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On the Value of Ethnography: Sociology and Public Policy

A Dialogue

By

HOWARD S. BECKER,
HERBERT J. GANS,
KATHERINE S. NEWMAN,
and
DIANE VAUGHAN

Reproduced below is a conversation and question/answer session about the relationship of ethnography to public policy. The conversation took place in Philadelphia on November 7, 2003. Comments by Jean-Michel Chapoulie follow the dialogue (p. 277).

Keywords: ethnography; public policy; sociological concepts; knowledge utilization; NASA; politicians; underdogs

Becker [B]: This session began because Herb Gans and I got into a discussion about Erving Goffman. I had written a piece about Goffman's language, suggesting that his very neutral language (in *Asylums* [Goffman 1961], for example) was a productive way for social science to proceed. I thought that that neutral language was central to his enterprise—or was, at least, to one of the crucial things he did, which was to get away from conventional language and conventional categories of thought. Those are especially foisted on us by contemporary definitions of problems, contemporary ideas about what's important, what's hot, what needs to be taken care of, what needs to be dealt with. I think we're often better off to take a somewhat larger and

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more general comparative view, never mind about the contemporary relevance of our research. That will find its own way to the surface. Herb took issue with that. I'm going to suggest that Herb respond right now to that and then we turn to Kathy Newman and Diane Vaughan, both of whom have been far more than me, and perhaps more than Herb, involved in things that are of contemporary interest, things that are problems now, to which sociology and ethnographic research might make some kind of contribution. Do you want to respond, Herb?

Gans [G]: Howie and I and Erving all went to school together, so we were brain-washed pretty much by the same people. Erving was always unfathomable, so this justified our discussion. I certainly didn't know what was in his mind. But I disagreed, first of all, because I thought *Asylums* was a plea for a mental health policy that rehumanized the patient. Whether Erving had that in mind or not is hard to tell. But I feel very much that sociology and anthropology, in order to survive—and I think this goes way beyond our e-mail—but in order to survive we have to be useful. But even if it wasn't a question of survival, we ought to be useful. Because to be general runs the danger of being irrelevant. Erving was certainly opposed to being irrelevant—fairly strenuously. That much I know. But also because sociology and anthropology ought to help in the making of social policy. You should not be in social policy unless you have expertise in it because it's a discipline unto itself and very different from the behavioral sciences, but we certainly ought to be relevant to it. And I think ethnography is particularly useful here because we come closest to the people who are being studied. We can tell policy experts what the implications are of their policy on other people. We can tell policy people what the people that we have studied need in the way of policy. Ethnography has always studied the underdog and the victim, partly because of how sociologists think and partly because that is funded by government and the foundations. The underdog that is troubled or makes trouble is one subject that keeps social science research money coming, after all. But we can communicate for the underdogs, for the victims to—and in opposition to—the perpetrators, as I think of them. We should also communicate with the decision makers, social hierarchies, institutions, etc., that enable perpetrators to victimize underdogs.

There's also another topic here. When you talk about the general, then you're coming close to the positivist analysis, the search for laws, for timeless laws. And the notion of having timeless laws for society boggles anybody's mind who's been on Earth for fifteen seconds. I think in that sense too, if Erving said that, I would be disagreeing very sharply.

B: I'll call your attention, Herb, to the last paragraph in Goffman's essay on total institutions, a category which includes mental hospitals but not only mental hospitals. It says something like this: that perhaps when we get the more general view that we get from the comparative study of institutions of this kind, when we look not only at prisons and mental hospitals but also army barracks, submarines, ships at sea, monasteries and convents, all of which come under his definition of a "total institution"—when we look at these kinds of places, perhaps we will see that we

can't blame the people who run them, that it's a mistake to think there are evil people who are running these places. I'm adding a lot to what he said, but this is the idea behind it.¹ And he leaves you with the sense that I think a lot of people who have done field research have, which is that when you get through studying some situation, some organization, you sure as hell know that something bad is happening, but you're damned if you know who did it. It's very hard to pin it down because everybody is operating under some constraints that make what they're doing seem to them reasonable, make it seem difficult to do anything else, etc. And the only way we can feel secure in pinning down the evildoers is to not talk to them and so not find out anything about their situation. That's often read as a kind of counsel of despair, as saying in effect, "The hell with it. There's nothing that can be done." Which is not the case because every link in a chain like that is a link that's potentially weak and can be broken. But it is to say that it's very hard to pin down fault and to see where the moral responsibility lies. And that seems to me one of the things you have to do when you decide on a political solution to anything.

G: Obviously, in the last paragraph, Erving got very structural, and he said that there are no evil people. He is mostly right, but there are many good or morally neutral people whose actions have evil consequences, although perpetrators or victimizers may be the wrong words for them. Most people are agents, and there are constraints on them. As Howie said very nicely, there are weak links and our job is to help the policy analysts find the weak links and then to do something about them. How to get them done and how to mobilize and organize power to get them done is the big question, but as ethnographers, we cannot and do not have to answer it (although I wish we could, as sociologists or citizens). But I think there's a fine dividing line between the agent under constraint and the evildoer, and I think this is where you played the role you did [Diane Vaughan], because if it weren't for you, every nth shuttle in the future would certainly blow up. Because of what you did, at least only every second nth will blow up because you did point to the agents and the weak links and said things had to be fixed. And I think here's a natural place for you to take over.

Vaughan [V]: What he is alluding to is a book I published in 1996 titled *The Challenger Launch Decision*. The book itself is a historical ethnography, which means that I was using mainly archival documents to reconstruct structure and culture and explain why NASA blew up the space shuttle and why they made a decision to launch it when they were warned that there were problems. The explanation itself is one that is multilevel in that it takes into account history, macrolevel structural factors, NASA's political environment, the organization itself, and then connects those to individual decisions and actions and how they interpreted signals of danger. At the end of that book, because I saw that the accident was a result of systemic macro-, meso-, and microlevel factors, I actually was surprised when I was rereading it recently to see what I had done there that in the last paragraph of the book I had predicted an accident. I was finishing the book in May 1995, and I wrote that at the time the political context of NASA was changing back to what it was prior

to Challenger. At that point, I had predicted another accident, but I had never really thought about the consequences for *me*. When Columbia blew up, my phone started ringing an hour and a half afterwards, and it virtually only recently has stopped. From February the 1st through the middle of September [2003], there were twelve-hour days that were very involved with what had happened. Initially, all the calls were from the press. I took this as professional responsibility in the sense that I had written something that was about history and the press didn't really have another source. But I also took it as a teaching opportunity. One of the things that was clear in the beginning was that a lot of journalists who had never done anything about space had been assigned to the space beat to cover this accident, and they didn't have a clue about NASA, NASA culture, what had happened with Challenger—nothing. So I spent a lot of time on the phone that was just teaching them. But soon I discovered that the sociological concepts that had driven the analysis in the book, like organization culture, the normalization of deviance, institutional failure—those words began appearing in the press whether I was being quoted or not.

The story that was going on at the same time was that a congressionally appointed investigation board had been called together, headed by Admiral Gayman. Two weeks after the accident, I learned, because of my association with the board, Admiral Gayman read my book. And what he saw was that the explanation of Challenger was analogical to what they were finding about Columbia. The press things continued. By mid-April, I was called to testify before the board investigating the Columbia accident and was there for two days, one day in which I was briefing the board. And then the second day was my testimony. I saw this as very adversarial. I'd never done anything like this. It was going to be live on NASA TV and videostreamed in all major press centers around the country and into home computers. I was also teaching, as you can imagine, so there was a scramble about how to prepare for this. But when I got there, I found a board that was already prepared and interested in and endorsing the sociological analysis. Everyone on the board—and they had 117 staff members—and they all had my book. Plus a short piece that I had written to translate the causal ideas and how you could connect strategies for change with the causes of organizational mistakes. And I put that in a management journal, so they had found that. And that had also led them to other social science literatures. After my testimony, they invited me to come and work with them.

So I spent the summer [of 2003], starting in June, in Washington, D.C., working with the accident investigation board and the staff—there were about forty of us—on the production of the report. And they had succeeding outlines of the board's report. When I got there for the testimony, the first working outline they had had a section on social causes that mirrored the chapters in my book that were on the social causes—the decision making, the macrolevel political history, and the organizational factors. So when I got there, I was truly in a teaching position that people were coming to me and asking me about these concepts and how they fit and showing me their data, so that I could begin to work with their data. And the concepts shaped their analysis in the book.

It was interesting. In one of my conversations with the admiral, who defies any military stereotype—the fact that (a) I was there and (b) he listened to me, as he did to everyone else—was one of the really amazing parts of these group experiences that I was fortunate enough to have. In the beginning, I saw that the outline in the “Social Cause, Part 2” of the accident was trying to distinguish between layers of cause. So one of the chapters was the proximate cause of the accident, contributing cause of the accident, and factors relevant to the accident. So, encouraged by the fact that he was obviously ecumenical, I went into his office and I said, “I can see what you’re trying to do here, but no one is going to understand these different causes.” So he was persuaded—and the degree to which he endorsed sociology shows up in the final report to the extent that this is the first accident report that has given equal weight to the social causes as to the technical causes. Being successful there, I thought I would try something else and so suggested that they move the history chapter away from the beginning of the social cause part to the end of it, and he wouldn’t buy that. And I said, “Why?” He said, “History is a scene setter.” And I said, “No, history is cause.” And I explained to him how NASA historic political and economic environment had shaped that accident. He was still unconvinced, so I proposed that I write a chapter that drew parallels between Columbia and Challenger and was called “History Is Cause.” I proposed it as an experiment—you don’t have to publish this or use this in the report in any way, but I would like to try to do this. He turned out to be delighted. He did not move his history chapter, but he was delighted with the chapter, and it’s sort of a wrap-up to this part 2 of the book.

He was very much in touch with the press, and he began using sociological concepts early on. Just prior to the report’s publication, he had a press conference. And there was an article that appeared in the *New York Times* that really outed me as the genie behind what he had in his magic bottle; that was the report and the author of chapter 8. So this volume of press continued. My name and the concepts of the book, especially “The Normalization of Deviance,” became so commonplace in the place, it was almost generic. And the day after the report was issued, Sean O’Keefe, who was a NASA administrator, had a press conference. He said a few words and just opened it up to the press, and the first question by the press was, “Have you read Diane Vaughan’s book yet?” Which got me an invitation the next Monday night to NASA headquarters for dinner, in which I tried to address something that’s not so clear in the report, which is: if culture is a problem, how do you change culture? So I saw that again as a teaching opportunity. And since then, I’ve been working more closely with NASA. One of the things that I’m recommending is that you don’t wait until you make a huge mistake in order to investigate your culture and think about how you thought it was and how it actually turned out to be, that you should have anthropologists and sociologists who are ethnographers in the agency at all times so that you would be getting feedback on your culture. And I suggested a fellowship program whereby they were enabled to do that. And people have been very enthusiastic about that. I don’t know how it’s going to work out.

There are a lot of things to say in the discussion in relation to Kathy’s work as well. But I think that the reason I was able to have success—I don’t know how you define success here, but if you define success as change, that’s still up in the air. But

I think the reason I was able to teach effectively the connection between the two accidents is because the first book was ethnographic. The description in addition to the concepts really told the story. So when the Columbia accident investigation board and reporters are seeing that NASA had a problem flying with known flaws over the years since the beginning of the shuttle program with the foam, that naturally connected with the idea of normalization of deviance that was in my book in relation to the O-ring problem, that proceeded much in the same way, where they accepted some and then more and then more and then more. So it wasn't like they made two bad decisions; the bad decision was the first decision to accept anything that was a technical deviation and proceed with it. So it was both the ethnographic description, but it was also that the concepts made sense to them because the concepts also fit the data on the second case. And that is what I think gives ethnography particularly a role, not necessarily in public policy, but at least in shaping the public discourse so that people understand the sociological roots of the problem and have a way of analyzing it as you were describing what Erving Goffman did at the end of his book—so that you get it that there aren't really evil people, that there are evil systems and you have to change the systems.

Newman [N]: I want to pick up on that last point about the role of ethnographic work in defining problems in ways that people completely outside of social science can understand. That is one of the vital things that ethnographic work can do that other forms of social science have more problems with. In fact, I would say that what the ethnographic tradition does is to define social problems that have either not been recognized or have fallen off the radar screen, either because the presence of this problem contradicts cultural expectations about what kind of society we live in, what sort of organization we're working in, or because the problem plagues people who have been written off. We don't see them because we don't care about them or because we somehow think their problems are endemic to the kind of people they are. Those are the kinds of circumstances in which I think ethnographic work has the greatest promise.

I was asked by Howie to reflect on how this general perspective has influenced my own work. I've written two books on downward mobility, which, to pick up on Mitch [Duneier]'s point about social amnesia, was actually a topic of great importance to sociologists in the 1930s. Then it dropped completely off the radar screen, I would argue, because downward mobility flies in the face of what American culture defines as the normative experience. Yet even in huge technical tomes like Featherman and Hauser, we find evidence that there have always been a significant number of people falling down the class ladder. We just don't focus on them or write about them because their experience doesn't accord with the culture we live in. An ethnographer can turn that expectation on its head and look at a problem that is standing right before us and ask how it is constituted.

Another problem that I've worked on in collaboration with four of my graduate students, one of whom is here today, bears a resemblance to this. We have a book coming out in February [2004] on rampage shootings in American high schools. These are horrible tragedies that take place in the "wrong place." They are occur-

ring in places that have very low background violence, that are known to be a great place to raise a family, and suddenly, they turn into murderous violence. It jars our sense of predictable social order and makes a perfect occasion for an ethnographic exploration. *Rampage* owes a great deal both to Diane's work and to Charles Perrow's work on normal accidents. Rampage shootings happen after an enormous amount of warning behavior, after a great deal of "evidence" piles up that in retrospect suggests that something horrible is going to happen. Yet nobody can see it beforehand. These concepts of structural secrecy, of organizational failure, turn out to be powerful explanations of why rampage shootings get as far as they do.

I've also written two books that fall into this latter category of issues, those that don't get attention in general, because their victims are on the wrong side of inequality. One book, *No Shame in My Game* [Newman 1999b], was about the working poor, and the other, *A Different Shade of Gray*, was about elderly minorities in the inner city who are aging in poor communities. Those books look at the lived experience of people who don't confirm the accepted paradigm of American culture. We think poor people are poor somehow because they don't want to work. Well, what about all those people who are working in low-wage jobs?

Another important service ethnography provides—and this may be a more anthropological take than a sociological one—is to expose or explore the subjective experience of victims or of perpetrators: What does it mean to ordinary people to be working poor or to be downwardly mobile, and why do they think this experience has befallen them? What is the folk sociology of this issue, and how does it differ from the way a sociologist or a more distanced analyst would explain the structural problem of downward mobility? Americans have a weak grasp on structural forces. They tend to pull their problems inward toward the self, to make them expressions of failures that are under personal control. Explorations of the subjective perspective are always embedded in a particularly critical approach to the understanding of American culture.

A third point that I think is crucial about ethnography in the public realm is its capacity, which I think Diane's work shows and Howie's and Herb's as well, to communicate beyond the academy through powerful narrative. Ethnography has the capacity to connect with the public, with students, and even with politicians—even with people who run NASA—because they can actually understand it, and they have a great difficulty often with structural equation models. It also then tends to attract media exposure. I've been on *Oprah* and the *Lehrer News Hour*, and at either end of that media spectrum, you find yourself with a teaching opportunity, a chance to convey to an enormous public the power of social science to capture a reality that matters, that helps to explain things that people find puzzling. Quantitative forms of sociology rarely have that opportunity, which is too bad because they offer insights into many important public problems as well.

Many ethnographers are tempted to move from the diagnosis to the remedy. You can't embed yourself for three or four years in a problem and then say, "Gosh, I have no idea how we could improve this situation, no opinion about how we could make the lives of the working poor better, no thoughts on improving the lives of the inner city elderly." But I do think there is a difference between diagnosis and rem-

edy. We shouldn't confuse public sociology, or the exposure of social problems, with policy evaluation. I've never thought of myself as a policy wonk. I wasn't trained as a policy analyst. At Harvard, I work alongside people who've done it for a living for many years, and they bring tools to policy analysis that I genuinely don't have. Nonetheless, I would never back away from the challenge I pose to myself of offering whatever I can in the way of informed speculations, ideas, thoughts, possibilities, about how to make the problems I study go away or be less harsh in their consequences. But I guess in the end that's not a special brief of ethnography. It is something we can do. Some people do it, some people don't. It's not necessarily unique to our trade.

The special mission of ethnography, in my opinion, lies in its capacity to redefine the social landscape, to explode received categories, to cause students, ordinary readers, we hope politicians and people who have power to understand the social realities of our society. Ethnography has the capacity to develop different ways of thinking about a social universe that they often take for granted, to explore and make real the experience of people who have gone through hardship, through loss, through confusion, and possibly even a triumph against all expectations. That is a special mission that I feel makes it legitimate for me to take that paycheck that I get for doing this work I'm privileged to do.

G: Let me say two things. I e-mailed Diane Vaughan after I learned about all this and told her she was the heroine of sociology because she was in the right place at the right time when she could do something. I've been in some situations like this and made proposals. However, my proposals were generally ignored or rejected, but you were there with the right remedy and at the right time.

But I got up mostly to respond a little bit to Kathy because I think ethnographers can do more than she says they can do. If you've studied and worked among the working poor, you know even just by listening to them what kinds of changes could be made to give them access to an opportunity for upward mobility. And you can tell the policy people that. You can draw from the implications of your research; you can draw from the consequences of your research. The implications will provide clues to policy. The consequences will help you evaluate the policy or take a policy evaluator aside and give him or her helpful advice. So I think we can do something. Whether we ought to do something is up to each individual, but I certainly think that ethnography, by the virtue of being close to people, by seeing how they live and seeing their problems, enables us to take a bit more of an active role than she suggested.

N: You misunderstood, Herb. I didn't say that you can't or you shouldn't evaluate policy. I'm saying that it's something some people would like to do and other people don't find to be consonant with their mission. But I don't think it's something that is necessarily the sole province of ethnography.

G: Of course not. But it's one of the capabilities that ethnography gives us. That's what I was trying to say.

B: I'm a mischievous kid and I can't resist making a little trouble if I can. Seriously, I think it depends so much on the situation around the case. You're exactly right—Diane was in the right place at the right time. Hardly any of us ever are. What made me a complete cynic about the value of sociology for influencing anything were two experiences I had. One is: I did research on marijuana fifty years ago [Becker 1963, pp. 41-78]. It was part of a wave of research of various kinds—biochemical, pharmacological, psychological, legal, etc.—all of which showed that governmental policies with respect to marijuana were foolish and counterproductive. Those arguments are well known and widespread. Everybody has heard them and knows all about them. And the result of all that research is that the laws are even more punitive today than they ever were. This is enough to make a boy unhappy, I'll tell you.

The other experience is on a much smaller scale. It had to do with the research my colleagues and I did in a medical school. When we finished our research, we wrote a manuscript, we gave it to the faculty to read, and we had a meeting with some of the faculty, who said, "Well, where are your recommendations?" We said, "We don't have any recommendations. This is what it looks like." They said, "No, you've been here three years. You must have some idea of what could be made better." I said, "Look, I'm not a medical educator. I don't know what you people are trying to achieve. But you tell me what you don't like, and I'll tell you how to fix it." They said, "Well, for instance, we don't like the way students cram for examinations. They study very hard to pass these exams. And then they immediately forget everything they've learned. It's just for passing exams." I said, "What would you like them to know how to do? What is the exam supposed to measure?" "We'd like them to know how to examine a patient—do a physical examination, take a medical history, order the appropriate laboratory tests, arrive at a diagnosis, and suggest a plan of treatment." I said, "Nothing easier. Give each student two patients. Let them do that. Then you examine the patient and you see how well they did." There was a dead silence. I said, "What's wrong?" "Well," they said, "that's not very practical." I said, "What do you mean?" They said, "It would take a lot of time." I said, "You'd better believe it. But if you're serious, this is what you would do." They said, "We can't do that." "Why not?" "Well, we have our research to do. We have our administrative work on various faculty committees. We have our own patients to take care of. We don't have time for that."

From this I developed the concept of a *panacea*. A panacea, in the technical definition I gave it, is a solution that takes care of exactly what you want to get rid of and doesn't cost a penny or a minute or anything that's of any importance to you. The problem I've always found with offering solutions based on ethnographic knowledge is that they cost too much and that we are never in a position to get the people who might be able to make the change we suggest to agree to bear the expense of doing it. I think, Diane, you may be in a position where what NASA is experiencing now is so heavy and so expensive that they're willing to pay the price to make some changes. But that's not a very common situation for a social scientist to be in.

G: Diane, you said before that you think the next blow up will be on the same schedule, so obviously you didn't think you achieved what you hoped to.

V: Well, I went to a conference of NASA's forty top leaders, and they are absolutely determined to make change and excited and having lots of ideas. I never saw a group of such smart people. They understand the social principles behind this. But a lot of it is outside their ability to make change. Their top leaders have responsibility to Congress and the White House, and as long as they are shortchanged in terms of funds, they are always going to have deadlines and schedule pressures, and their ability to make internal change is seriously challenged by that. As well as the fact that historically the roots of the agency are in the military. It's huge, so they have an exceedingly large system in terms of rules and protocol, which make hierarchy and change of command essential for them. It's a minisociety. It's just like changing society: there are inequalities throughout. You can't listen to engineers because engineers can only speak up under certain conditions. So systems are hard to change.

Audience Questions/Comments [Q]: With respect, there is another tradition of ethnography that might enlighten the discussion, and that is the ethnography of policy making itself. I would take issue a little bit with Herbert Gans's contention that we only look at the underdog. It's perfectly possible to look at the activities of politicians and policy makers, look at the way they proceed, and look at the way in which they have knowledge, and it becomes quite clear that we all as social scientists are still operating within a blue-book socialist or Fabian tradition, which believes, as it were, the facts will speak for themselves and they'll be effortlessly absorbed in the political process. That is not the way it works. It works very differently. What you're talking about is a world which is largely self-contained. You're talking about a world which has its own strong political priorities and its own discourses. Not least amongst the characteristics of those discourses is the anonymization of influences. Ministers, at least in the countries I've studied, are not interested in sources. They're not interested in the authors of particular documents. They're interested in what they conceive in a very immediate sense to be the political good sense of what is said. And the political good sense is not our sense at all. It's framed by constituencies. It's framed by political crisis. It's framed by long-term problems. It's framed by manifesto commitments. It's framed by obligations to parties and all the rest. That's one issue. The second issue is the timing is critically wrong. We often produce our results out of synchronization with the policy process. And the policy process has its own momentum. So there is not, as it were, a receptive audience. And it goes on.

In the field I know something about, which is justice, John Braithwaite has been the most distinguished and influential social scientist, but you will look very hard to see his name amongst any of the papers that talk about changes in justice systems. The whole way in which social science is treated is to bleach out authorship. And I

don't think we know very much about the way in which social science does impinge. It's much more oblique. It's much more tangential. It's much more a matter of trickle down. It's much more a matter of casual conversation. And if we persist in the idea that somehow we will represent the sufferings of the poor and then the politicians will respond, I suspect we're not going to work very effectively.

N: Let me take a crack at that. I don't measure the success of what I'm doing by whether or not politicians find it attractive and act on it. I'd love it if they did, but I still think it's a worthy enterprise because the mission is something larger than simply trying to affect public policy or to study how public policy is made—though all of those things are available for ethnographic study, I don't deny it. The study of the political process and the extent to which it *ever* pays any attention to evidence as opposed to interest groups, I think, is very worthy as well. But I think we would make a big mistake if we evaluated the success or the intrinsic value of what we do by whether or not we can show the kind of change that Diane is able to show. I think it's wonderful, but I think it's rare. I don't think I was able to massively affect the course of mobility in the United States by writing *Falling from Grace* [Newman 1999a], but I changed a few people's way of thinking about the problem, and that modest goal is actually sufficient for me.

Q: Let me just respond very quickly. I said that we studied the underdogs and the victims in part because we were paid to do so. However, I do not think that is all we should do. In the sixties, Lee Rainwater and I argued that we should stop studying the poor and start studying the people who make other people poor. We got nowhere, of course, and things haven't changed very much. But I agree thoroughly that we ought to be studying the policy people and see if we can get to them that way.

V: If I could say one quick thing in relation to Kathy's comment. I think that we cannot really measure our effect, that you don't know when you put something into print who's reading it. Kathy and I are both outliers in a sense in that we do deal with the public a lot in terms of our research, but if you think not of policy change but of social change, and you think of the sociologists who are really public sociologists but are not teaching in universities because they can't do their work and simultaneously stay on a faculty and live up to those requirements but are doing grassroots organizing and trying to empower other groups of people, that those teachings are based on sociological principles. And it may be that in terms of their work and in terms of my work, the people I may have most influenced are the people who are on this accident investigation committee. We have to think of social change in terms of our outreach to our students, in terms of other kinds of examples of public sociology than just the kind that Kathy and I have had the opportunity to do. That's the kind that shows. What we don't see are the invisible work of all the sociologists in their classrooms. When a student leaves your classroom, you

may have changed their life, and you may not, but often you don't hear. So I think changing organizations and changing lead structures is always going to be hard for us. But I think, as Kathy says, if we give people a different vision of how the system works, then we've done a lot right there.

Q: I think that skepticism is the enemy of sociology, and because of this skepticism, I ask, is that the reason that the Chicago slums are still there and that they are more miserable now than when this work was started in the Chicago School? And then I think sociologists sometimes are snobs. They are snobbish about their writing. They make their writing so complicated and incomprehensible for the policy maker, for the politician who is not always very smart maybe, but who needs to read simple texts. So don't we really have time to produce those, to help people? This is my question.

Q: I think that a lot of the graduate students in this room are probably thinking about a lot of what you've all been saying. And I think Diane and Kathy are beginning to answer the question that I have. A lot of us, especially those of us who study urban poverty, we want to do policy-relevant work, and we think about that, and we realize that there is a reality of the context that we work in. Eli [Anderson] and Mitch [Duneier] have both told me, and Kathy Edin has told me this as well, that our first goal should be to be good sociologists. We want to explain, we want to understand. And if we go into it motivated primarily by a policy agenda, we'll always be disappointed because policy makers will misappropriate and reinterpret our findings to say what they want to say. That's how people can read Kathy Edin and Laura Lane's *Making Ends Meet*, and liberals can say, "See, welfare moms can't make ends meet on welfare alone. Low-wage working moms can't make ends meet on a minimum-wage job." And conservatives can read the same book and say, "Look, here there is proof: welfare moms cheat on welfare. That's why we need welfare reform." So I guess the question that I have for all of you, for those of us who want to do that kind of policy-relevant work, what's the answer? Can we just hope we're in the right place in the right time, as Diane was? Or can we hope to influence just a few people, as Kathy Newman was saying? Or is there a way that we can try to gear our work to speak to policy makers and not have it misappropriated?

Note

1. The actual wording of Goffman's remark is as follows: "I have defined total institutions denotatively by listing them and then have tried to suggest some of their common characteristics. . . . the similarities obtrude so glaringly and persistently that we have a right to suspect that there are good functional reasons for these features being present and that it will be possible to fit these features together and grasp them by means of a functional explanation. When we have done this, I feel we will give less praise and blame to particular superintendents, commandants, wardens, and abbots, and tend more to understand the social problems and issues in total institutions by appealing to the underlying structural design common to them all" (Goffman 1961, 123-24).

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