

Convincing Sociologists: Values and Interests in the Sociology of Knowledge

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It is not particularly novel to note that knowledge is swayed by the interests of the knowers. This is one of the foundations of the sociology of knowledge, as first formulated by Karl Mannheim.¹ Even scientific analyses are vulnerable to the scientist's interests and values, as Max Weber argued in his methodological essays.² The recent "sociology of scientific knowledge" has generated a number of case studies in this vein.³ However, these studies fail to recognize that the values held by scientists are diverse and often mutually contradictory. Moreover, as I shall argue, scientists invoke different values according to their interests in specific contexts of the production of knowledge.

This chapter explores the relationship between values and interests in the production of sociological knowledge. The site is the graduate sociology class on participation-observation (P-O) methodology that produced the essays in this volume. The subjects are the dozen students and one professor who met twice each week for a semester. The knowledge in question consists of the statements and analyses made during class meetings, and tentatively accepted by the others present as accurate.

The conclusion is that we in the class did not evaluate hypotheses in terms of the evidence presented or in regard to the theoretical tradition proposed—the two reigning perspectives on the production of scientific knowledge. The first perspective views scientists as straightforward arbiters of evidence, calmly investigating hypotheses and judging them to be meritorious or unworthy.⁴ The second perspective views scientists as members of theoretical communities, willing to endorse or disregard hypotheses on the basis of their compatibility with the community's

paradigm.⁵ Yet I found little reference to evidence, and no consistent paradigms, in classroom discussions.

Instead, we in the class evaluated hypotheses in keeping with certain metatheoretical values. These values concerned basic attitudes toward the social scientific project, that is, basic approaches to the understanding of human subjects. What was odd about these values is that they were contradictory and mutually exclusive, and that we frequently shifted back and forth on these seemingly immutable fundamentals.

I identified two pairs of values governing the class's treatment of hypotheses. The first pair consisted of:

- "respect for the subjects," that is, listening to and respecting the subjects' own analysis of their situation; and
- "social silencing," whereby social forces systematically prevented the subjects from properly understanding their situation.

The second pair of values consisted of:

- "social determinism," whereby subjects' behavior was held to be caused by broad social forces; and
- "voluntarism," namely, the ability of subjects to transcend determining social forces.

Both of these dichotomies operated on a continuum of social distance from our subjects: when we felt closer to them, we were more likely to respect their analyses and to grant them free will. When we felt distanced, we saw their analyses as systematically deluded and their behavior as socially determined.

These sentiments of social distance, I argue, reflected our interests as P-O sociologists. The first pair of values served to define our role in the field setting, allowing us to pursue political goals and to gather information. The second pair served to carve out a niche in the academic setting and defend it against encroachments by individualist analyses on the one hand and dehumanizing analyses on the other.

Thus by maneuvering within the two dichotomies, we defined our relation not just to our subjects, but also to other social scientists. The contrast between these two arenas, the field and academia, is particularly sharp for P-O sociologists, sometimes even painful.⁶ However, all social scientists face a similar contrast, and thus the basics of the analysis presented here—conflicting values and interests governing the evaluation of academic hypotheses—might be applicable more widely.

IDENTIFYING KNOWLEDGE: METHODOLOGICAL HURDLES

There are two methodological problems in the study of how knowledge comes to be accepted as such. The first is the ontological problem of identifying proposed instances of knowledge: distinguishing hypotheses about reality from other statements whose goal is not primarily analytical. The second is the epistemological problem of identifying acceptance: distinguishing true agreement with a hypothesis from partial or insincere agreement.

I was dismayed to find during the semester that it was difficult to identify classroom statements as analytical hypotheses. There were questions, suggestions, and ruminations aplenty, but few bold conjectures and analytic arguments. This theme was also taken up by the rest of the class. Carol noted that classroom conversation should be construed as "in process," not finalized or definitive.⁷ Sepha made the point that much of the class's discussion was simply not intended to be "factual." In particular, Leslie S. added, students' comments on their classmates' projects should not be construed as hypotheses. With each of these objections to my project came the admonition that I had simply picked the wrong class to study the sociology of knowledge; other classes debated hypotheses, I was told, but not this one.

These objections were to a certain extent on the mark. Classroom discussion was more tentative than in other graduate sociology courses I had participated in; there was less debate of social theories or substantive sociological explanations; and there was much more of an emphasis on helping our classmates work on their projects. However, these objections did not rule out the existence of hypotheses or knowledge production in this class. Specifically, tentativeness did not mean that these statements were not proposals about how the world should be viewed. It does no violence to the intent of the speaker, I think, to study the fate of such tentative statements, and to examine the reasons why some of them were accepted while others were not.

Identifying acceptance was even more difficult. To begin with, the limits I placed on my study—paying attention only to in-class interactions, and only for one semester—restricted my study to short-term phenomena; over the long term people may come to feel they "know" things that they initially resisted. But even in the short term, it was difficult to tell whether people agreed with an analysis or not. The biggest obstacle was that not everyone spoke up on every issue, and a few members of the class spoke quite infrequently. Even those who spoke more often might have been putting forward ideas just to play devil's advocate, or to be polite, or as a gesture of kindness and support, none of which should necessarily be taken as agreement. Sometimes

people would nod their heads at some statement, but this too, as several class members told me in discussions of my project, might signify something other than agreement.

Indeed, there are difficulties with the idea of agreement itself. I took part of one class session to ask my classmates whether they had agreed or not with certain of the hypotheses I discuss here. Several students became quite upset that I asked them to write "agree" or "disagree," survey-style, even though I was also asking for longer comments as well. Sepha accused me of trying to "pigeonhole" opinions that were not binary but multidimensional. She was quite right; agreement is not an either-or matter, and it involves a number of subissues: for instance, I might agree with someone's conclusion but not with the reasoning behind it, as Josh pointed out.

Several partial solutions were available to me. I asked everyone straight out if they agreed with items I felt they had agreed with. I asked everyone to write brief statements on what they had learned in the class. I checked my impressions with my classmates informally outside of class. I made outrageous statements in class and watched how people reacted. But none of these efforts were systematic. In the end, I had to rely on my status as a relative "insider" to distinguish actual agreement from unspoken disagreement, and I have tried to stay sensitive to the variations and shades that acceptance may take. In this spirit, I have chosen examples from class that I felt were subject to fairly widespread consensus.

ALICE ON GENDER: THE VALUE OF RESPECT FOR SUBJECTS

Alice was working in and studying a public employees' union. When she first presented her research to the class, she noted that almost all of the public employees in the union were women, while almost all of the union representatives were men. Furthermore, she told us, her subjects divided their clients into two gendered groups: "bad homeless clients," who were mostly men; and "AFDC mothers," who were, in the subjects' eyes, "redeemable."⁸ The site seemed to be perfectly matched with Alice's interest in gender issues. This is how she introduced her first presentation to the class:

Alice: Initially . . . I went in very interested in women workers. Social work is a feminized profession, and I really wanted to see how that feminine ideology was being played out in the workplace. And what I discovered is that, at least from what I've seen so far, it's not that important to the workers.

Michael: What's not important?

Alice: Gender.

Michael: But what do you mean by that?

Alice: Well, there are policy issues like pay equity and comparable worth and child care. That's there, and that's really important. But in terms of the relations in the workplace and the relations between the [union] organizers and the workers, gender is not really very important.

Her justification for the relative unimportance of gender was that she saw few signs of gender consciousness, and she heard few of her subjects complain about sex discrimination. They complained, instead, about racial issues: "Right now I'm really attending to what people are talking about, so I think that was sort of why I was emphasizing [race, as against gender], sort of what's consciously out on the table as an issue."

This reasoning holds that the subjects themselves are capable of identifying the important issues confronting them. This is not an empirical argument: Alice presented no evidence to show the unimportance of gender—indeed she admitted that it did appear to be important. Nor is this a theoretical argument: she presented no alternative theory to explain the workings of her field site, at least not at this early point in the semester. Instead, she appealed to the class to respect the indigenous analysis of her subjects. This "respect for the subjects" argument was frequently adopted during the course of the semester; several other students abandoned or shifted their scholarly perspective because their subjects did not find the scholarly issues to be important. The professor had urged us in emphatic terms in the beginning of the semester to "let the field speak to you" and to "be surprised"; being surprised by one's findings in the field became a running joke throughout the term.

However, Alice's argument, and the others like it, face a constant rebuttal, which I will call the "silencing" argument: namely, that the subjects are systematically silenced by social forces preventing them from recognizing or discussing their true situation. The classic silencing argument is Antonio Gramsci's theory of ideological hegemony.⁹ Leslie S. suggested something similar when she objected to Alice's first presentation: "That this is a female workforce where women are doing traditionally female things seems to still be true whether or not people see themselves that way." By implication, the lack of gender consciousness among Alice's subjects did not necessarily signify the unimportance of gender. Indeed, feminist social science has frequently applied a gender analysis over the objections of its subjects, with the intention of thereby creating gender consciousness where it is lacking. Alice's response was to repeat the justification for shifting her focus away from gender:

I was probably being kind of flip when I said [gender] was not important. I guess what I mean is that I'm struck by the fact that in terms of discrimination, that's not what's out on the table. In fact, it's often a white female supervisor supervising a Black female and there's a lot of tension on the basis of race, and there's just not . . . all this solidarity around gender.¹⁰

This debate between the "silencing" and "respect" arguments was not one to be decided by the application of evidence or the adjudication of competing theories, traditionally held to be social science's primary methods of settling analytical disputes. Instead, both arguments appeared to be on the order of shared values, not open to empirical proof or disproof, guiding the class's treatment of Alice's nongender analysis.

Alice's argument overcame the "silencing" objection—at least, the objection was not raised again. Toward the end of the semester, I asked the class whether my impression had been correct and whether they now agreed with Alice's nongender analysis of her field site. The most vocal members of the class protested vehemently. They had not "agreed" with Alice's argument, they said; rather, they had assented to it, feeling it was not their place to tell her what to study. But in other cases, the class had felt it was its place to use the silencing objection to change a classmate's focus of study. Ann R.'s initial class presentation was one such case.

ANN R. ON AGE: THE VALUE OF SOCIAL "SILENCING"

Ann R. studied a senior citizens' center. Her second set of field notes appealed to the value of respect for subjects:

1. The original question I went into the field with, i.e., to listen for body images and metaphors old women use to talk about their aging as a way to get at if, and how, the old internalize a negative stereotype of aging as decline and loss, no longer seems relevant in light of the data I am gathering in the field.
2. In the first place *age itself does not appear to matter very much* in the interactions I have had or have observed; in fact age has never been referred to as a limiting factor and most people proudly tell me their age; when Gertrude expressed an interest in tap-dancing, her reason for not taking it up was that she had a bad knee, not that she was too old. [Emphasis in original]

Josh responded by picking up on Sepha's point that there might be a tacit contract at the seniors' center not to talk about negative things:

Josh: You say here that "age itself does not appear to matter very much," and one possible explanation is that there's this tacit contract not to

allow it to matter. There's a big difference between that agreement and it not mattering.

Ann R.: Maybe that was a clumsy way to say it. I'm not meaning to imply that age doesn't matter. . . . [It's just that] they don't ascribe anything about who they are to their age.

Josh: But the thing that I'm saying is how do you know that's not because there's an agreement in this context, there's a set of rules operating in this context—I'm not saying that they're consciously repressing these words, but it may be that in another context they may be—

Ann R.: —more conscious of that. Well, I think some of that has to do—and some of the literature indicates that too—with the fact that it's an age-peer setting. Other than me, they're all old.

Michael: It could be construed as a reaction formation: "We are not going to accept that." Aging *does* matter. People are responding to it and trying to deny it. The very fact that people are trying to deny it says that it's part of their self-understanding.

Ann R.: Yes. Yes.

In this episode, Ann R. was talked out of her field-notes position, which resembled Alice's in that it justified the downplaying of a particular theoretical issue by noting that the subjects did not identify it as important. But in this case the justification was defeated by a "silencing" argument. Social forces, namely the norms operating within the community of old people at this seniors' center, kept people from talking about something that was important to them all, namely age.

I asked the class at our next session whether they agreed with this silencing argument. Six of thirteen specifically said they agreed. One said she didn't know. The others gave no indication one way or another. But I sensed that approval was fairly widespread. Indeed, the discussion of Ann R.'s presentation took such a long turn away from respect for subjects that by the end of the session we were cracking, and laughing heartily at, old-folks jokes. Sepha did a wicked, though brief, impression of a doddering Katherine Hepburn. Ann R. later commented that this treatment was just the sort of age discrimination that she wanted her work to combat.

Here, again, both the "respect" and the "silencing" arguments were appealed to not as empirical propositions with evidence in their support, nor as theoretical contributions, but rather as classroom values. Ann R. argued, just as Alice had, that the subjects had the right to say what was important in their lives. Just as Leslie S. had observed with Alice's first presentation, Sepha and Josh pointed out that we cannot rule out the possibility that "silencing" mechanisms are at work.

SOCIAL DISTANCING AND P-O SOCIOLOGISTS' INTERESTS

Alice's appeal to the norm of respect for subjects succeeded, while Ann R.'s did not, because of social distancing. Class members felt themselves to be different from, and socially superior to, Ann R.'s subjects, while they tried hard to erase the social distance between themselves and Alice's subjects. Alice and Ann R. had—unwittingly, in the latter case—encouraged the different attitudes. Alice had treated her subjects, despite all her disagreements with them and her puzzlement at their actions, as "strategic actors," a formulation in which social distance is minimized. In addition, she clearly declared herself to be on her subjects' side: as poor, overworked, underpaid, Black working women, Alice's subjects had the class's sympathy.

Ann R.'s elderly subjects had the class's sympathy as well, but it was a sympathy tinged with a patronizing aspect that would have raised hackles immediately in the context of Alice's project. Ann R. inadvertently encouraged this attitude by describing a scene in a senior citizens' seminar she had participated in. At one point, the seminar leader had paid tribute to an extremely old woman in the seminar, kneeling before her for no apparent reason and proclaiming, "There is radiance coming from you." The episode, as Ann R. described it, seemed ridiculous and baffling, and all of us in the class made thorough fun of it. Only as an afterthought did Ann R. give us the reason for the seminar leader's action: this elderly woman, despite her own infirmities, was doing volunteer work for people with Alzheimer's disease.¹¹ Having distanced ourselves from Ann R.'s subjects—Sepha, for instance, wondered whether those of us raised in the era of consciousness-raising sessions and support groups will age differently—we were much more willing to discount what the subjects actually said and to impute greater importance to the social forces that kept them from talking about reality.

Social distance, in turn, reflected the class's interests as sociologists. The most direct interest was political: the class, composed of political progressives, was eager to ally itself with workers and workers' movements, while the elderly were not as high on our political agenda. In addition, we as sociologists had a research interest in maximizing information retrieval. Individually, within our field sites, we each manipulated our social distance so as to get the information we felt we needed, sometimes posing as authority figures, sometimes acting chummy and conspiratorial. Collectively, in classroom discussion, we often discussed and suggested roles that students might try to play in their sites so as to get the necessary information.

These two interests in politics and information defined our relation to our subjects, a relation that is especially important and problematic in P-O sociology. Unlike other sociological methods, which require only

short-term interactions with their subjects—an interview, perhaps, or even a series of interviews—participant observation requires researchers to become more intimately involved with their subjects. The “participant” portion of the P-O moniker describes just this aspect of the method, and raises the serious question of how the researchers fit in to a more-or-less alien social situation. Since most field sites have no ready-made role for participant-observers to play, the researchers have a particularly uncertain and anxious status, but one that is quite open to maneuvering.

Maneuvering is necessary because the two interests, politics and information, may sometimes be contradictory. Cozying up to authority figures in order to get information from subordinates may go against political values; conversely, a strong political identification with one's subjects may make the researcher hesitant to pry information out of them for academic scrutiny. Compromises may result: for instance, P-O researchers may supplement their regular, collegial, low-social-distance relations with their subjects with occasional formal interviews, whose researcher-subject roles involve greater social distance. In this way, P-O researchers can move back and forth along the continuum of social distance, in the service of their political and research interests, appealing at any moment either to the value of respect or the value of silencing to justify hypotheses they might make.

This is not the only case where competing values may serve competing interests.

THERAPISTS AND POVERTY PIMPS: THE VALUE OF SOCIAL DETERMINISM

A recurrent theme in the course was criticism of and disgust toward people who are trying to help the poor and the troubled. This was due to no stinginess of spirit, but rather, I think, to the individualistic approach of many of those doing the helping. In the first substantive class session, Paul, an advanced graduate student who pursued P-O research several years before and had had his term paper published—which lent him considerable prestige—described his study of an organization that counseled wife-batterers. The predominant group of volunteers at this organization consisted of therapists who had just gotten their professional degrees. He described one of them, whom he called “typical”:

Paul: He talked about working [at this organization] in the way you would talk about choosing a clientele. If you're a newly minted psychologist, do you want to work with drug addicts, do you want to work with men who beat their wives, or do you want to work with someone else? And he thought, this is a good choice to make.

Ann R.: A newly emerging field, not too many people in it. I mean, that's a very cynical view, but. . . .

Paul: I don't think that's necessarily cynical, and I think it makes sense.

Imputing such careerist motives to therapists had a pejorative tinge to it, and the class seemed somewhat depressed to hear that the organization was increasingly run by therapists.

The next week, Kathy presented her field work studying an ethnographic project that used ethnographic field work to distribute bleach and condoms to slow the transmission of AIDS among injection drug users, prostitutes, and other high-risk populations, while at the same time studying these groups. Kathy and the rest of the class repeatedly referred to the project staff—the professional ethnographers running the project and the outreach workers they had trained—as “poverty pimps.” Ann R. made the analogy to “the people who are hanging around the elderly, trying to make professional careers out of it.” Kathy responded by quoting one of the ethnographers, a professional social service worker, who had told her, “Well, I was really smart. I saw this thing coming way in advance. [The class laughed.] I knew that AIDS and homelessness were going to be the big issues in [this city] in the late eighties, so I switched early on and started learning about both issues.”

Nadine then spoke about the social service workers in New York who had hopped onto the methadone-maintenance “bandwagon” as “a way to make fast money.” Kathy's remark that her organization was tilting toward hiring more professional social service workers and fewer “indigenous” outreach workers—her word for people from the communities being served, such as former drug users and prostitutes—occasioned a disgusted shaking of heads.

This pattern of insulting those who cared, not always effectively of course, in social-service professions, was entirely consistent throughout the term and entirely acceptable. No one spoke up in defense of these people; surely, given the vitriol of some of the insults, someone who felt at all sympathetic to social-service workers would have spoken up to say that they were not being treated fairly.

Why, then, did the class hurl or consent to the hurling of such nastiness? There was no neoconservative sentiment that spending money to help the poor was in itself a misguided project. Instead, the disgust toward social service workers had to do with their individualistic approach to what the class saw as fundamentally social problems. Paul said of his therapeutic organization's approach: “That's how he understood what he was doing as a way of dealing with the [wife-]battery issue, that you deal with individuals as individuals, and all these social categories are not what's real. What's real is individuals being able to be free and love each other.” In her field notes, Kathy quoted a disgruntled subject

who blamed the anti-AIDS project for focusing only on helping individuals: "By focusing on one problem—THE EPIDEMIC, right?—people, agencies can look away from the real issues, like health issues or the conditions around these people which makes them perfect targets for the epidemic to spread." Kathy and several other class members said they agreed with this analysis.

This antipathy toward individualistic solutions was based on no evidence and no theory. Nobody in the class argued that individualistic social services were unhelpful or wrong or a waste of taxpayers' money. It was simply agreed in a series of cases that AIDS, wife-beating, and other issues were social problems: they were examples of human behavior that is determined by social forces. Individualistic analyses of such problems simply miss the point, and in so doing also keep the victims of these problems from seeing the point too.

SPECIAL PEOPLE: THE VALUE OF VOLUNTARISM

Despite the value of social determinism, the class sometimes allowed exceptions to be made. Some subjects were called "special," and their behavior was to be explained in individual terms, not by recourse to social forces.

The first "special" subject, though the class had not yet coined the term, was Kathy's disgruntled outreach worker, M., whom she quoted at length in the first field notes that she distributed to the class. M.'s analysis of the ethnographic project he worked for was quite radical, and wholly social-determinist. Kathy followed her extended quotation of this source with this analysis: "It is clear that there are some real problems with the way that these outreach workers identify themselves. Since the agency's policy is to try to hire indigenous members and former members [of groups affected by AIDS], they encounter a great deal of *role conflict*." (Emphasis in original.) Leslie S. commented on this analysis in class: "After reading all the description of what this guy said, I assumed that you agreed with him, actually. And then I was surprised to read that you were . . . putting it in a framework of role strain, because it seemed like he was the deviant within the kind of [social situation] you were looking at." In other words, Leslie continued, one shouldn't analyze subjects who have "true" perceptions (meaning, I suppose, perceptions we sociologists would agree with) in terms of role strain; rather, one should look at why the other subjects have *not* been able to come up with such accurate perceptions. Only two people in the class (Alice and Michael) later said that they did not agree with this critique entirely; Kathy, Leslie S., and I said we felt that there had been a good deal of consensus on the point.

In another instance, Ui also called her main subject, a dynamic immigrant-community leader, "special." She described her subject as a rare woman who had been able to transcend the traditions in the immigrant community limiting women's opportunities and keeping women from leadership roles. Ui was not interested in how the subject had been able to break the grip of social determinisms and did not pursue the implications of her observation that the subject was the only one in the community with a royal background. Classroom discussion also skirted this issue and focused on the effects of this leader's behavior, the role she played in the immigrant community, and so on. The rest of the community, though, was subjected to deterministic analyses.

Why should certain subjects have been exempted from social determinism? My feeling is that another value was being applied: namely, that social determinism is dehumanizing. It strips away the subjects' free will and treats them as objects, not as fully human beings. This is particularly a problem for P-O sociologists, who get to know their subjects better than interviewers or archival researchers can. This intimacy may cause P-O researchers not to wish to see their subjects reduced to impersonal social causes, and it may lead P-O researchers to become so aware of their subjects' idiosyncrasies and personalities that no social determinism would fit.

DISTANCE AND INTERESTS

As with the previous pair of values, "respect" and "silencing," social distance seemed to determine whether social determinism or the opposite value was applicable. Many students in the class chose field sites hoping to find something progressive occurring in this neoconservative era, and virtually everybody was disappointed to find their subjects not so progressive as anticipated, not so much *like us*. The class seemed to have no problem attributing social causes to such people's behavior; indeed, to have done otherwise would in some cases have been to engage in "blaming the victim." The few subjects the class really felt in agreement with, however, were granted the sociologists' self-exemption from social determinism. Leslie S.'s distinction between "deviants," whose behavior is socially caused, and "true" perceivers, whose behavior is not socially caused, might be translated into the terms "negative deviants" (not like us; behavior socially determined) and "positive deviants" (like us; behavior exempted from social determination).

Where the respect and silencing values served to define P-O sociologists' relations with their subjects, social determinism and voluntarism seemed to define P-O sociologists' relations with their academic competitors. These competitors took two forms: psychologists and welfare-

state analysts, on the one hand, and mainstream sociologists on the other.

In the first case, social determinism serves to defend sociology's home turf, namely, the analysis of social forces. This is the only phenomenon over which sociology can claim a monopoly, and it is what distinguishes sociologists—and all who take a “sociological” approach—from other social scientists. This battle between the disciplines is as old as the disciplines themselves. At the turn of the century, for instance, Emile Durkheim argued that sociology and its “social facts” ought to be given priority over psychology and its individual “psychical facts”:

It is therefore in the nature of society itself that we must seek the explanation of social life. We can conceive that, since it transcends infinitely the individual both in time and space, it is capable of imposing upon him the ways of acting and thinking that it has consecrated by its authority. . . . [E]very time a social phenomenon is directly explained by a psychological phenomenon, we may rest assured that the explanation is false.¹²

Of more recent vintage is sociology's battle with the welfare state for control of social analysis. In the two decades after World War II, as the American welfare state grew in size and in popularity, sociologists shifted their focus accordingly: for instance, Alvin Gouldner, cited approvingly by Kathy and others in class, has described how grand theorists and hip deviance specialists alike adapted to the welfare state, its academic agenda, and its social service programs.¹³ The welfare state declared society's problems to be social but went about solving them by helping individuals, by treating the symptoms instead of the disease: if there is poverty in society, then give the poor people a little money; if there is drug abuse, then help addicts deal with their addiction. Social problems were thus translated into personal problems. Social scientists willing to shift with the times received full federal funding and produced studies deemed socially useful.

In the 1980s, however, the welfare state became increasingly unpopular: the greatest problem facing the federal government was held to be the budget deficit, and presidents were elected on a no-new-taxes platform. Politically progressive sociologists may find neoconservatism anathema, but they are finding the anti-welfare-state climate surprisingly conducive to the reassertion of their professional domain, indeed to the reclaiming of their subject matter, the analysis of society, from state control.¹⁴ By insulting social service workers and their individualist analyses, we in the P-O class were at the same time reaffirming sociologists' rightful role as the primary analysts of the social world.

While the value of social determinism defines sociologists as against psychologists and welfare-state social analysis, the value of voluntarism defines P-O sociologists as against other sociologists. P-O, it should be noted, is not a mainstream sociological method. It has a fine pedigree, from anthropological ethnography to William-Foote Whyte's classic sociological work *Street Corner Society*, but it is not widely taught or practiced, and it is not yet treated on a par with, say, comparative-historical methods, much less with survey methods. In the current era of a fractionated or “multiple paradigm” sociology, minority viewpoints can achieve a certain degree of legitimacy, if only they can organize themselves communally and claim recognition as a new subfield.

Nine of twelve students in this P-O class said they hoped to continue doing P-O work in the future; two students said they didn't know, and one said it would depend on the project. The professor said he was strongly committed to P-O methods, and repeatedly said he considered them the “prototype” for all social science. Thus the value that social determinism is dehumanizing helped create a community of P-O sociologists by setting this group apart from all other sociologists. It was not important whether this actually did set this group apart from other sociologists; for instance, some hermeneuticians who use non-P-O methods also consider determinism dehumanizing; and the P-O class itself was willing to invoke social determinisms quite frequently. What is more important is that class members treated the value as setting them apart, treated one another as fellow community members, and treated their subjects, at least their sympathetic subjects, as deserving of respect.

The best example of this value, and its use as a communal boundary mechanism, came when I presented my first set of written field notes to the class. I managed to violate the value in every way possible, and the class's response was an emotional counterattack. My field notes focused on Carol's first presentation, and my first mistake was to describe Carol's approach as “touchie-feelie,” not once but twice. However, nobody mentioned this until I mentioned the term, halfway into the discussion of my project. Even then the class objected politely; Kathy said the term sounded “pretty pejorative” and Josh suggested I use the term “affective” instead. It was only after Carol said she “took offense” at this and other aspects of my description of her that the class started “emoting” (Leslie S.'s word). Ann R. called my field notes “typically masculine”; Kathy said, “the whole way you approached [your subject] was really pejorative”; Michael accused me of feeling there's only one right way of doing sociology. Ann F. drew the broader conclusion: “You objectified us and we're responding in a very emotional way, because I

did respond to [your field notes] emotionally. But it made me get very emotional about what I was going to have to do to my own subjects."

In other words, I think, I violated the idea of community by insulting a classmate; I violated the value of voluntarism by objectifying the least socially distanced subjects possible, namely my classmates; and I violated the link between value and community by showing that even P-O work could lead to the objectification and dehumanization of one's subjects. In the spirit of self-reflexiveness that P-O study encourages, I take this as evidence that the class was undergoing a process of community-building, one of the elements of which was the value of voluntarism.

The two interests that define P-O sociologists' relations with their academic competitors are even more clearly contradictory than were the field-site interests of political correctness and information retrieval. On the one hand, the P-O sociologists want to rally with other sociologists in defense of their social determinism. On the other hand, they want to differentiate themselves from other sociologists over the same issue. In sum, then, there are two pairs of conflicting interests and two pairs of conflicting values.

CONCLUSION

I have tried to argue that scientists' values and interests are complex, varied, and even mutually contradictory at times. The source of these contradictions is that scientists operate in several arenas at once. In the academic sphere, they have a variety of competitors with whom they are constantly battling; in the research sphere, they have to determine their relations with their subjects. The balance between these arenas varies from field to field. In the natural sciences, the subjects generally don't talk back (though sometimes their human supporters do). In the "harder" social sciences, such as macroeconomics, the relation between analyst and analysand has become so formalized and abstract that it can almost be ignored.

Participant-observer sociology is at the opposite end of this spectrum. The immediate and relatively intense contact at the P-O field site forces researchers to negotiate relations with their subjects and often creates strong sympathies. The academic arena no longer appears to be the only one, or even necessarily the most important one. Thus the contradictions within and between field-site interests and academic interests are particularly clear in P-O sociology. Moreover, the class I studied, with a progressive and sensitive attitude toward its subjects, and with constant encouragement from the professor, paid close attention to field-site relations. In addition, I focused on the class's verbal interactions, not on the final written products, where conflicts and debates

are generally less visible.¹⁵ For these reasons, my case study was especially well suited to the analysis of competing and conflicting interests.

However, the basic analysis may be widely applicable. With respect to their subjects, all scientists have political interests, even if they consist largely in the apolitical support of the status quo; and they also have an information interest in maintaining good relations with their subjects. With respect to their colleagues, all scientists have career interests and hope to carve out a niche for themselves academically; and they all have communal interests that their field, their subfield, and their specialty will all receive funding, achieve prominence, and prosper. It would be interesting to see, in other case studies, whether these interests are linked to when social scientists listen to their subjects, and when they deem their analyses to be deluded; when they grant their subjects free will, and when they deem their behavior to be socially determined.

AFTERWORD: SHARING ONE'S WRITINGS WITH ONE'S SUBJECTS

Sharing one's field notes and final paper with one's subjects recommends itself for at least four reasons:

- It allows the subjects to correct any mistakes the researcher might have made.
- It can create a sense of cooperation between subjects and researcher in the mutual quest to understand the subjects' social world.
- It empowers the subjects by making them active members in their own analysis.
- It keeps the writings "clean," that is, it keeps the subjects' possible reactions in the researcher's mind as the writings are being written.

Unfortunately, these are also the very reasons *not* to share written analyses with one's subjects.

Correcting mistakes. There are an infinite number of mistakes to be made in writing about a field site: facts that subjects may feel are taken out of context; imputed thoughts that subjects may deny altogether; generalizations for which subjects can produce counterexamples; and so on. Subjects may help you correct these. But subjects generally do not have the detached eye of a proofreader. Their "native" status that makes them qualified to correct the analysis also makes them interested parties. If they find mistakes, and they are sure to, they may suspect the researcher of being systematically mistaken about the field site. The researcher is already a suspicious character, someone who is studying a particular site for heaven knows what reason, and is going to report

back to heaven knows whom. If the subjects feel that the information being collected is off the mark, the researcher's project may take on a sinister aspect in their eyes.

This, I am unhappy to report, is what happened in my case. The graduate sociology class I studied picked up on a few fairly small mistakes—if I say so myself—in my first set of field notes as indicative of a “masculine” and unsympathetic perspective, “masculine” being perhaps the ultimate insult in the Berkeley sociology milieu. They went on to attack me and my project for the better part of an hour, and I was saved from more browbeating only by the ringing of the class bell. More importantly, the class's attitude toward me changed from “wait and see” to “we waited and saw.” The damage was not irreparable, largely because I was friends with most of the class, but the class's disapproval lingered throughout the semester and made my project both more difficult and less pleasant.

Perhaps my outlook *was* unsympathetic; for certain, I made a number of insensitive mistakes in my field notes. But the subjects were unable simply to correct mistakes. They were upset by the very existence of mistakes.

Cooperation in analysis. It may come as a surprise to many P-O researchers, but their subjects are rarely interested in theoretical analyses of their lives. At best sociological analysis is generally idle speculation; at worst it is disruptive meddling. This seems to be true even when the sociologist is trying to help the subjects attain some goal. Alain Touraine's interventionist methodology, for instance, had to struggle to overcome the mistrust and resistance of subjects who considered the researcher a threat.¹⁶

So when subjects read a researcher's writings, they tend to skip quickly over analytic portions and focus on the important things: who gets quoted, and how often; who gets insulted or praised. The researcher's goal of analysis simply is not a high priority in the subjects' lives, even, as I found, if the subjects are sociologists. It was very difficult to get the class I studied to discuss the theoretical issues that were important to my research. In part this was because of the mistakes I had made in my field notes, and in part this was because they did not feel qualified to discuss these issues. However, the subjects went further and denied that my theoretical interests in the sociology of knowledge were even applicable to them. I stubbornly overruled them and continued the study from this perspective, but it was not one that the subjects felt was important to them. I was pleased when a few of the concepts that I had proposed were adopted, usually as jokes, by my subjects, but they were not interested in analytic cooperation.

Empowerment. It is absurd for social scientists to debate the subjects' situation without letting the subjects speak up for themselves. The ideal, then, is to give the subjects a voice in the academic world. By sharing one's writings with them, the researcher can thus show the subjects the debate and incorporate their contributions to the debate. Unfortunately, this attempt at empowering the subjects can also have the opposite effect. Subjects may not feel qualified to participate in academic debates. By inviting them to participate, the researcher may unintentionally rub their noses in their feelings of inadequacy, under-education, or lower social status. This is especially likely when the researcher is young, white, middle-class, and college-educated and the subjects are not. The complex sentences and fancy words in the researcher's writings may remind the subjects of their inability to communicate this way.

Even when the researcher and subjects are on the same social and educational level, as in my case, empowerment may have complications. In the first set of field notes I distributed to my subjects, and in the first draft of my final paper, I tried to lay out the field of the sociology of knowledge for their comments. I succeeded only in confusing everybody. The subjects did not respond by feeling inadequate, as less educated subjects might have; instead, they responded by calling the presentation unclear. Indeed it was, partially because of my own shortcomings, partially because of the difficulties in boiling down a whole sociological subfield to a single short presentation. In any case, the subjects certainly were not empowered by the invitation to participate in their own analysis.

Clean writings. Researchers should not intentionally insult their subjects. They should not refer to them by their physical or behavioral idiosyncrasies. They should not make jokes at their expense. In this sense it is a good idea to keep the subjects' potential reaction in mind when one is writing about one's field site. Beyond these simple matters of good taste, however, the subjects' potential reaction can have a chilling effect on a researcher's writings, both on field notes and in final papers.

In writing field notes, the researcher includes all sorts of details, many of which might seem irrelevant: how somebody dresses, how old someone appears to be, what people eat for lunch, and so on. At the beginning stages when the focus of the project may not yet be settled, it is impossible to know which of these details will later seem very significant, and which might otherwise be lost to the recesses of memory if not written down. Yet these impressions may offend the subjects: if you estimate someone's age as forty and he's only thirty-five, or if you

note how deferential someone is toward her boss, and she doesn't feel deferential. Knowing that the subjects are going to read one's field notes can make a sensitive researcher second-guess every detail. It is hard enough to write field notes regularly without the additional pressure of trying to keep them clean.

As for final papers, these suffer both the field notes' problem of offending details and the second problem of objectifying analyses. Every social-scientific analysis objectifies its subjects to a certain extent. Some are quite blatant in the way they obliterate the subjects' individuality, even their free will. Others are more sensitive to their subjects, but still may offend them by taking them as typical, as representative, or as telling. Subjects, by and large, think of themselves as individuals, with more or less special characteristics. To reduce them to a single aspect or a limited bundle of attributes is to ignore a large part of who they are, or who they think they are.

In my case study, for instance, I treated the subjects as hypothesis-makers and hypothesis-judgers, as sociologists, and as P-O sociologists. This is not necessarily how they would have treated themselves, and I knew when I was writing my paper that they would object to this characterization when they read it. It gets quite tricky, then, to find the balance between censoring one's paper to avoid offense and making a social-scientific argument that one feels is justified. I suggest in my paper that sociologists' response to this dilemma hinges on a variety of competing and conflicting interests. In my own case, I felt secure enough in my subjects' friendship, and in my own field-site role as a humorously offensive devil's advocate, to risk offense and objectify my subjects. Other researchers may try to reduce the objectification or refuse to show their papers to their subjects.

In sum, then, the idea of sharing one's writings with one's subjects is fraught with problems. I am not suggesting that researchers refuse to show their writings to their subjects, merely trying to make the issue complex. My view is that field notes should not be shared; they are too raw and too liable to offend. However, I feel that subjects have a right to see and comment on at least one draft of the researcher's final paper.

Conclusion