

# Retrospect: Ethical Issues in Social Research

Team Trade

HQ 76

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1975

Holt Rinehart & Winston  
Publishing  
Chicago

Laud Humphreys

WHEN I BEGAN graduate work in sociology at Washington University in June of 1965, I had already acquired one graduate degree and ten years of experience as a parish priest in the Episcopal Church. Largely due to my intense involvement in the Civil Rights movement of the early 1960s, I found myself increasingly at odds with vestries and bishops and disenchanted with the church as an effective instrument for promoting the social change that I thought essential for dealing with the problems of individuals.

I was not long in discovering that sociologists have their own prejudices, a common one being a strong distaste for those who leave the ministry to build a career in the social sciences, the so-called "refugees from the parsonage." Whatever justification there may be for this anti-clerical bias, it involves one false stereotypical image, that of the clergyman as a naive, other-worldly individual who lacks knowledge of the streets. When my first graduate professor, in discussing a proposed research project, urged me to study

the gay bars of St. Louis, I took it as a serious challenge. "Get out of your sheltered tower," she advised. "Get out in the streets and get your hands dirty!"

In the twenty years since my mother's death, I had been "out in the streets" more than I cared to remember: first, as a delinquent kid in dust-bowl depression Oklahoma; then working my way through college as a newspaper reporter; later in mission work in Chicago slums and such "Wild West" towns as Cripple Creek, Colorado; finally, in the sit-ins and protest marches of the movement for black liberation. The suggestion that I engage in some rough ethnography I took as a challenge to my identity as a person and a nascent social scientist.

The early stages of this research were largely motivated by that original challenge. As tearoom behavior came into focus, however, the obstacles often seemed too great to overcome. I frequently considered changing my dissertation topic or even moving out of the deviance field into my second major area, medical sociology. However, events in the spring of 1966 assured the continuation of my research into impersonal sex: I was granted a predoctoral fellowship for research in homosexual lifestyles, and I was introduced to David, the necessary informant.

Chronicals of field research bear repeated testimony to the central role of a key informant. Both Whyte<sup>1</sup> and Liebow<sup>2</sup> tell us of the importance of Doc and Tally to their respective projects. I should like to provide similar praise for David, the informant who facilitated this research, but I fear that comments approaching a full disclosure of his vital role might reveal his identity.

The danger of exposing David to stigma illustrates a unique problem that plagues any account I attempt of my research. I would tell many tales of warmth and humor about the interaction between my respondents and myself. Some became lasting friends of my family, and my sense of the identity of nearly all of them goes far beyond a code number on an interview schedule. But now, as in writing the book, I must suppress the sort of anecdotal material that makes the "Appendix" of Whyte's *Street Corner Society* so

1. William Foote Whyte, *Street Corner Society*, (Chicago: University of Chicago Press, second edition, 1955).

2. Elliot Liebow, *Tally's Corner* (Boston: Little, Brown, 1967).

appealing. With much caution and removing all identifying tags, I told everything I could in the original manuscript. Each time I start to involve another person, to provide a human account of how a nameless research subject became a cooperating respondent, I risk exposing that person to stigmatization. Thus I return to talking about myself and my reaction to individuals who must remain faceless and anonymous, as if the impersonality of tearoom sex has to be duplicated in every description of those who participate in that furtive ritual.

Most reviews of *Tearoom Trade* did not surprise me a great deal. My greatest pleasure came from the consistently favorable reaction of the gay press, and the attacks from positivist devotees of survey research were expected. Von Hoffman's comparison of me with Attorney General Mitchell, odious as it was, gained some ironic value when I was later arrested, tried, and imprisoned under Mitchell's authority for destroying a photograph of Nixon during a draft board demonstration in 1970. But three reviews really stung by accusing me of lacking compassion for my respondents. Typical of these is one that appeared in *Issues in Criminology*, written by Barry Krisberg:<sup>3</sup>

One feels a tremendous sense of uneasiness while reading Humphreys' ethnography or, in fact, the ethnographies of other "hip sociologists" such as Howard Becker, Elliot Liebow, Ned Polsky, or the grand master, Erving Goffman. Perhaps the root of the dis-ease is that one senses that the sociologist appreciates his deviant subjects but is not compassionate with them. To be compassionate, it seems to me, requires that the researcher understands the fundamental passion or suffering of his subjects. This lack of sensitivity to human suffering leads the "hip sociologist" to ignore important dimensions of the social problem which he is describing.

After noting that the book won the C. Wright Mills Award of the Society for the Study of Social Problems, he continues:

It is not hard to imagine how Mills, the great radical sociologist, would have responded to such an irony. The moral relativism of the hip

3. Barry Krisberg, "Tearoom Trade," in *Issues in Criminology* (Winter, 1972).

ethnography would in Mills' terms be evidence of "moral cowardice." Compassionate analysis of the victim or consequences of social injustice seems central to Mills' radical perspective.

About the time Krisberg's review appeared in print, my second book, an analysis of the gay liberation movement, was published.<sup>4</sup> Krisberg and others who objected to the "clinical," "hip," or "dehumanized" nature of my tearoom sociology have not reviewed *Out of the Closets*, but I suspect some of them might find its style more to their liking. In that work, however, I was free to discuss "the fundamental passion or suffering" of my respondents, chiefly because they were overt gay activists whose identities I did not need to protect. In that study the liberation of my subjects liberated the researcher; whereas the anonymity and lack of commitment of the tearoom participants forced me to write of them in a cool, dispassionate manner that some readers interpreted as a lack of concern.

Given that my primary ethical concern in the study was to safeguard respondents from the many dangers that might have attended their exposure, I can only hope that those who read the book and this retrospect will understand the necessity for ellipses and cautious reporting on my part. If that means I must be labeled as "cool" or "hip," I find it preferable to being destructive.

### Ethical Criticisms

With these considerations in mind, I must direct the reader back to the first four chapters of the book for the bulk of what can and should be said regarding my research strategies. I shall concentrate here on some of the ethical questions that have been raised. In doing so, it is not my intention to counter all of the objections raised by von Hoffman, Warwick, and others, but I shall try to recall how ethical concerns appeared to me and how I dealt with them in the field.

As noted in the "Postscript" to the main text, I suffered little from doubts or hesitation about observing sexual acts in the tea-

4. Laud Humphreys, *Out of the Closets: The Sociology of Homosexual Liberation* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972).

rooms. These were, after all, public restrooms, and my role, a natural one in those settings, provided extra protection for the participants. In my nonsexual way, I was a true participant. Erikson's excellent "Comment on Disguised Observation" was published toward the end of my systematic observations but summarizes, after the fact, my feelings about this stage of the investigation:

Some of the richest material in the social sciences has been gathered by sociologists who were true participants in the group under study but who did not announce to other members that they were employing this opportunity to collect research data. Sociologists live careers in which they occasionally become patients, occasionally take jobs as steel workers or taxi drivers, and frequently find themselves in social settings where their trained eye begins to look for data even though their presence in the situation was not engineered for that purpose. It would be absurd, then, to insist as a point of ethics that sociologists should always introduce themselves as investigators everywhere they go and should inform every person who figures in their thinking exactly what their research is all about.<sup>5</sup>

Erikson appears to be referring primarily to research in those places of daily concourse where the social scientist plays a natural role not engineered for the purposes of his study. It does not seem an unfair extension, however, to follow the same principle in public settings where the sociologist steps into a natural role, particularly when that unobtrusive role is not central to the action observed. It may provide some insight into my lack of doubt about the ethics of my observations to know that I spent an hour discussing my research with Erikson at about the same time his paper on ethics was published. Neither of us recall that he raised any ethical objections at that point; we were both more interested in the nature and quality of the data being gathered.

Warwick makes an issue of my use of a hidden tape recorder for <sup>use of</sup> recording observations while seated in my automobile. He assumes <sup>a</sup> that "the point of this procedure was to avoid the possibility that those frequenting nearby tearooms would discover his true <sup>hidden</sup> identity." The truth of the matter is that I covered the recorder with a <sup>tape</sup> pasteboard box for two reasons: to avoid tempting thieves, and to <sup>record</sup> avoid giving passersby the impression that I was a policeman. My

5. Kai T. Erikson, "A Comment on Disguised Observation in Sociology," *Social Problems*, Vol. 14, No. 4 (Spring, 1967), p. 368.

reasoning was much more in the cops and robbers line than cloak and dagger. Although I used a less portable tape recorder on a number of occasions to interview fully informed and consenting respondents, I did not do this in my automobile. In addition, I find transcribing from tape (a task I could not entrust to others in this research) a terribly time-consuming operation. I tried using a notebook, and then a clipboard, in my car, but despaired of that method of note-taking after a couple of near accidents while making notes on the park drives. Actually, a number of the conspiratorial machinations attributed to me by reviewers were no more than mundane solutions to problems of research technique.

my  
plots

### Legal Problems

Much more serious are some of the legal objections to my field work. Along with a number of questions about the protection of my respondents, all of which I believe were well answered, the Chancellor of Washington University argued that I had committed numerous felonies in the course of my research. He was not successful in having my degree revoked on this basis, but he did terminate both my teaching contract and my participation in a research grant. By observing, perhaps facilitating, and failing to report some 200 acts of fellatio, was I not guilty as an accomplice to the acts? Stated in that form, the answer may appear obvious. Strangely enough, I don't think the question even occurred to me until late in my research.

In retrospect, I have tried to probe my conscience about this apparent disregard for the law. The only answer I have found, if not totally satisfying, is of some criminological interest. Some might call it a case of "improper socialization." There are many laws I have never had any intention of obeying, including most of those governing victimless "crimes" against "public order." When I conform, which is most of the time, it is only because I have no interest in committing a certain act or because I fear the possibility of being caught at it. I remember my terror when counselling at a summer camp in Colorado upon finding that I had violated the law by picking columbines to decorate the tables of the mess hall. The priest in

charge of the camp made me throw them all out for fear of being caught and bringing shame upon the institution.

To be raised in prohibition Oklahoma, where all the social drinkers in the church regularly voted for the sheriff who promised to crack down on bootleggers, is to be brought up in contempt of certain kinds of law. As a state legislator, my father was most diligent in promoting the passage of Sunday "blue laws." He also helped establish a law school in the attic of the State Capital so that blacks would not have to be admitted to the University. I am one of those people who have been officially "rehabilitated" by months in jail and years on probation, yet I still have utter contempt for a number of statutes on the law books.

If I did commit felonies in the course of my research, it never occurred to me that I was wrong in doing so. Had I been prosecuted and convicted, I would have been "wrong," at least in a sociological sense; but it would have been impossible for a prosecutor to prove that any particular act observed took place at any particular time and place between specified individuals. Now that the statute of limitation has run out, I can address this matter with some bravado.

My self-assurance fails, on the other hand, in the face of another objection raised by von Hoffman, Glazer, and Warwick. The latter expresses it with frightening directness: "This is one of the few social scientific studies which would have lent itself directly to a grand jury investigation." In the wake of front-page publicity, fostered by members of the administration and faculty at Washington University soon after the completion of this research, I am surprised that no such investigation followed. Even with the care I took to safeguard my data, I spent some weeks early in the summer of 1968 burning tapes, deleting passages from transcripts, and feeding material into a shredder. Memories of that time of panic have helped me understand, though not approve, the procedures of some White House conspirators in the Watergate cover-up.

As I pondered during sleepless nights what I would do to protect my respondents if called to court, my resolve was to plead the Fifth Amendment and risk contempt citations rather than reveal the identity of a single tearoom participant. There was no question in my mind that I would go to prison rather than betray the subjects

of my research. My lawyer, my advisers, and I spent some time discussing these matters, and I now realize they were not as placid as I was about the prospects. Since those days of uncertainty, however, I have spent three months of a Federal sentence in a county jail and am no longer so certain that I could have withstood the pressures of the criminal justice system.

There was little danger either to researcher or respondents in the observation of sexual encounters between individuals whose identity was unknown. For those subjects who knew the nature of my research and freely consented to participate in it, the ethical and legal responsibilities were at least shared and reciprocal, though no less pressing on the researcher in protecting the confidentiality of his data.

With these considerations in mind, I am forced to agree with my critics regarding that part of my study in which I traced license numbers and interviewed respondents in their homes. At the time, although troubled and cautious about my research strategies, I justified them in much the same terms as I have outlined on p. 42. It seemed that I was interviewing subjects in the least disturbing and least dangerous manner possible. I now think my reasoning was faulty and that my respondents were placed in greater danger than seemed plausible at the time.

I know when my change in thinking on this matter took place. Two events occurred while I was an inmate of Albany County Jail in the spring and early summer of 1972. The first mail I received in prison was a copy of Glazer's *The Research Adventure*, giving me the opportunity to read and reflect upon it over many hours. (Later, I passed this book around to fellow inmates and a few guards as part of my policy of informing everyone at the jail of the sort of research they might expect of me while incarcerated.)

The second event brought home some dangers of research. I was called out to the visiting room one afternoon for a conference with two inspectors from the New York State Police. They had learned (probably from students in my course in Field Research Methods who were policemen on leave) that one of my students was doing research in certain bars where Albany's drug and prostitute traffic was believed to center. As the result of aggressive reporting in a local newspaper, a Grand Jury had been called to investigate these

establishments. The inspectors wanted to know the identity of my student, along with any notes or recollections I might have of his findings. Fortunately, I had no notes and only a faulty memory, but I could not avoid identifying the student. He had a bad memory, too, I suppose, for he was never called to testify.

Since then, although I remain convinced that it is ethical to observe interaction in public places and to interview willing and informed respondents, I direct my students to inform research subjects before interviewing them. Were I to repeat the tearoom study, I would spend another year or so in cultivating and expanding the category of willing respondents into which the "intensive dozen" fall. Perhaps the sample of participants would not be as representative as in the original study, if I were limited to these methods, but the richness of data gained would certainly surpass those obtained from the interview schedules. Perhaps by these means I could discover, as Rainwater urges in his Foreword to this book, more about "the personal significance to the participants of their homosexual behavior."

Years have passed since I studied the tearoom encounters and those who enact a hidden portion of their lives in them. There is no reason to believe that any research subjects have suffered because of my efforts, or that the resultant demystification of impersonal sex has harmed society. The scandal called Watergate has reached its glorious climax, perhaps leaving the nation somewhat humbled. Among the lessons we should have learned is the need for each of us to make frequent ethical self-examinations—and this applies to research as well as to politics and business. But we also need to learn the danger of secrecy and of maintaining illusions. Warwick states that "men in every age—including the social scientists of the present—need illusions to be free." I shall always disagree with that romantic statement. We are in desperate need of knowledge to be free.

Somewhat chastened by the controversy surrounding my study of the tearoom trade, I remain proud of the work done. I often wish other sociologists would give more attention to some of my substantive findings that I believe provide an increment of understanding of social behavior in our society. At the same time, I realize that serving as a focus for ethical debate is no mean contribution. Above

all, I know that this study is increasingly cited by attorneys seeking acquittal for clients arrested in public restrooms. What began as a relatively uncomplicated ethnography of the gay community grew in complexity as the logic of research took hold, and ended as much in a quest for justice as for knowledge alone.

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