

THE EMPEROR'S NEW CLOTHES: THEORY AND METHOD IN GANG FIELD RESEARCH

John M. Hagedorn, University of Illinois - Chicago

INTRODUCTION

In the last few years there has been a resurgence of field research on gangs. In New York, Los Angeles, San Francisco, Detroit, St. Louis, and elsewhere social scientists have gone "back to the field" to study gangs, drugs, and violence. This has been a welcome development. However, the recent profusion of field research has also brought to light a few problems.

This paper intends to initiate an academic discussion about the representativeness and validity of data in modern gang studies.¹ I think we need to ask two rudimentary questions of every study: 1) how representative are their samples? and 2) how have the researchers separated lies and "hype" from more valid and reliable data? Let me briefly explain how our research in Milwaukee has prompted me to ask these questions.

In my early research (1988) I learned what Thrasher (1963) meant in saying "no two gangs are just alike." Milwaukee's gangs vary by neighborhood, ethnicity, age, and gender and are quite different from gangs in other cities (Moore 1988). But while each gang may be different, my current research (1994a) has taught me there also are differences *within* each gang. Some studies I've read interview only one or two members of a gang, and we're not told why the researchers think their respondents are representative of all or most gang members. If there is substantial variation within a gang, selectivity in sampling may strongly influence the findings and distort any theoretical conclusions based on those findings.

Further, even if a sample is representative, gang members simply don't always tell the truth. Yablonsky (1966) warned long ago that to gang members "every researcher could be a 'cop.'" I've learned that gang members, like everyone else, present "accounts" of themselves which project an image they wish to maintain in the eyes of outsiders (Campbell 1984). Gang members manage their appearances to researchers and few of us have reported on how we have seen through such "presentational" data (Goffman 1959).

These methodological problems have theoretical consequences. Could some

studies which conclude that gang members are strongly committed either to deviant or to conventional norms be based on interviews with unrepresentative outliers? Might some proponents of cultural deviance theories have interviewed or observed only atypical gang members who were "loco," violence-prone, or drug-crazed? Might some strain theorists have taken at face value self-serving comments from gang members or "wannabes" who were just trying to "look good?"

These issues are also crucial if the new nineties gang research is to be used for humane policy ends. I am particularly concerned with research that paints gang members as "hopelessly deviant" and thus provides a justification for right wing policies of increased incarceration. But I also cannot neglect critiquing studies, like my own, which see gang members as basically conventional, and may be incorrectly minimizing the gang problem.

Many of the nineties gang field studies are important contributions to the literature. However, until we examine the methodology of these studies we cannot be convinced of the validity of their findings or theoretical conclusions. Without such a methodological critique, our appreciation of any these studies may be no more than sociologists admiring the emperor's new clothes. This article examines how the findings and theoretical conclusions of contemporary gang research—including my own—have been influenced by selectivity and how gang members distort information. In conclusion, I discuss some political and ethical consequences of our research.

SELECTIVITY IN SAMPLING

The truism that "gangs vary" is as old as gang research itself. Variation between gangs in different cities and between gangs within a city have long been acknowledged (Cloward, Ohlin 1960; Moore's introduction to Hagedorn 1988; Spergel 1964). Gangs also vary by ethnic group (Spergel 1989) and by gender (Campbell 1990).² Specific gangs may also become more or less violent over time (Moore 1993). Klein (1995) has forcefully reminded both the social science and law enforcement communities that not all gangs deal drugs or are violent.

There are only a few studies which have overlooked variation between gangs. However, I think researchers today who find no between-gang variation have a sociological burden of proof. Attentiveness to variation becomes even more theoretically relevant when we look at differences within a gang.

The "gang" in research is often seen as a monolithic entity. Observations or interviews with one or a few gang members are sometimes assumed to be descriptive of the gang as a whole. Since accessing street gangs is quite difficult, often studies only interview those gang members who are willing to be interviewed and ignore the implications of such selectivity (Moore 1978; Whyte 1943). Our study (Hagedorn 1994a) has demonstrated that even among core members of gangs, there are both conventional and deviant lifestyles as well as different orientations toward the future (Vigil 1988). We found most adult gang members ("homeboys," "legits," and most "dope fiends") had a conventional orientation with a varied work history, while only a minority of the gang, some "dope fiends" and the "new jacks," eschewed work and glorified violence.

I suspect some studies which portray gang members as adhering to deviant or violent norms may have sampled only new jacks or other outliers. Other studies which have downplayed organization or violent behavior may have interviewed only legit or less involved "wannabes." Ignoring variation within the gang can distort our understanding of the extent to which gang members are committed to gang norms or whether they aspire to mainstream American cultural goals, as suggested by strain theory (Cloward, Ohlin 1960).

For example, Martin Sanchez Jankowski (1991) bases his cultural deviance model on his conclusion that gang members as a whole are "hard nuts" with "defiant individual" personality traits quite different from other residents in their neighborhoods. His respondents appear to have similar hard-nosed outlooks and lifestyles. They appear unlike the complicated and conflicted people usually found in field work (Becker 1970) as well as the respondents in our Milwaukee study. While admittedly Jankowski's interest was in analyzing the gang as a unit, he virtually ignores within-gang variation. This may have led Jankowski to mistakenly attribute the personal characteristics of some gang members to the entire gang.

Sanders (1994) also neglects variation

within the core membership of the gang and finds core gang members are *defined* by their adherence to violent subcultural norms. Sanders spent ten years riding with police gang squads investigating drive-by shootings. His interviews with gang members involved with homicides gives a chilling, but perhaps not representative, picture of amorality. For example, he reports that "gang members"—apparently as a whole—"lack remorse" for the accidental killing of babies in drive-by shootings (Sanders 1994). Surely this finding would shock most veteran field researchers. In our own study, the vast majority of gang members we interviewed reacted with deep remorse to incidents of tragedy and death within their communities. Sanders does not quote anyone who felt remorse, and we don't know if the gang amorality he found is typical of San Diego gang members. Might Sanders' method of access and homicide-based sample have influenced his findings?³

Finally, in a much quoted study, Skolnick (1990) interviewed 39 inmates in a one-shot, one hour interview. He concluded from this study that northern California African American gang members were part of "instrumental" gangs, similar to organized crime, while southern California Latino gangs were more "cultural," or neighborhood based. Were Skolnick's respondents representative of gang drug dealers of each ethnic group? Skolnick interviewed only those members of a gang who had been arrested, convicted, and sentenced to prison for committing crimes and then were picked out for an interview by correctional officials. Might his prison-based, prison-guard-selected sample have contributed to his rather sweeping findings?⁴

The problem with all of these studies, as well as many more from contemporary gang research,⁵ is selectivity. We don't know how the respondents who were interviewed or quoted differ from others within their gang or how prevalent the cited views are within their gang. Could the methods of selection in these studies have led these researchers to mistakenly conclude that gang members are deeply committed to criminal or violent norms? Might more attention to sampling within gangs have led these researchers to different conclusions?

Sampling Strategies

One well respected and often used sampling strategy in gang field research is the "snowball sample" (Biernacki, Waldorf 1981).

A snowball sample can be constructed by asking one respondent to refer a second, and so on. This is normally done when "there are no known lists or populations from which respondents could reasonably be selected" (Lauderback, Hansen, Waldorf 1992). However, while gangs may be "hidden populations" like drug users, burglars, or the homeless, they do have a definite, if diffuse, social organization with loosely definable populations which can be sampled. This makes snowball sampling a strategy of second choice, and a choice that presents its own problems in selection.

Decker and Van Winkle's (1994, 1996) study is a good example. Through a snowball sample they interviewed 99 active gang members from 29 gangs with respondents varying in age from 13 to 29 years. Unfortunately we do not know how responses differed by age, a reasonable question for a sample with such a wide age range. Decker and Van Winkle do not tell us why they believed those interviewed were representative of all gang members, or members of any specific gang. They did corroborate gang membership by field observation, but that still begs the question of representativeness.

A snowball sample is also only as good as the gatekeeper or the key link on the chain (Whyte 1943). If the chain referral begins with non-gang members, like Decker and Van Winkle's (1996), or with community agencies, like Fagan's (1990), we may have problems with selectivity. This problem then compounds itself as initial respondents refer researchers along "chains" which may also be unrepresentative.

How can a researcher be sure those contacted by snowball are active gang members and not "wannabes" who might puff themselves up, ex-members with an axe to grind, or non-gang hustlers out for a buck? In Biernacki and Waldorf's (1981) snowball method, theoretically the researcher exercises increasing control over the referrals, deliberately searching for representativeness. Respondents also could be asked if they could refer to the researcher someone else within the gang who is "different" than the respondent in some key aspect (Guba, Lincoln 1989). I have not read many instances, however, where gang researchers utilized these techniques. Our Milwaukee research has found that referrals from non-gang sources tend to produce a disproportionate number of respondents who

are on the fringes, not the core of the gang (Medico 1995).

Sampling from Rosters

Another method of sampling is based on constructing rosters of gang members. Whyte's (1943) study was with a known population of a single Cornerville gang as was Padilla's (1992) in Chicago. Short and Strodbeck's (1965) classic test of cultural deviance and strain theories worked from rosters of Chicago gangs. Joan Moore (1978) randomly sampled from age-graded rosters of multiple "klikas" from gangs in East Los Angeles. Both our Milwaukee studies were based on sampling from gang rosters. All of these studies, it should be pointed out, have found variation within the gang and shifting membership rosters. Each of them also found conventional success goals were important to gang members and questioned the validity of cultural deviance theories. So how is a roster compiled and maintained?

Few gangs issue membership cards or keep written rosters and police lists of members of a gang are notoriously inaccurate (Klein 1971). In our 1988 research we simply asked those interviewed to list the street names of all those people who were present when the gang took a name. We wanted only those "core" members who hung out everyday, not hangers-on who came and went. The first respondents were gang members who I had either hired or had worked with me for several years in my capacity as a gang intervention program director. I knew from years of work with these respondents that they were original or founding members. The rosters they compiled were then checked with each successive respondent, who was asked whether there was anyone left off the list or anyone listed who wasn't really a member. The respondent was asked whether each of the members was working, had been to prison, and how involved each member still was with the gang.

In our initial study we interviewed only two or three people from each gang, mainly leaders. We developed an accurate roster from the gang my collaborator, Perry Macon, belonged to, and from a few others. However, some of the other rosters proved to be incomplete. In the current follow-up research, all our staff were founding members of their gangs and they interviewed all or nearly all of the members on their own gang's roster.⁹

Problems of Sampling from Rosters

Unfortunately, drawing up a roster does not, in itself, guarantee representativeness. For one thing, gang members vary by age-group. Moore (1978, 1991) solved this problem by randomly sampling all known "klikas" or age-groups of East Los Angeles gangs. In Milwaukee, where gangs formed in 1980s, we interviewed only members from the *founding* group of Milwaukee gangs, those men and women of roughly the same age who were present when the gang first took a name. Therefore we can't be sure the Milwaukee founding group is representative of succeeding groups of gang members. For example, the founders may be more enterprising or daring than gang members who later joined an established gang. Core members, like our founders, also differ from peripheral members or wannabes (Vigil 1988). On the other hand, our strategy was to learn as much as possible about one age-group of gang members over time in order to develop "working hypotheses" about gang drug selling (Lincoln, Guba 1985). For this purpose, our sample worked well.

I have spent quite some time criticizing how others have sampled, and it would only be fair to subject our own work to the same scrutiny. While most of our rosters were confirmed by each of our respondents, some gang members never did agree on who was properly a member, though the disputed list was quite small. Sometimes "gang members" were improperly included on the roster so our community researcher could interview them and pay them the \$50 fee. We weren't always successful in developing satisfactory rosters for gangs from which we had not hired staff.

Once a roster was drawn up, even more problems ensued. While in three of the gangs we interviewed at least 90 percent of the original founding gang members, in two other gangs, we were able to interview only about a third (15 of 35 founding members and 11 of 36). Anyone who does gang research knows that getting interviews is not always easy. We didn't always succeed either and that means we had to be alert for problems of selectivity.

In one of these two gangs where we didn't interview everyone, we limited the number of interviews ourselves due to constraints of time and after getting a detailed picture of the gang drug business, our main objective. While we can't be sure our sample was representative, no one refused to be interviewed. But in the other gang we did run

into problems of some members refusing to be interviewed. Refusals may introduce systematic bias, perhaps hiding involvement in drug use, violent behavior, or gang members who have gone "legit" and do not want to dig up their past.

Refusals in this one case turned out to be a staff related problem. The staff person for the gang where we had many refusals was a "legit" (Hagedorn 1994a) - someone who had been a member of the gang about the time of its founding, but had moved away, held a job as a security guard for a while, and had gone on to college. He and his family were well known and had been well respected among the gang, but his upward mobility led to tensions with the gang. We generally had the most success getting good interviews when our homeboy interviewers had been to prison or had once been a "new jack" themselves. These interviewers were more trusted by their homeboys than legit's or someone who had never done time.

Our legit was not trusted by some within the gang and his first interviews were with more deviant "dope fiends" who mainly wanted the interview fee. Those interviews contained little information and lots of lying. Further interviews came slowly. Our staff member felt discouraged and panic set in that maybe we weren't going to be able to get the interviews we needed. At that point the gang looked to us like an anomic association of cocaine addicted dead beats.

We solved this problem by continuing to interview. We did get several more interviews of high quality over the next few months, but still we had not located most members of the gang. The gang, however, then began to look more like other gangs we were researching. We looked back over those we interviewed and made sure we had interviewed some who were involved with drug sales and some who were not, as well as some who had actively sold dope and some who had quit the dope game. We ended up interviewing about a third of the entire roster, and fortunately we discovered we had interviewed "homeboys," "dope fiends," "legits," as well as "new jacks." Had we generalized about the entire gang from our initial interviews, we would have received a much different picture of the gang than the one we patched together after all the interviews were done. Still, we can't say that our interviews in this gang were "representative."

To summarize: gang research needs to

avoid the convenience sample, where gang members are interviewed and observed not because of representativeness, but because of their availability or their uniqueness. *Our research suggests that interviewing everyone on the roster, or sampling rosters on theoretical grounds, would, in most cases, find gang members who resemble middle class Americans more than underclass demons*. But we have too few studies of the entire membership of individual gangs to confidently generalize. I suspect many findings of extreme deviance of gang members are tied up with issues of selectivity which interact with a researcher's prior theoretical assumptions.

THE PRESENTATIONAL ACCOUNTS OF GANG MEMBERS

Representativeness is not the only problem in gang research. We also have the problem of validity. What kind of information are we getting when we interview a gang member? Campbell points out there are many "realities" from which gang members, like all of us, "present" to others.

Social interaction is a creative process in which we select to present ourselves as a particular type of person and then offer accounts of our actions which support that view of ourselves... So the accounts the girls give are likely to be a function of their conceptions of themselves and the persons they wanted to present to me. In that sense, everything they say is true. Sometimes however, the facts may have been altered. (Campbell 1984)

An important issue for research is to understand when "the facts have been altered" and when they are reported accurately. Otherwise we report whatever our respondents have to tell us without any context or regard to validity or reliability. Their "accounts" may tell us something about the individual respondents, but little about the social reality in which those individuals are embedded. This is an important consideration in all research, but particularly today when the media has discovered the new and scary role of "gang member" (Ice-T 1994). The violent, drug-selling "gangsta" has attained "a special place in the commercially organized fantasies of the nation" (Goffman 1959).

I question whether cultural deviance theories of gangs may be based on data drawn from gang members' hyped-up

"presentational" accounts of their lives as "gangstas" (Shakur 1993). But the rigors of social science also compel me to explore the opposite possibility: could strain theories of gangs, which stress the adherence of gang members to common American cultural goals, be based on "accounts" which emphasize gang members' conventional side and minimize deviance? How can we methodologically differentiate "presentational" from "operational" data (Van Maanen 1979) and untangle its effects on our research?

"Interviewees," Oakley (1981) reminds us, "are people with a considerable potential for sabotaging the attempt to research them." Use of "hype" is a common strategy for gang members, particularly younger kids, who want to build themselves up to an outsider. Taylor (1989) reports that members of "scavenger" gangs were "very talkative and some were great braggarts." The problem is made worse when interviews are done in a group where such braggadocio can be contagious as gang members seek to impress one another as well as the interviewer. Younger people also tend to brag more than older ones (Waldorf 1993).⁹

The less familiar the interviewer is with the respondent, the greater opportunity exists for the respondent to exaggerate or to produce an "account" which creates a role of "gang member" to match the "background expectations" of the researcher (Hyman 1954; Scott, Lyman 1968). Thus studies which seek out an interview with a gang member and then never see him or her again are particularly susceptible to "hype." Conclusions drawn from such interviews could mistakenly find gang members are firmly committed to deviant gang norms and paint an exaggerated picture of a gang.

For example, is this respondent interviewed by Carl Taylor (1989) giving his real feelings, or playing a role for the interview?

I likes to bust heads. Violence? What's that? [laughing] You got to dog everybody or they gonna dog you. Doggin' is my speciality... I'll just see someone and start doggin' them in the street

On the other hand, why should we believe Jankowski's (1991) class conscious respondents, like this one, who uniformly claim their gang "helps the community."

Me and the guys in our group we try to help the

community because that's the only way that all of us can protect ourselves from those rich bastards!

It is extremely difficult for outsiders to cut through hype or lies in gang interviews. Even sincerity is a poor test. As Goffman (1959) points out, an actor's performance must be sincere to be successful. In most one-shot gang interviews the respondent has an "account" to present which he can freely develop at the time of the interview, unconstrained from past experience with the interviewer or the anticipation of future contact. How can any sensible theoretical conclusions be drawn from such presentational data?

In our study, through peer interviewers, we tried to capture the individual perspective of gang members looking back over their lives reflectively. The accounts we wanted were not those which might be given to outsiders, but rather "insider" information, "operational data" concerning how the drug game worked and how it affected the life of the respondent. We wanted to go "back stage," to listen to how friends talk with one another privately, where "suppressed facts make an appearance" (Goffman 1959). While such data are themselves accounts, and not some objective reality, we believed these insider accounts would yield an insightful picture of the drug game minus the hype often given to outsiders.

Hype was a minor problem in our interviews mainly because we knew the people we were interviewing. When the respondents tried to play a "gangsta role" to their homeboy interviewer, it was considered crass and was confronted. We did our interviews privately, one on one, with the express purpose of allowing the respondent to talk about his/her life and reflect upon it. Vigil and Long (1990) explain that in certain situations gang members drop their "cholo" front, and this was our expressed intention. The interview situation was not set up to be a chance to brag about exploits as a gang member but, as much as possible, as a talk between friends.

Sometimes hype is the result of a strong belief in the mythology of the gang. There are certain norms that some gang members hold that they may not want to admit are broken and may even deny to themselves. They want their "account" of the gang to reflect the myths they firmly believe. Consider the question of snitching. While most of our respondents discussed instances of snitching, some denied

any such thing ever took place within the gang. In discussing a practice which strongly violates gang norms, some respondents would suddenly switch their identity from "friend" of the interviewer to the stereotypical role of "gangsta":

Q: Have any of your gang ever snitched on you or anyone else in the gang?

#44. No! That was against the rules

What is remarkable about this answer is that the interviewer and respondent were from the same gang. Several years back the interviewer himself had been snitched on by a fellow gang member and went to prison. This incident was well known by everyone in the gang and it caused serious repercussions within the gang, a tightly knit group of friends. The respondent was well aware of the incident but allegiance to mythical gang norms forced him to deny it even to a friend of his - the interviewer - who was the one who had been snitched on! How would an outsider have figured out that this respondent was lying?

On the other extreme, respondents may also lie or give socially acceptable answers to outsiders on some questions on difficult topics. Hyman (1954) pointed out that questions of extreme sensitivity like those asked about communist sympathies in a "period of public fear" like the McCarthy era were most likely to be distorted to researchers. Getting valid data on drug use and gang activities in today's "period of public fear" about gangs and drugs is a rather good analogy to doing research during the 1950s red scare. Denial of involvement in drug dealing is a common response in gang research today (Taylor 1989; Waldorf 1993).

It happened to us too. While we got detailed descriptions of drug sales by the gangs of our staff, we were often lied to by respondents from other gangs. We interviewed two members of one gang which had come to dominate one area's drug business in the last few years. The first interview, with the leader, was a disaster. Although we knew he ran the gang, which was no more than a drug business, he steadfastly denied any involvement with drugs:

Q: How has selling dope changed from when the gang started selling it?

#22: What gang? Far as gangs, period? Oh, you know, they got the money, bigger guns, more cars, posse members trying to get deeper, you know how that goes. Like I said, I just say no to drugs.

We knew this was nonsense. When our interviewer was selling drugs several years before in the same neighborhood, he had become friends with this respondent, who was a major dealer in the area. Still, this respondent did not want to admit on tape of his involvement with drug selling. We then followed up that interview with another member of the same gang who gave us detailed information on the gang's lucrative drug trade.

The issue here is that reporting information from this respondent or the one who said snitching was "against the rules" tells us something about the individual gang member, but gives a false picture of reality on those issues. We used Whyte and Deans's (1969) first check in detecting distortion - "implausibility." Our community researchers, by virtue of their prior gang status, simply *knew* what was a plausible story and what was not. Then, by interviewing others within the gang, they could get beneath the "presentational" account and reconstruct the "real story" - or at least the "real story" as it is presented among intimates.

Problems of Peer Interviews

Hype may be the main threat to validity in most gang research, but minimizing deviant behavior was the main problem in our study. While cultural deviance theorists need to look at how their methods may have led to their conclusions, those of us who see gang members as more conventional than deviant also need to look at how we capture data.

Other research has shown that minority populations in general have less trust in research than whites. Researchers must be concerned that "respondents will provide socially desirable responses or will not be able to provide accurate retrospective reports of behavior" (Collins 1992). While we thought interviewing by persons very familiar with the respondent would counter the tendency to provide socially desirable responses, the strategy at times backfired. While almost 90 percent of the interviews were scored by our community researchers as "truthful" or "mostly truthful" there were several problems.

First were three concerns about the

nature of the interaction between the interviewers and the interviewed. Our community researchers interviewed all the members of their own gang, meaning any interviewer effects would be constant across each gang (Hyman 1954). For example, one interviewer was involved in acts of gang violence and consistently failed to question his homeboys about those acts. Another community researcher was discharged when his interviews were few and poor in quality. His homeboys didn't trust him enough to do an interview and we never did get a good picture of that gang.

A second problem was that our community researchers had a certain status within their gangs, and had a long history with each of the respondents. Where relationships were strained, the interview suffered. More significantly, the respondents reacted to the gang status of the community researcher, and perhaps did not share details that might compromise that relationship. In one extreme case, a community researcher's current husband had been previously married to another homegirl. This created some rather touchy problems in the interview.

Third, the "community researcher" role adopted by our interviewers led to some jealousy from peer respondents, who would have loved to have the job themselves. The community researcher's "advocacy" role also may have led some respondents to want to give their "advocate" what he or she wanted, rather than give out the unvarnished truth. For example, one of our interviewers was very active in finding jobs for respondents and some people may have sought out an interview in the expectation of getting work. They would not have wanted to offend or disappoint their link to a job.

It is important to understand the "accounts" our homeboys and homegirls received as the voices of gang members talking to their friends, as opposed to talking to outsiders. This tight relationship between interviewer and interviewed gives our study its uniqueness. But just as you, the reader, are unlikely to be perfectly frank about all aspects of your lives to every friend, that's also the case for gang members. Accounts varied among gang members and even within an interview.

Gang research needs to learn the lessons of feminist methodology which has been exploring variation in accounts by reporting "polyphonically" (Cancian 1992) all the voices they hear, not just the ones that agree with the

researcher. In anthropology as well there are increasing concerns with how research may report stereotypes or average frequencies, rather than a more complex reality. Hopper (1995) examines several ways that respondents distort information to ethnographers, even when the ethnographer is well aware of a different reality.

In some cases, outsiders might actually get more information than insiders. For example, many of the gang members I interviewed revealed differences within the gang and criticisms of leaders that might not have come out in peer interviews (1994b). Some respondents confessed beliefs and actions to me they might not have shared with homeboys. I also knew what I was looking for theoretically, and could probe on certain questions where peer interviewers might not. On the other hand, my interviews were not shared reminiscences between friends, and certainly lost important detail. Having a variety of interviewers would probably improve validity.

Minimizing Deviance

Overall, both in interviews by peers and by myself, respondents clearly stressed conventional aspirations. It is not clear if those same sentiments would have emerged in interviews given to strangers. The conventional orientation we uncovered cannot be entirely ruled out as an effect of the collaborative method.

For example, respondents consistently minimized or even denied cocaine use. I discovered this as I interviewed the members of one gang, all of whom I had known for ten years. As I did each interview, the respondent claimed his own drug use was minimal, but others were heavy users. This bothered me and concerned me even more when I interviewed a celebrated "dope fiend" of the gang and asked him about the prior respondent.

Q: Was Bob using heavily that year too?

#33. Yeah, we all was smoking ... heavy

Perhaps since I was a non-gang outsider, some respondents did not want to admit heavy use to me. A few other respondents from other gangs had claimed light or moderate usage when I knew they were crackheads and they knew I knew. But surprisingly, community researchers also reported their own homeboys minimized cocaine use in their interviews.

Why?

Several explanations were explored by staff. Among Latinos, who were still heavy into the gang, drug use, if known, could get them "violated." Respondents may have been unsure whether our staff, some of whom once had "rank" within their gangs, might report them to the current "chiefs." Cocaine use, unlike heroin use in Moore's study of East Los Angeles, was relatively new in Milwaukee. Norms did not exist which recognized and legitimized its use.

The order of the interview questions also contributed to some respondents downplaying their drug use. The first hour of the interview concerned the operation of the drug business and the reflections of the respondents over what happened to them and the gang over the last five years. By the time we got to the section where respondents report their drug use, the interview was already two hours old. Even our interviewers complained by then they were "tired out."

The basic problem, however, was how the interview situation itself led to gang members minimizing drug use. Our two to three hour taped interview focused on the adult gang experience and respondents were asked to detail their involvement in drug dealing. In most cases they were looking back at a life they had left or wanted to leave. They often spoke deprecatingly of the dope fiends who had bought their drugs. To their minds, the role of "heavy drug user" was inconsistent with their more "manly" role of "drug dealer." To be seen, even by their homeboys, as a heavy drug user was considered "shameful." Gang members saw their drug use as demonstrating weakness, as this African-American respondent explains:

#90: As I see it it's a mind game, and if you ain't a strong person it's (cocaine) the wrong thing to mess with

Their reports consistently minimized drug use, even when the interviewer knew better. Our male respondents simply did not want to admit to their homeboy interviewers or to me that they were not "strong." This pattern was repeated by female respondents, who similarly did not want to be stigmatized as "dope fiends" by their homegirls or those listening to the tape.

There is some confirmation in the literature that familiarity with an interviewer may

contribute to under-reporting drug use. In the 1984 National Longitudinal Survey of Youth, Mensch and Kandel (1988) found that when follow-up interviews were conducted by the same interviewer, respondents under-reported drug use as opposed to those who were interviewed by a different interviewer. They concluded:

We speculate that interviewer familiarity increases salience of normative standards and that participants respond not only in terms of their past familiarity but also in terms of their subjective expectations regarding the probability of a future encounter with the interviewer. (Mensch, Kandel 1988)

If we ask ourselves whether our collaborative method may have influenced our findings, we have to answer "yes." Our research prompted gang members to reflect on their lives and that process itself may have encouraged more conventional judgments. Some deviance, particularly if it cast the respondent in a poor light, was minimized to peer interviewers.

On the other hand, our conclusions concerning the conventional orientation of gang members were based on many questions in the interview and on harder data like work and family histories (Hagedorn 1994a). In fact it was our collaborative method which allowed us to detect distortion and report it to you along with the rest of our findings. Adherents of cultural deviance models need to similarly examine how their methods may have contributed to the nature of their findings.

DISCUSSION

The use of multiple methods (Webb, Campbell, Schwartz, Sechrest 1966) is still the best way to "triangulate" and verify data. Our own study utilized official statistics, interviews with gang members, surveys of a probability sample of neighborhood residents, interviews with "old heads" from those neighborhoods, the life experiences of our staff, and participant observation by the author in various drug selling locales. All these methods have contributed to our emerging interest in strain theory.

Some researchers who have found deviance where we've found conventionality may be working from biased samples or unsuspectingly reporting "hype" as data. When researchers sample from rosters and understand some of their data as "presentational,"

I believe they will find that most gang members are not socio-paths, but a lot like the rest of us. Confronting selectivity and the presentational aspects of our own data has been a rewarding and healthy process. Today's gang researchers must more seriously examine how their methods have influenced their findings and report it to us. Otherwise, we are in danger of engaging in theoretical debates which are little more than fashion writing about the emperor's new clothes.

I am convinced of the overall utility of our collaborative methods, despite a tendency of our respondents to minimize some types of deviance. Collaborative research is designed to describe the reality of the streets from a back stage perspective which is seldom attained by outsiders, even if they may live among the "natives" for a spell. By involving gang members in every facet of the research, and using representative sampling, the entire gang can be described, including conventional and deviant aspects. Thus while our study argues that most gang members have a conventional orientation, we avoid romanticizing gang life.

These issues are not abstract and significant only for theory - they also have public policy implications. There are at least two mainstream political agendas on gangs which have their own distorted "official definitions" (Mills 1959) of the reality of gang life. Politicians pick and chose from research to buttress their own agendas.

The main policy thrust toward gangs, the law enforcement agenda, wants justification for its war on drugs and build-up of the criminal justice system. It supports and encourages research which presents gang members as especially violent, imperialistic drug dealers, or as purely evil underclass villains (Reeves, Campbell 1994). Law enforcement officials embrace field research whose findings can lend credibility to their self-interested notions of the "threat" posed by gangs. Cultural deviance theory is especially suited to be used by politicians to demonize gang members and make it seem that the only solution is more police and more prisons.

On the other hand, some researchers who are looking to counter this law enforcement juggernaut tend to underplay the organization of drug-dealing, violence, and other ugliness of gang life. William Julius Wilson (1987) pointed out that liberal sociology has suffered a "confused and defensive" reaction to the pathology of the ghetto. One such reaction

may be to selectively look for field data which dispute talk-show stereotypes and to report such findings, even though the data may be questionable. I believe such presentational data, like the gang leader in our interviews who "said no to drugs," are not ultimately convincing and their use will backfire.⁵

While this discussion of our research methods has theoretical and political aspects, there are also ethical reasons for paying attention to representativeness and selectivity. Susan Sontag (1973) has said that "to photograph is to appropriate the thing photographed." This is also true of academic descriptions of gang members. As one of our community researchers put it, distorted studies "do violence" to the lives of our respondents. They stigmatize and label gang members and impose on them an outside definition of their lives. Improper labels, like "sociopath," are not only used by authorities, but may be destructively embraced by some of the labeled (Lemert 1967). Portraying gang members as helpless victims is similarly demeaning, takes away agency, and paints a false and unbelievable picture of gangs in poor communities.

We should never forget that what we as sociologists write has consequences. We must exercise extreme care that the new theoretical realities we construct do not contribute to the dehumanization of a segment of the American people.

END NOTES

Reliability and validity are old problems in gang research. Here is Spergel's candid admission on his classic research thirty years ago:

It should be clearly understood that the research was neither rigorously designed nor executed. While much care was exercised in the collection and processing of data, there were no systematic checks on validity or reliability. (Spergel 1964)

There are few current discussions of gang member veracity in the literature. Waldorf (1993) raises the issue briefly in the Final Report of his San Francisco gang study. Also see Vigil and Long (1990) on emic and etic perspectives in anthropological research.

² Some still forget female gangs. For example Jankowski claims to be studying the "gang problem in general" but he omits any discussion of female gangs (Chesney-Lind 1993). Jankowski's overall methodology has also come under severe attack (Fagan 1993; Klein 1992; Sullivan 1994).

³ Sanders' study is informed by Goffman's work, but he apparently neglected Goffman's rather categorical advice to field researchers:

There's no way in which, if you're dealing with a lower group, you can start from a higher group... You can't move down a social system. You can only move up a social system. So, if you've got to be with a range of people, be with the lowest people first. (Goffman 1989)

⁴ Skolnick's defense that his interviews with both inmates and correctional officials were consistent confounds reliability with validity. That everyone gave roughly the same story should raise suspicion or at the least prompt a negative case analysis or use of multiple methods (Lincoln, Guba 1985; Webb et al 1966). For other problems of gang interviews in prison, see Moore 1993, Hagedorn 1990, and Decker and Van Winkle 1994.

⁵ For example Daniel Monti (1994) conducted interviews with "approximately 400" students in a suburban school district. Many - but we don't know how many - of these were selected by the principal of each school and interviews took place in the school office. In at least one school, administrators were present during the interview. Monti simply abandoned any notion of scientific sampling - and confidentiality, for that matter.

⁶ Our original 1988 rosters were revised by staff and cross-checked with each person interviewed until we were confident of their accuracy. In the first study, N= 19 gangs and 260 members. For the second study we were not satisfied with updated rosters for three gangs and we added rosters from two other gangs which were not included in the 1987 study but had special characteristics which made them important to include. We thus tracked 18 gangs with 296 founding members.

⁷ Some studies regrettably do not report the age of those interviewed (Lauderback et al 1992) and this can have distorting effects. For example, a sixteen-year-old may report his/her activities in the gang quite differently than a twenty-five-year old. If samples are to be drawn across age groups, each age-graded group should be sampled (Moore 1978, 1991) or else the gang should be sampled by developmental age (Waldorf 1993).

⁸ Our interviews were done individually, not in groups, to minimize gang members saying things to please friends, leaders, or influential members. Subordinates may play more exaggerated roles to impress leaders or, on the other hand, do little more than second what leaders say as a way to hide divisions from outsiders. Group interviews tend to mask variation (Short, Strodbeck 1965). They are particularly susceptible to what Goffman (1959) calls "team performance." A group will typically cooperate "to maintain a particular definition of the situation" toward the audience, i.e. the researcher. A key aspect of any team, according to Goffman, is it "must be able to keep its secrets and have its secrets kept". Gang members with intense group solidarity may also demonize the researcher in the same way as they have been demonized by the media and some research. Group interviews are more useful as a means to triangulate with other data than as a sole source of information (Fontana, Frey 1994).

⁹ Researchers might not confront a respondent who is giving "politically correct" information minimizing deviance. Goffman explains that such a researcher may be:

motivated to act tactfully because of an immediate identification with the performers, or because of a desire to avoid a scene, or to ingratiate themselves with the performers for purposes of exploitation. (1959)

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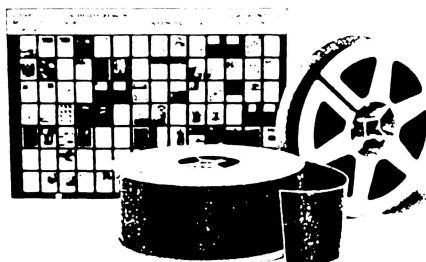
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