimize disk space, edit directory information, and search files and directories for selected text. The search program in Norton Utilities searches through each sector of a disk and displays highlighted text strings in the context of surrounding text and in their order of location. The user may request identifying information on the cluster number, sector number, file name, and directory name for each "found" text string. Norton does not allow the user to edit text, but it will write text to a new file that can later be loaded into a word processing program.

Norton Utilities is inexpensive, readily available, and offers several additional utilities besides the search feature. But those needing speedier word searches and greater versatility in specifying the criteria for customized searches, and who can afford the price, should consider "dedicated" text-retrieval packages.

To date, the best of these appears to be a program called ZyIndex. This program literally indexes each content word while ignoring noise words

such as "a," "an," "the," and "it." As do all other "index-based" programs, ZyIndex "tags" the location of all meaningful words. The most expensive version of ZyIndex currently costs about $700 but can be networked among six IBM-compatible personal computers. It will index up to 15,000 files of approximately 200 pages each. Files may be initially located in different directories, but copies of the indexed files are stored within ZyIndex's own subdirectory after indexing. (This procedure uses extra memory but speeds the search process). Once indexed, the files are treated as a structured data base: each word is given an "address" that can be located almost instantaneously. "Found" words are highlighted and shown in context and are presented in their order of location. The user can cut and paste any designated section of text to a new file that can later be edited on a word processor. ZyIndex employs Boolean delimiters to specify search criteria, thus allowing relational searches for word combinations that may be separated by one or more intervening words.

The main advantage of ZyIndex is speed. Once the indexing procedure is completed, the search program can literally race through several megabytes tracking down the desired text. It should be noted, however, that while the search program is exceptionally fast and comprehensive, the indexing procedure itself is relatively slow. For example, a test run on an IBM compatible XT Turbo took 12 minutes to index 18 documents averaging 22 pages each. These same 18 documents took 6 minutes to index on an AT Turbo model. A researcher with several hundred files to index might want to consider catching up on journal articles while waiting for the indexing program to finish. Once indexed, however, be ready to fly through the data.

Another fast indexing program is Search Express. This program indexes files and then lets the user specify search criteria. One appealing feature allows the user to title and number each file and then employ these identifiers as "guides" in the search procedure. Thus, for example, one can instruct the program to "search file 99." Search Express also has a "zoom feature": pressing the F3 function key takes you to a document with a found word; pressing it again takes you to the relevant paragraph; pressing F3 a third time takes you to the sentence with the found words. Search Express is fast, but not as fast as ZyIndex. Its main advantage lies in its scope of reach; the manufacturers claim that Search Express will index and read up to 1 million short documents.

There are also a number of nonindex-based text-retrieval programs that offer a variety of useful features. A program called Gofer, for example, allows the user to customize search specifications using up to eight Boolean operators and to specify the scope of the search (within files, subdirectories, or disks). It also accepts upper and lower case distinctions and can search alphabetically, by date, or by extension. Another nonindexing program, Text Collector, is considerably more limited in its ability to customize

relational searches but offers a phonetic search capability that tracks down text even if it is misspelled. Text Collector also searches for wild cards and will accept hyphenated words. A third nonindex-based program, Dragnet, allows the user to specify searches for groups of words under a single rubric. For example, a search for heroin, cocaine, amphetamines, codeine, and other drugs can be grouped as "drugs." Dragnet will also print documents directly without accessing a word processor or DOS. Dragnet only operates under MicroSoft Windows, however, a restriction that may limit its applicability.

Another important element of the data exploration process is "browsing," an activity analogous to thumbing through old books or casually perusing one's own old field notes and interviews. Browsing may play a significant role in data analysis at a variety of stages. For example, ethnographers who are in the early stages of analyzing their data may have only fuzzy images of broader or more abstract patterns and relationships, especially if they are still immersed in the concrete realities of the field setting and have not had time to step back from the data. In such situations, it is beneficial to scan one's data at leisure, letting the mind wander from one idea to the next without imposing any constraints on the creative process. In the later stages of analysis, on the other hand, the researcher may want a fresh outlook or approach to familiar territory. A relaxed, informal hour or two spent browsing through one's computer files may in fact educe previously overlooked patterns and connections or rekindle the creative fires.

One software package that seems ideal for browsing is Apple's HyperCard. This program stores data on "notecards" that can hold text, numbers, pictures, or musical scores. HyperCard files are organized into "stacks" of notecards that can be shifted and sorted according to a user's needs. An important feature is HyperCard's use of "buttons" to link data. Buttons are user-selected symbols that connect a notecard to all other notecards sharing the same buttons. The user can assign several such buttons to each notecard. When selected, a button calls up another notecard bearing the same symbol.

The key to HyperCard is its ability to jump to related ideas, regardless of their location within the file structure. Rather than read from front to end, HyperCard springs to the new notecards with the same button. Since movement between cards is not sequential, the researcher may spot new connections that might be overlooked in a "straight line" search that plods remorselessly from one file to the next. (Many veteran researchers who once used McBee cards will readily acknowledge that some of their most creative insights occurred when "the cards got out of order.") The fact that a card may have several buttons, moreover, allows a "browser" to explore new relationships or search for new connections between superficially unrelated concepts simply by shifting buttons during the course of a search. HyperCard thus has great potential as an analytic tool. It does, however,

suffer from two noteworthy drawbacks: first, text must be organized according to the program's formatting specifications before transcription; and second, the program is only available for Apple computers.

Another readily available hypertext program is Owl International's Guide. Like HyperCard, Guide uses buttons to link data. Unlike HyperCard's file-stack-notecard structure, Guide organizes data into files consisting of a single document. In fact, one of the drawbacks of the current version of Guide is that it links data only within a single document. However, Guide does operate in MS.DOS under Windows, thus making it accessible to PC users.

Guide's buttons offer several useful features. For example, the user can write detailed notes on a passage of text, place them in a separate location, and link the notes to the original text with a "note button." The notes are easily accessed by selecting the button when it is encountered in the text. Guide also offers a button that links text to references, as well as a replacement button that substitutes new passages for old text. Although Guide is somewhat more limited than HyperCard as a browsing tool, it does provide an inexpensive means of exploring related ideas within existing documents.

Contemporary hypertext programs and text-retrieval packages thus come with a wide variety of features. The speed, convenience, and collective versatility of these applications makes the topic-exploration process more productive and even more enjoyable. Researchers who relish the investigative aspects of data analysis often find it easier to spot "clues" to relationships when zipping through highlighted files at their own optimal pace rather than plowing through stacks of undifferentiated documents. The task of analyzing large quantities of qualitative data will always take time and careful attention to detail. But some of the drudgery of analysis can be reduced if not eliminated by research tools that are readily available to researchers with limited budgets.

LEXICON DEVELOPMENT

Another important step in the path toward ethnographic understanding of a sociocultural group is the development of a dictionary or lexicon of meanings. Among the many formal and informal definitions of ethnography, Giddens' (1976) observation that it is a process that "mediates frames of meaning" most directly taps the complex, multifaceted character of the methodology. Human social realities are subjective and socially constructed. The meanings shared among contemporaries and across generations are the products of collective "frames of reference" generated by common experiences and reinforced by language, custom, and beliefs.

Ethnography attempts to comprehend and catalog the complex elements of group culture (including language), unravel the subjective meanings shared by a collectivity, and reconcile or resolve any seeming discrepancies or breakdowns that might confuse or befuddle the outside observer (Agar 1982; Agar 1986). Agar (1982), in fact, goes so far as to argue that the purpose of ethnography is not to use an insider's meanings to define subjective reality to outsiders, nor to explain another culture to outsiders in terms that comply with their own frame of reference, but rather to employ an interpretive tradition that mediates the disjunctions between sociocultural groups and the audience. In effect, messages are meaningless unless the symbols and codes employed convey the researcher's meanings to the audience. The researcher's interpretations, however, must be grounded in the realities of the research setting, thus requiring an intimate familiarity with the shared meanings of the group under study.

Some of the fundamental building blocks of shared meanings are often found in the words and phrases peculiar to the local culture of a group. To paraphrase Schutz (1970), language is a treasure house of stored meanings that, when invoked in the routine interactions of everyday life, help an actor express his acts and intentions and also help the observer make coherent sense of those acts and intentions.

From Schutz's perspective, language is used to make generalizations about the typical motives and activities of others, thus providing a sense of stability, predictability, and comprehensibility. Words and phrases, the building blocks of language, embrace traditions and conventions that transcend the background, schemes, and purposes of any individual actor. From the observer's perspective, the words used to express and describe motives are treated as if they also explain behaviors and intentions. In effect, one understands the subjective realities of others as long as one has a universalistic knowledge structure (or linguistic skeleton) on which to hang one's perceptions of the typical goals or intentions underlying a particular act. Interpretations of another's motives and meanings are thus shaped, homogenized, and standardized through language. Unique events are more difficult to comprehend than everyday acts; therefore, language is employed to routinize the unusual and level the differences between the ordinary and the extraordinary. For the ethnographer, it is essential to examine and comprehend the nuances of everyday language, including the formal expressions of polite discourse and the informal jargon of the street.

In the field of drug abuse research, for instance, it is often important to know the broad and varied street terms used to identify various drugs or drug-related activities. The experienced observer knows that "smack" and "horse" are standard street names for heroin and that "China white," "tar baby," and "Mexican mud" represent different grades of quality. But street language is fluid and volatile. Drug terminology varies according to time and place, with users introducing new terms to veil the nature of their

activities or simply to use a more graphic and colorful imagery. For example, in the early 1970s, a group of white Appalachian male IVDUs on Chicago's North Side briefly eluded the scrutiny of police informants by referring to heroin as "chicken feed." This not only reflected their rural background (by associating needle tracks with chicken tracks, and heroin as the feed for the chicken (needle), but also threw listeners off the scent. As one police informant stated, "Gee, all that talk about chicken feed. I thought they were talking about their pay checks."

Needless to say, a dictionary of local meanings often facilitates communication with study populations. There are, in fact, a number of ways in which current computer software programs can aid in the development of an ethnographic lexicon. The simplest procedure would be to write a customized glossary and/or thesaurus straight into a standard word processing file. This strategy suffers from a number of limitations, chief among them being that the user would be forced to start the dictionary from scratch and thus replicate much of the effort that went into the creation of readily available general-purpose dictionaries. Another strategy is to build on the thesaurus subprograms offered by standard word processing programs. Those who feel comfortable with reprogramming the basic features of their word processor or who have access to a competent computer programmer can add a supplementary thesaurus and dictionary to their word processing program. This procedure might be beyond the expertise of the average user and may exceed the memory capacity of the typical word processing program.

A more parsimonious approach to lexicon development is to take advantage of the thesaurus subprograms available as optional features with text search-and-retrieval programs. ZyIndex, for example, offers an optional package called ZyFeatures that includes, among other things, a thesaurus capability. ZyFeature's thesaurus is ideal for developing an ethnographic lexicon, because it allows the user to enter new words and then build sets of synonyms. Thus, the user could establish the general category "drugs" and then enter specific related meanings such as heroin, speed, coke, uppers, downers, and so on. Or the user could enter a more particularistic category, such as "heroin," and include slang equivalencies such as "horse," "smack," "H," "mud," and "tar." The researcher is thus able to build a customized thesaurus that may in turn be used in text searches. Searches themselves may be customized, with the user specifying a search that calls up all instances of the word "drugs" as well as all of the synonyms specified in the thesaurus. These word searches can of course take advantage of the other standard search capabilities, such as Boolean operators and multiple file searches. But more to the point, the analyst can use the speed and flexibility of this computer "tool" to chart the linguistic structure and track the meanings of key words and concepts of a sociocultural group.

INDUCTIVE AND DEDUCTIVE RESEARCH

One of the strengths of ethnographic training is that it discourages the tendency to prejudge sensitive issues and instead encourages ethnographic researchers to approach complex, multifaceted topics such as the AIDS/IVDU nexus with an open, inquiring mind. As Schwartz and Jacobs point out, the job of the ethnographer is to "make a set of integrated observations on a given topic and place them in an analytic framework" (Schwartz and Jacobs 1979, p. 289). There are, of course, different research philosophies and methodological preferences that influence the ethnographer's orientation toward the development of theories and the utilization of analytic schemes.

Ethnographers who feel at home with the deductive, *a priori* approach that remains predominant within the social sciences often prefer a structured program of theory construction and hypothesis testing. The tendency within this school of thought is to generate a prefabricated "analytic framework," the validity of which is tested, and subsequently supported or refuted, through empirical observation and objective analysis. Kerlinger (1973) describes logico-deductive hypothesis construction and the advantages that derive from it as follows:

> After intellectualizing a problem, after turning back on experience for possible solutions, after observing relevant phenomena, the scientist may formulate a hypothesis. A hypothesis is a conjectural statement, a tentative proposition, about the relation between two or more phenomena or variables. Our scientist will say, "If such-and-such occurs, then so-and-so results." [The next step involves the activities of reasoning and deduction] . . . The scientist now deduces the consequences of the hypothesis he has formulated . . . Here is where experience, knowledge, and perspicuity are important. Often the scientist, when deducing the consequences of a hypothesis he has formulated, will arrive at a problem quite different from the one he started with. On the other hand, he may find that his deductions lead him to believe that the problem cannot be solved with the present technical tools. [Thus], reasoning can help lead to wider, more basic, and thus more significant problems, as well as provide operational (testable) implications of the original hypothesis (Kerlinger 1973, pp. 12-13).

From this perspective, the deductive process does, in fact, rely on prior knowledge and past experiences as a basis for proposing explanations and predictions of probable relationships. Despite its rootedness in the past, deduction remains primarily a future-oriented undertaking. Propositions are made and then rejected or not rejected on the basis of the "goodness of fit" between the observed and expected results. Deductive research thus operates within a fairly rigid, well-defined system of rules and procedures that

channel and constrain data collection activities, theory construction, and data analysis.

A growing number of contemporary ethnographers prefer a more free-wheeling inductive approach to data collection and analysis. For them, hypotheses, theories, facts, and findings emerge from the data. An "analytic framework" is not imposed as a preconceived model but is instead treated either as a way station in the path to understanding or as an outcome of the research itself. Within this *a posteriori* orientation, naturalistic observations are "integrated" into a fluid, everchanging "analytic description of the behaviors that characterize and distinguish cultures or sociocultural groups . . . and the knowledge and beliefs that generate and interpret those behaviors" (Walters 1980, p. 17).

In effect, the inductive approach treats data as a catalyst for generating theories that summarize and explain the normative structures, collective belief systems, and shared social realities of unfamiliar groups. This "discovery of theory from data," according to Glaser and Strauss (1967), results in grounded theories. Such data-based theories are verifiable in that their conceptual components may be dismantled and tested through further observation. More important, the very fact that such theories arise from the data increases the likelihood that they will present crisp, accurate portrayals of social reality. Their intimate linkage to the data moreover ensures that grounded theories are open to modification and reformulation. A good theory, from the inductive perspective, is one that fits the facts; a better theory is one that has been reshaped to more accurately describe those facts and revised to more clearly articulate the subjective, elusive nature of social reality.

Although many researchers tend to subscribe to either inductive or deductive strategies exclusively, each approach has its own strengths and weaknesses. Deductive research, for example, lends itself to the specification of causal relationships and formal hypothesis testing, especially in controlled settings. Most ethnographic research, however, is conducted in "real life" situations that minimize the researcher's control and cloud the nature of causality. Viewed in the context of its *a priori* nature, deductive methodology tends to force blinders on the analyst: by defining the social world in terms of a favored hypothesis and its plausible competitors, the researcher limits the scope of inquiry and focuses attention on a narrow range of possibilities.

Inductive research, on the other hand, is generally more broadminded, wide-ranging, and *a posteriori* in its outlook. Consequently, it benefits from the advantages of open-minded hindsight: self-correction and adaptability. The inductive approach is therefore remarkably effective in the detection and identification of social patterns (Walters 1980), especially in cases where those patterns contradict the rational predictions of logico-deductive reasoning. The method of natural observation that is so closely linked with

this style of research clearly operates most effectively in those real life situations that thwart and frustrate the efforts of the deductive, *a priori* researcher.

Strauss (1987), in fact, argues that three elements of scientific inquiry—induction, deduction, and verification—are mutually interdependent and vitally essential to the research process. According to Strauss:

> deduction without verification . . . of an hypothesis or set of hypotheses is truncated inquiry. Obviously, too, verification cannot occur without deduction: Hypotheses for data collection without reference to implications of theoretical hypotheses are useless. And how can there be hypotheses without either thinking through the implications of data or through "data in the head" . . . that eventuates in so-called hunches, insights, and very provisional formulations of hypotheses? (Strauss 1987, p. 12).

In short, ethnographic inquiry, as with all forms of qualitative research, is most likely to contribute to our knowledge of social phenomena when it incorporates informed inductions and deductions that are verifiable through rigorous scientific observation and analysis.

The purpose of this chapter is not to debate the relative merits of inductive and deductive strategies, nor even to argue for a marriage of these divergent approaches (although that would be the author's preference), but rather to explore the advantages offered the ethnographic researcher by computerized text-management programs. As in the earlier discussion of topic exploration, there are programs that lend themselves more to one form of analysis than another. However, such distinctions often prove artificial. An analytic aid such as a text-retrieval program may alternately function as "deductive tool" or an "inductive device" depending on the stage of analysis and the researcher's personal research style.

Ethnographers conducting deductive research should consider using HyperCard's button system as an instrument for verifying observations and/or testing hypotheses. In effect, HyperCard buttons serve as a precoding scheme. As a result, the researcher is all but forced to prestructure the data and thus engage in a rudimentary form of preanalysis. Quite simply, in order to code or button a passage on a notecard the ethnographer must first have some preconceived notion of a classification scheme that places concepts into appropriate categories. Buttons are thus, in some ways, analogous to anticipatory analytic categories.

Accordingly, the analyst can perform preliminary tests for conceptual linkages simply by following a particular button selection path in HyperCard. When testing a hypothesis or examining a relationship derived by deduction, this procedure can provide a means for tracking evidence to

support or refute propositions. For example, if field observations suggest the proposition that heroin addicts often use alcohol to maintain their high, one would expect notecards buttoned for heroin-related data to consistently show linkages to mentions of alcohol, beer, wine, and drinks.

For the rare ethnographer who wishes to extract numerical findings from qualitative data sets, hypertext and text-retrieval programs also provide a means for quantification, albeit at a rudimentary level. For example, ZyIndex counts the frequency of every indexed word within a document, thus supplying a rough data base for content analysis. Moreover, the researcher can count the number of times a word, event, or phenomenon appears in conjunction with another concept, thereby producing a crude but nonetheless potentially useful cross-classification of categorical data. These simple operations can be performed either by using relational searches in text-retrieval packages or button links in hypertext programs. Although some ethnographers may be disinclined to look for quantification in their data, an occasional descriptive table may, in fact, present a richer, more global picture of the social realities under study.

A relatively new software package that promises to serve as an inexpensive but powerful research tool is The Ethnograph. As its name implies, this program was developed explicitly for ethnographic analysis. The Ethnograph offers the ability to code and number every line of text in a document and then to search and retrieve text according to line numbers and coding categories. Although the program accepts text from most standard word processors (for example, Wordstar or WordPerfect), it also requires some simple preliminary reformatting.

Briefly, The Ethnograph permits only 40 columns of data per line; the remaining columns are reserved for codes, user identifications, and line numbers. The researcher can create and use an unlimited number of codes per document, but no more than eight codes per line. Actual codes must be 10 letters or less, a restriction that occasionally proves somewhat limiting. The Ethnograph allows up to 9,999 lines per transcript and will conduct relational searches using up to 8 Boolean operators. Unlike other text search-and-retrieval packages, The Ethnograph does not conduct its searches within the main body of text but instead searches through the codes in the margins. The program will sort by document, extension, or subdirectory and will search through as many documents as exist in The Ethnograph format. It also offers a global search-and-replace feature for codes but will not alter text. Once a transcript has been completed, The Ethnograph will print out code lists, count the number of times each code was used, and the number of times each speaker "spoke." It will also write selected output to disk where it can be edited and printed.

Although The Ethnograph can be used for preliminary data exploration or browsing, its real power is as an analytic tool. For one thing, The

Ethnograph's search procedures compel the analyst to grapple with classifications and meanings even before engaging in activities that might formally be described as inductive analysis. In effect, The Ethnograph's code search, in contrast to the text searches of indexing programs, requires not only an intimate familiarity with the data but also a good deal of *a priori* classification that is more frequently associated with deductive research. Once the analyst has completed coding, the program is extremely helpful in conducting the free-flowing, data-based *a posteriori* analysis associated with induction. Global search-and-replace functions allow the researcher to change codes during the course of analysis, thus moderating some of the rigidity of *a priori* classification and supplying the flexibility to revise and update the elements of analysis. Furthermore, the fact that code searches take place among conceptual categories constructed by the researcher means that the analytic process will always be grounded in the researcher's own emergent understanding of the data, facilitating an optimal linkage between theory and data at each stage of analysis.

The Ethnograph thus offers a variety of features that blur the distinctions between inductive and deductive research but, nevertheless, speed and simplify all facets of the ethnographic enterprise. Other programs that are considerably more circumscribed in their scope as analytic tools may also offer a great deal of utility within their sphere of applicability.

For example, dedicated text search-and-retrieval programs such as ZyIndex and Search Express are extremely useful in conducting general inductive research. For one thing, these programs do not involve the initial data preparation required by The Ethnograph, thus reducing some of the time demands that frequently plague the ethnographer's schedule. For another, they provide complete freedom in the text-search process. This can be extremely important for researchers who have reached a dead end using their own coding schemes and want a fresh start with the data. At an even more fundamental level, not all researchers will have access to multiple software packages and must therefore make use of the programs at hand. For those who have them, text-retrieval programs provide an excellent analytic tool for inductive research. Their speed and flexibility allow the researcher to identify and trace linkages between concepts quickly and to cultivate a broader perspective that is so often conducive to theory construction.

Another advantage of computer-assisted qualitative research is that it provides an opportunity to examine and understand the context surrounding the issues and events under investigation. For lack of an existing term, this subset of inductive research can be called "contextual analysis."

The social context surrounding a phenomenon or event is important, because it reflects and articulates the exogenous factors influencing social realities, human behaviors, and collective actions. Analysis of the social context may

sometimes provide meaningful and helpful insights into those factors that shape and define the external reality within which human actors act. For example, preliminary data on IVDUs' attitudes toward AIDS may indicate a variety of social contexts operating in different groups. Briefly, some IVDU social networks may appear to react with complete denial, others might respond with fear, some with mistrust of public health authorities, while others may show indifference and apathy. Clearly, these responses to the epidemic would, in and of themselves, be regarded as important findings. But, for the individual living and acting within the world of the IVDU, a social context of denial may encourage different behavioral responses and risk-reduction decisions than one of generalized fear or one of apathy. In turn, these social contexts have implications for public health intervention efforts. Public health agencies confronted with IVDUs who continue to share unsterilized needles because of generalized denial of their infectability must consider different intervention strategies from agencies that are flooded with requests for help from IVDUs coming from a social context of fear and anxiety.

Once again, text retrieval programs such as ZyIndex and Gofer offer the capability to analyze large bodies of data in relatively short periods of time. ZyIndex, for example, displays "found" words within the context of the surrounding page. Since the found word is highlighted, it is a simple matter to scan the adjacent lines for their content. Recurring patterns, if they exist, frequently become readily apparent. For instance, patterns of fear, denial, and apathy emerged during a ZyIndex search through interviews with IVDUs in three different cities. It quickly became apparent that each group manifested a distinctive collective reaction to the threat of AIDS: within one group, members spoke of fears and anxieties generated by the death of friends and acquaintances; in another group, members claimed immunity to AIDS. These findings do not constitute a relationship or association in the statistical sense. In other words, there is no apparent correlation between the category of "watching a friend die from AIDS" and "fear of AIDS," although such an association may in fact exist. Instead, they suggest variations in the general climate pervading different groups.

In the above example, computer analysis of social contexts indicated that, although each group may internally experience multiple emotional reactions to the AIDS epidemic, there is a tendency toward a predominant collective response such as fear or denial. The immediate social environment of a group exerts its own subtle influences on the perceptions and actions of the individual actor. In later stages of analysis, a working knowledge of social contexts and variations between groups may help to explain differences in behavioral responses to AIDS (for example, why one group cleans its works with bleach, while another continues to share unsterilized needles among its members).

The point here is that, although this type of analysis may be conducted with or without the aid of computers, rapid text-search software facilitates the analytic process. At minimum, the speed of computer text searches allows the inductive researcher to reach into data and quickly extract the data being sought. More important, the speed of the search process may itself act as a catalyst to conceptualization. Just as the motion picture presents a different and in some ways richer and more comprehensive view of the world than the still photograph, the rapid motion and flow of computer-assisted qualitative analysis may reveal a gestalt pattern that is not visible at a slower pace.

RELIABILITY AND VALIDITY

In any research study, the analyst eventually runs into the thorny issues of reliability and validity. At the simplest level, reliability deals with questions of consistency, stability, and accuracy of measurement, while validity refers to whether or not the researcher is actually measuring the property purportedly being measured.

An ongoing awareness of the need for reliability and validity checks is important to the research process for several reasons. It raises tough but necessary questions about the quality and accuracy of one's data collection instruments and measurement techniques. It also focuses attention on the quality and dependability of the data itself. Moreover, it forces the researcher to grapple with the meanings and the measurability of the concepts under study.

Kerlinger (1973) argues that reliability is characterized by three properties: consistency, accuracy, and degree of error. Briefly, the consistency component answers the question: Will repeated measures with the same instrument consistently get the same results? Accuracy refers to whether or not the measuring instrument calibrates the true underlying units of the phenomenon or property being studied. The third component, error of measurement, addresses the question: Does the measurement instrument itself contain flaws that generate random errors? (Or more precisely, given the imperfect nature of all measurement instruments, how much error does a given instrument produce?)

Validity, on the other hand, deals with the question: Are we measuring what we think we are measuring? (Kerlinger 1973). The three types of validity in Kerlinger's scheme are content validity, criterion-related validity, and construct validity. Content validity refers to "the representativeness or sampling adequacy of the content—the substance, the matter, the topics—of a measuring instrument" (Kerlinger 1973, p. 458). Criterion-related validity is determined by comparing one's own results with external measures or criteria presumed to measure the attribute under study. Construct validity is

a somewhat elusive idea: Has the researcher adequately identified, defined, and isolated the property, attribute, or concept that is being measured?

The focus of construct validity is on the concept rather than on the measurement instrument. To use a physical example, the concept of velocity (or speed) can be understood independent of the measuring devices (speedometers) employed to measure and express the concept. The concept "miles per hour" is easiest to visualize in terms of a dashboard speedometer. But "miles per hour" can be measured without ever referring to a speedometer simply by clocking passage over a known distance with a stopwatch. The core concept or construct remains the same even though the measurement instrument varies. (This procedure would also help establish criterion validity.) The central question remains, Have we actually tapped the concept of velocity? If velocity means the rate of travel (distance traversed per unit of time), then the central construct has been tapped.

Historically, the preponderance of theoretical and methodological concern with reliability and validity issues has been generated by quantitative researchers, especially in the areas of psychology, physics, chemistry, and biology. In a recent monograph, Kirk and Miller (1986) address the issues of reliability and validity specifically in relation to qualitative research.

The point of conducting qualitative social research, to loosely paraphrase Kirk and Miller's argument, is to accumulate knowledge about people by "watching [them] in their own territory and interacting with them in their own language" (Kirk and Miller 1986, p. 9), and to acquire and present these observations in an "objectively" verifiable manner. The search for objectivity, in their view, is an important element of all scientific inquiry (including qualitative studies), because it eschews biased viewpoints, including doctrines claiming a belief in or a monopoly on a "final, absolute truth," and instead encourages the rigor, determination, and discipline to perceive the social realities operating in the external world. Objectivity represents "taking an intellectual risk—the risk of being demonstrably wrong" (Kirk and Miller 1986, p. 10), but it also means that false assumptions, inaccurate observations, and flawed theories will be ultimately exposed and eventually discarded rather than retained as erroneous and misleading "truths."

Qualitative research that maintains the rigorous standards of objective inquiry is thus just as valid and scientific as any other form of research. Qualitative research tools and methods may include informal questions and field observations instead of calibrated instruments and controlled laboratory experiments, but the findings and conclusions of qualitative studies are open to the same scrutiny and subject to the same objective standards of all sciences. Consequently, Kirk and Miller argue that objectivity plays a fundamental role in the qualitative scientific research process.

Objectivity, according to Kirk and Miller, can itself be partitioned into two components: reliability and validity. These concepts in turn reflect the qualitative researcher's need and obligation to control and ensure the quality of one's data collection instruments and measurement procedures and to firmly understand the meaning of one's theoretical constructs (Kirk and Miller 1986). Kirk and Miller argue that the reliability of qualitative data depends primarily on the researcher's methods. Consequently, there are as many forms of reliability as there are methods or research approaches. Kirk and Miller, however, discuss three basic forms: quixotic, diachronic, and synchronic.

Quixotic reliability occurs when "a single method of observation continually yields an unvarying measurement" (Kirk and Miller 1986, p. 41). The unvarying measurement may, of course, be reliable or unreliable, depending on the nature of the method. For example, asking IVDUs the question, "How often do you inject drugs?" could reliably (consistently) elicit the highly inaccurate and erroneous answer "Never." Diachronic (or temporal) reliability refers to "the stability of an observation through time" (Kirk and Miller 1986, p. 42). The observation that IVDUs reduce the number of needle-sharing partners after hearing about AIDS would be diachronically reliable if the same observations are made over a period of several months. Synchronic or concurrent reliability reflects the "similarity of observations within the same time period" (Kirk and Miller 1986, p. 42). This concept is especially useful when there are two or more observers measuring similar events or phenomena, or when the same observer is employing two or more different research methods. For example, the observation that IVDUs start to clean their works more frequently after hearing about the threat of AIDS would be considered synchronically reliable, or at least more reliable, if independently made by two ethnographers. Similarly, a single researcher who obtains the same results using questionnaires, unstructured interviews, and field observations could reliably conclude that the data are synchronically reliable.

Validity, according to Kirk and Miller is "the degree to which a finding is interpreted in a correct way" (Kirk and Miller 1986, p. 20). The emphasis here is on the researcher's perception and comprehension of the concepts underlying the data, rather than on the substantive properties of the data itself. Kirk and Miller distinguish between three types of validity: apparent, instrumental, and theoretical.

Apparent validity or face validity represents an obvious linkage between a measuring instrument and the phenomenon under observation, for example, when an observer presumes that track marks on a subject's arms indicate a history of IV drug use. Instrumental validity or criterion validity occurs when "the observations match those generated by an alternative procedure that is itself accepted as valid" (Kirk and Miller 1986, p. 22). For instance, track marks may be regarded as a valid indicator of IV drug abuse if an

independently administered known instrument, such as urinalysis, produces the same results. Finally, theoretical validity or construct validity exists if there is "substantial evidence that the theoretical paradigm rightly corresponds to the observations" (Kirk and Miller 1986, p. 22). For example, the theoretical construct "drug-induced psychosis" gains validity or may be represented as valid if its component elements (psychotomimetic episodes occurring within a specified period after the ingestion of certain drugs) conform with empirical observations.

Despite their abstract and elusive qualities, validity and reliability are extremely important to the practicing researcher. Reliable data are accurate, consistent, and dependable. Valid data are comprehensible, comparable to data compiled through other known measures, and representative of the properties being measured. Given the time-consuming and sometimes tedious nature of the ethnographic data collection process, it could prove extremely frustrating to end up with data that are worthless or unreliable because they represent measures of the wrong concept. It would be similarly disheartening to waste one's efforts generating data that are invalid or impaired because they fail to tap, exemplify, or substantiate a central theoretical construct. To avoid these frustrations, it behooves the ethnographer to (1) continually check the appropriateness and accuracy of data collection techniques, procedures, and instruments; and (2) constantly reassess notions of the meanings and representative characteristics or properties of central theoretical constructs.

Although it would be impossible to review every means of checking for reliability and validity, there are nevertheless several central operations that warrant discussion. For example, an important data-cleaning function is to isolate and account for the effects of leading questions, confusing or poorly worded questions, inconsistent questioning procedures, or glaring stylistic differences among interviewers' techniques. Such improprieties might bias or contaminate answers and thus impair the reliability of one's data. Similarly, it is often important to examine interview passages containing references to key theoretical constructs to determine what correspondence, if any, exists between those constructs and the world of observable reality. This procedure provides a means of checking the validity of the conceptual framework the researcher has elicited from or imposed on the data.

For the individual ethnographer working alone, or even for the small ethnographic research team, the process of checking the validity and reliability of qualitative data undoubtedly presents a monumental and foreboding task. Yet computerized text management programs can greatly accelerate many of the steps involved in checking data, instruments, and procedures. In fact, any of the several text-retrieval and hypertext programs previously discussed would at minimum help the analyst skim through fieldnotes and interview transcripts looking for sources of unreliability and threats to validity.

When checking the quality of data, the ethnographic researcher may once again want to consider using text-retrieval programs such as ZyIndex, Search Express, or Gofer as a tool for establishing reliability and validity, or conversely, uncovering unreliability and invalidity. These programs can help search the text for evidence of factors that might impair the reliability and jeopardize the validity of qualitative data. For instance, are the questions being asked consistently measuring the thing purportedly being measured? Are the same questions eliciting the same answers time after time? Are those answers accurate reports of real events and activities, or do they represent lip service to the researcher's expectations? Do different researchers get similar answers to the same questions time after time? By specifying appropriate search combinations, the analyst may exploit the speed and flexibility of text-retrieval programs to quickly reveal inconsistencies between answers; expose methodological errors, such as vague or leading questions, that may bias responses; and uncover patterned response sets in which the respondent gives the preferred answer rather than the real one.

An ideal program for checking the validity of coding schemes and theoretical constructs is The Ethnograph. By summoning up the codes presumed to tap or represent key theoretical constructs, the ethnographer may begin to examine their degree of correspondence to the data in the text. A simple (albeit time-consuming) procedure for cross-checking the reliability and validity of analytic codes would be to have a second analyst generate independent codes for interview transcripts or other documents and then compare the degree of correspondence between coding schemes. Since specific codes may be nested in broader or more abstract concepts, the researcher may also engage in conceptual analysis at a variety of levels. For example, the concept of "denial" in a study of IVDU attitudes towards AIDS may include components of "unrealism," "misinformation," and "macho self-concept," each of which may be coded as an independent subcategory of the more general concept. Nested codes thus share a good deal of commonality. By conducting a continuing and intensive comparative analysis of the relationships among analytic codes and between analytic codes and the data, the researcher may make use of The Ethnograph's search facilities as a tool for tapping this commonality and testing the validity of constructs, data, and measurement instruments.

CONCLUSION

For the ethnographer who already employs the computer to store and manage qualitative data, contemporary text-retrieval and hypertext programs provide a much-needed additional tool that can be invaluable in the data analysis process. Several text-retrieval programs have the ability to race through innumerable files looking for specific words or character strings. Others enable the researcher to browse through diverse files, exploring linkages between seemingly unconnected elements. In either case, the end

result is a heightened capacity to probe questions, make connections, generate and test working hypotheses, and follow the flow of ideas in an organized, firmly grounded manner. These technological developments are an important resource that can help lighten the often overwhelming workload of the dedicated researcher.

REFERENCES

Agar, M. Toward an ethnographic language. *Am Anthropol* 84:779-795, 1982.

Agar, M. *Speaking of Ethnography.* Beverly Hills: Sage Publications, 1986. 78 pp.

Giddens, A. *New Rules of Sociological Method.* New York: Basic Books, 1976.

Glaser, B., and Strauss, A. *The Discovery of Grounded Theory: Strategies for Qualitative Research.* Chicago: Aldine Publishing, 1967. 271 pp.

Kerlinger, F. *Foundations of Behavioral Research.* 2nd ed. New York: Holt, Rinehart and Winston, 1973. 741 pp.

Kirk, J., and Miller, M. *Reliability and Validity in Qualitative Research.* Beverly Hills: Sage Publications, 1986. 87 pp.

Schwartz, H., and Jacobs, J. *Qualitative Sociology: A Method to the Madness.* New York: The Free Press, 1979. 458 pp.

Schutz, A. *On Phenomenology and Social Relations.* Chicago: University of Chicago Press, 1970.

Strauss, A. *Qualitative Analysis for Social Science.* Cambridge: Cambridge University Press, 1987. 319 pp.

Walters, J. What is ethnography? In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policymakers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Monograph. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980. pp. 15-20.

AUTHOR

Richard B. Fritz, Ph.D.
Research Director
Comprehensive AIDS Prevention and
Education Program
Chicago Department of Health
Daley Center
50 W. Washington Street
Chicago, IL 60602

128615

Ethnographic Field Stations

Paul J. Goldstein, Barry J. Spunt, Thomas Miller, and Patricia Bellucci

INTRODUCTION

Varieties of field research exist on a continuum of styles of observation. At one end of the continuum is total participant observation, in which the researcher takes on the attitudes, behaviors, norms, and values of a studied population. Such researchers live the lives of their subjects. At the other end of the observational continuum is the researcher who remains completely separate from the studied population. These researchers observe from afar. For example, Cohen (1980) reported studying street prostitution in New York City by sitting in his parked car night after night and observing how prostitutes went about their business.

Somewhere toward the middle of this continuum of observational styles lies the ethnographic field station. This chapter will describe the nature, functions, strengths, and weaknesses of this particular style of field research. The experiences to be discussed come from three research projects that utilized four different ethnographic field stations to collect data about drug users. The Economic Behavior of Street Opiate Users project used three different field sites in the East Harlem area of New York City between 1978 and 1982. The Drug Related Involvement in Violent Episodes (DRIVE) project and the Female Drug Related Involvement in Violent Episodes (FEMDRIVE) project both operated out of the same field site on the Lower East Side of Manhattan between 1984 and 1987. Either directly or indirectly, all the authors were influenced by Ed Preble (1980a; 1980b) in their approach to field work.

WHAT IS AN ETHNOGRAPHIC FIELD STATION?

Ethnographic field stations may be described both in terms of their function and in terms of their appearance. Functionally, an ethnographic field station is a research "outpost" in a community of interest to the researchers. It is a place where data are collected. It serves as a base of operations for the research staff, providing an environment in which research subjects and research staff may interact over an extended period of time.

In appearance, field sites may look quite different. The three that were used for the Economic Behavior study were all storefronts on tenement blocks. The first was on 95th Street between First and Second Avenues. The second was on 105th Street between First and Second Avenues. The third was on 117th Street between Second and Third Avenues.

The block that housed the 95th Street storefront has since become an object lesson in the rapidity of change in New York City. This block has been absorbed by the expansion of the fashionable Upper East Side. Abandoned buildings and tenements have been renovated into high-priced condos. A sidewalk cafe now flourishes where drug dealers used to cluster on the stoops of abandoned buildings.

The 105th Street location was one of the most active blocks for drug sellers in East Harlem at the time of our research. It contained both occupied and abandoned tenement buildings. Directly across the street from our storefront was an abandoned school building guarded by monstrously beautiful gargoyles. The copper roof of the school had been stripped all around the upstairs windows by heroin users seeking the copper for resale. One person leaned too far out of one of the windows and fell to his death in the courtyard below. Though not as dangerous as copper collecting, data collecting in the 105th Street site was rather difficult.

> Plaster fell from the ceiling. The toilet would not flush, and there was seldom any heat in winter. Recording data was a problem because of the cold. If the interviewer sat without gloves for any length of time, his hands would be too numb to hold a pen or pencil. The alternative was wearing gloves, which made it difficult to write. Completed interview schedules often bordered on the illegible. Interviewers finally developed a system of taking off their gloves, recording interview data for about 5 minutes, rubbing their hands vigorously and perhaps putting gloves back on for a minute or two while chatting with subjects, and then returning to the formal interview. Between interviews they would walk around outside, if there was any sun, to warm up a bit. (Johnson et al. 1985, p. 204)

The third storefront, a former bodega on 117th Street, was owned by a plumbing contractor, which boded well for heat in the winter. This site was much larger than our previous ones, had two working toilets, and was well lit. In fact, our major environmental problem was the 5-year-old who lived directly above the site. He and his friends enjoyed playing in their bathtub and would frequently let the tub overflow until the water gushed through the ceiling.

Research staff maintained a congenial atmosphere in the 117th Street storefront. Coffee was available to all. Subjects often brought their

children with them, and at least one baby took her first steps there. Neighborhood residents, some of whom were former subjects, used the field site as a place to stop in, sit, have a cool drink or snack, and trade yarns and the latest neighborhood gossip.

The DRIVE and FEMDRIVE field site, on the Lower East Side of Manhattan, consisted of three rooms in an old, dilapidated building owned by the City of New York and administered by a community organization as a neighborhood center. Other organizations occupying space in this building included two dance companies, a Hispanic cabaret group, the Third World Women's Archives, the Women's Health Collective (a source of primary health care for lesbians offered by lesbians), a cadet corps, and a carpentry workshop. The building was covered with graffiti, both inside and out. Our bathroom was functional for about 6 months of the nearly 4-year research project. Staff members would vie for the privilege of picking up lunch for everybody at a nearby delicatessen because of the opportunity it afforded to void a swollen bladder. Those with spastic colons received special consideration.

On a more positive note, the building, which was formerly the Church of All Nations, contained a chapel with lovely stained glass windows. Meetings of the National Institute on Drug Abuse (NIDA)-sponsored Community Epidemiology Work Group were held in this chapel in 1986. There was also a large room in the building that featured a gymnasium floor with a full basketball court and a stage at one end. Staff members and research subjects would occasionally shoot baskets in the gym. The gym was also used for avant-garde art shows, plays, dance recitals, and community meetings.

The "suite of offices" occupied by DRIVE and FEMDRIVE was in relatively good shape because it had been fixed up by the former occupant, a Cornell University nutritional project. A large entry room served as a waiting area and a place for "rap sessions." Hot coffee and crackers were generally available. A small room served as the project office. A medium-sized room in the back was partitioned into three cubicles for interviewing. The field site was staffed on a daily basis (excluding weekends) by at least three project staff.

FINDING THE FIELD SITE

The research process really begins with the search for a field site. During this time researchers begin to establish a street presence in an area. It is an opportunity to develop street contacts and to informally discuss issues to be studied. Potential research subjects may be screened for general veracity, cooperativeness, and so on.

A description of 1 day in search of a field site was extracted from the DRIVE and FEMDRIVE Principal Investigator's field log. It conveys a sense of the neighborhood and of the interrelationships between store owners, landlords, police, drug users, and the social researchers who study them.

Field Log, 8/84. I was looking for a storefront in which to do DRIVE research. I wandered the streets of the Lower East Side looking for "store for rent" signs and jotting down phone numbers listed on signs. I used the search for a field site as an opportunity to meet some of the neighborhood people. I frequently stopped into stores and asked persons if they knew of any places for rent. This often led to conversations and new information on the current state of the Lower East Side. In one small tobacco store, for example, I met Brian, who managed the store for the owner. We have had several nice chats. He echoed the statements of others about how gentrification has driven the rents very high and is driving many of the small store owners out of business or forcing them onto less desirable blocks. Since I am looking for a place on a less desirable block I am now in competition with these store owners; hence, even the rents on less desirable blocks are going higher. I wound up buying a pewter Sherlock Holmes chess set from Brian.

One example of the storefronts that I found on Essex Street was a single large room, about 20 feet by 24 feet. It was in pretty good condition. Lots of rubble on the floors but a pretty solid ceiling and floor. Two of the walls were brick. Very nice. The electricity was "broken" and it would be my responsibility to fix it. Likewise the plumbing. The landlord said the heat would be very weak in winter and I should plan to invest in gas heaters. The tenant in the store next door on the same pipes said there was no heat in the winter. The rent was $800 a month. Expenses would include fixing the electricity and plumbing, buying heaters, and partitioning the room in order that confidential interviews could be done.

A landlord showed me three storefronts on Ridge Street. The first was about 29 feet by 22 feet. It was unheated. Apparently, many years ago, the storefront had a glass front but the glass had long since vanished. Pieces of wood and rusty metal were haphazardly nailed to the front. There were many gaps that the wind whistled through. The ceiling was peeling badly and looked like large pieces might fall down at any time. It was badly in need of painting. There was a john, but it was not working. The rent was $700 a month. It had formerly been a shoe store, and many pairs of shoes were lying about. The landlord examined the shoes while I examined the place. He took a pair when we left.

The second storefront on Ridge was too small, about 12 feet by 15 feet. A strong smell of urine permeated the place. There was no heat, and the plumbing did not work. It too had wood and metal nailed to the front that

did little to protect the interior from the elements. The rent was $400 a month.

The third storefront on Ridge Street was long and narrow, about 10 feet by 40 feet. There was no heat, and the front was again nailed up with rusty metal. However, the toilet did work. The rent was $400. The storefront was located on the ground level of a tenement that appeared about 50 percent abandoned. This place was a possibility, but the extreme narrowness of the place presented problems. Also, significant expense would have been involved in heating the area and repairing the front. It used to be a social club, and there was a hidden room behind a steel wall. The landlord professed not to know what went on in the hidden room.

The landlord was a character. In his late fifties, he exuded lecherousness and unscrupulousness. He was wearing a pink shirt, dark grey pants, and a black hat and had a 3-day growth when we met. His favorite expression was "You pay for everything, I pay for nothing." The following incident took place at the third storefront. While he was showing me the place, two tough-looking Hispanic street women, one looking very stoned, appeared. They were trying to talk to him about money. He did not appear to want the conversation to take place in my presence. He kept trying to get them to wait until I left. However, they appeared too stoned to understand and kept bringing up the subject of money. They had come down from upstairs in the tenement. They appeared to be in their early twenties. One put her arms around the landlord's neck and rubbed her body against him. He grinned appreciatively. She knocked his hat off his head to the floor. He laughed and said, "You got to show more respect." The girl replied, "Oh, we respect you." I left soon afterwards. The landlord was still talking to the girls.

Later that day I was walking the streets of the Lower East Side with Barry and Marietta. We were on Houston Street between Ludlow and Essex. A police van with three officers pulled up alongside. Two cops were in the front and one was in the back. One called to us. The following dialogue ensued:

Cop: Hey you!

Paul: Who me?

Cop: I don't see anybody else. We've been watching you three. You were on Avenue A. You were on B, C, and D. What are you doing in the neighborhood?

Paul: Research.

Cop: Well, I'm stupid. I don't know what that means. Explain it to me.

Paul: We work for Narcotic and Drug Research, Inc., which is a not-for-profit affiliate of the New York State Division of Substance Abuse Services. We're doing research into the drug problem on the Lower East Side. We are looking for a storefront. We were just up on Ridge Street looking at one.

Cop: What number on Ridge Street?

Paul: (Number deleted for reasons of confidentiality.)

Cop: Oh, you don't want to rent from that guy. He's got a real record. We bust him regularly. We got him for selling needles to an 11-year-old girl. He rents mainly to drug addicts. We bust his place all the time. It would be real embarrassing to you to rent from him. Why don't you stop by the station house and the sergeant will tell you where there are good places to rent.

Paul: Thanks. We'll probably do that.

Cop in front seat: Why don't you go to 6th Street and ask Jimmy Carter if he has space? (NOTE: Former President Jimmy Carter was at that time on the Lower East Side working with the Habitat group repairing tenement apartments to create housing for poor people.)

Paul: I was with him a few weeks ago in Atlanta. Maybe I will ask him.

Cop: Well, thank you for your time, sir.

Paul: Take care. **End Field Log, 8/84.**

CONDUCTING THE RESEARCH

Overview

Each of the research projects that are reported on herein as utilizing ethnographic field stations were attempts to combine qualitative and quantitative techniques. These studies sought to retain the richness and depth of ethnographic understanding, while recording specific behaviors of individuals over time in a quantified way. In the Harlem research, our focus was on economic behavior, so we carefully documented all income and expenditures of drug users over a specified time period (33 days). In the Lower East Side research, our focus was on violence, so we documented all instances of violent victimization and perpetration over a specified time period (56 days).

The basic approach in all the studies was to have particular respondents reporting to a field site on a routine basis for interviews. Reporting

schedules were on a weekly basis for the most part. Interview questions were both open-ended, e.g., a complete description of a violent encounter, and closed, e.g., specification of types and amounts of drugs purchased and consumed on a daily basis.

The routine weekly interviews constituted the critical core of our data collection. However, other forms of data collection were also used. All subjects received a life-history interview. Staff members spent time with subjects on the streets, in their apartments, and in bars and restaurants and recorded their observations. Informal rap sessions took place constantly in the field site. Special taped interviews were done about topics of interest to the research staff, e.g., the workings of a specific drug-dealing hierarchy, life in shelters for the homeless.

Sampling and Recruitment

The existence of a permanent, fixed-location field station greatly simplifies the process of recruiting research subjects. Subjects may be recruited from field contacts established while searching for a field site. Snowball-sampling techniques may be employed as initial subjects refer friends, family, or other associates. Researchers may contact other local organizations, e.g., methadone maintenance treatment programs, and recruit subjects from their client populations. News of the project circulates on the street grapevine. The important factor here is that the fixed location of the field site means that potential subjects always know where they can go to find the researchers.

The DRIVE/FEMDRIVE field site happened to be located two blocks from the Third Street Men's Shelter (a central processing point for homeless males in New York City), about five blocks from a main womens' shelter, and very near to a number of Bowery flophouse hotels. Word of the project spread rapidly among the homeless of the area, and subjects were also recruited from this population.

Novice field researchers, or inexperienced outsiders, usually ask how it is possible to get strangers to talk about deviant and/or criminal activities. In fact, this has never proven to be a problem. Subjects are paid an interview fee as compensation for their time. In the late 1970s, interview fees were $5 for an interview of 1 hour or less. In the 1980s, these fees have risen to $10 for an interview of similar length. Preble commented as follows about the practice of paying subjects for interviews.

> Money is usually the sole motive for participants at the beginning, and this is often the cause for professional skepticism about the validity of research findings based upon such a foundation. But if money is a mean motive for those we recruit, it is the same for us who do the recruiting; and so we

> are even. The fact is that one can work with this motive and parlay it into honest, thoughtful participation. (Preble 1980a, p. 69)

Carefully constructed sampling plans have a tendency to go awry in the field. For example, on the DRIVE project, initial plans to engage in quota sampling based upon subjects' use and/or distribution of heroin and cocaine proved to be unworkable. The $10 interview fee offered to subjects was a real incentive to participate in the project. Unfortunately, this incentive was also sufficient to motivate potential subjects to claim they were engaged in heroin and cocaine use or distribution and qualify for the sample, when in fact they were not involved in these behaviors.

The interests of reliability and validity of DRIVE data appeared best served by limiting, to the greatest extent possible, any incentives for subjects to lie or exaggerate. Therefore, subjects were told that we were interested in talking with users and distributors of all sorts of drugs, and also that we wanted a "control" sample of nonusers and nondistributors. By opening up the DRIVE sample to everybody, we removed the desire to be included as an instrumental reason for "conning" the researchers. DRIVE staff felt more confident about the veracity of subjects' claims to be using or distributing specific drugs because the pecuniary motivation to make fraudulent claims had been eliminated.

Demographic characteristics provided vivid confirmation of the tendency for subjects to try to con their way into the sample. For example, at one point our sample was skewed toward older subjects. Field staff were instructed to recruit younger persons. As soon as this became common knowledge, aspiring respondents in their forties or fifties presented themselves at the field site claiming to be in their early twenties. Such persons were rejected.

An even more difficult and amusing situation occurred because of the temporal overlap between the DRIVE (male) and FEMDRIVE (female) projects. The DRIVE sample included a number of gay subjects, some of whom were transvestites. Toward the end of DRIVE data collection, no new male subjects were being accepted. However, we were accepting new female subjects for the FEMDRIVE project. Several persons who were obviously transvestites claimed to be female, hence eligible for inclusion in the FEMDRIVE sample. These persons were rejected. In a few other cases, staff were not sure if an aspirant for FEMDRIVE was, in fact, a female. In such cases, different staff members conducted interviews and then reached consensus as to whether the respondent should be kept in the sample or terminated.

These situations illustrate a basic reality of field research: the problem is seldom how to recruit subjects, but how to reject those who, for one reason or another, do not meet criteria for participation. Rejections must be done

politely but firmly. Those who are rejected may become angry, create a scene in the field site, and even threaten staff members.

Various techniques may be employed to reject potential subjects without creating a violent scene. One way, not recommended, is to tell the person that all recruitment is handled by a specific staff member, who is not present. The ploy may work well at the time, but is really rather cruel. If the potential subject returns on repeated occasions looking for that staff member and not finding him or her, the situation becomes frustrating for all. If the potential subject does find the sought-after staff member, a somewhat sticky situation may develop, which is generally resolved by the staff member telling the person that he or she is no longer responsible for recruitment of subjects and that a different staff member (not present, of course) must now be seen.

Better ways to reject subjects include telling the potential subject that the study is "full up" right now, but that he or she should come back in a month or two. Potential subjects who have telephones may be asked to leave their number at the field site and "don't call us, we'll call you." Such persons may also be told that scientific processes of randomized sampling are being employed, and that if their number comes up they will be called.

Key Informants

Over time, ethnographic projects should develop key informants. Key informants are trusted and respected members of the population under study. They may be current or former drug abusers. These individuals are utilized as neighborhood guides, showing research staff the principal copping (drug distribution or drug dealing) areas, gaining access to specific population segments (e.g., ethnic groups, dealing hierarchies), and explaining peculiarities of events and subcultures to research staff. Utilization of key informants widens a project's network of contacts and adds to its credibility and legitimacy on the streets. Key informants may also play a critical role in the recruitment of research subjects. This is especially true when a particular sort of person, e.g., burglar, female cocaine dealer, is sought for the project.

An account of a visit to a nearby "shooting gallery" was extracted from the principal investigator's field log. The guide involved, code-named Ragtime, had completed his regular reporting cycle at this time, but continued to hang around the field site. A good rapport had developed between him and project staff. He was perceived as worthy of trust.

Field log, 11/85. Ragtime asked me if I wanted to visit a shooting gallery with him. I said OK. We walked around the Lower East Side. Ragtime crossed the street real crazy. Kept going against the light, dodging cars. I

waited until it was safe and was always trying to catch up with him. He said I didn't cross like a New Yorker. I said I was practicing my Jersey guy look. I told him I knew now why bags marked "death wish" sold so well. He laughed.

Ragtime didn't like the looks of the street that the shooting gallery was located on. He pointed out people that he said were junkies and said that they all seemed to be dispersing away from the gallery. He indicated about five different people. To him this indicated that there was some sort of trouble there, perhaps police. He said that he would take me to a different shooting gallery.

As we walked, he discussed how he would introduce me. He wanted to introduce me as a guy from Jersey. I wanted to be introduced as exactly what I was, the director of a research project. Ragtime said that it was too soon to be honest with people like that. I said that I was uncomfortable lying and that it was possible that other subjects who knew me would see me at the gallery and say something about who I was. Ragtime agreed that this was a possibility. The issue remained unresolved as we neared the gallery.

The gallery was near Avenue B. Ragtime said that if a motorcycle was parked outside, that meant that the gallery was open. The motorcycle was there. The entrance was through a hole in a chain link fence near a gas station. A parked van provided a screen (intentional?) so that people going through could not be readily seen from the street. We went through the fence and were in a rubble-strewn backyard. Ragtime left a shopping bag that he was carrying in the yard by the fence. An entrance into the building had been cemented up, and a hole had been smashed in the cement. We crawled through. We were in a totally dark room. It took a while for my eyes to adjust to the darkness. The ceiling, walls, and floor were all collapsing. I had the feeling that giant rats were lurking all over the place. There were some big holes in the floor covered with boards, like little bridges, so that you wouldn't fall through. I had no idea how far one could fall.

Ragtime reached under a slab of concrete and pulled out a brown paper bag. He told me that it was the works that he and his friend shared. I looked at them. There were two needles and syringes and a soda bottle cap wrapped in a paper towel. I asked him how he would know if anybody else had used them while he was away. He said that he didn't know.

I began picking up some empty dope bags that the floor was littered with. I could barely see in the dark so I stuffed a few into my pocket and examined them when I got outside later. They were stamped The Titanic and Jaguar.

Another man, a thirtyish Hispanic, crawled into the room through the hole. Ragtime said hello. The man looked surprised to find anybody there. He unzipped his fly and pretended to pee against the wall. I could see that nothing was coming out. He was just pretending that was the reason for his coming into the building. He soon left. He returned a few minutes later with three other guys. Ragtime said hello to one of them, who was named Angel. Ragtime had previously told me that his friend who ran the gallery, and with whom he shared works, was named Angel. I guessed correctly that this was the guy. Ragtime made no move to introduce me. I said nothing, which Ragtime later told me was the right thing to have done. Angel asked Ragtime to go and wait outside. He said that he had some business to conduct. Ragtime said OK, that we would go and get a beer. We left.

Ragtime said that he had gotten nervous when the four had come in. He thought that he might have to fight his way out, with me behind him. He said that Angel carried a gun and was a little crazy. He said we could have gotten decapitated. I was pleased to be on Houston Street at that point.

I asked Ragtime a few questions about the gallery. He said that it was only for people getting off with needles. Nobody went in there just to smoke reefer or drink wine or do pills. He said that heroin users could stay as long as they wanted, but that coke users had to leave as soon as they had gotten off because "nobody wants to listen to them talk all day long." **End Field Log, 11/85.**

Theft and Violence

Theft and violence are two issues that must be dealt with when trying to operate an ethnographic field station. Tape recorders, jackets, cash, tools, coffeemakers, and a clock have been stolen from field sites. Thefts are typically difficult for staff members to accept and are often perceived as a violation of the trust between subjects and field researchers. Sometimes staff members interpret a theft as meaning that they have personally failed as researchers, that they are outsiders and "marks," and that their feelings of being accepted by subjects were just illusions. Victimized staff members may need informal therapy lest feelings of resentment and suspicion color their interactions with subjects and render them less effective. Thefts that involve items that belong to the project, e.g., a tape recorder, are typically easier to accept than theft of personal items, e.g., a leather jacket saved for from months of paychecks.

Theft may affect not only staff members, but also subjects and the research. Subjects who steal from the project tend to feel guilty about it and may fear punishment. Having stolen, they probably will not return as subjects.

The project and its staff thus may lose not only a valued item, but also a valued subject.

Staff members are told that most thefts that occur at field sites are cooperative ventures. The person who left a valuable tape recorder sitting unattended on a desk is as responsible for the theft of that tape recorder as the person who picked it up. The person who flashed a wad of cash is as responsible for an ensuing robbery as the person who was tempted by that wad. Some simple precautions may prevent the theft.

Staff members may be holding several hundred dollars of both interview and personal money at any given time. An account of an unavoidable robbery in one of the Harlem storefronts follows, showing how the loss was limited by the researcher's carrying two separate bundles.

> I was in the storefront one day with K. There was a knock on the door and I went to open it. A research subject stood there. He asked me for some money for carfare. I gave him a few dollars. He wanted more. I said I didn't have any more. I closed the door and went back to talking to K. A few minutes later, another knock, same research subject only this time he was with two others. They were standing off to the side where I couldn't see them. I opened the door a little annoyed and then I saw the other two. I thought to myself, this didn't look too good. One guy had his hand inside his jacket. The other had his hand in his pocket. The research subject had his foot in the door. He said, "Tom, I want money." I knew what he meant. I had a few hundred dollars in one pocket and less than a hundred in the other pocket. I tried to stall to remember which pocket had the most money in it. I said to him, "What do you want to do this for? Don't you always get carfare here? Don't we always give you money to go to your program or see your P.O.?" His answer was "I'm doing it because I'm a junkie and I need money." I wasn't sure which pocket held the larger amount, because I was kind of scared, but I knew I couldn't stall much longer. I reached into a pocket and gave him the money. It was the right one. He got about $68. Before I gave up the money I was thinking about slamming the door on his foot, but I didn't know what the guy with his hand in his belt was holding. If I was sure he was only holding a knife I would have slammed the door. After I gave them the money and they left, my knees started to shake. They were shaking for 10 or 15 minutes. But the main thing is . . . nobody got hurt.

A violent episode at the Lower East Side field site occurred when a staff member was mugged by a research subject and two others in March 1985. Following is an account of the mugging from his diary:

> I had arrived at the site a few minutes early, after going to the bank to get interview money. "Singer" (a subject I had interviewed three or four times previously) was sitting in front of the building with two males. Singer said they were friends who wanted to get involved in the project. While we were making small talk, Dave, who was also hoping to get interviewed, joined the group. I felt comfortable with the situation and decided to go upstairs to schedule appointments. Upstairs, the two friends were really pushing to get interviewed immediately; I tried to be nice and said that we would do our best. As I turned around to make coffee, Singer's friend came at me from behind, picked me up, and slammed me to the floor. One covered my mouth with his hand, the other held my arm behind my back. They went through my pockets, where they found the interview money and my wallet. They asked if I had any more money. I said, "No." The three of them ran down the stairs and disappeared. Dave sat there and did nothing. I don't think he was in on it but I'm not sure. The whole thing happened in 10 to 15 seconds. I ran out of the building and saw Tommy (DRIVE staff member) walking toward the building. I told him what happened. He asked if I was hurt. I said, "No." He said, "That's all that matters." At that time, Paul arrived. We all went back upstairs.

The field site was closed for the day. The staff member experienced a certain amount of self-blame and even some hostility toward fellow staff for not being present at the time (even though he had arrived early). The event was reported to the police, and the staff member went to the local precinct to examine mug shots (Singer and his friends were not spotted). Security procedures at the site were strengthened; from then on staff met at a local diner and went to the field site together.

Partly for security, trusted subjects who had completed the interview cycle were encouraged to hang out at the field site. Some were put on a retainer to act as "security," general "go-fors," and traffic monitors, e.g., making sure that interviewees were interviewed in the proper order. A total of five subjects served in this capacity at the Lower East Side field site, never more than one at any given time.

CONCLUSION

Strengths of Field Sites

The major strength of ethnographic field stations is that they facilitate the rigorous collection of longitudinal data from reasonably large samples. Field researchers who do not have a field site to work from must contend with such difficulties as finding quiet and private places in which to do interviews and being able to make time and space connections with subjects. In the latter regard, I have spent hours in various cities being taken about by fellow ethnographers. Frequently, street corner hangouts that I am taken to are deserted. Bars where certain types of persons hang out are empty. My colleagues shake their heads in dismay and ask, "Where is everybody today?"

I have been similarly embarrassed in New York, but not since I began operating out of ethnographic field stations. The field site provides a point where subjects and researchers comfortably congregate. The field site allows a more effective use of researcher time: less time is spent seeking subjects out, or waiting for people to show up in a bar or on a street corner, and time is spent in direct interaction and data gathering.

The relationship between a field site and traditional ethnographic street work may be seen as analogous to the relationship between the health clinic and the physician's house call. The field site/health clinic model allows the maximum number of people to be seen in a day, allows needed resources and equipment (in the case of research, tape recorders, interview instruments, and so on) to be stored where they are used, and provides a team environment in which staff members may help each other out and watch each other's backs.

The field site provides an important stability and continuity to researcher-subject relationships. Subjects know that they are welcome to stop in for a cup of coffee and a chat. Much interesting and relevant information is collected during the course of these informal conversations and through sharing discussions between subjects.

Study participants who have been out of circulation for some time (e.g., due to hospitalization or incarceration) find it a simple matter to reestablish contact with the project. Subjects who need a safe haven find it at the field site, and staff may become privy to accounts that they might not have.

Also, having the field site means that project staff have "a place" in the neighborhood. It engenders a feeling of rootedness and belonging in the study area. It serves to negate the perception of staff as strangers and facilitates interaction with neighborhood residents. The project becomes accepted as belonging to the local community.

Weaknesses of Field Sites

The ethnographic field station model contains a number of weaknesses. One is the cost. Rent, utilities, renovation, furnishings, coffee, coffee cups, cream, sugar, napkins, crackers, mop, broom, cleaners, and so on, add up to significant monthly expenditures. A major strength of ethnographic research, that it can be done by concerned and knowledgeable researchers on a shoestring, disappears. Funding becomes necessary.

Another weakness is the potential for inertia. The ethnographic field station tends to breed complacency. One's expectations for getting out into the street become blunted over time. Too much time is spent sitting around the field site, not enough time is spent with subjects in their milieus, e.g., apartments, schoolyards, bars, and so on. In this regard, the field site is, after all, an environment manufactured by the research team and does not constitute a "natural" setting.

Use of an ethnographic field station tends to create a "9-to-5" mentality. There are times when the field site is open, and times when it is closed. Staff members tend to prefer something akin to a 5-day week from 9 to 5. This is usually a preference endorsed by family and loved ones. But good ethnography is not a 5-day, 9-to-5 undertaking. Important goings-on tend to occur at night or on the weekend. If the researcher is not present at these times, he or she can only hear accounts of these events. Potential subjects who are themselves employed during normal working hours have no opportunity to participate in the research.

The quality of subjects attracted by ethnographic field stations tends to deteriorate over time. Those most in need of shelter during the winter, a free cup of coffee, a safe place to sit, will increasingly congregate at the field site and, perhaps, deter more successful subjects from coming in. The "moochers" become increasingly annoying. These are the persons who come by solely for "carfare." They may come by every day, and they can tolerate an enormous amount of discouragement.

The health clinic analogy that was made in the discussion of strengths also illustrates one of the weaknesses of ethnographic field stations. The health clinic is more bureaucratized than the personal physician. When an interviewer tells a client who has been "spilling his guts" that he or she will not be in next week but, "Not to worry, you can talk to someone else," then something of the personal connection between field researcher and subject is lost.

SUMMARY

Ethnographic field stations are not a substitute for traditional ethnography, because they provide an "unnatural" setting for research interaction and

observation to take place. However, they do provide perhaps the optimal setting for qualitative and quantitative methodologies to be undertaken in a single research effort. In this sense, they are critically important for advancing our knowledge about drug abusers and their behaviors.

REFERENCES

Cohen, B. *Deviant Street Networks: Prostitution in New York City.* Lexington: Lexington Books, 1980.

Johnson, B.D., and Lipton, D.S. Creative tensions: Issues in utilizing ethnographic research within a single State agency. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policy Makers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Monograph. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980.

Johnson, B.D.; Goldstein, P.J.; Preble, E.; Schmeidler, J.; Lipton, D.S.; Spunt, B.; and Miller, T. *Taking Care of Business: The Economics of Crime by Heroin Abusers.* Lexington: Lexington Books, 1985.

Preble, E. What an ethnographic field station looks like. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policy Makers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Monograph. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980a.

Preble, E. Problems utilizing ethnography in a single State agency. In: Akins, C., and Beschner, G., eds. *Ethnography: A Research Tool for Policy Makers in the Drug & Alcohol Fields.* National Institute on Drug Abuse Monograph. DHHS Pub. No. (ADM)80-946. Washington, DC: Supt. of Docs., U.S. Govt. Print. Off., 1980b.

AUTHORS

Paul J. Goldstein, Ph.D.
Barry J. Spunt, Ph.D.
Thomas Miller
Patricia Belucci, M.A.

Narcotic and Drug Research, Inc.
11 Beach Street
New York, NY 10013

Ethnographic Research on Hidden Populations: Penetrating the Drug World

Patricia Adler

INTRODUCTION

Ethnographic research methods have, for many years, followed principles elaborated by members of the Chicago School. Building on the guidance of University of Chicago sociologists Robert Park, Ernest Burgess, and W.I. Thomas, a spate of now classic studies were conducted. These studies focused primarily on the areas of community and deviance (Anderson 1923; Cressey 1932; Thrasher 1927; Landesco 1929). The naturalistic research methods of these fieldworkers served as inspiration for the scholars who comprised the department in the 1940s and 1950s, faculty members such as Herbert Blumer, Everett Hughes, William Lloyd Warner, Anselm Strauss, and David Riesman. While the fieldworkers of the earlier era had practiced a wide range of research strategies, they had not written any accounts of their methods or delineated strategies and principles for ethnographic research. Scholars and their students produced early methodological works in the 1950s and 1960s, which articulated the principles, practices, and problems of the newly named "participant observation" (Becker 1967; Becker and Geer 1960; Gold 1958; Junker 1960; McCall and Simmons 1969; Miller 1952; Schwartz and Schwartz 1955; Vidich 1955).

The Chicago School tradition of field research urged its practitioners to gain an understanding of the empirical world by seeking out the members' perspectives (Blumer 1962; Schutz 1962; Schutz 1967), observing human group life naturalistically (Denzin 1970; Schatzman and Strauss 1973), and being *in situ* (Hughes 1971). Researchers were told to enter the setting and acquire the trust of members by being straightforward, honest, and cooperative. The overt role, in which the researcher's intentions and identity are revealed to the subjects, was preferred over the covert role. In conducting their research, individuals were advised to tread a fine line between subjectivity and involvement on the one hand, and objectivity and detachment on the other (Becker and Geer 1960; Blumer 1969; Burgess 1982; Gans 1982; Gold 1958; Hammersley and Atkinson 1983; Junker 1960; Lofland 1971; Miller 1952; Pollner and Emerson 1983; Schatzman and Strauss 1973;

Willis 1977). Too much of the latter could fail to yield an accurate and insightful account of the scene and its members, while too much of the former could bias the researcher into adopting the members' point of view and interfere with analytic objectivity. If one had to err, however, it would be best to maintain detachment. This was based on the fear of "going native," a situation in which the researcher becomes a member of the scene, uncritically adopts its perspectives, and fails to return to the scientific community to present findings. The Chicago School advocated marginal roles for researchers in field settings, poised between familiarity and strangeness (Powdermaker 1966), between being a stranger and a friend (Everhart 1977), to observe the essence of ongoing activity while inconspicuously fading into the background (Bogdan and Taylor 1975; Bruyn 1966; Burgess 1982; Gans 1968; Hammersley and Atkinson 1983; Jarvie 1969; Junker 1960; Paul 1953; Pollner and Emerson 1983; Powdermaker 1966; Trice 1956; Vidich 1955; Williams 1967).

The desirability of the Chicago School guidelines for field research in the 1980s has weakened because of changes in thinking about epistemology since the 1950s and 1960s. Some field researchers have questioned the emphasis on objectivity and detachment, as found in quantitative research methods; they believed that fieldwork should build on its strength as a subjective approach to data gathering (Douglas 1970; Douglas 1976; Douglas and Johnson 1977; Feyerabend 1972; Johnson 1975; Mehan and Wood 1975; Phillips 1974). The value of fieldwork lies not in how the researchers maintained distance from the data, but rather in how close they came to the phenomena. In the 1970s, it was suggested that the more deeply a researcher penetrated the scene, the more accurate and valid were the data. Contemporary fieldwork epistemologies, such as existential sociology's investigative paradigm and early ethnomethodology's advocacy of becoming the phenomenon, urge researchers to "cast off the shackles" of the classical approach and strive for subjectivity and involvement. The human side of researchers gives field research its great advantage: empathy with those studied by immersion in the subjects' empirical world. Instead of hanging back lest they influence the setting, researchers should realize that there is no such thing as a fieldworker not influencing the setting (Jarvie 1969). By drawing close to the phenomenon, the feelings and insights that researchers develop can be utilized as data. Active participation should be done both to penetrate the scene and to understand it.

RESEARCH STYLES AND HIDDEN POPULATIONS

When studying hidden populations, the issue of research approaches becomes even more germane. Deviants are more difficult to locate, befriend, and investigate than other subject populations. Special techniques apply to participant observation with these groups. First, being forthright and friendly is not enough to gain entry; more calculated strategies may be necessary for these secretive and suspicious groups. Exclusive reliance on

the overt role is unrealistic. Covert roles, used alone or in concert with overt roles, may yield insights that the exclusively overt approach would miss. Second, understanding the multiple layers and hidden realities submerged within secret worlds may be facilitated by a team field research approach (Douglas 1976). A team comprised of different genders, ages, and backgrounds gives the advantage of multiple perspectives into the complexity of the empirical world (Warren and Rasmussen 1977). This is especially useful in deviant worlds, fraught as they are with inner contradictions. Third, adopting an investigative approach, in which the researcher probes beneath the surface, often produces results more quickly and effectively. This is not to suggest that researchers abandon social norms of interpersonal tact, which would be deleterious to their field research relationships, but rather that they keep in mind that things are often not as simple or innocent as they seem.

Membership Roles

These new ideas in field research epistemology press ethnographers to take membership roles (Adler and Adler 1987) in their researches. They need not assume full-time, complete involvement in the scene, but they should establish a role that is more than marginal. Three types of membership roles are possible: peripheral, active, and complete. The peripheral membership role is the most marginal and least committed to the world under study. Researchers taking this posture would assume an insider's position by interacting closely, frequently, and significantly with members of the group under study. They would not assume the position of a central member and would refrain from participating directly in activities that stand at the core of group membership and identification. As a result, they generally do not assume functional roles within the group, although they are often members of the group's social world.

The active membership role moves researchers away from the marginally involved role of the traditional participant observer into a more central position in the setting. They do more than participate in the social activities of group members; they take part in the core activities of the group. In so doing, they generally assume functional roles, thus interacting as colleagues or coparticipants with members.

The complete membership role entails the greatest commitment on the part of the researcher. Beyond merely participating, complete-member researchers immerse themselves fully in the group as "natives." They and their subjects relate to each other as status equals, sharing a common set of experiences, feelings, and goals. Inevitably, they come closest of all researchers to approximating the emotional stance of the people they study. In conducting their research, then, they often adopt an overt role (i.e., they are open about their work to those they are studying). While becoming the phenomenon is one way to achieve the complete membership role, it is

more common, in studying hidden populations, for the converse to occur. That is, members of the group become researchers and take advantage of their personal history to "opportunistically" (Riemer 1977) exploit their biography. Thus, they turn a scene in which they are current or former members into a research setting. Much of the best work on deviance, in both pure and applied research, is currently being done by former members of these subcultures (Brown 1988).

The advantage of membership roles over other forms of research lies in members' recognition of the researcher as a fellow member. This recognition by the group allows the researcher to participate in the routine practices of members and, as one of them, to experience the members' world. Membership roles force researchers to take on the obligations and liabilities of members. In repeatedly dealing with the practical problems members face, researchers ultimately organize their behavior and thinking about the setting's everyday reality in much the same ways as do members, providing, it is hoped, a deeper and more accurate understanding of the hidden populations that they study.

MEMBERSHIP IN PRACTICE: RESEARCHING DEALERS AND SMUGGLERS

From 1974 to 1980, my research partner (my husband) and I engaged in daily participant observation with members of a drug-dealing and smuggling community (Adler 1985). These operators constituted the drug world's upper echelons, as they imported and distributed tons of marijuana and dozens of kilos of cocaine at a time. The extremely illegal nature of their trafficking activities made these individuals cluster together for both business and social relations, forming a deviant subculture that reflected their common norms and values.

The methods we used to study this group were direct and personal. Although we did not deal ourselves, we participated in many of their activities, attending social gatherings, traveling with them, and watching them plan and execute their business activities. We came to know members of this subculture and formed close friendships with several of them. In addition to observing and conversing casually with these dealers and smugglers, we conducted indepth, taped interviews and cross-checked our observations and their accounts against other sources of data whenever possible. After leaving the field, we continued to conduct followup interviews during periodic visits to the community until 1983. The methods we used reflect the membership-role epistemological philosophy.

The highly illegal nature of the occupation makes drug dealers and smugglers secretive, deceitful, mistrustful, and paranoid. To insulate themselves from the straight world, they construct multiple false fronts, offer lies and misinformation, and withdraw into their group. Detailed,

scientific information about upper-level drug dealers and smugglers is lacking because of the difficulty sociological researchers have had in penetrating such groups. The only way we could get close enough to these individuals was to take a membership role in the setting. Although our different values and goals precluded our adopting complete membership in the subculture, and our fears prevented our becoming actively involved in their trafficking activities, we were able to assume peripheral membership roles. We became members of the dealers' and smugglers' social world and participated in their daily activities on that basis.

Strategies and Tactics

Getting In. Our discovery of the research setting happened quite accidentally. After moving into a new area, we began to get acquainted with the neighbors. Several were in their late twenties to early thirties and, judging by their clothing and automobiles, appeared to be rather wealthy. We began to notice some unusual behavior patterns: people entered and left their houses at all hours, parties sometimes continued for days, yet they seemed to have no visible means of financial support. In most places, this would have invoked community suspicion, but few people in this area held traditional jobs, so we overlooked this phenomenon and continued to develop neighborly relations. Gradually, we developed friendships with a few individuals, whom we saw as frequently as our academic schedules permitted. Their behavior began to seem increasingly odd; although we inquired mildly about their occupations and social sphere, we received no satisfying answers. We began to suspect that we might be living amidst a group of drug dealers. This piqued our sociological interest, and we continued to build friendly relations by doing, quite naturally, what Becker (1963), Polsky (1969), and Douglas (1972) advocated for the early stages of field research: giving them a chance to know us and form judgments about our trustworthiness by jointly pursuing common interests and activities.

One day, while at Ben's house (our closest neighbor), we were introduced to two of his friends from out of town. They started talking about a smuggling run they had just completed for Ben. Ben told them to button their lips, but they thought he was joking, that anybody as close to Ben as we seemed undoubtedly knew the nature of his business. After the story was revealed, Ben discussed with us the nature of his occupation. He said that he knew he could trust us, but that it was his practice to say as little as possible about his activities to outsiders. We thus became admitted into his inner circle, informally introduced to the members of his smuggling family, and formally apprised of what their roles were within his organization.

Once we realized the scope of the activities, we saw the enormous research potential. This scene was different from any analysis of drug trafficking that we had read in the sociological literature, because of the quantities in

which they were dealing and the fact that they were importing it themselves. We decided that, if it was at all possible, we would like to capitalize on this situation, to "opportunistically" (Riemer 1977) take advantage of our prior expertise in the field of drugs (Cummins et al. 1972) and of the rapport we had already developed with key people. We therefore discussed with our closest friends from among the group the idea of doing a study of the general subculture. After being assured of the anonymity, confidentiality, and innocuousness of our work, they were happy to reciprocate our friendship by being of help to our professional careers. In fact, they basked in the subsequent interest and attention we gave their lives.

Use of Key Informants. We began by turning group-member friends into key informants and collecting their life histories in detail. We conducted a series of taped, indepth interviews using an unstructured, open-ended format. Topics we addressed included: their backgrounds; their recruitment into the occupation; the stages of their dealing careers; their relations with others; motivations; lifestyle; and their general impressions about the community as a whole. Our relationship as friends took on an extra dimension: the research relationship. As Douglas (1976), Henslin (1972), and Wax (1952) have noted, research relationships involve some form of exchange between the parties involved. In our case, we offered everything friendship entails: routine favors in the course of our everyday lives; insights and advice from the perspective of our more "respectable" position; letters on their behalf to the authorities; testimony as character witnesses at nondrug-related trials; prison visits; loans; and, on one occasion, shelter in our house. We thus lived through the experiences of their lives in intimate detail.

We continued to do taped interviews with our key informants throughout the next 6 years. After that, we occasionally updated our data. Followup interviews focused on ongoing events and probed into specific areas such as dealing networks, types of dealers, secrecy, trust, paranoia, reputation, the law, occupational mobility, and occupational stratification. Taped interviews with each key informant ranged from 10 to 30 hours of discussion.

In addition to these interviews, we engaged in intensive participant observation with group members. We socialized with them on a near-daily basis, especially during the early years, even crossing paths during vacation travel. We worked in their legitimate businesses, attended their weddings, and cared for their children. Over the course of their relationship with us, several participants adopted the researchers' perspective and began to seek instances of behavior that would fill holes in the conceptual scheme we were developing. This methodological strategy was particularly discouraged by Gold (1958), who cautioned against overly close friendship or intimacy with informants lest they lose their ability to act as informants by becoming too much of an observer. Whyte (1955), in contrast, recommended the use of informants as research aides, not for helping in conceptualizing the data, but for their assistance in locating data that supports, contradicts, or fills in

the researcher's analysis of the setting. One person became so intrigued by our conceptual dilemmas that he undertook a "natural experiment" entirely on his own, offering an unlimited supply of drugs to a lower level dealer to see if that dealer could work up to higher levels of dealing and what factors would enhance or impinge upon his upward mobility.

Besides helping us by relating their own experiences, our key informants widened our circle of contacts. They let us know when someone we might be interested in was planning on dropping by, vouching for our reliability as friends who could be included in business conversations. Often, nighttime phone calls woke us, saying that someone had dropped by for a visit, should we want to drop over "casually" too. Of course we went. We thus were able to "snowball," through the active efforts of our key informants, into an expanded study population (Henslin 1972; Douglas 1976). This was supplemented by our own efforts to cast a research net and befriend other dealers, moving from contact to contact slowly and carefully.

The Covert Role. The illegal nature of the activity and the degree of suspiciousness found among dealers made the adoption of an overt research role problematic. Our key informants agreed that we should be extremely cautious, screening individuals carefully before admitting to studying them. We thus took a covert posture in the general research setting. As non-participants in the business that bound members together, it was difficult to be accepted as equals. We therefore strove for peripheral, social membership, to be accepted as "wise" (Goffman 1963), and granted a courtesy membership. This was easier, since we already had this relationship with our key informants. In this wise rather than overt role, we were able to observe and interact with people who would otherwise have avoided us. Adopting a courtesy membership caused us to bear a courtesy stigma as well, however, and we suffered associated rejection from lay outsiders and academicians. Kirby and Corzine (1981), Birenbaum (1970), and Henslin (1972) provide more detailed discussions of the nature, problems, and strategies for dealing with the courtesy stigma.

In our covert posture, we showed interest in dealers' activities, encouraged discussion about them (within limits, so as to avoid acting like narcs), and wrote field notes at home. This role gave access to unapproachable people and avoided research effects (i.e., alterations in observed phenomena as a consequence of the observer's presence), but it prevented us from asking probing, indepth questions or tape-recording conversations. Secret tapings, aside from the ethical problems involved, seemed too dangerous. Therefore, we sought continually to change to the overt role, by working to build trust.

Developing Trust. Just as when initially entering the group, developing trust with members of unorganized deviant groups can be slow and difficult. In the absence of a formal structure separating members from outsiders,

each individual must form his or her own judgment about whether new persons can be admitted into their confidence. No gatekeeper existed to smooth our path, although our key informants helped by providing introductions and references. Another result of the group's unorganized nature was that we met people at different times and were constantly at different levels in our developing relationships with them. We were thus trusted more by some people than by others, in part because of their greater familiarity with us. But as Douglas (1976) notes, knowing someone or even liking them does not automatically guarantee that trust will be extended to them.

We actively tried to cultivate the trust of our respondents by tying them to us with favors. Small things, like providing the use of our phone, were followed by bigger favors, like providing the use of our car, and finally really meaningful favors, like providing the use of our home. Here we often trod a thin line, trying to ensure our personal safety while putting ourselves in enough of a risk position, along with our research subjects, to encourage trust. As one act followed another, we tried to build a "web of trust" (Douglas 1976). As hard as we worked at this, we found that gaining trust was not a simple feat in the drug world. Johnson (1975) points out that trust is not a one-time phenomenon, but an ongoing developmental process. Our experiences show that it cannot be assumed to be a regular or reliable process either, for it can be diminished, withdrawn, reinstated to varying degrees, and requestioned at any point. Carey (1972) and Douglas (1972) have remarked on this waxing and waning, which may be influenced by the psychological volatility induced by using large amounts of cocaine over an extended period of time. We thus lived through a series of ups and downs with people we were trying to cultivate as research informants.

The Overt Role. After a period of friendly, covert interaction, we began to feel that some members trusted us. When we assumed an overt role, we used direct and indirect means of informing members that we were involved in a study of dealing and smuggling. In some cases, our key informants approached their friends or connections, vouched for our trustworthiness, and convinced them to talk to us. In other instances, we approached people ourselves, asking for their help with our project. With these secondary contacts, we discussed the dealing scene first, and then moved to taped life-history interviews. Some people reacted well, but others were skittish, making appointments for taped interviews only to break them and alternating honesty with falsity about their dealing activities. For some, this varied with varying involvement in the business. When they renounced dealing, they would tell us about their present and past activities, but when they became active again, they would withdraw. Like us, our research subjects combined overt and covert roles.

This progression of roles generated a few tactical difficulties. The first was the problem of coming on too fast and blowing it. Early in the research we had a dealer's "old lady" ready for a direct approach. She knew we

knew many dealers in common and we had discussed tangential subjects without actually mentioning dealing. When we asked to do a taped interview of her bohemian lifestyle, she acquiesced without hesitation. But when the interview began, and she found out why we were interested in her bohemian lifestyle, she balked, gave us a lot of incoherent jumble, and ended the session as quickly as possible. Even though she only lived three houses away we never saw her again. We learned to move more slowly after that.

A second problem involved juggling overt and covert roles with different people, with the danger of blowing cover (Henslin 1972). It became confusing and difficult to separate the people who knew about our study from those who did not. Our informants would make veiled references, especially when loosened by intoxicants, that made us extremely uncomfortable. We also worried that our snooping and inquisitiveness might be mistaken for police tactics, so that we became casualty statistics.

Cross-Checking the Data. The hidden nature of the drug-dealing world increased the concern about the reliability of our data. Therefore, all of the major conclusions were based on independent sources that were carefully verified. We adopted an attitude of suspicion, assuming people did more than they would originally admit, and we were generally right. First we checked information against our own observations, since we were involved with many of the principals on a daily basis and knew exactly what they were doing. Secondly, we cross-checked our data against alternative independent accounts. We were fortunate in locating several sources who had known various respondents at different times during their lives and were able to corroborate key bits of information about these people. We even managed to utilize outside sources to verify information about people we had never interviewed directly. Finally, wherever possible, we checked accounts against other types of information: newspaper and magazine reports; arrest records; material possessions; and visible evidence (Douglas 1976).

After 4 or 5 years of active and intense participant observation, we began to diminish our involvement. We did, however, maintain close ties with research informants with whom we were overt, seeing them regularly and doing followup interviews periodically.

PROBLEMS AND ISSUES

Effect of Drugs on Data Gathering

Carey (1972) has elaborated on some of the problems he encountered when trying to interview respondents who were using amphetamines, while Wax (1952; Wax 1957) mentions the difficulty of trying to record field notes while getting drunk on sake. In our research, marijuana and cocaine were

found to have nearly opposite effects, one helping the interview process, while the other hindered it. Attempting to interview respondents who were stoned on pot did not prove highly effective for a number of reasons. The effects of the drug provided the first obstacle, as people became either confused, sleepy, or involved in eating. The most exaggerated symptoms were displayed by people seeking to circumvent the interview. They simulated overreactions to marijuana to avoid divulging information. Cocaine, in contrast, proved to be a research aid. The drug's influence diminished respondents' inhibitions and increased their enthusiasm for both the interview experience and us. The comments of one respondent illustrated this effect:

> I've been thinking a lot about this taping and I woke up at three today and I started going, "Oh Bruce, let's cancel it," and I started getting really paranoid. Then all of a sudden—I don't know what it is about cocaine that makes people so loose, but it's typical. This has really been fun.

Risks of Field Research

A second area deserving consideration is the problem of assuming risks while doing research. Dangerous situations are endemic to research on deviant behavior. These dangers can come from various sources. One danger was from the research subjects themselves. As Carey (1972), Henslin (1972), and Whyte (1955) have noted, members of deviant groups can turn on a researcher if they believe some wrong is being committed. This can stem from blowing cover or from a simple misunderstanding. In this setting, the research subjects proved to be particularly volatile, capable of becoming malicious toward each other or us with little warning. This was stimulated not only by their drug consumption, but by the constant risks from police and other dealers. They vacillated between trusting friendliness and angered hostility.

Respondents' anger was directed toward either our research tapes or ourselves. We received several threats to our collection of tapes from interviewees. Since we had taken great pains to acquire them and felt strongly about maintaining the confidences entrusted in us by our informants, we shifted the tapes between different hiding places and finally transferred the tapes into an uninvolved person's possession.

Anger from the respondents toward ourselves was more unnerving. Some of the individuals we were studying were violent and erratic; others worked as enforcers and enjoyed inflicting injury. Some would read offense into a communication when none was intended. On a couple of occasions we were caught in the crossfire of arguments or misunderstandings between group members and were forced to flee from our house.

The other general source of danger was the police. We worried about local police or drug agents discovering the nature of our study and confiscating or subpoenaing our tapes and field notes. Sociologists have no privileged relationship with their subjects that would enable us to legally withhold evidence from the authorities should they subpoena it (Brajuha and Hallowell 1986). For this reason, we avoided any publicity and refrained from publishing articles in scholarly journals until we were ready to move out. We voluntarily forfeited obtaining a multiperspective view on the scene by studying police efforts to stop dealing, although several opportunities were offered. The closest we came to public exposure as drug researchers came when a former sociology graduate student (turned dealer, we had heard from inside sources) was arrested at the scene of a cocaine deal. His lawyer wanted us to testify about the dangers inherent in drug-related research, since he was using the research role as his defense. Fortunately, the crisis was averted when he settled out of court before the trial began. Had we been exposed, however, our key subjects would have acquired guilt by association through their friendship with us.

Our fear of the police was rooted not only in our concern for protecting our research subjects, but in the real danger of arrest ourselves. Many sociologists (Polsky 1969; Becker 1963; Whyte 1955; Carey 1972) have remarked that field researchers studying deviance must inevitably break the law in order to acquire valid participant observation data. This occurs in its most innocuous form from having "guilty knowledge": information about crimes that are committed. Our awareness of major dealing and smuggling operations made us an accessory to their commission, since we failed to notify the police. We broke the same law through our "guilty observations," by being present at the scene of a crime and witnessing its occurrence (Carey 1972). We knew it was possible to get caught when others were arrested, yet dealing behavior was so pervasive that to leave every time it occurred would have been unnatural and highly suspicious. Some kind of illegal action was also found to be a necessary or helpful component of the research by Whyte (1955), Polsky (1969), Becker (1963), Carey (1972), and Johnson (1975).

Cultural Differences Between Researcher and Subject

Another methodological issue arose out of the cultural clash between us and our research subjects. While other sociologists have alluded to these kinds of differences (Humphreys 1970; Whyte 1955), few have discussed how the research relationships affected them. Relationships with research subjects are unique, because they involve a bond of intimacy between persons who might not ordinarily associate, or who might otherwise be no more than casual friends. But when a field researcher undertakes a major study, he commits himself to a long-term relationship with whatever key informants he can develop. As the researcher probes his subjects' experiences and motivations to understand causality and human nature, he will necessarily

encounter fundamental differences between them and himself in character, values, and attitudes. We were confronted by differences in present vs. future orientations, desire for thrill seeking and risk vs. security, and upholding or breaking self-imposed rules. These differences often caused us great frustration. When we saw dealers act in ways they denied doing, setting themselves up for repeated failure, we wrestled with the desire to offer advice, pulled between our observer role (not to influence the natural setting) and our participant role (to offer friendly help in whatever way possible). Henslin (1972) and Douglas (1972; Douglas 1976) discuss this dilemma and various solutions to it. Struggling to cope with the effects on ourselves of these cultural differences led to increased self-understanding.

Ethical Issues of Participant Research

Various ethical problems were engendered by this research. Most sociologists who engage in participant observation fieldwork encounter ethical dilemmas during the course of their research experiences (Carey 1972; Klockars 1977; Klockars 1979; Humphreys 1970; Johnson 1975; Douglas 1976). Fieldwork raises ethical questions, because researchers never fully throw their allegiance in with their subjects, but maintain their commitment to the scientific community. Ethical dilemmas are directly related to the amount of deception undertaken in gathering the data and the degree to which the deception has been accepted as necessary by the researcher.

Guarding secrets told during taped interviews was not always easy or pleasant. Dealers occasionally revealed things about themselves or others that we had to pretend not to know. Guarding these confidences often required lying or building elaborate stories to cover what some people did not want others to know. Their fronts became our fronts; we had to weave our own web of deception to guard their performances. Even during the writing of the research report, we were torn by conflicts between utilizing details to enrich the data and glossing over descriptions to guard confidences, a problem that has been faced by previous researchers (Polsky 1969; Rainwater and Pittman 1967; Humphreys 1970).

Furthermore, use of the covert research role generated feelings of guilt, despite its necessity for maintaining relationships with our key informants. Their experiences with living covertly were more deeply entrenched than ours, being a part of their daily coexistence with the nondrug world. Some of our subjects had used the covert role with us, trying to convince us they had quit trafficking. Despite the universality of covert behavior in this setting, we still felt like traitors every time we ran home to make research notes on observations we had made under the guise of innocent participants.

There was also some guilt from efforts to manipulate people. Although not major manipulations, they involved courting people to procure information about them. Manipulation has immediate and long-term effects on the

researcher. During it, we felt awkward and phony; over the long run, we thought friends might later look back and feel exploited. Douglas echoes similar feelings, although based on a less trusting research relationship:

> We liked Jon, regardless of the lack of trust . . . It was our liking for him that made us feel badly, not only betrayal, since he never entrusted us with anything and since field research is inevitably a partly traitorous activity we had accepted as necessary. (Douglas 1976, p. 139)

Ironically, one other problem was the feeling of "whoring" for data. Often, we felt that we were being exploited by others, that we were putting more into the relationship than they, that they were taking us for granted or using us. Subjects were allowed to lie to us, borrow money and not repay it, and take advantage of us, but we were expected to behave honorably. This was an effect of our research strategy of allowing people to take our favors for little return. Nonetheless, it made us feel as if we were selling ourselves.

CONCLUSIONS

The aggressive research strategy employed was vital to this study. Our modern, pluralistic society is filled with subcultures whose interests compete or conflict with each other; each subculture has a set of knowledge reserved exclusively for insiders. To survive, members do not ordinarily share this knowledge with those outside the group. To obtain the information we needed, we had to become like the members in certain ways. Because they dealt only with people they knew and trusted, we had to become so before we could reveal our research interests. Confronted with secrecy, hidden alliances, misrepresentations, and unpredictable changes of intent, we had to use a delicate combination of overt and covert roles. Throughout, our deliberate practice of reciprocal exchange enabled us to trade friendship for knowledge, rather than waiting for the unlikely event that information would be delivered into our laps. We built a web of research contacts, used them to obtain highly sensitive data, and carefully checked to ensure validity.

Throughout, I profited from having my husband serve as partner in this project. It would have been impossible to examine this world as an unattached female without falling prey to sex role stereotyping, which excluded women from business dealings. Acting as a couple allowed us to relate in different ways to both men and women. We protected each other when we entered the homes of dangerous characters, buoyed each others' courage, and helped each other keep the conversation going. Conceptually, we helped each other remain detached and analytical, and we corroborated, clarified, or (most revealingly) contradicted each others' observations and conclusions.

Finally, to ensure accuracy, research on deviant groups must be conducted in the settings where it naturally occurs. As Polsky has forcefully asserted:

> This means—there is no getting away from it—the study of career criminals *au natural*, in the field, the study of such criminals as they normally go about their work and play, the study of "uncaught" criminals and the study of others who in the past have been caught but are not caught at the time you study them . . . Obviously we can no longer afford the convenient fiction that in studying criminals in their natural habitat, we would discover nothing really important that could not be discovered from criminals behind bars. (Polsky 1969, pp. 155-156)

Studying criminals in their natural habitat shows them in the complexity of their surrounding subculture, rather than within the artificial environment of a prison. Otherwise inaccessible dimensions of their lives can be observed and analyzed firsthand, including the nature of their social organization, social stratification, lifestyle, and motivation.

REFERENCES

Adler, P.A. *Wheeling and Dealing*. New York: Columbia University Press, 1985.

Adler, P.A., and Adler, P. *Membership Roles in Field Research*. Newbury Park, CA: Sage Publications, Inc., 1987. 95 pp.

Anderson, N. *The Hobo*. Chicago: University of Chicago Press, 1923.

Becker, H. *Outsiders*. New York: Free Press, 1963.

Becker, H. Whose side are we on. *Soc Prob* 14:239-247, 1967.

Becker, H., and Geer, B. Participant observation: The analysis of qualitative field data. In: Adams, R.N., and Preiss, J.J., eds. *Human Organization Research: Field Relations and Techniques*. Homewood, IL: Dorsey, 1960. pp. 267-289.

Birenbaum, A. On managing a courtesy stigma. *J Health Soc Behav* 11:196-206, 1970.

Blumer, H. Society as symbolic interaction. In: Rose, A., ed. *Human Behavior and Social Processes*. Boston: Houghton Mifflin, 1962. pp. 179-192.

Blumer, H. *Symbolic Interaction*. Englewood Cliffs, NJ: Prentice-Hall, 1969.

Bogdan, R., and Taylor, S.J. *Introduction to Qualitative Research Methods*. New York: John Wiley, 1975.

Brajuha, M., and Hallowell, L. Legal intrusion and the politics of field-work: The impact of the Brajuha case. *Urban Life* 14(4):454-478, 1986.

Brown, J.D. Toward a revision of labeling theory: The deviant career of alcoholics—from patient to healer. Unpublished master's thesis, University of Denver, 1988.

Bruyn, S.T. *The Human Perspective in Sociology: The Methodology of Participant Observation.* Englewood Cliffs, NJ: Prentice-Hall, 1966.
Burgess, R.G. Some role problems in field research. In: Burgess, R.G., ed. *Field Research: A Sourcebook and Field Manual.* London: George Allen & Unwin, 1982. pp. 45-49.
Carey, J.T. Problems of access and risk in observing drug scenes. In: Douglas, J.D., ed. *Research on Deviance.* New York: Random House, 1972. pp. 71-92.
Cressey, P.G. *The Taxi Dance Hall.* Chicago: University of Chicago Press, 1932.
Cummins, M. *Report of the Student Task Force on Heroin Use in Metropolitan Saint Louis.* Saint Louis: Washington University Social Science Institute, 1972.
Denzin, N.K. *The Research Act.* Chicago: Aldine, 1970.
Douglas, J.D. Understanding everyday life. In: Douglas, J.D., ed. *Understanding Everyday Life.* Chicago: Aldine, 1970. pp. 3-44.
Douglas, J.D. Observing deviance. In: Douglas, J.D., ed. *Research on Deviance.* New York: Random House, 1972. pp. 3-34.
Douglas, J.D. *Investigative Social Research.* Newbury Park, CA: Sage Publications, Inc., 1976.
Douglas, J.D., and Johnson, J.M., eds. *Existential Sociology.* New York: Cambridge University Press, 1977.
Everhart, R.B. Between stranger and friend: Some consequences of "long term" fieldwork in schools. *Am Ed Res J* 14:1-15, 1977.
Feyerabend, P.F. *Against Method.* London: New Left Books, 1972.
Gans, H.J. *The Urban Villagers.* New York: Free Press, 1982.
Goffman, E. *Behavior in Public Places.* Glencoe, IL: Free Press, 1963.
Gold, R.L. Roles in sociological field observations. *Soc Forces* 36:217-223, 1958.
Hammersley, M., and Atkinson, P. *Ethnography: Principles in Practice.* New York: Tavistock, 1983.
Henslin, J.M. Studying deviance in four settings: Research experiences with cabbies, suicides, drug users and abortionees. In: Douglas, J.D., ed. *Research on Deviance.* New York: Random House, 1972. pp. 35-70.
Hughes, E.C. *The Sociological Eye.* Chicago: Aldine, 1971.
Humphreys, L. *Tearoom Trade.* Chicago: Aldine, 1970.
Jarvie, I.C. The problem of ethical integrity in participant observation. *Current Anthro* 10:505-508, 1969.
Johnson, J.M. *Doing Field Research.* New York: Free Press, 1975.
Junker, B.H. *Field Work: An Introduction to the Social Sciences.* Chicago: University of Chicago Press, 1960.
Kirby, R., and Corzine, J. The contagion of stigma. *Qual Soc* 4(1):3-20, 1981.
Klockars, C.B. Field ethics for the life history. In: Weppner, R., ed. *Street Ethnography.* Beverly Hills, CA: Sage, 1977. pp. 201-226.

Klockars, C.B. Dirty hands and deviant subjects. In: Klockars, C.B., and O'Connor, F.W., eds. *Deviance and Decency.* Beverly Hills, CA: Sage, 1979. pp. 261-282.
Landesco, J. *Organized Crime in Chicago.* Chicago: Illinois Association for Criminal Justice, 1929.
Lofland, J. *Analyzing Social Settings.* Belmont, CA: Wadsworth, 1971.
McCall, G.J., and Simmons, J.L., eds. *Issues in Participant Observation.* Reading, MA: Addison Wesley, 1969.
Mehan, H., and Wood, H. *The Reality of Ethnomethodology.* New York: John Wiley, 1975.
Miller, S.M. The participant observer and "over-rapport." *Am Soc Rev* 17:97-99, 1952.
Phillips, D. Epistemology and the sociology of knowledge. *Theory and Society* 1:59-88, 1974.
Pollner, M., and Emerson, R.M. The dynamics of inclusion and distance in fieldwork relations. In: Emerson, R.M., ed. *Contemporary Field Research.* Boston: Little, Brown, 1983. pp. 235-252.
Polsky, N. Research method, morality and criminology. In: *Hustlers, Beats and Others.* New York: Doubleday, 1969. pp. 109-143.
Powdermaker, H. *Stranger and Friend: The Way of an Anthropologist.* New York: Norton, 1966.
Rainwater, R.L., and Pittman, D.J. Ethical problems in studying a politically sensitive and deviant community. *Soc Prob* 14:357-366, 1967.
Riemer, J. Varieties of opportunistic research. *Urban Life* 5(4):467-477, 1977.
Schatzman, L., and Strauss, A.L. *Field Research: Strategies for a Natural Sociology.* Englewood Cliffs, NJ: Prentice-Hall, 1973.
Schutz, A. In: Natanson, M., ed. *Collected Papers I: The Problem of Social Reality.* The Hague: Martinus Nijhoff, 1962.
Schutz, A. *The Phenomenology of the Social World.* Evanston, IL: Northwestern University Press, 1967.
Schwartz, M.S., and Schwartz, C.G. Problems in participant observation. *Am J Soc* 60:343-353, 1955.
Thrasher, F.M. *The Gang.* Chicago: University of Chicago Press, 1927.
Trice, H.M. The "outsider's" role in field study. *Soc and Soc Res* 41:27-32, 1956.
Vidich, A.J. Participant observation and the collection and interpretation of data. *Am J Soc* 60:354-360, 1955.
Warren, C.A.B., and Rasmussen, P.K. Sex and gender in field research. *Urban Life* 6(3):349-369, 1977.
Wax, R. Reciprocity as a field technique. *Human Org* 11:34-37, 1952.
Whyte, W.F. *Street Corner Society.* Chicago: University of Chicago Press, 1955.
Williams, T.R. *Field Methods in the Study of Culture.* New York: Holt, Rinehart & Winston, 1967.
Willis, P. *Learning to Labour: How Working Class Kids Get Working Class Jobs.* New York: Columbia University Press, 1977.

AUTHOR

Patricia Adler, Ph.D.
Department of Sociology, Box 327
University of Colorado at Boulder
Boulder, CO 80309-0327

Recovery From Opiate Addiction Without Treatment: A Summary[1]

Patrick Biernacki

INTRODUCTION

For decades, the conventional wisdom has held the notion of "once an opiate addict, always an addict." Correspondingly, national policy about illicit drugs encouraged abstinence, promoted methadone maintenance, and exercised extreme measures of drug control. This view of the addict and existing social policy go hand-in-hand. If opiate use is seen as an intractable condition resulting in inevitable addiction and other consequent unwanted behaviors, then a policy of abstinence and control would appear to some policymakers to be a logical approach to the situation. However, this view of opiate use and addiction may not be fully supported by scientific evidence.

More than 25 years ago, Charles Winick (1962), in his "maturing out" hypothesis, suggested that addiction might be a "self-limiting" phenomenon and that addicts might be able to recover on their own. Winick offered a psychodynamic explanation of the reasons why some addicts quit using after early adulthood. He argued that young adults become addicted as a way to cope with problems encountered during that point of the life cycle. As the problems are resolved and young people pass into adulthood, they learn to meet ongoing challenges and responsibilities of life without undue stress and emotion; thus, the shrouding effects of opiate drugs are no longer desired. Consequently, young adults can stop their use of addictive drugs without therapeutic intervention.

Later, Lee Robins (1973), in her classic studies of Vietnam veterans, showed that people who were addicted to opiates during the war could stop using without professional intervention after they returned from overseas. Although these and other studies documented that some individuals were

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[1] This paper summarizes the major findings and analyses that are presented in greater detail in Biernacki, P. *Pathways From Heroin Addiction: Recovery Without Treatment.* Philadelphia: Temple University Press, 1986.

capable of self-initiated recovery, they did not address social and psychological processes that may result in natural recovery.

STUDY SAMPLE

To develop a better understanding of opiate addiction, a qualitative life history study was conducted of 101 addicts who recovered on their own, without the help of any formal treatment programs. To qualify for the research, each respondent had to have been addicted for at least 1 year and then abstinent for at least 2 years. Addiction was defined as having experienced uncomfortable physical symptoms when withdrawing from an opiate drug, for example, chills, cramps, nausea, headaches, or muscle pain. The average reported years of addiction were 5.69, and the average length of nonaddiction was 6.91 years.

The study found that the so-called process of "spontaneous remission" may be neither "spontaneous" nor, strictly speaking, a "remission" to some prior state. Rather, it appears to be a rich social and psychological process that brings about dramatic life changes. There is a sequence to the changes, but they are not necessarily stages, because many were found to occur simultaneously. However, for analytic and explicative purposes, the process of natural recovery will be described as a sequence of events.

FORMING A RESOLVE

For a drug addict, a life of addiction is an arduous one, providing ample reasons to want to stop using drugs and change one's lifestyle. The consequences and risks from drug addiction are amplified today with the second wave of the acquired immunodeficiency syndrome (AIDS) epidemic spreading through intravenous-drug-using communities.

Surprisingly, some of the people in the study stopped their opiate use without developing a strong resolve to do so. Usually, these people never were deeply immersed in the social world of heroin and were addicted more through the actions of another person than through their own. For example, a spouse or lover would hustle and obtain the needed drug for them. When that person was removed from the scene, possibly as a result of death or imprisonment, the other found himself or herself associating with another social world unrelated to drug abuse without making a conscious, explicit decision to do so.

In other instances, however, resolutions to stop abusing drugs were rooted in dramatic life crises emanating from the drug world itself. Common to these cases were fundamental reorientations in frame of reference and perspective. With these reorientations, alternatives to continued drug use emerged. In many instances, the resolve to quit had a rational quality, in which the person carefully weighed alternatives and decided to put his drug

use behind him after considering the dangers continued use would entail. In still other cases, resolutions to stop were rooted in a dramatic and emotionally charged life crisis. The often-discussed "rock bottom" phenomenon and existential crisis, in which suicide is thought of or attempted, illustrate the latter.

BECOMING ABSTINENT

As any cigarette smoker can testify, making a decision to stop smoking does not necessarily mean that corresponding actions to do so will be taken, nor does it mean temporary abstinence can be maintained. A myriad of obstacles lie in the path of addicts who decide to quit and to change their lives. First, it is common for addicts themselves to believe the conventional wisdom of "once an addict, always an addict," often expressed by them as having an "addictive personality," or being a "hope-to-die dope fiend." Such beliefs work to undermine users' decisions to stop and weaken their ability to resist relapse.

A second obstacle to addicts who are trying to end their drug abuse is the absence of any role model or subcultural folklore to give them insight into how they might implement their resolution. The reason for this lack of information about successful, self-initiated termination is that addicts who are able to maintain their abstinence without having utilized some form of treatment generally cease to associate with those who remain addicted. Consequently, successfully abstaining ex-addicts who remove themselves from the drug scene are believed to have failed in their resolve and, perhaps, are assumed to be readdicted in another city, imprisoned, or dead.

CREATING AN ALTERNATIVE

Although some form of treatment was reported to be available to the great majority of people in the study, more than 90 percent rejected it as a possible alternative to their situations. Most rejected treatment because they thought they could manage the situation themselves. Others did not believe treatment would help them. Still others reported that they feared further stigmatization or that they did not wish to be humiliated or degraded. These reports are not unusual. Surveys of addicts in San Francisco show that even if treatment slots were available, many would not volunteer to take them, because they are not interested in treatment or because they view treatment programs as too intrusive, controlling, and regimented (Walters et al. 1986).

When addicts desire to give up their drug use and change their lives, they frequently are confused about what they should do instead. This confusion is especially dramatic for those who have been deeply immersed in the world of addiction for long periods of time. They may see themselves as having nowhere to turn, having burned their bridges with family and

ordinary friends. For such people, life alternatives may present themselves in a fortuitous manner and have the quality of conversion experiences. For example, recovering addicts may discover political or religious movements and completely immerse themselves in these social worlds, much as they had in the drug world when they were addicted.

In contrast, addicts who have not submerged themselves in a world of addiction may have an easier time sustaining their resolve to change, by turning to a relationship that has not been destroyed or by rebuilding a bridge to one that has not been damaged beyond repair. In either case, the first step in breaking away from addiction is a literal or symbolic move away from the drug and the drug world. Following the initial period of withdrawal, addicts face an existential problem of filling time in their lives that previously was occupied by hustling, buying, and using drugs.

Regardless of which activities fill the void—religion, family, work, politics—they may become the exclusive focus of the person's life. During this period, which usually lasts about a year, a moratorium takes place on what might be considered an "ordinary" round of life. The individual rarely moves beyond the confines of the group and activities with which he has recently become involved. Simultaneously, other changes are taking place that can help the abstaining addict maintain his resolve. First, the drug scene may change. Old friends, dealers, and running partners may leave the area, go to jail or into treatment, or some may die. These changes may make it more difficult, but not impossible, for abstainers to obtain the drug should they be tempted to do so. Second, while staying abstinent, ex-addicts undergo experiences that they can and do share with nonaddicts. With time, these experiences lay the foundation for a commonality of discourse that helps abstaining addicts to conquer fears that they cannot get along with or be accepted by nonusers. With the development of ordinary relationships, the abstaining addict can construct a nonaddict lifestyle that he becomes committed to and may not wish to jeopardize.

THE CRAVING PROBLEM

Becoming abstinent is one thing, staying clean is another. The relapse of abstaining addicts is something that has perplexed drug researchers and addicts themselves. The cycle of abstinence and then the renewal of drug use commonly has been attributed to something called the craving phenomenon—jarring, uninvited memories and feelings of past drug use.

In this research, two broad categories of cravings were reported. One resulted from associations made with withdrawal distress or sickness, and the second emanated from associations with past instances of using the drug and experiencing its effects. Each type of craving is triggered by environmental cues related to drug use or withdrawal. The ex-addicts in this study reported using two strategies to manage opiate cravings,

especially in the initial year of opiate abstinence. The first was to substitute some other, nonopiate drug—marijuana, alcohol, and tranquilizers (diazepam, in particular) were the most popular. The second strategy involved a more complex cognitive and behavioral process of negative contexting and supplanting. That is, when the abstaining individuals experienced drug cravings, they reinterpreted their thoughts about using drugs by placing them in a negative context and supplanted them by thinking about and doing other things. Important here is that the basis for this process is provided by the new relationships, identities, and corresponding perspectives of the abstaining individuals. For example, if the initial break from addiction involved a religious conversion, the abstaining person might impose a negative context on an emerging drug craving by defining it as a temptation of Satan and supplant the thought with prayer. Each time the abstaining person succeeds and is able to manage and overcome cravings and engage in behavior unrelated to drugs he is more likely to maintain his new identity.

With time, the frequency with which cravings occur and the intensity with which they are experienced diminish. Gradually, as the abstaining addict becomes more extensively and deeply involved in social worlds that are not related to illicit drugs, the various cues associated with drug abuse are reinterpreted and lose their strength to evoke drug cravings. This usually occurs after about 1 year of abstinence. Finally, those cues no longer represent harbingers for potential relapse.

BECOMING ORDINARY

The courses that people's lives take when they stop abusing opiate drugs without the aid of professional intervention are closely related to the degree to which, as addicts, they were involved in the world of addiction to the exclusion of activities in other, more ordinary social worlds, and to the extent that they had ruined conventional social relationships and spoiled identities in them.

The way to an identity transformation hinges considerably on the general lifestyle an addict has maintained, especially the commitments to and involvements in conventional aspects of life. Major problems faced by ex-addicts who have stopped the use of opiate drugs, beyond problems associated with the craving phenomenon and its management, spring from societal stigmatization of addiction. Social stigmas present a formidable barrier that must be overcome if abstaining addicts are successfully to stop drug use and transform their lives. Difficulties in this transformation may be compounded if the stigmas of addiction are coupled with other stigmas (e.g., those associated with race, sex, and class).

In spite of problems facing people who are attempting to break addiction to drugs, addicts can and do recover without treatment. They can successfully

transform their identities and come to be treated as "ordinary" people. They can do this by reverting to an earlier identity that has not been damaged too badly as a result of the addiction. Or they can extend an identity that was present during the addiction and that has somehow remained intact. Or they can engage an emergent identity that was not present before or during the addiction. A successful transformation from an identity with opiate use requires the availability of identity materials from which the "new" identity can be fashioned. Identity materials are those aspects of social settings and relationships (e.g., social roles, vocabularies) that can provide the basis to construct a nonaddict identity and a positive sense of self. In part, the availability of such materials is related to the stigma associated with addiction.

Those addicts wishing to change their identities may first have to overcome the fear and suspicion of nonaddicts before they will be accepted and responded to in ways that will confirm their new status. Gaining recognition and acceptance from the nonaddict world often is a long and difficult process. Eventually, acceptance may be gained by the ex-addicts' behaving in conventionally expected ways. Following "normal" pursuits, remaining gainfully employed, meeting social obligations, and possessing some material things will often enable nonaddicts to trust the abstainer and, over time, to accept him and respond to him in "ordinary" ways. At the same time, addicts' feelings of uncertainty and doubt will lessen as they more fully accept the new nonaddict life. Ultimately, the identity and perspective of an addict can become so deemphasized and distant that cravings for the addictive drug become virtually nonexistent. For all practical purposes, the addict can be said to have recovered.

CONCLUSION

Obviously, as is common with most research, this work raises a series of additional questions. The study was modest in that it was done in retrospect among a relatively small sample of people located through a chain-referral method. The work represents only a beginning in understanding the natural recovery process, a process that is important enough to warrant further research.

All the data used in the analysis were gathered subsequent to the events in question. The use of retrospective materials, although it has certain advantages, also has drawbacks. Problems stem from the vulnerability of human memory, including the inevitable reconstruction of past events on the basis of new experiences and vocabularies. Consequently, it is recommended that longitudinal studies be conducted of addicts as they move through a course of natural recovery. These should be ethnographic studies done "up close" that focus on the problems addicts face when they attempt to abstain and on how such problems are or are not overcome. Especially important would

be observations of how the various reactions of nonaddicts to the recovering addict either facilitate or stymie the recovery process.

ACKNOWLEDGMENTS

The research that provided the materials for this analysis was supported by U.S. Public Health Serivce grant #1 H81 DA 01988 from the National Institute on Drug Abuse.

REFERENCES

Robins, L.N. *The Vietnam Drug User Returns*. Washington, DC: Supt. of Docs., U.S. Gov. Print. Off., 1973.

Watters, J.K.; Newmeyer, J.A.; and Cheng, Y. HIV infection and risk factors among intravenous drug users in San Francisco. Paper presented at the American Public Health Association Meeting, Las Vegas, NV, October 1986.

Winick, C. Maturing out of narcotic addiction. *Bull Narc* 14:1-7, 1962.

AUTHOR

Patrick Biernacki, Ph.D.
Youth Environment Study
1779 Haight Street
San Francisco, CA 94117

128617

Women and Addiction: Process, Treatment, and Outcome

Marsha Rosenbaum and Sheigla Murphy

INTRODUCTION

Women opiate addicts have received little attention in drug abuse research since the Harrison Act of 1914, which marked the beginning of the criminalization of the use of narcotics. The words "woman addict" often produce a stereotypic image of an addict who is also a prostitute, a "woman of the streets," who has frequent encounters with the criminal justice system.

The field of drug abuse, including drug abuse research, has until approximately a decade ago largely ignored women. This may be because it was commonly believed there were few women addicts. In addition, women addicts were not recognized as having unique characteristics and problems and were, therefore, not distinguished from male addicts in analytic research on drug abuse.

The scant attention given to women addicts began to change in the early 1970s, at the time of the women's liberation movement and the Nixon administration. Richard Nixon's "war on drugs" heightened awareness of the drug epidemic and resulted in a large investment in drug abuse treatment and research. The thousands of addicts who entered new treatment facilities validated the severity of the drug abuse epidemic. Simultaneously, the women's movement brought attention to the possibility that women had drug experiences and problems that, as in some other facets of life, may be unique from those of men. Consequently, by 1976, the National Institute on Drug Abuse (NIDA), became formally interested in drug abuse research and treatment for women drug addicts. Among NIDA's solicitations were proposals for ethnographic "street" studies of women addicts. At that time, the focus was on women, with crime as a concomitant concern, in contrast to today's focus on women, drugs, and the acquired immunodeficiency syndrome (AIDS).

We began work on "The Career of the Woman Addict" in 1977. It was to be an ethnography that included 100 indepth interviews with unincarcerated, not-in-treatment women addicts in the San Francisco Bay area. The objective of the study was to explore the careers of women heroin addicts

from the theoretical perspective of symbolic interactionism and from the philosophy of phenomenology. The snowball, or chain referral method, was used to locate respondents initially. We were closely affiliated with the Prisoners' Union, an organization of convicts, ex-convicts, and others who were committed to ensuring the civil rights of the incarcerated. The chain referrals were begun through the girlfriend of an ex-prisoner. We also posted notices about our research in high-drug-abusing areas of the city. In this way, we were able to do fieldwork while simultaneously publicizing the study: two women (both pregnant) posting signs that read "Women addicts needed to participate in a research project . . . . ." were not ordinarily seen in the community at that time (now, with AIDS outreach, such scenes are more commonplace). Since people wanted to know what we were doing, we spent time talking with addicts in the streets while posting signs. We were sensitive to appropriate locations for reaching women, such as laundromats, supermarkets, and hospital clinic waiting rooms. Near the end of the study, after it appeared that the pool of subjects from "street" categories had been exhausted, interviews were begun with women addicts in treatment programs (n=10) and in jails (n=10). The interviews generally lasted 2 1/2 to 3 hours and consisted of a tape-recorded, open-ended "life history" and a short closed-ended quantitative component. Some of the topics explored were early childhood, drug use, family, relationships, work, and criminality.

While conducting interviews with women addicts, we learned that drug abuse treatment was not separate from, but rather integral to, the heroin world. Addicts routinely went into treatment, just as they went to jail (Rosenbaum and Murphy 1981). Methadone maintenance in particular had become a phenomenon to be reckoned with, and women in methadone treatment appeared to have special, distinctive problems. NIDA supported research in this area, with a grant awarded for the study of women on methadone. The project involved additional indepth interviews with 100 women who were currently enrolled in methadone programs. Using an interview format based on addiction as a progressive development, we interviewed each of the women about their initial and subsequent drug abuse behaviors.

This chapter begins with a discussion of our major research findings on women addicts and heroin abuse and then moves to a discussion of women on methadone. It closes with a brief examination of outcomes associated with women and addiction and a discussion of women, drugs, and AIDS.

WOMEN AND HEROIN: A CAREER OF FUNNELING OPTIONS

Women enter the heroin world for a number of reasons, among them a perception that it provides an expansion of life options. They are attracted to heroin, because they want to be part of a social scene complete with the appearance (and sometimes the reality) of money, excitement, and the

euphoric properties of drugs. Heroin has a reputation as the "hardest drug," and, as such, its users take on an outsider or outlaw status, which is sometimes appealing to young women. These attractions may add a new dimension to women's lives: lives that they often describe as boring, alienated, and without money. Occasionally, women are introduced to heroin through a boyfriend or spouse, but this research found that this does not occur as often as the literature suggests (Gerskin et al. 1977; Rosenbaum 1981a; Waldorf 1973). Some of the middle class women (a minority among addicts) who were interviewed began using heroin as an attempt to alleviate physical pain (sometimes connected with menstruation). They began with legal prescriptions (e.g., pain killers such as codiene) and in time purchased street drugs and eventually heroin when their prescriptions ran out or were withdrawn by physicians (Prather and Fidell 1978; James 1975).

Initially, women addicts enjoy the beginnings of a heroin career. But the reality of life as a "junkie" quickly turns out to be different from life in the initial or honeymoon phase. In the addicted stage, women become locked in, and the earlier illusion of a possible expansion of options transforms into a *funneling* or reduction of life options. Gifts of heroin that are common for beginning and women addicts begin to dwindle. Male heroin addicts often claim women addicts have an easier life, because they can resort to prostitution with comparative ease. Support of one's life is a major issue for most people, but, for the female heroin addict, the problem is compounded by the need to acquire and use drugs. The heroin world is a chaotic one, nothing is predictable. Finding a consistent source of good street drugs occupies most of an addict's time. Holding down a legitimate job is nearly impossible. Legal employment available to an unskilled, undereducated woman rarely pays enough to support an addict's habit. Most women resort to illegitimate jobs with better pay and more flexible hours.

Upon entering the world of illegal work, women addicts' lives become full of risk and chaos. Such work is often sex-role related: prostitution, forgery, and shoplifting, i.e., the guise of innocence allows women to pass bad checks and shoplift without being noticed and suspected as often as men are (Lemert 1951). Encounters with or entry into the criminal justice system escalate the funneling of life options out of the conventional world. Criminal records are acquired. Involvement in criminal activities further serves to lock women into deviant lifestyles.

Women may suffer physically more than men, which can present problems concerning presentation of self. Daily drug use often causes premature aging. Women appear older and worn out, while their male counterparts seem to recuperate more easily. One woman told us, "Heroin preserves men and tears women down." Scarring from "track" or injection marks (sites on the skin where addicts inject) are often seen as remnants of a wild youth on men. On women, they represent marks of a "fallen woman."

Ultimately, most women addicts want to change their lives, and this appears to be for four main reasons. The first is inundation in the heroin life. Because women addicts have no time for anything but heroin acquisition and use, children tend to be neglected, jobs get lost, and numerous health problems, including a gnawing, constant withdrawal, result. Second, women addicts desire recovery when they become pregnant. Birth control is not common; many women addicts cease menstruation and consequently believe they are incapable of becoming pregnant while addicted. When they discover they have become pregnant (often not until they begin to "show," because a missed period is meaningless), they are usually compelled to seek treatment for their addictions to avoid having an addicted baby. Third, when women reach 30, they may experience "burnout," coupled with panic, often asking themselves, "Is this who I want to be forever?" Finally, the recognition of a funneling of life options is a major factor that motivates recovery from heroin addiction: losing children through neglect and/or social service agencies' interventions, either not having had children or becoming pregnant while addicted, and loss of ties with the conventional world (Rosenbaum 1981b).

Since the proliferation of heroin treatment in the late 1960s and early 1970s, women (and men) who want to end their addictions frequently turn to treatment programs for help. The largest single treatment modality available for heroin addiction is methadone maintenance. As primary caregivers for children, women find that methadone maintenance is often their only option. Therapeutic communities usually do not accept children. It is only within the last few years that a small number of programs have begun to meet the needs of chemically dependent women and their children (Reed 1987).

SURRENDER TO CONTROL: WOMEN ON METHADONE

The initial phase on methadone maintenance can be characterized by surrender to control. Methadone replaces the chaos of the heroin world with a structured routine: mandatory clinic attendance, payment of fees, urinalysis, and counseling. There is often a sense of relief: the daily job of getting money is gone; women have more time; and, best of all, they are not sick from heroin withdrawal every day. For pregnant addicts, methadone provides stability. Some programs even have a special component for pregnant addicts, in which they are given support and education about childbirth and child-rearing. There is an initial appreciation of the control exerted by the clinic and its accompanying structure. The unspoken posture is, "Help me."

In the next phase, often termed "stabilization" (some refer to it as "addiction"), an enduring relationship to methadone is established. This phase indicates successful adjustment, at a correct dosage, to methadone as a drug: the effects of heroin are blocked, and withdrawal is prevented. Clients may begin to earn take-home doses of methadone and begin to assert some con-

trol over their lives. This is a critical stage in the client's recovery. There are clinic "successes," who are able to break from the deviant world and use methadone while attempting to reenter conventional life. There are also clinic "failures," who continue to see themselves as part of the heroin world (often they have tried but failed to reenter conventional life) and use methadone as a fallback drug and lifestyle.

Finally, there is a phase of disillusionment, when initial surrender to control becomes resentment of control. Resentment develops toward mandatory reporting to the clinic, the methadone dose, commuting and travel problems, health concerns, parenting problems (e.g., attempting to be a good role model while on methadone), and issues of identity. Identity problems center on being a "half-junkie" and on aging. Women complain that they are in a sort of limbo—no longer a junkie but still an addict. While attempting to function in the conventional world, these women feel they must conceal their status as methadone clients; hence, they continue to bear the stigma of addiction within themselves, although they have technically exited from the heroin world.

The strength of the stigma attached to opiate abuse is illustrated by the following. One of our respondents was a postoperative transsexual and had been living as a woman for 4 or 5 years at the time of interview. The respondent had a good job at a bank and was most anxious that no one at the bank find out about her methadone use or her transsexual background. The interviewer asked, "If you were at the office Christmas party and somehow a fellow employee found out about one of your secrets, which would be worse?" The respondent answered without any hesitation, "I'd much rather they knew I used to be a man. If they knew I was on methadone, they'd no longer trust me and then they would start watching their purses. I'd probably lose my job." The stigma of an addict's identity remains, even for clients receiving methadone maintenance treatment.

Aging plays an important role in disillusionment with methadone, just as it did with heroin. As most of us do, these women take stock of themselves and wonder if methadone is what they will be doing for and with the rest of their lives. Most do not want to be on methadone indefinitely (Rosenbaum 1982).

Obstacles to methadone detoxification, however, are numerous. There are few visible formulas for success or role models who have been detoxified without returning to drug or alcohol abuse. Successful detachment from methadone means staying away from the clinic. Methadone counselors have told us that, if they see a detoxified client visiting the clinic with frequency, they assume the client is not doing well. Such a client is likely to be either looking for heroin or trying to buy someone's take-home dose of methadone. Therefore, for the most part, there are few if any successful ex-methadone patients at clinics that can act as role models. Paradoxically,

achieving success as a methadone patient can impede detachment from methadone. Women on methadone who are adjusting successfully to the conventional world, often holding down jobs and raising children, cannot afford to go through an unsuccessful detoxification. It would require that they endure the sickness of withdrawal for an indeterminate period of time, possibly jeopardizing the order of their lives. Through methadone, women addicts can achieve a delicate balance in their lives, and detoxification from methadone could threaten any gains made. Additionally, clinic successes are reinforced by staff, and losing this positive feedback can be difficult.

Perhaps the most pervasive obstacle to ending use of methadone is fear. As one clinic counselor told us, "With detoxing, what you hear is what you get." Negative descriptions about detoxification circulate around the clinic. Patients are concerned about long-term withdrawal symptoms, emotional vulnerability, and a lack of social life (clinic attendance and going out for coffee with other clients become a mainstay of some women's social lives) as problematic aspects of attempts to detoxify. An experienced clinic counselor told us, "It's like asking you to visit hell for awhile." As a result, only a small percentage of women successfully detoxify from methadone and remain street-drug free (Rosenbaum 1985).

OUTCOMES OF WOMEN'S ADDICTION AND TREATMENT

Outcomes of women's addiction and treatment are linked to social class. For those who enter addiction through the working and lower classes, life options are limited from forces both outside and inside the deviant world. After addiction, it may be more difficult for women to readjust to the conventional world than it is for men. Women addicts may believe they carry a more negative stigma from their drug abuse than men, and, as a consequence, they may find it more difficult to secure and keep jobs. Within the "deviant" world, aging may make it more difficult for women to compete in sex-role-linked activities such as prostitution. As a consequence, working and lower-class women who become and remain heroin addicts often live on government support and/or become permanently enrolled in methadone maintenance programs. This may change if new fiscal policies and fee-for-service treatment programs are adopted.

Women who have entered addiction through the middle class (e.g., "hippies" and "medical" addicts) may have more options if they have not become too immersed in the deviant world (e.g., too many incarcerations). It is possible for them to readjust more readily, because they often possess the resources necessary to start a new life. Perhaps more important, they already have middle-class status and can detach from their addiction as a phase rather than a career (Rosenbaum 1985).

For women addicts and their children, the prognosis is often grim. The need for improved treatment opportunities has long since been recognized,

but the gap between what currently exists and meeting the need remains large. In the midst of an AIDS epidemic among intravenous drug users (IVDUs), women addicts and their children represent high-risk populations, especially if women share their needles or engage in prostitution. In California, short-duration methadone maintenance treatment is common, and there are decreases in available treatment slots, both of which may lead to higher recidivism among methadone treatment clients and a corresponding increase in the risk of AIDS transmission.

CONCLUSION

In this era of AIDS, ethnographic information and available resources must be applied to intervene in the spread of the AIDS virus. In many ways, women IVDUs are the greatest challenge to outreach workers and treatment personnel, because issues concerning sex roles and the stigma of addiction make women addicts one of the more difficult to reach at-risk populations.

Cultural differences in women's roles affect the outcome of drug abuse treatment interventions. For example, in the Hispanic communities, women seldom associate with others on the streets or outside their homes unless accompanied by their husbands or boyfriends. Women trained in innovative outreach methods are needed to help women addicts who may be relatively isolated.

Efforts to recruit women addicts into treatment programs should take into account their roles as primary caregivers for children. Providing services for children, such as coupons for well-baby examinations or organizing play groups for mothers and children in neighborhoods with chemically dependent populations, are likely to attract women into participation. Perhaps most important, facilities for children should be a component of all treatment programs that serve women.

Women addicts as well as men should feel encouraged to reenter the conventional world without the burden of stigma. Most recovered addicts assume new identities and/or resume viable old ones in order to make successful transitions (Biernacki 1986). The old drug-using lifestyle must be replaced by a new, conventional one. An organizing principle in many women's lifestyles today is the job outside the home. Efforts could be made in the workplace to destigmatize addiction and provide positive support to women who recover from drug abuse. In sum, ex-addict identities should be attainable, without shame or stigma, for women as well as for men.

Finally, ethnographic research should be continued to identify special and distinctive experiences, careers, and problems women have with drugs. Data collection techniques that take into account women's lifestyles and field researchers who can appreciate women's unique experiences are

essential. Of late, AIDS outreach programs have recognized the importance of ethnography in locating subjects and in effective intervention to curb the spread of the AIDS virus among drug addicts. Consistent with such programs, women ethnographers should become more involved in fieldwork to reach this difficult but pivotal population.

REFERENCES

Biernacki, P. *Pathways from Heroin Addiction: Recovery from Treatment.* Philadelphia, PA: Temple University Press, 1986.

Gerskin, D.; Judd, L.L.; and Rovner, S.A. Career dynamics of female heroin addicts. *Am J Drug Alcohol Abuse* 6:1-23, 1977.

James, J. Female Addiction and Criminal Involvement. Presented at the Pacific Sociological Association Meeting, Victoria, B.C., April, 1975.

Lemert, E. *Social Pathology.* New York: McGraw Hill, 1951.

Prather, J., and Fidell, L.S. Drug use and abuse among women: An overview. *Int J Addict* 13(6):863-885, 1978.

Reed, B. Developing women-sensitive drug dependence treatment services: Why so difficult? *J Psychoactive Drugs* 19(2), 1987.

Rosenbaum, M. Sex roles among deviants: The woman addict. *Int J Addict* 16:859-877, 1981a.

Rosenbaum, M. *Women on Heroin.* New Brunswick, NJ: Rutgers University Press, 1981b.

Rosenbaum, M. *Surrender to control: Women on methadone.* Final Report, Grant 1 R01 DA 02442. Rockville, MD: National Institute on Drug Abuse, 1982.

Rosenbaum, M. *Getting off methadone.* Final Report, Grant R01 DA 02242. Rockville, MD: National Institute on Drug Abuse, 1985.

Rosenbaum, M., and Murphy, S. Getting the treatment: Recycling women addicts. *J Psychoactive Drugs* 13(1):1-13, 1981.

Waldorf, D. *Careers in Dope.* Englewood Cliffs: Prentice-Hall, 1973.

ACKNOWLEDGMENTS

Research reported here was supported by U.S. Public Health Service grants 1 R01 DA 01793 and 1 R01 DA 02442 from the National Institute on Drug Abuse.

AUTHORS

Marsha Rosenbaum, Ph.D.
Sheigla Murphy

Institute for Scientific Analysis
2235 Lombard Street
San Francisco, CA 94123

Chicano Intravenous Drug Users

Reyes Ramos

INTRODUCTION

Not long ago, I went to a shooting gallery. An outreach worker, an ex-addict, had permission to take me and my research assistant there to give a presentation on acquired immunodeficiency syndrome (AIDS) and intravenous (IV) drug abusers to the clientele of the gallery.

We spoke with 12 Chicano men, mostly in their twenties. The owner and two others appeared to be in their late forties or early fifties.

During a question-and-answer period after our talk, an addict said:

> This stuff is interesting. If you are going around doing this, I'll be your consultant. For a fee, of course. We can get the wire out [i.e., the word out]. But, you understand, we need to look into it.

The day after the meeting, the "consultant" and another individual from the gallery came to our office. During the interview, the consultant told my assistant that they had come to check us out. The consultant said that they wanted to check out the relationship between the AIDS project and the host agency, a drug treatment agency. As the consultant put it, "We want to see if you people are independent from this place. We don't trust some of the people here." He was referring to the host agency and staff.

The consultant, like other IV drug users, was trying to make sense of the world in which he lives. Sometimes professionals forget this basic fact of human nature and treat IV drug users as unidimensional people. Researchers, like social service people, may think of IV drug users as if they live in a vacuum. They do not. IV drug users lead busy and complicated lives, and they constantly assess who is doing what with whom and for what reason. Thus, any effort to reduce the IV drug user's chances of acquiring AIDS must address the complex features of the addict's life and how the addict goes about making sense of his practical circumstances.

The purpose of this chapter is to describe the complex and dynamic world of Chicano IV drug users and to describe one method we use to discover

this world. Ethnographic data are presented to illustrate various aspects of the Chicano IV drug user's world. Some of these data come from cities on the U.S. and Mexican border where ethnographic work is now underway. In addition, we show how the methodological aspects of the "Indigenous Leader Outreach to IV Drug Abusers" model enriches the quality of ethnographic data that we obtain, data that we then use to influence behavioral change among IV drug users at risk for acquiring the AIDS virus.

DRUG CULTURE

Much is said about IV drug users in general, but little is known about the drug culture of the Chicano IV drug user (Aumann et. al. 1972; Bullington 1977; Jorquez 1984; Moore 1978; Moore and Mata 1982; Morales 1984). What Chicano IV drug users themselves say about their culture and the people who make up that culture is informative.

Chicano IV users claim that there are at least three aspects to their drug culture. One aspect is the way Chicano IV users classify themselves. Another aspect is linguistic, the language that Chicano IV users create and use. The third aspect deals with the ways "helpers" hinder drug addicts.

Chicano IV Users' Self-Classifications

Chicano IV drug users refer to themselves as "tecatos." It is said that the term tecato was applied to heroin users in Mexico in the 1940s. Then, it was used to distinguish heroin users from other drug users. The other major drug of preference at that time was opium. Even though some tecatos use cocaine today, the term is still used to refer to heroin users.

Tecatos stratify themselves into three groups: high class, medium class, and cucarachos.

High Class. Individuals in this group are known as tecatos buenos, good tecatos; they provide for themselves. They have money, and they do not ask for credit. They go to the connection or drug source and buy what they need. If they do not have the money for the drug, they do without. If they are arrested and they have to "kick" (withdraw from the drug habit), they kick quietly and do not complain about the pain.

The older and good tecatos see the young tecatos as crybabies. As some of the good tecatos put it:

> The guys today are spoiled. Or, rather, they have made themselves spoiled because, when they get arrested, they know that they are going to get medicine in the county jail, and they start to cry. They can't take it like in the past. Then, you were arrested and you knew that you were going to suffer and you

> prepared yourself. It is bad to break [kick], but when you break, like I tell you, you learn to take it and not to fear breaking.

There is truth in what the older good tecatos say about some of today's Chicano addicts. Older good tecatos can talk a supposedly good young tecato out of being sick for a while or into waiting for his fix. There is a lesson to be learned here for AIDS prevention. For example, good tecatos can train younger tecatos into waiting for their fix until they clean their needle with bleach.

This account may serve to illustrate the point Mata and Jorquez make when they write, "We found that controlled IV drug users tended to be older and more experienced" (Mata and Jorquez 1987, p. 49). Mata and Jorquez go on to say that these individuals "use various precautions, e.g., less frequent drug use and less drugs used, and they are more careful about who they share their drugs and IV injection works with" (Mata and Jorquez 1987, p. 49). This may be the case, but another explanation for controlled use may be that older tecatos operate under the old drug culture norms of a good tecato.

Other characteristics of good tecatos are that they dress well and act and talk responsibly. It is also said that they are reliable. Moreover, they may be employed at a "regular" job, or they may have a hustle (i.e., an illegal activity that makes money for them). Finally, these individuals may be married and appear to live ordinary lives.

Medium Class. (In Spanish, these individuals are called "medianos," so the label "middle class" does not translate into mediano.) These individuals are also seen as good tecatos. They do not ask for credit or a handout. But, they are different from tecatos buenos. While the good tecato is a leader or inner-directed type of person, the tecato mediano is not motivated to go out either to "connect" or to find a way to get money (i.e., steal). The tecato mediano waits around for someone to invite him to do a job or to connect. This person is a follower.

Cucarachos (Cockroaches). The cucaracho is an individual who hangs around the connection. Usually, there are one or two cucarachos around a connection. This individual always asks for a handout. The cucaracho does not pay his own way. He depends on real tecatos. He just waits around to see who will give him a "taste" or provide him with the drug.

Although the cucaracho behaves and talks as if he were a real tecato, this individual is not seen as a real tecato by other tecatos. It is said that these individuals rarely get hooked. If they get any drug, it is what is left in the cotton, or the cooker, or the syringe.

Cucarachos get a taste of heroin by doing favors or little jobs for the users and the seller. The cucaracho may also get a taste for lending his works (drug equipment) to a user who may not have his at the time of purchase. Or, he may run an errand either for the buyer/user or seller. For example, the cucaracho may go get cigarettes or syringes at the drug store, or do little odd jobs, like clean the car or yard for the seller or user.

There are some exceptions to the above description of the cucaracho. There are cases where a cucaracho lends or turns his home into a shooting gallery. In these cases, the cucaracho gets more than a taste. The cucaracho may get a large quantity of drugs because of the service he is rendering and may therefore become addicted. In a recent case in El Paso, TX, a cucaracho overdosed and died in this way.

A cucaracho plays a critical role in the life of IV users who shoot up when they buy in the home of the connection. More often than not, the cucaracho provides the works. While waiting for a tecato to buy and use, the cucaracho may spend time rinsing his works with water. Cucarachos could be trained to use bleach for cleaning works. An outreach worker on our project is helping cucarachos to develop a routine of rinsing injection equipment with bleach to prevent the spread of AIDS.

Cucarachos have a negative side to them. They may "burn" some people. It is said that cucarachos do not burn real tecatos, but that they may burn a newcomer, a "narc" (a police informer) or an informer for a narc. An informer for a narc is said to wear a chaqueta de relaje, or jacket of shame. Often, in a group of people, an individual may stare at another who is not in his group and say, "He has a jacket" (trae chaqueta).

Informers will use a cucaracho to buy drugs from a connection, so that the narc may locate the connection with the drug. In these cases, the cucaracho will take the money and not come back with the drug. These people, narc or narc informer, usually will not harm the cucaracho. An informer or individual with the jacket of shame will not go after the cucaracho who burned him. The informer cannot ask for the whereabouts of the cucaracho because nobody will cooperate with him. People in the community tend to know who is a narc informer.

A newcomer, either to the neighborhood or town or to drug use, who gets burned by a cucaracho often does nothing, perhaps because the newcomer does not know where to find the cucaracho or because he thinks he got burned by a real tecato. As pointed out, cucarachos present themselves as real tecatos.

Finally, the cucaracho depends on others for his support. He may be married, but he rarely has a steady job. Often, his wife is on welfare, and

he lives off her check. Or, as pointed out above, he may go work for a tecato for a day. He may fix a car, clean a yard, or take trash to the dump.

Implications of Self-Classifications. Needle sharing is common in these three groups of people. The high-class tecato bueno mostly shares with his partner, who may be his wife, girlfriend, or male buddy. Prior to the AIDS epidemic, these people only rinsed their works with water before passing it on to the partner. Now, these people tend to use new needles or needles rinsed with bleach if they are going to share with someone other than their wife.

As noted, the tecato bueno tends to be more controlled in his drug use. He or she will not share needles with anyone who is not in his group of tecatos buenos. This norm is similar to the one dealing with money. The tecato bueno will not buy drugs if he or she does not have the money. Instead, they will wait until they have the money.

The tecato mediano, like the high-class tecato, shares with his drug-using buddies. The cucaracho shares his needle with whoever gives him a taste of the drug. As pointed out earlier, the cucaracho can play a significant role in cleaning needles with bleach.

The status of a tecato may also be influenced by other factors. For example, the tecato may be a connection, a pusher, or a consumer. Connections are seen as being higher in status than the pusher and consumer. The pusher is higher in status than the consumer.

There is a behavior that may transcend these three classifications of tecatos. In the southwest, some recent arrivals from prison are using drag queens. If, in the past, an individual had one or several female prostitutes to provide sex, drugs, and income, he now may have a drag queen in his stable of prostitutes.

In some border cities, drag queens and gay prostitutes appear to make more money than female prostitutes. An interesting aspect of this situation is that these tecatos do not think of themselves as homosexual, and they are not seen by others as homosexual. They are seen as tecatos with a good hustle.

Tecatos and Language Use

In the emerging literature on AIDS prevention, AIDS researchers and social services delivery people call for "culturally and linguistically" appropriate prevention programs (Amaro 1987; Worth and Rodriguez 1986). These suggestions look great, but most writers do not specify what they mean by "cultural" and "linguistic" (Amaro 1987). They assume that the reader knows what these terms mean. This section addresses the linguistic aspects

of these recommendations; another section of this chapter will address the problems created when researchers do not define "culturally appropriate."

The few writers who define "linguistically appropriate" do so in a limited way (Amaro 1987). For example, the claim to have linguistically appropriate prevention programs only means that written materials and verbal interactions will be in Spanish and/or "spanglish." The assumption made is that the Spanish used is standard and that the spanglish is a mixture of Spanish and English.

This basic notion of presenting materials either in the subject's standard native language or in some dialect is fine, as far as it goes. But it is limited because it does not address how individuals use language, create and use language codes, and do code switching. Moreover, it does not address the contextual aspects of the social and physical environment that IV users and sex partners use to make sense out of what they and others do (e.g., buy drugs, steal, fence stolen goods, and get and hold jobs).

Tecatos, like any other people, create words and word usage that make up a language unique to them. The words that follow are used by tecatos as codes in specific situations.

Algoda. Algoda comes from the word algodon, which means cotton in standard Spanish. But, in the drug culture, cotton can mean any item that the tecato uses to filter the drug after it is cooked. The "cotton" often is the filter from a cigarette. Usage: Preste las algodas. (Lend me the cottons.) Móchate con las algodas. (Share with me the cottons.) Mochate is not often used in this way. The root word in standard Spanish is mochar, which means to cut. With the use of the phrase, mochate con las algodas, the individual reveals his status. It is said that only beginners and cucarachos ask for the algodas/cottons.

Aliviane. Aliviane comes from the standard Spanish word alivianar, which means to heal or cure. In slang, aliviane means to lighten up or get better. Usage: Pasame un aliviane. (Pass me a cure or help me out for the moment by giving me a fix or at the very least a few drops, or a cotton.) Andaba malillia, pero ya me aliviané. (I was sick, but now I have gotten healed or cured.) In other words, the individual is saying, "I was sick, but I have just fixed."

Apáre. Apáre comes from the Spanish word aparar, which means to stop. Here it means armed robbery. Usage: Hacer un apáre (to do an armed robbery).

Carga. Carga means load in Spanish. In the tecato world, it means heroin. Usage: Traes carga? (Do you have heroin?)

Chafa. Chafa means cheap. Usage: Es tecato chafa. (He is a cheap or not a serious/real tecato).

Chiva. Chiva means goat in Spanish and heroin in the tecato world. Usage: La chiva esta buena. (The heroin is good.)

Cincho. Cincho comes from the Spanish word cincha and the English word cinch. In the tecato world it means the same. Usage: De cincho la puedo agarrar. (For sure, I can get it, in a cinch [easily].)

Clávate. Clávate comes from the Spanish word clavo, which means nail. Clávate in the tecato world means to wait or hide. Usage: Clávate porque hay ropa tendida. Literally translated, this means: Nail yourself because clothes are on the clothesline. For the tecato, the sentence means "Wait, don't talk, because there are people listening."

Cotorrear. Cotorrear comes from the Spanish word cotorra, which means parrot. For the tecato, cotorrear is the back and forth head movement that an addict engages in when he is fixed (i.e., following recent injection of the drug). Usage: Estas cotorriando? (Are you fixed?) Being able to recognize this behavior is very useful to outreach workers and researchers. Saying, "Estás cotorreando?" gives the workers credibility with addicts and helps to establish rapport. As we have learned, the tecato opens up more when he perceives that we are knowledgeable. We assume that this perception leads him to believe that we are members of his culture.

Cura. Cura comes from the word curar, which means to cure or heal. In the tecato world, it has several meanings. Usages are given with the definitions. It can mean "to fix" as in the expressions: Para la cura, o me voy ir a curar. (For the cure, or I am going to cure myself.) Cura can also mean to laugh at something or someone, as in: Es bien cura. (He is funny.) It can also mean fun activities, like curadas, or indicate the nodding behavior that addicts engage in after a fix.

Enjable. For the tecato, enjable means to break into a house or a business to steal. Usage: Vamos enjablarnos a esa casa or negocio. (Let's go rob a house or a business.)

Esquinear. Esquinear comes from the Spanish word meaning corner. In the tecato world, the word means support. Usage: Esquinéame. (Give me support, or give me a corner so that I may help myself.)

Fardear. For the tecato, fardear means to shoplift. Usage: Vamos a fardear, o anda fardeando. (Let's go shoplift, or he is shoplifting.)

Filerear. Filerear comes from the Spanish word filo, which means cutting edge. For the tecato, it means to inject with a needle or syringe. Usage: Se filereáron? (Did they inject themselves?)

Jále. Jále comes from the Spanish word jalar, which means to pull or haul. For the tecato, jále means a job or employment. Usage: Vamos hacer éste jále. This sentence can have two meanings. It can mean let's go do this job. Or, it can mean let's go steal. Tecatos always talk about jáles or jalesitos.

Jugar el Número. Jugar el número (play the number) means to play a game or to develop a strategy/movida. Usage: The person assesses a situation and determines whether it requires that he play a fool or appear innocent or smart. He then plays el número or the part of a fool or an innocent or smart person. A good interviewer who knows tecato life can say to a tecato, "no me jueges el número." In saying this, the interviewer lets the respondent know that he or she knows the situation and that the respondent needs to be "straight" with him or her.

La Muleta. Muleta means crutch. This is one of the important words in the tecatos' vocabulary. For the tecato, la muleta refers to something that can become a problem. Usage: Me voy a quitarme esta muleta. This means that I am going to take away something that is or can become a problem. For example, I am going to a job that I don't really want to do, but I'm going to do it. Or, if I have a gun on me, I will go hide it because if I don't it can be a problem. The same thing applies to having drugs on you. You need to hide them. Usage: Para no traér la muleta. (So that I will not be carrying the problem.) An addict may say, "Mi quiciera morir para quitarme esta muleta." (I would like to die to be rid of my problem of being an addict.)

Malilla. Malilla comes from the Spanish word mal, which means bad or ill. The tecato may use the word to mean that he is sick in the "straight" sense or that he needs a fix. Usage: Ando buti malilla. (I am very sick.) (The word buti may be a word indigenous to El Paso, TX.)

Movida. Movida means strategy. Usage: Andar en una movida (having a motive or being in a strategy).

Parchar. Parchar means to patch. For the tecato, the word means to have sexual intercourse. Usage: Esta madre no se pega nomás parchando. Con erres. (You can get this mother [the AIDS virus] in other ways besides sexual intercourse. With works or rigs.) Some tecatos call condoms bandaids, and they request them because they are going to parchar.

Piña. Piña means pineapple in Spanish. For the tecato, piña means story or lie. Usage: Es piña? (Is it a story/lie?)

Ponle por la puerta. Literally, put it at the door, this phrase is used to mean to confront the person. Usage: Sálele por la puerta, no me descuentes. (Come out the door, don't discount me.) The confrontation may result in a physical fight. But, whatever may occur, the point is that, at the start, the activity is face to face, not catching the other off guard or coming at him from behind. If someone has pulled a movida (literally, strategy) on you, and you meet him on the street, he knows that he pulled a movida on you, so he may say, "Sálele por la puerta, no me descuentes."

Rayarse. Rayarse means to get something. This word is an important word in the tecato's vocabulary. Usage: Te rayas con algo. (You came out with something.) The something is usually money or an item that can be exchanged for money or drugs.

Rollo. Rollo means roll in Spanish. Rollo means talk/conversation or "rap" for the tecato. Usage: Tirar rollo. (Give them a talk.) Our outreach workers often say, "Les tiré el rollo de AIDS." (I gave them the AIDS talk.)

Soda. Soda means cocaine. Usage: Traen soda. (They have cocaine.)

Talonear. Talonear means to hustle. Usage: Vamos a talonear. (Let's go hustle.) Anda taloneando. (He is hustling.) Ese bato es muy talón. (This guy is always looking for something to do, like a job, or something that he can steal.)

Traer Cola. Literally, having a tail, traer cola means being on probation or parole. It can also mean being followed or being searched for. Usage: Traer cola de tres años. (I have a probation period of 3 years.)

To explain this term with a brief digression, there are some interesting treatment aspects to the idea of having a tail or traer cola. A tecato on parole or probation is not supposed to be on drugs during this period, but many tecatos do use drugs. If they want to enroll in a treatment program that requires payment, the tecato can't go to his parole or probation officer (PO) for help, even though the PO might be able to obtain financial help to pay for the treatment program. The situation is a catch 22.

If the tecato admits to his PO that he is using again, he runs the risk of having his parole or probation revoked. Thus, a tecato can only go to a counselor in a treatment program whom he trusts.

A tecato may go to a counselor/friend and say, "Ando malilla, y también ando buscando puerta para quitarme ésta muleta. Pero, traigo cola." As translated: "I'm strung out, and I am also looking for a way out to get this thing off of me. But, I have a tail."

If the counselor knows the PO and knows that he can work with him, the counselor may approach the PO to tell him the actual situation. And, thus, he may help the tecato.

In this situation, the tecato must have someone who can understand his plight and language and can "front" for him. The advocate needs to know the tecato's language, to be able to esquinear/support the tecato.

Transa. Transa means transaction or deal. The concept of transa implies that the deal should be "under the table" and that the items being exchanged for the money are probably stolen. Usage: Vamos hacer esta transa. (We are going to go make a deal.)

Toriquear. Toriquear means to lie. Usage: Me estas toriquiando? (Are you lying to me?) As pointed out before, an outreach worker and researcher may use this word like the other words listed here. In so doing, he can communicate and establish or reveal that he has membership in the tecato community.

Vaquita. Vaquita means little cow in Spanish. In the tecato world, it means that two people share the price of a fix. Each person gets half of the drug in the cooker, and these two people share the syringe. Usage: Busco una vaquita. (I am looking for a little cow.) Tecatos with little money practice the vaquita process. Many tecatos in the border cities of Mexico do this.

The words given are codes. Tecatos use them to communicate with one another and to make sense of what they are doing in specific situations. These words convey meaning to the tecato in terms of the context in which they are used by particular individuals.

For example, an individual can say to a friend, "Let's go do a job." (Vamos hacer este jále.) A third person or listener to the verbal exchange may want to know what job they are talking about. He may ask, "Qué jále?" Often the response is something like the following: "Un jále." (A job). In the drug culture, this response tells the person asking the question that he is not going to be told. The response also lets the questioner know that he should not probe. In the drug culture, people who probe engage in unacceptable behavior. Membership in the culture is maintained by knowing the norms of appropriate behavior.

Helpers and Hinderers

Tecatos play out their lives in particular contexts, which are populated by significant others who help them and hinder them at times. People in helping agencies like drug treatment centers and in private business are

often tempted by the inexpensive items that tecatos can provide for them. Here I will point out how some helpers hinder the tecato.

Pharmacists, drug treatment center employees, policemen, and ordinary community people, for example, are part of the drug culture. They help perpetuate tecatos' deviance by buying stolen goods from them. Worse, these people give tecatos "shopping lists" of items that they need.

For instance, a methadone employee may "help" a tecato pay for his methadone by buying from him or her stolen items that she, the employee, ordered. Of course, a $70 dress will cost only $18. If the tecato complains, he or she has trouble getting methadone. As an employee put it to the tecatos, "I am the goddess, and you are to bring me gifts."

Some policemen, the very people who arrest tecatos for their drug use, buy goods from tecatos for their own use and to sell at a profit. As in the above example, if the tecato doesn't produce, he can get burned.

Housewives also may seek out tecatos to put in their orders. Some tecatos do a thriving business selling clothing and meat to housewives.

There are two interesting aspects to the buying of stolen goods from tecatos. One aspect is that tecatos develop a list of customers that he or she supplies. The other aspect is that these "helpers" and "good" community people reinforce the tecato's idea that everyone is a crook.

OUTREACH AND RESEARCH: A CASE STUDY

In AIDS prevention among IV drug users, much is said about the role of research and outreach intervention practices. Often, the roles of research and outreach are not clearly spelled out in prevention projects. Here, I present a descriptive account of how researchers and outreach workers of the El Paso, TX, project of the University of Illinois at Chicago work together to learn about the tecato's actual situations, and to show how pushers who run galleries can further the aims of the project by becoming prevention advocates. They can become "informal staff" because they do outreach in their galleries. They continue our work by using bleach to rinse their needles and by telling other tecatos who come to connect and/or shoot up to use bleach and condoms to prevent getting the AIDS virus. It can be said that these pushers in galleries represent an indigenous leadership outreach. The ex-tecato or nontecato indigenous outreach workers and researchers represent another important aspect of effective outreach intervention.

The following material consists of our observations and taped recorded interviews with tecatos. Some of the material is presented in Spanish. An English translation follows the Spanish account. The material is presented

in both languages for two reasons. First, to show how some of the vocabulary presented above occurs in real life. Second, to remind the researcher and the reader that being bilingual and sharing the tecato's experiences by sharing the language are important for ethnography and for effective outreach.

Here we describe some of the ways we do our work. The cast of characters are the following people whose names have been changed, except for the research assistant and the outreach worker:

Lorenza, research assistant
Pete, outreach worker, ex-tecato and old friend of Berta, Mono, and Norberto
Berta and Mono, two 55-year-old tecatos/pushers and sex partners
Gloria, 36-year-old tecata lesbian
Dolores, 26-year-old tecata lesbian/bisexual and sex partner of Gloria
Norberto, 50-year-old tecato and shooting gallery proprietor
Bruno, 30-year-old tecato and shooting gallery proprietor

The context of the account will reveal the relationships we have established with tecatos. An objective of our project is to work with IV drug users who can influence others to change their behavior to reduce the risk of getting and passing the AIDS virus. These gatekeepers or leaders often are the pushers and proprietors of galleries.

We established rapport through Pete, our outreach worker, and by helping tecatos get housing and other basic necessities. We met Gloria through one of her lesbian friends who asked if I could help get Gloria out of jail. We met Dolores through Gloria. When Gloria got out of jail, she returned to Dolores, her lover and shooting partner. Both women went back to using and selling drugs to support their habit. Norberto and Bruno were acquaintances of Pete.

Lorenza:

> I met Mono today, finally. I had heard so much about Mono for the past 2 months that I wanted to meet him. I first heard about him from Gloria. She said that she had talked with Mono and Berta, his girlfriend, about coming for an interview. She thought that I would like to interview them because they are interesting people. But they refused.

Gloria:

> It's good for you to hear what they have to tell because they are sharing needles. Berta and Mono are sharing needles, and they should know more about AIDS and tecatos.

Lorenza:

Later, when I was inside the building, Pete asked me what Gloria wanted. He thought that I had given her money. I told him I'd only given her food stamps. Then, I asked him if he thought that they would go buy groceries because of the little boy.

Pete:

No, andan con la Berta, y el Mono. Yo los conozco. Ahorita van ir a conectar. Van a vender las estampías. (No, they are with Berta and Mono. I know them. Right now they are going to connect. They are going to go sell the stamps.)

Lorenza:

The third time I heard about Mono was when Gloria came to visit. Apparently, she and Dolores had been staying off and on at Berta's house. She told me that Berta had some kind of income, either social security or a widow's pension, something like that. And that income gave Berta a way to live. Gloria also said that Berta has a nice home and that Mono lives with her. Gloria went on to tell me that Berta never really needed to go out (to get drugs), because she always had money and a good home. But, in the last year, Berta has had to go out in the streets. Gloria reported that she had again encouraged them to come for an interview with me and you [i.e., Ramos]. They said, "No, we already heard about what they are doing there at the agency. El Pete nos invitó. Pero le dijimos que no." (Pete has invited us. But, we told him no.)

Today, el Mono came. Pete came and said, "There is this guy outside. And, he came in for an interview. I didn't expect him. No lo esperaba, pero si no lo quieres entrevistár yo le digo que venga otro día." (I didn't expect him, but if you do not want to interview him, I'll tell him to come some other day.)

As usual, my question was, "Es tecato?" I wanted to know if he was a current user.

Pete:

Oooh, de los buenos. (Oooh, of the best.)

Lorenza:

Then, I asked him, "Who is it? Do you know him?"

Pete:

Yes, es el Mono. (Yes, it is Mono.)

Lorenza:

El Mono! Finally! I told him that I would be ready in 5 minutes. Then I went to the lobby to meet him and to tell him that I would interview him in 5 minutes.

During the interview, Mono was very open and cooperative. He was very honest. I believed the influence was Pete's, because Pete had already talked with him about the program.

After the interview, he went to Pete's cubicle, and Pete did a risk assessment. That is, they explored ways of changing behavior that could reduce the risk of getting the AIDS virus. I joined them and listened.

As I said, he was very cooperative. But, this may be an exception. During the interview, I asked him, rather than ask Pete later, where he connected. He told me that he connected downtown. I said, "Mono, I know where some of the connections are. And, you are not divulging anything. I just want to know where it is that you find your drugs best."

Of course, I knew from Gloria that he sold drugs. So, perhaps Mono didn't need to go to the different areas that users go to. But I still asked, "Do you buy it at Florencia, or Pugas, or the church, or San Jose?"

Mono:

No, I buy it downtown.

Lorenza:

During the time he visited with Pete, he mentioned two places, Bruno's and Norberto's. From the conversation, I learned that his actual connection is Norberto's and not downtown.

Norberto's and Bruno's names came up because Mono mentioned that last night or early this morning Bruno, at San Jose, had gotten busted. The police had not found anything except a marijuana cigarette. As he said, "no más les encontraron un cachito de grifa." He added, "Ahorita estaba muy caliente la situacion ahí, y que por un rato

nadien se íba acercar. El San Jose esta seco ahorita." (Presently, the situation there is very hot, and for a while nobody is going to get near it. El San Jose is dry right now.)

I noticed how quickly drug users know the situation with connections. Of course, if he knew, it was likely that many more knew by now. They wouldn't hang around San Jose because they would run the risk of arrest.

Mono also mentioned that the cathedral (Norberto's gallery) was going to be next. "Porque ya la han estado cuidando desde la ventana de la iglesia o San Pedro." (Because they are already observing the cathedral from San Pedro church.) I realized that although Mono didn't want to tell me where he connected, he knew Norberto, or the cathedral, and he frequented the place. As the conversation developed, Pete told him, "You can speak with her, because she has been to Norberto's. We have been there twice, and they have accepted us. Also, we have shown them two videos."

Mono:

Si ahoríta ya me estoy acordando que sí, porque Norberto carga una botellita con cloro y ahí la tiene cerca de él. Y, también le he visto unos papelitos que tiene por ahí del AIDS, y me lo ha enseñado. Y, aparte que cuando entran y cuando se presentan—que porque no todos se presentan para oír, él, Norberto, les platica del AIDS, y les platica del cloro. (Yes, right now, I am remembering that, yes, you've been there. Because, Norberto carries a small bottle of clorox, and he has it near him. And, also I have seen several little papers on AIDS. And, he has shown them to me. And, besides when people come and present themselves—not everyone is open to listening—he talks to them about AIDS, and he talks to them about clorox.)

Lorenza:

Then I asked him, "De verda, no piña. Verdad? (Really, it is not a pineapple, i.e., you are not lying? The truth?)

Mono:

Porque le voy a decir piña? Y se reía. (Why am I going to say pineapple, i.e., lie to you?)

Lorenza:

But what is going to happen? I asked Pete what is going to happen to Norberto if they arrest them?

Pete:

No, they won't arrest them. They won't arrest him. In the past, they have arrested him only twice, and they have only given him a ticket.

Lorenza:

Pete changed the subject of the conversation from Norberto to Berta. He asked Mono, "Y la Berta?" (And, Berta?)

Mono:

Malilla, como lo debes de saber. (Sick, as you must know.)

Lorenza:

Then, Mono told us why he had come for the interview. Mono came for the $10. Because he and Berta have hit hard times, they needed the $10. Berta was sick; they needed the money to buy Berta a fix.

While in Pete's cubicle, Mono called Berta and told her that he was on his way. Pete took the telephone and talked with Berta.

Pete:

Aguante como una soldada, que llegarán en un rato. De hoy en adelante piensa venir nada más para platicar—a tirar un rollo. Para ver si te podemos quitar esa muleta de encima. (Endure like a soldier woman, they will arrive in a short while. From here on, think about coming to do nothing else but talk—to rap. And to see if we can help take away the problem that is upon you.)

Lorenza:

Mono waited for another person being interviewed. As he waited, he continued talking with Pete, and he gave me and Pete information on the drug scene in El Paso.

The information Mono gave Lorenza and Pete is typical of information that we often turn into lines of inquiry to follow in the future. For example, before he left, he told Pete that a tecato whom they knew had been cut off by several connections (i.e., was no longer able to obtain drugs from usual sources). They didn't know, and Lorenza thought it would be interesting for us to know, why a tecato gets cut off, the length of time a person is cut off, how the tecato manages while cut off, and how the tecato eventually reestablishes himself with the connections.

CONCLUSION

Descriptive material on tecato culture and a list of vocabulary words are evidence of the ways that tecatos, in relationship with others, create the tecato culture. It is not simply the case that the culture determines how the tecato copes with his practical circumstances. Yes, cultural norms do influence behavior, but they do not explain totally how tecatos interpret the norms and take into account other information that may not be part of the tecato culture (Ramos 1979).

To prevent the spread of AIDS among Chicano IV drug users, we need to discover what the tecato takes into account as he defines the many situations that he creates to manage his life as a tecato. To this end, I have some recommendations:

(1) AIDS researchers and service delivery people need to discover the practical circumstances of the specific IV drug user and sex partner under study.

(2) AIDS researchers and service delivery people need to discover what the specific IV drug user and sex partner under study take into account when they define, construct, and manage the situations they create with others. Specifically, researchers should study:

- how high-risk people define reality for themselves;
- how high-risk people create and cope with the events in their lives;
- how high-risk people create and use language codes;
- how high-risk individuals define help; and
- how and where researchers and social services delivery may enter into the lives of high-risk people.

REFERENCES

Amaro, H. Hispanic women and AIDS: Considerations for prevention and research. Presented at the NIMH/NIDA Research Workshop on Women and AIDS: Promoting Healthy Behaviors. September 28-29, 1987.

Aumann, B.; Hernandez, M.; Medina, M.; Stewart, C.; and Wherley, N. *The Chicano Addict: An Analysis of Factors Influencing Rehabilitation in a Treatment and Prevention Program.* Phoenix, AZ: Valle del Sol, 1972.

Bullington, B. Heroin Use in the Barrio. Lexington, MA: Lexington Books, 1977.

Jorquez, J. Heroin use in the barrio: Solving the problem of relapse or keeping the tecato gusano asleep. *Am J Drug Alcohol Abuse* 10(1):63-75, 1984.

Mata, A., and Jorquez, J. Mexican-American intravenous drug user's needle-sharing practices: Implications for AIDS prevention. In: Battjes, R., and Pickens, R. eds. *Needle Sharing Among Intravenous Drug Abusers: National and International Perspectives.* National Institute on Drug Abuse Monograph 80. DHHS Pub. No. (ADM)88-1567. Washington, D.C.: Supt. of Docs., U.S. Govt. Print. Off., 1988. 189 pp.

Moore, J., and Mata, A. *Women and Heroin in a Chicano Community. A Final Report.* Los Angeles: Chicano Pinto Research Project, 1982.

Morales, A. Substance abuse and Mexican American youth: An overview. *J Drug Issues* 14(2):297-311, 1984.

Ramos, R. Movidas: The methodological and theoretical relevance of interactional strategies. In: Denzin, N., ed. Studies in Symbolic Interaction, Vol. 2. Greenwich: JAI Press, 1979. pp. 141-165.

Worth, D., and Rodriguez, R. *Latina Women and AIDS.* New York: SIECUS, 1987.

ACKNOWLEDGEMENTS

Pete Estrada and Lorenza Mena, of the University of Illinois at Chicago AIDS Demonstration Project in El Paso, TX, helped in data collection and shared their knowledge of the tecato culture.

AUTHOR

Reyes Ramos, Ph.D.
PAHO
Suite 600
6006 N. Mesa
El Paso, TX 79912

128619

The German Bridge: A Street Hookers' Strip in the Amsterdam Red Light District

Hans T. Verbraeck

INTRODUCTION

In Amsterdam, as in every city in Western Europe, there are many kinds of prostitution. There are women who work at home; there are escort services, very chic and very cheap ones; and there are clubs, both exclusive and sleazy ones. Amsterdam and some other Dutch cities are famous for the phenomenon of the window girls, a very amorphous group. In Amsterdam, there are windows outside the Red Light District: expensive and large windows on well-known streets and canals, cheaper and smaller windows in alleys. Finally, there are, of course, prostitutes who work in the streets.

This chapter focuses on heterosexual prostitution, but the same story could be told about homosexual prostitution. There are, of course, many other kinds of paid sex in Amsterdam (Belderbos and Visser 1987), but even the subject of street prostitution in Amsterdam must be restricted. Based especially on fieldwork done in the summer of 1987 in the Red Light District in Amsterdam, this chapter discusses addicted female street prostitutes in the Amsterdam Red Light District.

GEOGRAPHY OF PROSTITUTION IN AMSTERDAM

The street hookers' strip in the Red Light District is not the only one in town. Another group of addicted street prostitutes worked, until the end of 1986, at the de Ruyterkade. Until 1986, the policymakers at city hall had allowed an area where street prostitution was allowed, a "gedoogzone." Translated, it is something like "permissive zone." Prostitutes were allowed to work there; working in other parts of town was prohibited. In January 1987, this gedoogzone was abolished following protests from area residents. A few dozen addicted prostitutes stayed, moving just a couple hundred yards, thus creating a new area behind the central station.

This street hookers' strip resembles those seen in other European cities. Clients cruise by in cars and are approached by the prostitutes; a price is

negotiated; and they depart to a cheap hotel, a parking lot, or the docks nearby.

Although they no longer work in a gedoogzone, the street hookers behind the central station still have facilities enjoyed by prostitutes working in gedoogzones in other Dutch cities like Rotterdam, The Hague, and Utrecht. A center has been established where the street hookers can get professional drug help, stay and rest, drink coffee, eat, have a shower for a guilder ($0.50), obtain clean needles and condoms for $0.25, and receive information on acquired immunodeficiency syndrome (AIDS) and general information.

The strip behind the central station is not part of the Red Light District. Indeed, it is one of the more depressing streets in Amsterdam, whereas the Red Light District is not depressing at all, but one of the more vibrant neighborhoods in town.

The Red Light District

The Oudezijds Voorburgwal, the Oudezijds Achterburgwal, the Gelder-sekade, and the alleys in between make up the Amsterdam Red Light District (figure 1). The area is crowded with sex shops, adult movie theatres, live show theatres, and street and window prostitutes. One can find here the oldest church in town, next to windows of prostitutes, and a souvenir shop, next to one of many hashish coffee shops. In the same area, ordinary Amsterdammers live their everyday lives. There are local cafes, shops, supermarkets, and all the normal facilities found elsewhere in town.

Drug Practices

One can also find here perhaps the most notorious heroin copping (drug distribution) street in Western Europe: the Zeedijk. Heroin and cocaine are sold on this street in small sealed plastic bags 24 hours a day. Dealers openly offer "grote en kleine bruin," which are large and small doses of heroin, for 40 guilders ($20) and 20 guilders ($10) a bag. They also offer "grote en kleine wit," which are large and small doses of cocaine, at the same prices. Crack is not currently sold in Holland. On and near the street may appear a junkie taking a shot in a doorway and many others who are smoking heroin and cocaine by heating the drugs on aluminum foil and sucking the smoke up through a straw. The junkies in Holland call this way of using heroin and cocaine "chinezen," to "Chinese," or "chasing the dragon." It is important to know that a majority of the addicts in Amsterdam are not intravenous (IV) drug users; instead, they smoke the drugs. Only 37 percent of the Dutch drug users, 4 percent of the so-called ethnic users (mainly from Surinam and Morocco), and 69 percent of the foreign drug users are IV addicts. Moreover, since the AIDS epidemic, IV drug users are switching from IV drug use to chasing the dragon.

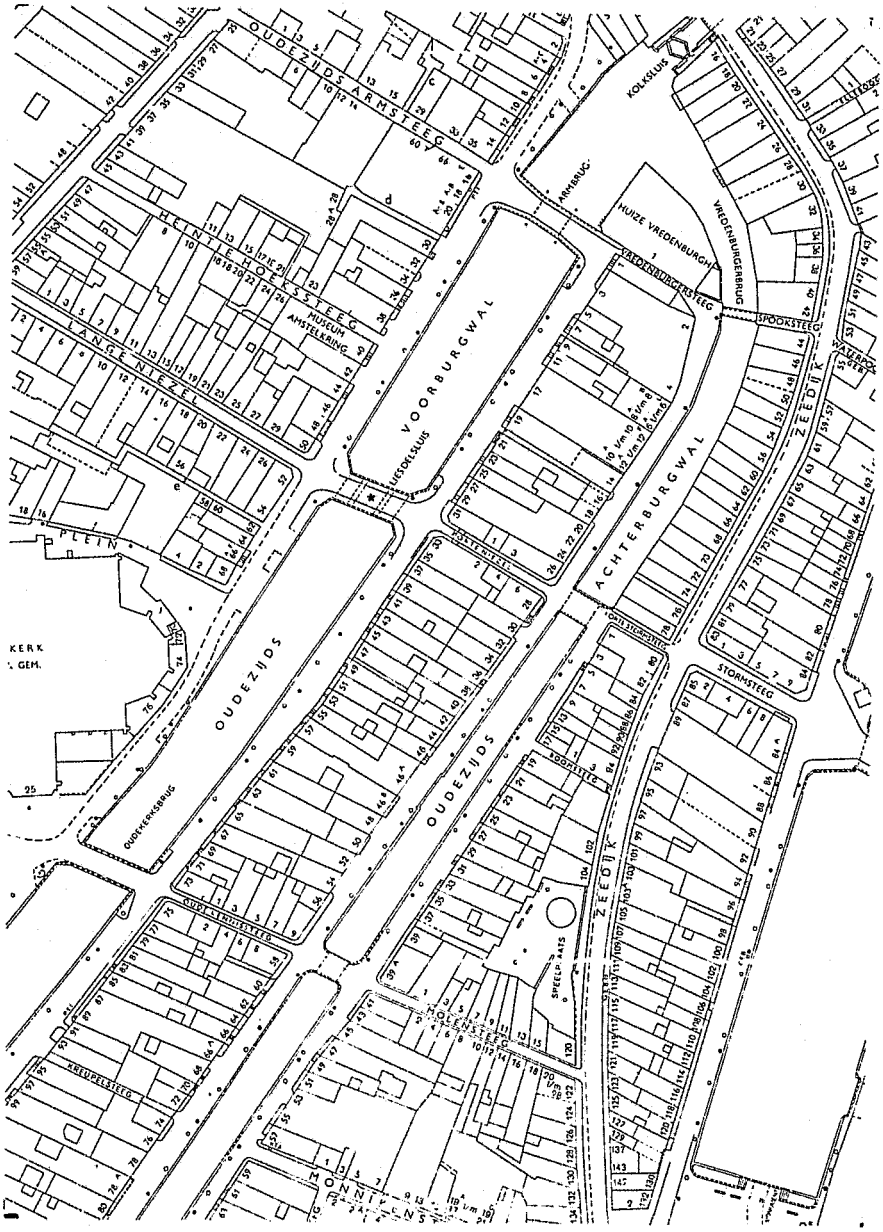

Figure 1. *Map of Amsterdam red light district, with asterisk indicating the bridge*

ENFORCEMENT POLICY AND ITS EFFECTS ON STREET ADDICTS IN THE RED LIGHT DISTRICT

Over the past couple of years, policymakers in Amsterdam have developed "ontmoedigingsbeleid," a policy intended to discourage the hard-dope scenes in town, to make the city safer and more pleasant for the local residents and nonaddicted tourists visiting the city. Since the policy of discouragement has been implemented, many addicts have left the inner city and gone to other neighborhoods and suburbs (Korf 1987).

The peak of the policy of discouragement was called Summerplan 1987. Summerplan 1987 was an intensified scheme of uniformed surveillance and actions by the detective force against fences, burglars, pickpockets, shoplifters, addicts, and dealers to create and consolidate an acceptable level of order and safety.

In the Red Light District, this Summerplan 1987 was very tough. It meant that more policemen were walking around in the area than in years before. The police force in the area was assisted in the summer of 1987 by 55 "marechaussees," or military policemen, and by police with dogs, mounted police, and other backup. During the summer, six uniformed policemen walked the Zeedijk 24 hours a day. Sixteen others walked other parts of the area on and off during the day and night.

Most of the following observations are based on fieldwork done in the area from April 1 to October 1, 1987, by the author and a colleague.

The policy of discouragement and Summerplan 1987 had some notable effects on those working in the Red Light District.

- When 6 policemen patrolled the Zeedijk 24 hours a day and 12 others patrolled on and off in other parts of the area, the Amsterdam Red Light District was no longer a dope fiend's paradise.

- Many junkies and addicted street hookers fled to other parts of Amsterdam and to other cities in the country. Additionally, a lot of junkie tourists were arrested and deported.

- A hard core of 400 junkies and addicted street hookers stayed in the Red Light District. This hard core was a special group. Mostly, they stayed because they had no choice. Most of them had purchased or copped heroin and cocaine in the area for years. Either they did not know any dealers in other neighborhoods, or these dealers did not want to sell to them. People of the hard core were arrested frequently, detained for questioning, and only occasionally brought to trial. They usually got short sentences, and they always returned to the Zeedijk in the Red Light District. The hard core who stayed in the District

consisted of the "hard-core" levels of both the dope and prostitution scenes in town. In the area, they were called the extreme problematic drug addicts or "losers."

- Because of the large police force in the District, addicts and dealers cannot hang out on the Zeedijk as they did a couple of years before. The police force them to move on or arrest them. To avoid the police, dealers and junkies must walk blocks through the entire neighborhood, and they must avoid these policemen to survive. They go to the canal of the window girls known as Oudezijds Achterburgwal; to the Kloveniersburgwal; to the Oude Hoogstraat, where most of the pills in town are sold; and to the Oude Doelenstraat en Damstraat, where street dealers are trying to sell horse manure as hashish to young tourists (Verbraeck 1988).

THE GERMAN BRIDGE: CHRONOLOGY AND PERSONNEL

Dealers and junkies also go to the Lange Niezel, the Liesdelsluis, and the Korte Niezel, on the street hookers' strip in the Red Light District. When the heroin and cocaine dealers are forced off the Zeedijk, they go to the street hookers' strip. In fact, one can get heroin and cocaine on the strip when all the street prostitutes are still asleep; early in the morning, there are usually a few heroin and cocaine dealers hanging out on the street hookers' strip. Only early in the morning are drugs offered openly. During the rest of the day, dealers are present, but deals are made on the Zeedijk or in the alleys between the canals.

At 10 in the morning the alcoholic tramps arrive, having slept at the Salvation Armies or near the central station. They sit along the Liesdelsluis, and, while the prostitutes are still asleep, open their first bottles of beer and cheap wine, which they drink all day.

Toward the end of the morning, the first addicted street hookers arrive. Although some come from cheap hotels in the area, most have a room or an apartment somewhere else in town. Before they go to work, the prostitutes have breakfast in one of the many snack bars or coffee shops on the strip and chase some dragons in the doorways of the Oudezijds Voorburgwal. They wait for the men working in the center of town who visit the Red Light District during rush hour at noon. The prostitutes wait in the doorways, in front of the windows of the Lange Niezel and the Korte Niezel, lean against the rail of the bridge, or walk around on the strip.

It is not yet busy at noon on the street hookers' strip. Never have more than six prostitutes been seen working before 1 p.m. Although there are many window girls—there are at least 200 windows in the District—no more than 40 addicted street hookers work the strip, even during peak hours.

During the afternoon, more and more prostitutes arrive and choose a spot on the strip. The addicts and dealers of the Zeedijk have to leave now and then, when they are chased by the police. When this happens, they head for the street hookers' bridge nearby. By midafternoon the bridge is crowded with people. Many of these people are clients or potential clients of the window girls and the street hookers. Every day the same groups of elderly men loiter on the bridge. Many of these men are steady clients. Some street hookers choose a place among the elderly men or accost people who enter the Red Light District via the Lange Niezel.

Prostitutes' Social Isolation

The street hookers on the strip in the Red Light District are a very loosely knit group. The majority of the prostitutes work on their own. They arrive in the area, choose a place on the strip, and go home at night without having any contact with the other prostitutes.

The isolation of the prostitutes is intensified by the fact that the police station in the area is in the Warmoesstraat, so that every policeman on his way to the Zeedijk walks by way of the street hookers' strip, scattering the prostitutes in the process. The majority of the street hookers have contact with the hard-dope scene on the Zeedijk only when they are copping or purchasing their heroin and cocaine after some tricks (customers).

Some prostitutes have regular contact only with their steady clients. A few live in a room of the house of a steady client and frequently come with this client to the strip.

Some prostitutes have addicted boyfriends. Only a few of these boyfriends are also protectors, or pimps. They keep an eye on who the prostitutes are leaving with, follow them to the cheap hotels in the area, and, when the prostitutes get into a car for a trick, they note the plates.

The traditional pimp who has a couple of prostitutes and who is a link between them is completely unknown on the street hookers' strip in the Red Light District.

Usually, the boyfriends have their own hustle and their own place in the hard-dope world of the Zeedijk. This is especially true when the boyfriend is black, because most dealers on the Zeedijk are black. Having a dealer boyfriend affords the prostitute a place in the world of the copping street.

But, as previously noted, the majority of those working on the street hookers' strip in the Red Light District operate in complete isolation in both the drug and prostitution worlds.

Tourists

Tourism affects the life on the street hooker strip in the summer. One has to realize that the Red Light District is without any doubt the biggest tourist attraction in town, and the Lange Niezel is an important access route to the district. Because the streets are narrow and so many tourists walk around, only a few cars can cruise by. Some tourists are potential clients for the street hookers and window girls but most of them are not. Many tour operators in town organize sex and drug "safaris" in the Red Light District. The street hookers and window girls complain about these sex and drug safaris. A group of 50 tourists with a guide will have a quick walk through the area but will not go to a pub, to clubs, or to prostitutes. Sex and drug safaris do not provide any business at all in the Red Light District.

Other tourists, those who come on their own or in small groups, definitely are potential clients for the street hookers. Some of the prostitutes approach every man who visits the Red Light District. Others mainly meet steady clients in cafes, coffee shops, and snack bars on the strip. When a deal is made, client and street hooker leave and go to one of the cheap hotels in the Beursstraat, Warmoesstraat, or Damrak, or the client and prostitute walk together to a car parked outside the district.

Drug Use

After every trick or after every two tricks, the prostitutes cop (purchase) a grote or kleine bruin (heroin) or a grote or kleine wit (cocaine) from one of the dealers on the bridge. Sometimes they go to the Zeedijk to cop or buy their drugs.

The strip at the bridge is crowded until midnight. Sex and drug safaris pass by, visitors of the Red Light District hang out on the bridge for a while, and now and then waves of junkies and dealers come from the Zeedijk. After midnight, it gets quiet on the street hookers' strip. The cafes, coffee shops, and snack bars, where the prostitutes meet their steady clients, close down. After midnight, most visitors of the Red Light District simply walk up and down the Oudezijds Achterburgwal, which is the canal with the live show theatres and window girls.

Only a few addicted street hookers wait desperately for some last clients so that they can cop (buy) their last bruin and wit for the night.

HARD-CORE POPULATION AND AIDS

As mentioned earlier, because of the policy of discouragement, the tough Summerplan 1987, only a hard core of addicts and addicted prostitutes is left in the Red Light District. This hard core represents the lowest social class of both the hard-dope and prostitution worlds. Although this is the

lowest social class, there are still other, different classes or levels of professionalism among prostitutes. Research recently published in Holland shows that 50 percent of all the street hookers in town started as nonaddicted prostitutes; the other 50 percent were addicted before they turned to prostitution. Of those who were addicted before they became prostitutes, the "junkie" tourists, many from Germany, are considered the least professional. Many of the German prostitutes are heavily hooked; nearly all are IV drug users; and most work at less cost, go home with nonsteady clients, rob clients, and do not charge anything extra when a client does not want to use a condom (van de Berg and Blom 1987).

All groups, including those who were prostitutes before they started using hard dope, those who were addicted before they became prostitutes, and female junkie tourists, still work on the street hookers' strip in the Red Light District. A few years ago the bridge on the strip was called "die Deutsche Brücke," the German Bridge, because many German junkie tourists worked there. After a few years of the policy of discouragement, one cannot accurately call the bridge in the Red Light District the German Bridge. Most of the German street hookers, who are considered the least professional, have moved to other areas in Amsterdam or other cities in Holland, or they have been sent back to Germany by the police.

Some Dutch prostitutes work unprofessionally too, of course. Nonetheless, some prostitutes working the strip are quite professional. They have many steady customers, meet most of them in cafes, coffee shops, and snack bars on the street hookers' strip; work for fixed prices; always work in cheap hotels except for very steady customers; and never exclude the use of the condom, not even when they are dope-sick.

When discussing addicted prostitutes, prostitutes who are sometimes IV drug users, and those who sometimes work unprofessionally, some remarks about the AIDS problem in Amsterdam are necessary.

There is not one square mile in Holland where condoms are so readily available, and there probably is not a square mile in the world where one can get as many clean needles as in the square mile of the Amsterdam Red Light District.

The area is crowded with sex shops, and every sex shop sells condoms, of course. If you simply ask sex shop owners for condoms, they smile. One can get at least a hundred kinds of condoms in every sex shop in the area. In the Warmoesstraat, where there are gay clubs and gay cafes, two women recently opened an interesting shop called "Condomerie." They sell only condoms and anti-AIDS tee-shirts and posters. Free condoms and AIDS information are also distributed by Stichting Regenboog, a drug-help organization on de Oudezijds Achterburgwal in the heart of the Red Light District.

This organization, Stichting Regenboog, also has a needle exchange program. There are extensive needle exchange programs in Holland. In or near the Red Light District, there are other places from which addicts can get clean needles. Main needle exchangers are the local health service, which distributes needles as part of the methadone programs, and the "Junkiebond." The Junkiebond, or Junkie Union, is an organization that works for the rights of junkies.

The most unusual place to obtain needles is a shop on the Zeedijk. Although one window is packed with pornography and another with typical Dutch souvenirs, everybody in the area knows that the shop mainly sells, not exchanges, works. Addicts can buy a clean disposable needle in the shop for 2 guilders ($1), which is a ridiculously high price for a needle in Holland. Also available in the store is a quarter of a lemon to dissolve Turkish heroin, priced at a guilder, and a piece of aluminum foil and straw, the works for addicts who are chasing the dragon, at a guilder each.

The local health service calculated that the official needle exchangers distributed 800,000 clean needles among 2,400 IV drug users in 1987. That means that every IV drug user used one clean needle nearly every day (Buning 1987).

The local health service also found that of the 370 AIDS patients in Holland in 1987, 16 (4.3 percent) were IV drug users. They also found that one-third of all the IV drug users in town were seropositive (van den Hoek et al. 1988).

Although not as alarming as the AIDS-prevalence figures in some other European countries and in the United States, these figures are still frightening. The statistics raise the question: What happened in Holland during the extensive needle exchange programs? The following statements summarize the possibilities.

- Although 800,000 clean needles were distributed to IV drug users in Amsterdam in 1987, only 400,000 were distributed in 1986; 100,000 in 1985; and 25,000 in 1984. It could be said that the extensive needle exchange programs started too late to combat the AIDS epidemic in the local drug scene. Besides, junkies can get new, clean needles only during certain periods of the day; it is very hard to get a clean needle between midnight and 10 a.m.

- More than 50 percent of all the IV drug users in Amsterdam are foreigners. Most of them were addicted before they came to Amsterdam, although many people do not believe that. There are no needle exchange programs outside Holland. Many foreigners already were infected in their homelands before they came to Amsterdam.

- Although every addict knows by now that there is an AIDS epidemic in town, some still share needles, especially when they are dope-sick during the night, when it is hard to get a clean one.

- Addicted prostitutes still do not usually use condoms when they have sex with their addicted boyfriends.

- There are still clients of addicted prostitutes who do not want to use condoms. Some clients wait until the prostitutes are dope-sick to arrange a trick without a condom at a ridiculously low price.

In 1987, the first addicted prostitute who took drugs only by chasing the dragon all her life got AIDS.

REFERENCES

Belderbos, F., and Visser, J.B. *Beroep: Prostituee.* (Profession: Prostitute.) Utrecht: SWP, 1987. 231 pp.

Buning, E.C. *De GG&GD en Het Drugprobleem in Cijfers II.* (The Public Health Service and the drug problem in statistics.) Amsterdam: GG&GD, 1987. 27 pp.

Korf, D.J. *Heroïnetoerisme II: Resultaten van een Veldonderzoek Onder 382 Buitenlandse Dagelijkse Opiaatgebruikers in Amsterdam.* (Heroin tourism II: Results of a field research among 382 foreign daily opiate users in Amsterdam.) Amsterdam: University of Amsterdam Press, 1987. 125 pp.

van de Berg, T., and Blom, M. *Tippelen Voor Dope; Levensverhalen van Vrouwen in de Heroineprostitutie.* (On the strip for dope; life histories of female heroin prostitutes.) Amsterdam: SUA, 1987. 159 pp.

van den Hoek, J.A.R.; van Haastrecht, H.J.A.; van Zadelhof, A.W.; Goudsmit, J.; and Coutinho, R.A. HIV-infectie onder druggebruikers in Amsterdam; prevalentie en risicofactoren. (HIV-infection among drug users in Amsterdam; prevalence and risk factors.) *Ned Tijdschr Geneeskd* 132(16):723-728, 1988.

Verbraeck, H.T. De Staart van de Zeedijk: *Een Bliksemonderzoek Naar Enkele Effecten van het Zomerplan 1987 in Het Wallengebied.* (The trail of the Zeedijk: A short-terms field research to the effects of the "Summerplan 1987" in the red light district.) Amsterdam: University of Amsterdam Press, 1988. 119 pp.

AUTHOR

Hans T. Verbraeck, Drs.
Freelance Ethnographer
University of Amsterdam
University of Utrecht
Erasmus University of Rotterdam
St. Janshovenstraat 192
3572 RC Utrecht
HOLLAND

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