

PART 3 · NEW IMMIGRANTS / 133

Introduction to Part 3 / 135

- ✓ 7. A Maid by Any Other Name: The Transformation of "Dirty Work" by Central American Immigrants
Leslie Salzinger / 139
8. "Unlikely Heroes": The Evolution of Female Leadership in a Cambodian Ethnic Enclave
Shiori Ui / 161

PART 4 · FROM CLASSROOM TO COMMUNITY / 179

Introduction to Part 4 / 181

- ✓ 9. Mr. Henry Makes a Deal: Negotiated Teaching in a Junior High School
Leslie Hurst / 183
10. Coming Together: An Interactive Model of Schooling
Nadine Gartrell / 203

PART 5 · RESEARCHING THE RESEARCHERS / 221

Introduction to Part 5 / 223

11. The Politics of Prevention: Ethnographers Combat AIDS Among Drug Users
Kathryn J. Fox / 227
12. Convincing Sociologists: Values and Interests in the Sociology of Knowledge
Charles Kurzman / 250

CONCLUSION

13. The Extended Case Method
Michael Burawoy / 271

APPENDIX

14. Teaching Participant Observation
Michael Burawoy / 291

NOTES / 301

BIBLIOGRAPHY / 331

INDEX / 345

PREFACE

This was almost the preface to a cookbook instead of a collection of ethnographies. The volume grew out of a graduate sociology seminar in participant observation. When the seminar ended, we decided to continue meeting informally over dinner to revise and rework our research projects into a book, and the menu became a barometer of our progress. When the writing was going well, we ate take-out. When we were having difficulty, however, the meals were sumptuous, and there were times when some of us wistfully considered abandoning the social sciences in favor of the culinary arts. But we helped each other through those rough spots, and two years (and many meals) after entering the field, we emerged with this collection of essays.

In truth, when we began the seminar in fall 1988, we were probably more qualified to be cooks than ethnographers. Only one of the twelve, all of us graduate students at the University of California at Berkeley, had ever done participant observation before. We had merely two weeks to select our sites and begin research, a time constraint that permitted neither a leisured nor circumspect entry into the field. Moreover, during the course of the seminar, the scope and direction of many of the projects changed. At the end of the semester most of us felt that our projects were far from complete, but by that time we had become committed to our research.

So we agreed to meet weekly at the home of Michael Burawoy. Perhaps it was the informality of the setting, perhaps the conviviality of the gatherings, or perhaps it was a growing interest in and dedication to one another's work. Whatever the reasons, we were fortunate to be graced with a highly collaborative and collegial working relationship. This spirit had been evident during the seminar, but it deepened over

the course of our evening meetings. Rather than the posturing and jockeying for status that commonly characterize seminars, it was the norm for us to present unfinished work, to offer ideas and theories only half-formed, which in turn allowed us to benefit from each other's rigorous and constructive feedback.

Perhaps those inclined toward participant observation as a technique of research are also more inclined to a participatory approach to learning. Certainly there was a congruence between our own interactions and those we had with our subjects. As Burawoy argues in chapter 1, the process of working alongside those we study necessitates a dialogue between observer and observed. While much sociological work may appear seamless—the researcher invisible behind the scenes—there is in fact always a relationship between ourselves as researchers and our subjects. Participant observation brings this conversation to the fore. For that reason, he argues, participant observation is paradigmatic of all social science and not merely a quaint technique at the margins.

Participant observation also generates rich and detailed data about everyday life. These studies, for example, attempt to convey the flavor and texture of life in the San Francisco Bay Area and specifically, as the book's title suggests, how people's daily lives are disrupted, threatened, and impinged upon by forces outside their control. In his discussion of the extended case method in chapters 2 and 13, Burawoy explains how we were able to extrapolate outward from our particular sites to explore the more general themes of power and resistance, and how from there we were able to reconstruct existing explanatory theories.

Thus, the significance of the studies resides in both their particularity and their generality. Chapters 3 through 12 deal with five features of the modern metropolis—social movements, work organization, immigrants, education, and knowledge. Each chapter concludes with methodological reflections that call attention to distinctive aspects of the research process. In chapter 14 Burawoy offers an account of his experience as teacher of the seminar.

Sadly, two members of our seminar, Carol Heller and Ann Robertson, couldn't continue with us. We missed them as our collaboration developed, and we thank them for their many contributions during the first semester. We also thank Nancy Scheper-Hughes for an inspiring talk about her field work in Brazil; Judy Stacey, Rick Fantasia, Nina Eliasoph, Paul Lichterman, Carol Stack, Bob Freeland, Ida Susser, and one anonymous referee for their comments and help on different parts of the manuscript; Amy Einsohn and Andrew Alden for sharpening the essays by their copyediting; and Naomi Schneider, whose enthusiasm for the project gave us the confidence to pursue it to the end.

Introduction

Michael Burawoy

This book examines the way in which everyday life in the modern metropolis is continually eroded, distorted, overpowered by, and subordinated to institutional forces that seem beyond human control. In part 1 Joshua Gamson and Josepha Schiffman thematize the importance of power in new social movements (an AIDS activist group and two peace groups), particularly the way civil society is not outside but traversed by regimes of micro-power. In part 2 Alice Burton and Ann Arnett Ferguson criticize the exclusive focus on hierarchical control in studies of work. Instead they underline the importance of horizontal ties for creating the conditions of resistance to bureaucratic control in a welfare agency and for maintaining alternative organizations such as a baking cooperative. In part 3 Leslie Salzinger and Shiori Ui downplay what is conventionally stressed in the literature on immigrants, namely their cultural background. Instead they highlight the way in which state and economy have shaped and limited strategies for occupational advancement among refugees from Central America and Cambodia. In part 4 Leslie Hurst shows how the separation of family, school, and classroom contributes to the breakdown of teaching of lower-class teenagers, and Nadine Julius shows how restoring connections between teacher, parent, and student can improve education. In part 5 Kathryn Fox shows how the state, through its laws and regulations and through its control of funds, constrains ethnographic outreach work among drug users, while Charles Kurzman studies the autonomy of academic ethnographers to adopt different values and interests.

All the studies examine how power and resistance play themselves out in social situations that are invaded by economic and political systems. They highlight what Jürgen Habermas calls the colonization of

the lifeworld by the system.¹ In the face of commodification through money and administration through power, everyday life loses its autonomy and shared purpose. But their analyses do not simply record this colonization, they also explore resistance to it in the forms of negotiated orders, alternative institutions, and social movements.

If the students share a substantive theme, they also share a common research technique—the technique of participant observation, or what some call the art of ethnography. Participant observation is usually viewed as one among a number of techniques of social research—archival, survey, demographic, and experimental. What distinguishes participant observation is the study of people in their own time and space, in their own everyday lives. It is often referred to as natural sociology, studying subjects in their “natural habitat” as opposed to the “unnatural” setting of the interview or laboratory.²

According to convention, each research technique has its own advantages and its own distinctive biases, ways in which it distorts the reality it seeks to comprehend. Thus, the advantages of participant observation are assumed to lie not just in direct observation of how people act but also how they understand and experience those acts. It enables us to juxtapose what people say they are up to against what they actually do. The dangers of participant observation are said to derive from the same source as its virtues. Too close contact with the participants can lead to loss of objectivity or to contamination of the situation. The problem of objectivity is compounded by the problem of validity, namely, intensive research limits the possibility of generalization. It is sociology’s “uncertainty principle”: the closer you get to measurement on some dimensions—intensity and depth—the further you recede on others—objectivity and validity.

As a technique of research there are courses on participant observation as well as excellent books that outline ways of countering or compensating for its pitfalls.³ They take the aspiring participant observer through entry into the field, being there, and finally leaving. They describe field notes and how to analyze them systematically. They distinguish between overt and covert participant observation, conducted in open or closed settings. They consider the range of membership roles from full participant, at one extreme, to participant-as-observer to observer-as-participant to complete observer at the other extreme. They discuss the ethical dilemmas of entering the lives of others and then of broadcasting the information collected there. This book is different. It is not a cookbook; it does not provide any recipes. If it demonstrates, it does so by example. Each study presents its own unique constellation of problems, and only in their afterwords do the authors discuss the dilemmas they confronted in their studies.

This book is intended less as a contribution to the technique of participant observation and more as a contribution to the methodology of social science. We seek to unchain ethnography from its confinement as a quaint technique at the margins of social science.⁴ In our eyes participant observation is the paradigmatic way of studying the social world, and from this point of view anthropology becomes the paradigmatic social science. By “paradigmatic” I do not mean that participant observation is the only technique or necessarily the most appropriate technique of social research, but rather that it best exemplifies what is distinctive about the practice of *all* social science. Situated at the crossroads of the humanities and natural sciences, social science combines both understanding and explanation. Understanding is achieved by virtual or actual participation in social situations, through a real or constructed dialogue between participant and observer, or what we call the *hermeneutic* dimension of social science. Explanation, on the other hand, is the achievement of an observer or outsider and concerns the dialogue between theory and data, or what we call the *scientific* dimension.⁵

From this standpoint there are two reductions we seek to avoid. The first is the positivist reduction that reduces social science to the natural science model and suppresses the hermeneutic dimension. In this view the interaction of participant and observer is a source of “bias”—a nuisance to be minimized rather than the distinguishing feature of all social science, indeed without which there could be no social science. Rather than allowing us to regard ourselves as inextricably part of the world we study, positivism demands we aspire to the position of the neutral outsider. The second reduction we seek to avoid is the humanist and, more particularly, the “postmodern” suppression of the scientific dimension. Here scientific theories are exposed as simply another world view, this time that of the observer, in no way superior to the world view of the participant. Social science is reduced to a dialogue between insider and outsider aimed at mutual self-understanding—“the comprehension of self by the detour of the comprehension of other.”⁶ In the words of Alain Touraine, sociology becomes a “discourse that interprets other discourse, an ideology criticizing other ideologies, all the while remaining blind to effective behavior and situations.”⁷ Explanation loses any distinctive meaning. Defending this reduction, Clifford Geertz says our business is limited to the “understanding of understanding.”⁸ This leads social science down the path of textual analysis, where it merges with literary criticism.

For us participant observation has the distinct virtue of highlighting the limitations of both forms of reduction. It brings together both the perspective of the participant who calls for understanding and the perspective of the observer who seeks causal explanation. It necessarily

combines both hermeneutic and scientific moments and thereby casts exaggerated light on the tensions and dilemmas that are definitive of all social science.

Let me deal with the hermeneutic axis first, that is, the problem of understanding. Like natural scientists, social scientists face the task of interpreting data. However, they differ from natural scientists in that the data are themselves constituted by a community—the community of participants. In the social sciences there are not one but two interpretive tasks, what Anthony Giddens calls the “double hermeneutic.”⁹ Data are the preconstituted theories and concepts of participants, and their meaning can be gauged only in relation to the context of their production. What respondents say in interviews is shaped by the context of the interview. Whether a death is counted as a suicide depends on how and in what circumstances the death was registered. This context dependence of meaning, or what Harold Garfinkel calls indexicality, requires careful examination of the situation in which knowledge is produced, which in turn requires actual or virtual participation in the lives of those one studies.¹⁰

But how does one conduct such a situational analysis? In order to appreciate the self-understanding of the participants, some advocate that observers strip themselves of their biases in order to become like their subjects. Ethnographic work is then a feat of empathy in which we immerse ourselves in the community we study. Others argue the opposite: that objectivity comes only from distance. Herbert Gans, for example, embraces the image of the participant observer as a marginal person—detached and emotionally removed.¹¹ From a different standpoint Geertz makes the same point: “Understanding the form and pressure of, to use the dangerous word one more time, natives’ inner lives is more like grasping a proverb, catching an allusion, seeing a joke—or as I have suggested reading a poem—than it is like achieving communion.”¹²

We advocate neither distance nor immersion but dialogue. The purpose of field work is not to strip ourselves of biases, for that is an illusory goal, nor to celebrate those biases as the authorial voice of the ethnographer, but rather to discover and perhaps change our biases through interaction with others. Thus, an “I-You” relation between observers and participants replaces a “we” relation of false togetherness and an “I-they” relation in which the I often becomes invisible. Remaining on the sidelines as a marginal person or positioning oneself above the “native” not only leaves the ethnographer’s own biases unrevealed and untouched but easily leads to false attributions, missing what remains implicit, what those we study take for granted. The practical consciousness of everyday life—whether of oneself as social scien-

tist or of those one studies—contains a great deal that is tacit, what Peter Winch calls nondiscursive, and therefore not explicitly articulated.¹³ The pursuit of nondiscursive knowledge, that is, knowledge that is assumed rather than unconscious—both the observer’s as well as the participant’s—calls for participation but not immersion, observation but not marginality. Once more this privileges participant observation.

Dialogue between participant and observer poses the question of power. Insofar as the relationship between participant and observer is that between power unequals, to that extent the dialogue is distorted. Recently anthropologists have become sensitive to the way power differentials affected the study of colonial societies.¹⁴ Classical anthropologists too easily bracketed the domination that made their field work possible. Coming in under the auspices of a colonial regime to study preliterate societies, anthropologists were not compelled to be responsive to the interests of those they studied—people who would never read or even be aware of the books that were written about them. Following recent trends in anthropology, we too are sensitive to inequalities of power between participant and observer. But being sensitive to power inequality doesn’t remove it. Although many of us had considerable loyalty to the people we studied, and revised our papers in the light of their comments, nevertheless in the final analysis what we wrote was outside their control. This is not to justify either complacency or paralysis but to recognize the limits of responsiveness. As Michel Foucault has taught us, social science as we know it today rests on an irreducible level of domination.¹⁵

I now turn to the scientific dimension of social science, the dialogue between theory and data whose goal is explanation. Participant observers can be distinguished from those whom they study by their participation in the academic community. Indeed, the academic community gives them the reason for being field workers in the first place. Although social science depends on lay concepts as the foundation of its analysis, it also tries to go beyond the lived experience of participants. We are interested not only in learning about a specific social situation, which is the concern of the participant, but also in learning from that social situation.¹⁶ In contrast to the participants, we want to be able to make causal claims that have validity beyond the situation we study. It is the task of methodology to explicate methods of turning observations into explanations, data into theory.

What methods are available, then, for moving from the data of participant observation to the level of social theory? One can attempt to make generalizations across different social situations, looking for what they have in common. This is a process of induction associated with grounded theory. Alternatively, one can try to uncover the tacit under-

standings, the taken-for-granted knowledge that underlies competent performances of those we study. We can set about revealing the non-discursive knowledge that makes social action at all possible. This is the project of ethnomethodology. Both grounded theory and ethnomethodology are methods that use participant observation to develop micro-sociology.

* While we have drawn on these microsociologies, we are more concerned to examine what they bracket—the institutional context that shapes and distorts what happens in the lifeworld. In this connection there are also two approaches. On the one hand, what we call the interpretive case method regards the micro context as a setting in which a particular “macro” principle, such as commodification, rationalization, or male domination, reveals itself. The uniqueness of each situation is then lost as it becomes an expression of the whole, of some essential defining feature of the totality. While we are concerned with a single principle, that of domination and resistance, nevertheless we also pursue a different strategy we call the extended case method, which examines how the social situation is shaped by external forces, or, in the terms of C. Wright Mills’s sociological imagination, tries to connect “the personal troubles of the milieu” to “the public issues of social structure.”¹⁷ The extended case method thus bursts the conventional limits of participant observation, which stereotypically is restricted to micro and ahistorical sociology. (If one wants to do research of a macro or historical kind, one had better leave the field site for the archive, the survey research center, or the institute of demography.) We challenge the conventional correspondence between technique and level of analysis and argue that participant observation can examine the macro world through the way the latter shapes and in turn is shaped and conditioned by the micro world, the everyday world of face-to-face interaction.

* But such an outward extension calls for a particular mode of theorizing, described in chapter 2. In traditional ethnography, participant observation tends to produce detailed descriptive accounts that have no obvious relevance beyond the immediate situation. When traditional ethnography makes good its theoretical claims, they usually concern what Erving Goffman called the “interaction order” in settings of “co-presence.”¹⁸ Such theorizing emerges directly out of the data. It is very different from the extended case method, which is realized not through induction of new theory from the ground up but through the failure and then reconstruction of existing theory. But what existing theory? We search for theories that highlight some aspect of the situation under study as being anomalous and then proceed to rebuild (rather than reject) that theory by reference to the wider forces at work, be they the state, the economy, or even the world system.

As the following chapter makes clear, our intent is not to reject bad theories but to improve good theories. We don’t believe there is a final truth which once arrived at gives incontrovertible insight. Nor do we start with a tabula rasa, as if social science begins with us. Rather we seek to place ourselves in a wider community of social scientists by taking the flaws of existing theory as points of departure. This is not a token recognition that perfunctorily appears at the beginning of an article, but a deep engagement with the ideas of others.

Thus, in our view participant observation is not only a paradigmatic technique for studying others; it also points to a distinctive way of understanding ourselves. The dialogue between participant and observer extends itself naturally to a dialogue among social scientists—a dialogue that is emergent rather than conclusive, critical rather than cosmetic, involving reconstruction rather than deconstruction.*

Reconstructing Social Theories

Michael Burawoy

In the social sciences the lore of objectivity relies on the separation of the intellectual product from its process of production. The false paths, the endless labors, the turns now this way and now that, the theories abandoned, and the data collected but never presented—all lie concealed behind the finished product. The article, the book, the text is evaluated on its own merits, independent of how it emerged. We are taught not to confound the process of discovery with the process of justification. The latter is true science whereas the former is the realm of the intuitive, the tacit, the ineffable, in short, the “sociological imagination.” In this chapter I try to break open the black box of theory construction by regarding discovery and justification as part of a single process.

One of the most appealing features of Glaser and Strauss’s exposition of grounded theory is the way they bring order to the process of discovering theory. They too deny that the only true scientific process is “verification.” They too refuse to insulate the “process of discovery” from the “process of justification.” But where Glaser and Strauss are concerned to discover new theory from the ground up, we on the other hand seek to reconstruct existing theory.¹

Grounded theory approaches social phenomena from the standpoint of their *generality*. Whether we study peace movements or AIDS activism, a baking collective or social workers, domestic workers from Central America or refugees from Cambodia, grounded theory treats each case study as a potential exemplar of some general law or principle that applies across space and time. Here, theoretical advance is the movement toward greater generality; that is, the inclusion of more phenomena under a single covering law. In pursuing generalizations, grounded

theory remains at the same level of reality. By contrast, the extended case method attempts to elaborate the effects of the “macro” on the “micro.” It requires that we specify some *particular* feature of the social situation that requires explanation by reference to particular forces external to itself.

But how does one decide what is particular and has to be explained? We look for what is “interesting” and “surprising” in our social situation. That is, we look for what is unexpected. Initially, it is not important whether our expectations derive from some popular belief or stereotype or from an academic theory. What is important is that to highlight the particularity of our social situation we self-consciously and deliberately draw on existing knowledge to constitute the situation as “abnormal” or “anomalous.” That is to say, in the first place we treat social phenomena not as instances of some potential new theory but as counterinstances of some old theory. Instead of an *exemplar* the social situation is viewed as an *anomaly*.

Grounded theory moves from substantive theory, developed for “an empirical area of sociological inquiry,” to formal theory, which pertains to a “conceptual area of sociological inquiry.”² For example, Glaser and Strauss studied dying as a nonscheduled status passage, and in developing substantive theory they compared hospital wards where patients died at different rates. To then develop the formal theory of status passage, they would compare dying with becoming a student or entering a marriage. They proceed from the ground up through a theoretically guided process of constant comparison to develop transhistorical and trans-spatial generalizations.³ We, on the other hand, move from anomaly to reconstruction. We begin by trying to lay out as coherently as possible what we expect to find in our site *before* entry.⁴ When our expectations are violated—when we discover what we didn’t anticipate—we then turn to existing bodies of academic theory that might cast light on our anomaly. But here, too, the focus is on what that theory fails to explain. The shortcomings of the theory become grounds for a reconstruction that locates the social situation in its historically specific context of determination.⁵

Rather than theory emerging from the field, what is interesting in the field emerges from our theory. Rather than seek ever more general theories that cover diverse sites, we move from our own inchoate conjectures to the existing body of literature in search of theories that our observations show to be anomalous. Rather than treating the social situation as the confirmation of some theory, we regard it as the failure of a theory. But failure leads not to rejection but to rebuilding theory.⁶

We, therefore, agree with Karl Popper’s critique of induction and verification.⁷ We also follow Popper’s own logic of scientific discovery in

emphasizing processes of conjecture and refutation. Where we depart from him is that we use counterinstances to reconstruct rather than reject theory. That is to say, instead of proving a theory by corroboration or forsaking a theory because it faces falsification, our preferred approach is to *improve* theories by turning anomalies into exemplars. In a sense we take Popper to his logical conclusion. Instead of abandoning theory when it faces refutation, we try to "refute the refutation" by making our theory stronger.⁸

If counterinstances are something not to be avoided but seized upon to reconstruct our theories, then one can go beyond Popper and think of falsifiability not simply as demarcating science from nonscience but as a criterion of theory choice. We look for theories that are refuted by our observations, but, of course, we don't choose any theory. We look for a theory or body of theory that we *want* to improve, a theory that is of interest, and then show how it is challenged by the social situation we are studying. This approach, therefore, leads us to strengthen preferred theories.

So far I have restricted myself to one sort of theoretical failure that prompts the reconstruction of theory—the existence of anomalies or refutations—but other types of failure call for reconstruction as well. *Internal contradictions*, highlighted by empirical studies, can also stimulate reconstruction. Alvin Gouldner's study of a gypsum plant led him to highlight a latent tension in Weber's theory of bureaucracy between promotion based on expertise and promotion based on loyalty, out of which he constructed his different patterns of bureaucracy.⁹ In the studies that follow, the focus is often on yet another kind of theoretical failure, namely *theoretical gaps or silences*. A given theory may fail to address an aspect of a particular empirical phenomenon that, once included, compels the reconstruction of theory.

The way we interrogate our field notes depends on whether our goal is to reconstruct existing theory or to discover new theory. Thus, Glaser and Strauss are very concerned to develop concepts, categories, dimensions, and sampling that are grounded in the data and reflect the data's complexity and richness: "In short, our focus on the emergence of categories solves the problems of fit, relevance, forcing, and richness. An effective strategy is, at first, literally to ignore the literature of theory and fact on the area under study, in order to assure that the emergence of categories will not be contaminated by concepts more suited to different areas."¹⁰ They ransack the data for emergent categories; the focus is on organizing and reorganizing, coding and recoding of data.

In pursuing theory reconstruction, on the other hand, we conduct a running exchange between field notes and the analysis that follows them. The conjectures of yesterday's analysis are refuted by today's

observations and then reconstructed in tomorrow's analysis. But there is a second running exchange, that between analysis and existing theory, in which the latter is reconstructed on the basis of emergent anomalies. Analysis, therefore, is a continual process, mediating between field data and existing theory.

Although we take a respectful view of existing bodies of theory, we don't mean to imply that one has to have read "the literature" before beginning field work. To be sure, knowledge of the literature is not the contaminating influence that Glaser and Strauss attribute to it, but neither is it a *sine qua non* of research. We can start, as I suggested above, with our own conjectures to highlight what is surprising, but there does come a time when we turn to some existing literature with the goal of improving it. We begin by experimenting with a number of different theories, perhaps, that highlight different aspects of the social situation as anomalous. Over time, if we are successful, we will home in on one particular theory that calls for reconstruction.

In the following discussion of the studies in this volume, I try to show how the process of theory construction evolved, in each case contrasting it with grounded theory. For those accustomed to thinking of the scientific process as a linear movement in which a theory or hypothesis is presented and then tested, my exposition will appear unflattering to the authors. On the other hand, to those who think of the scientific process as inscrutable, as tacit, personal knowledge, I hope to convey the logic underlying the extended case method; in chapter 13 I systematize that logic. In their afterwords, the authors reflect on the research process, highlighting those features they found particularly salient. Here I write as a participant observer of the seminar, describing how the authors made sense of their data—the way they built and rebuilt theories.

"NEW SOCIAL MOVEMENTS"

The literature on "new social movements" is almost the prototype of the generalizing mode. From Robert Park to Neil Smelser the goal has been to pursue the most general, transhistorical theory of collective behavior.¹¹ Not surprisingly, critiques of this generalizing mode have come from historically minded sociologists such as Charles Tilly and his collaborators, who have located social movements in specific contexts of urbanization and industrialization.¹² More recently European theorists, always more sensitive to historical context, have sought to specify the peculiarities of what they call "new social movements" (NSMs), which mobilize primarily middle classes to defend an augmented civil society against state and economy.¹³ In this volume Gamson and Schiffman

take this literature as point of departure, using their case studies to highlight specific silences in the way theories of NSMs treat power.

Joshua Gamson began his study of the AIDS activist organization ACT UP by walking into one of its meetings. The scene, with its progressive left politics, was quite familiar and unexciting, but the actions the members planned and, even more, the ones they fantasized captured his interest. Members were spending a great deal of energy devising ways to reappropriate symbols of blood and death—symbols the media had mobilized against people with AIDS (PWAs) and gays. Gamson noted how these symbolic dramas were attempts to reclaim power through an inversion of their meaning. But he was puzzled: Was this gay activism or AIDS activism? Who was the audience for any given action? He went to a national conference of AIDS activist groups in Washington and returned convinced that these strategies to reclaim power were typical of groups trying to combat the stigmatization of PWAs and gays.

But what was the significance of this distinctive symbolic politics? Gamson became a man in search of a literature and a theory that would shed light on this peculiar form of expressive politics. He turned to theories of the new social movements. ACT UP showed all the marks of a new social movement, yet theories failed to illuminate what he was observing. In part this was because what was new about the NSMs remained obscure or unconvincing. His first insight was to distinguish between movements oriented to strategy and those oriented to identity. Certainly both threads could be observed competing for prominence in ACT UP, but why this concern for identity? At this point he began to focus on the way gays and PWAs confronted mechanisms of domination, particularly their invisibility. They had to face strategies of “normalization,” power exercised over everyone by labeling a particular group as deviant. Here lay the dilemma faced by ACT UP: open resistance drew attention to stigmatization and thereby reinforced it.

Having extended out in this way, the explanation stalled once more. After all, normalization, as Foucault insists, can hardly be regarded as new, yet the resistance was new. What was it about the contemporary political order that accounted for this particular response? Casting around for comparison groups, Gamson decided that in the past stigmatized groups had resisted differently because normalization was coupled to the state. He argued, therefore, that the distinctive symbolic politics of ACT UP was a response to a specific form of normalization in which the state became less visible.

But at this point he halted. Because of his highly unusual experiences in the field, Gamson was uncomfortable with turning his extension into a generalization: that what new social movements shared, what

made them “new,” was their response to new forms of invisible domination. With only one case to go on, he did not feel able to define the scope of his reconstructed theory of NSMs.

Joseph Schifman found herself in a different quandary when she began her study of SANE/Freeze. Her goal, as she laid it out in her original proposal, was to examine the way peace organizations resolved internal conflicts—did they resolve their differences through peaceful, democratic means or in a coercive, authoritarian manner? However, she was not anticipating what she found: a moribund organization that was primarily involved with the Democratic election campaign. There was a limited, overworked, and somewhat inexperienced staff and a board that had nearly dissolved. Peace issues were rarely discussed. Her project inevitably shifted to examining this apparent state of decay.

The most obvious explanation was that this very active and vibrant social movement had succumbed to bureaucratization—it had simply played out Michels’s iron law of oligarchy. Delving into the history of SANE/Freeze, however, suggested that the organization had gone through similar periods of decline in the past. So she countered the argument that all social movements have a similar “natural history” with an argument specific to SANE/Freeze, namely that its radical goals were at odds with its reformist strategies. Furthermore, its futuristic and utopian aims of international harmony provided the basis of a vibrant social movement only in the wake of concrete experiences of the dangers of war—after a nuclear scare, fallout episode, or international conflagration. But such a theory could only be evaluated by undertaking a historical analysis, and she was, at least for the moment, confined to field work.

This was the unsatisfactory state of affairs at the end of the semester. Schifman returned to the field after Christmas to discover that her hitherto moribund organization had now sprung to life. The national elections were over and the organization had joined the coalition of the Bay Area Peace Test (BAPT), which was planning its annual spring action at the Nevada Test Site. Schifman now became interested in the dramatic differences between the politics of SANE/Freeze and BAPT and how both contrasted with the “consciousness-raising” politics of Beyond War (BW), another peace organization with which she was familiar. She felt as though she was beginning her project all over again.

Why should the peace movement adopt such different and unconventional strategies for achieving its goals? Like Gamson, Schifman turned to the literature on new social movements, but she problematized it in a slightly different way. Where he saw new social movements as a response to the invisibility of domination and strategies of normalization, she saw them as responding to the penetration of the state,

particularly the nuclear state, into private realms. In this view, NSMs define themselves as carving out arenas of civil society autonomous from the state. But how does this explain the very different strategies of BAPT and BW? This silence in the literature became her puzzle.

She turned to the conceptions of power that informed the strategy and organization of BAPT and BW. Both were opposed to institutional forms of politics, but for very different reasons. BAPT saw power as a relationship of domination that pervades all arenas of social life. Participation in state politics only reproduces that domination. BW, on the other hand, saw power as a neutral energy that operates outside institutions of the state. International peace will come about through personal transformation, through "right thinking."

Schiffman goes on to conjecture that similar bifurcated tendencies can be found in other new social movements, such as the ecology and women's movements. In declaring themselves to be organizing in "civil society" and opposed to institutional politics, they turn toward anarchistic confrontation or consciousness-raising. Thus by locating the peace movement in its context of determination, namely the state, Schiffman reconstructs the theory of NSMs and thereby sheds light on other contemporary movements.

REORGANIZING PRODUCTION IN A SERVICE ECONOMY

Everett Hughes's pioneering work on occupations represents the best of the Chicago School's grounded theory. He saw his mission as drawing out commonalities between the most disparate occupations: "[W]e need to rid ourselves of any concepts which keep us from seeing that the essential problems of men at work are the same whether they do their work in some famous laboratory or in the messiest vat room of a pickle factory."¹⁴ Hughes was particularly interested in how professionals and service workers negotiated their relationship to their clients, how they dealt with problems of visibility, mistakes, and power. Although Hughes had great insight into the matrix of roles that defined work situations, he did not locate that matrix within its historical context. Nor did he examine how and why the matrix changed, over time and between workplaces, or the consequences that change might have for collective resistance. While both Burton and Ferguson in this volume examine the relations between producer and consumer, they do so by locating them within the changing context of the welfare state on the one hand and the contemporary capitalist economy on the other. Moreover, they are interested not only in the way these wider systems structure the possible forms of work organization, but also in the circum-

stances under which workers organize themselves collectively against those systems.

Alice Burton enlisted as an intern with a labor organizer of a union of local state welfare workers. She was interested in the relationship between union organizers and rank-and-file membership, but attending union meetings only brought her in touch with union activists and staff. Nevertheless a pattern began to emerge. Union officials often divided along the lines of the two groups they represented: social workers (SWs), who dealt directly with and often visited families with dependent children, and eligibility workers (EWs), who were confined to the office and processed applications for welfare. The EWs were more militant than the SWs but at the same time were more hostile to their clients. Nor was it difficult to see why. Social workers advanced their interests through the autonomy they exercised in dealing with their clients. They pursued their grievances on the basis of their professional prerogatives. The EWs, on the other hand, were subjected to intensive surveillance. They had no autonomy to manipulate the work context and so instead confronted the state in militant but usually unsuccessful strikes.

So what? Burton had an answer but no question. The division between EWs and SWs seemed quite "natural" until she turned to the literature. There she discovered a very different perspective, one that portrayed state workers as potential carriers of labor radicalism. According to James O'Connor, for example, welfare workers would forge an alliance with their clients based on their common interest in the expanded provision of social needs.¹⁵ Burton's observations challenged this argument: the militant workers, the EWs, turned out to be those most hostile to their clients, while the most sympathetic workers, the SWs, were neither militant nor radical.

What began as a question—why the EWs rather than the SWs were inclined toward collective mobilization—now became a puzzle: where did O'Connor go wrong? Burton's study suggested that O'Connor's mistake was to *impute* to state workers and clients a common interest in the expansion of welfare, an explanation that failed to take into account the way work organization structures interests. Therefore, Burton introduced an auxiliary hypothesis into O'Connor's theory, positing that day-to-day relations of EWs to their clients engender antagonism, not solidarity, just as the work autonomy of SWs engenders professionalism rather than militant unionism.

Her analysis led her to spend more time examining the workplace. In the second semester she undertook intensive interviews with welfare workers about their work and how it had changed over the last two decades. O'Connor's anticipations were based on the radical labor-community alliances that sprang up in the early 1970s. Why was O'Con-

nor correct then but wrong now? What had happened to the organization of work? Burton traced the history of the division of case workers into SWs and EWs. She discovered that, following the early struggles, the welfare system began carving up each client into a set of distinct problems and distributing these to different workers. By fragmenting welfare work and by fragmenting clients, the system effectively undermined any solidarity. The state's strategy of divide and rule, budget cuts, and union busting took the wind from the sails of labor radicalism. Thus, Burton's observations of grievance meetings led her to enter into a dialogue with and eventually reconstruct O'Connor's theory so as to accommodate and interpret the historically changing forces shaping public sector unions.

Ann Arnett Ferguson also studied collective organization of workers, but of a very different kind: a bakery cooperative producing healthy food, which she felicitously calls Wholly Grains. Cooperatives have often developed out of social movements, and Wholly Grains was no exception. Most of its members were white middle-class exiles from the student movement of the sixties for whom Wholly Grains, always teetering on the brink of bankruptcy, was more appealing than career paths in the state sector or the corporate world. Ferguson wondered what it meant to work in such a cooperative: was it any different from other small enterprise? She became interested in the "empowerment" of its members—their sense of fulfillment through participating in a community with others—a characteristic that the members valued in the collective and the standard by which they evaluated its operation. She was struck by how jealously they guarded collective decision making, yet she couldn't discover its mechanisms. Indeed, the secret of the operation seemed to be the absence of rigidly enforced rules. But this fluidity also made the collective vulnerable to usurpation by individuals in structurally powerful positions. Anticipating this possibility, the collective, somewhat half-heartedly, tried to organize a rotation of members through different "shifts." Certain groups, though, particularly the drivers on delivery, seemed exempt from rotation. Ferguson was also struck by the unwillingness of anyone to assume the role of coordinator. This, she determined, was at least in part due to the members' unwillingness to deal with the imbalance of power between shifts, such as between the "delivery" and "wrap" shifts.

After circling around this complex of problems, Ferguson looked for a theoretical point of entry—a key that would unlock what she was observing. She turned to the longevity of Wholly Grains, then thirteen years old. Certainly the literature suggested that cooperatives should be ephemeral organizations, unable to withstand the inevitable internal tendencies toward bureaucratization or external pressures to place

profit before collective decision making. So how had Wholly Grains survived? Without doubt these subversive pressures existed; indeed, they led to a never-ending succession of crises. Ferguson examined three such crises that threatened the collective: the use of white flour (which would compromise the political commitment to healthy food), the development of efficiency norms, and the abdication of production coordinators.

While these crises originated in external pressures, their solution depended on the mobilization of the energies of the collective's members. In this regard Wholly Grains relied on a continuing supply of workers with ingenuity and commitment to the collective. The collective could draw on people who circulated through the relatively dense network of cooperatives in the Bay Area, leaving one cooperative for another and taking with them their accumulated skills. Wholly Grains was also able to draw on the specialized resources at reduced rates made available through the same network of cooperatives. Finally, although market forces always threatened to turn Wholly Grains into a losing enterprise, it could protect itself against competition from mass production bakeries by providing for the specialized tastes of young urban professionals. What began as an anomaly, the longevity of Wholly Grains, could now be explained. External pressures, which continually threatened the existence of the cooperative, called forth countervailing responses from the membership, responses whose efficacy depended on resources available in the Bay Area economic environment.

Wholly Grains violated theories that regarded cooperatives as unstable organizations. In explaining its longevity, Ferguson reconstructed these theories by highlighting the importance of the specific economic and political context. Extending out thereby became a vehicle for making generalizations about the conditions under which cooperatives can reproduce themselves.

NEW IMMIGRANTS

The study of immigrants was another area in which Chicago sociology developed grand generalizations. Beginning with Thomas and Znaniecki's *The Polish Peasant in Europe and America*, studies of the effects of industrialization on immigrants to the city deployed the theory of social disorganization. This was further generalized to all urban populations in Louis Wirth's classic summary statement, "Urbanism as a Way of Life," and from there found its home in theories of deviance, delinquency, crime, and youth cultures. In developing the theory of social disorganization, Thomas, Znaniecki, and their followers lost sight

of the specific political and economic context within which communities were forged, both in Chicago and their place of origin.

Leslie Salzinger's and Shiori Ui's studies of new immigrants in California downplay social disorganization and focus on responses to the economic and political context of the United States. Not their countries of origin but the distinctive array of economic opportunities, which is in part shaped by their political status as immigrants or refugees, defines what new immigrants have in common and distinguishes them from earlier waves of immigration. However, when it came to comparing the different responses among immigrants, Salzinger emphasized their institutional connections here in the United States, whereas Ui stressed their different class background and political status in the country of origin.

Salzinger attached herself to two job-distribution cooperatives for Central American immigrants. She had intended to use these as sites to study how oppressed peoples develop common identities. Would the clients see themselves as Central Americans, as immigrants, as women, as poor, and with what consequences for collective mobilization? At first she found few signs of the degree of organization she anticipated. Central American women came to the centers to find domestic work and that seemed to be all. Although Salzinger noted differences between the two co-ops—the style of job distribution, the attitude of the staff toward immigrants, the affiliations of the job center—she couldn't fathom their significance. At this point she began working with her first "extension," following Sassen in locating immigrants within world economic changes that created the demand for menial domestic work.¹⁶

But still she regarded the women, first and foremost, as immigrants. She therefore couldn't make sense of the endless discussions of the ins and outs of domestic work at one of the co-ops. Frustration with her whole project triggered an insight that transformed the research. She began to recognize a self-conscious creation of work identity in all the talk about cleaning. This had been difficult for her to appreciate because she considered domestic work demeaning. She had assumed that it was simply the only job these women could find. Listening to them she realized that there was more to domestic work than met the eye. As an occupation it had some virtues: autonomy and flexibility, and indeed a certain satisfaction in a job well done. The switch from "immigrant" to "worker" led her to a second extension, an examination of the historical changes in the character of domestic work from servant to wage laborer.

Through the lens of "work" the two co-ops took on a new significance. At first it seemed that one catered to newer immigrants who had few skills, didn't speak English, and were paid particularly low wages,

while at the other co-op women had generally been in the United States longer, constituted a more developed occupational community, and even organized English lessons and training sessions for their members. But social characteristics of the women couldn't explain the differences between the two co-ops. Instead Salzinger focused on the limited economic opportunities available to immigrants from Central America. If women were largely confined to domestic work, then upward mobility could be assured most effectively by professionalizing the occupation through credentialing and establishing an occupational community.

But why did this not occur at both co-ops? The answer lay in her third extension, examining the structure of the demand for domestic workers. She discovered that there were also two types of employers. On the one hand, female-headed households or working-class families needed help in day care for their children. They paid low wages and were not (or could not be) concerned about the status of their employees. On the other hand, richer employers were prepared to pay higher wages for workers with a more polished image. The organization of the two co-ops corresponded to these two types of employer. However, Salzinger uses the extended case method to examine not only how social and economic structures limit opportunities but also how they enable actors to reshape those limits. The co-ops were not only reacting to the labor market but, by their different occupational strategies, contributing to its bifurcation.

So what? Sassen's studies place the flow of immigrants in its widest context. New immigrants, she argues, are slotted into a "service economy" by providing for the domestic needs of a new professional class. But Sassen fails to analyze the responses of immigrants to their occupational fate. In examining these responses, Salzinger sheds new light on the structure of the lower end of the service economy. The two occupational strategies, survival and professionalization, reflect and promote two distinct tiers of employers with different household needs. Underlying this balkanization of the labor market is the historical transformation of domestic work from neofeudal servant relations to regulated, market relations of a service sector, that is, from servant to wage laborer. Moving through a series of extensions, Salzinger was able to locate Central Americans within an economic, political, and historical context which they themselves had helped to shape.

With interests similar to Salzinger's, Shiori Ui set out to examine the place of women among Cambodian refugees. She spent some time in Bay Area cities trying to gain access to families but without much success. Leaders of the Cambodian community told her that women play no significant role. Pursuing a contact from her Khmer language course, she visited the Cambodian community in Stockton where, much

to her surprise, she found a group of women leaders. A tension developed between her original interest in women as family members and her discovery of an influential group of women leaders. For a time her research pursued both tracks simultaneously, until she saw a link between them.

She reconstructed her project around the puzzle of why women assumed such importance in Stockton but not in the Bay Area. She discovered that the greater leadership role for women there, as compared to the bigger cities, was tied to their employment possibilities. She found women involved in the informal economy while at the same time receiving welfare payments and learning English. A few were working in welfare agencies that dealt with Cambodians. The men, on the other hand, were often unemployed and dependent on the earnings of their wives or children.

Employment possibilities favored women and thus often gave them a dominant position within the family as well as the resources to become community leaders. But which women became leaders? She turned to the background of the Cambodian refugees, in particular their experience under Pol Pot. Many of the women had been rescued from concentration camps, where they had been forced to marry much older men, often from a lower socioeconomic background. Moreover, it turned out that the Cambodian community leaders in Stockton came from educated urban classes rather than from the rural peasantry.

How unique was this community? Here Ui turned to the literature on ethnic enclaves and compared the Cambodians to the Vietnamese. She found the Vietnamese to be doing much better, reflecting their different class background and their earlier immigration. Cambodians were coming to California as secondary migrants from other parts of the United States because of the more generous welfare and educational provisions. When they settled in the smaller communities, as many were doing, economic opportunities gave women a dominant role in the family, and some of them translated this into community leadership. By strategic comparisons first with other Cambodian communities, then with other Southeast Asian immigrants, and finally within Stockton on the basis of class background, Ui was able to extend out to the political and economic forces that gave rise to the anomalous role of women.

FROM CLASSROOM TO COMMUNITY

Education became a focus of sociological research only after World War II, when immigrant communities had established themselves. It was then that sociology turned to questions of integration and consensus,

and particularly to the role of education in promoting inter- and intragenerational mobility. Riding the tide of large-scale surveys, sociologists would try to establish correlations between social origins, education, and occupational mobility, but without careful attention to the social processes that produced the effects they were measuring. This came much later, with the development of the ethnography of schooling, studies that located schools in their specific cultural and economic context.¹⁷ The studies of junior high school students by Leslie Hurst and Nadine Julius follow in this tradition.

Leslie Hurst volunteered to tutor eighth graders at Emerald Junior High School. At first she was taken aback by the anarchy that reigned in the classroom, but soon she began to discern patterns of bargaining between students and teacher. Students continually strove to expand their rights while the teacher attempted to invoke the disciplinary order to restrict those rights. In her first presentation to our seminar, Hurst conceived of the classroom as a negotiated order. But it could have been any organization. Asked to specify what made this a school, she spontaneously responded that it served a babysitting function, keeping kids off the streets and out of the home.

From here she turned to the schooling literature to discover that it did not really address the issues she found compelling. On the one hand, schools were studied in terms of their "future" effects—slotting people into the class structure, training people for jobs, socializing people for adult life, and so forth. On the other hand, the few studies that examined the school in "presentist" terms, such as the building of peer groups, missed the specificity of the classroom as a negotiated order.

She proceeded to reconstruct the classroom as a contest between students and teachers over how much time should be spent teaching and how much simply babysitting. To highlight what others might find quite normal but what she found so interesting, she contrasted her classroom with Roald Dahl's autobiographical account of the authoritarian order of a classroom in a British public school. There the teacher commanded control over the body, mind, and soul of the pupil. At Emerald the teacher had legitimate authority only over the pupil's mind, so that body and soul could be continually mobilized to disrupt teaching. Teaching was supposedly supported by the school, which policed the student's body, and by discipline in the family, which shaped the soul or moral character of the student. But Emerald was not a middle-class school, where pupils come from a rather homogeneous cultural background and where relations between family, school, and teacher are fairly coherent. At Emerald the balance of power in the classroom and the separation of spheres undermined teaching and promoted babysitting.

It was precisely the failure of schools to deliver adequate education to African-American children that motivated Nadine Gartrell's study of an experimental after-school tutorial college for African-American students in East Oakland. Project Interface (PI) employed tutors to give extra lessons to students in small groups. It worked with what Gartrell calls an "interactive" model of education in which parents were not obligated to pay fees but had to enter into a contractual agreement that they would provide appropriate conditions for their child to work at home. The students were also contractually bound to the project.

Gartrell began her research skeptical that such a program would be effective. Theories of cultural or linguistic capital assert that instruction works for middle-class children because the form of schooling is congruent with their experience at home. In explaining the reproduction of class or racial differences, writers such as Pierre Bourdieu and Basil Bernstein paint a bleak picture because they rely so heavily on irremovable cultural obstacles.¹⁸ Contrary to Gartrell's own expectations and those of education theory, her observations at PI and the results it achieved suggested that the project was very successful. Why? The patterns of recruitment seemed to rule out the possibility that students had been specially selected on the basis of their abilities. Instead, two factors suggested themselves. First, the higher quality of instruction at PI—smaller classes and more highly motivated tutors—could more effectively stimulate students. Second, the cooperation between PI and parents created better conditions for students to learn both at school and at home. Unable to separate these two factors, Gartrell argued that the success of PI lay in the mutual reinforcement of the separated spheres, a feature absent at Emerald. PI offered an interactive model of education—interaction among students, teachers, administrators, and parents—rather than the normal fragmentation and isolation of spheres.

In the second semester, Gartrell began her extended case method by investigating whether there was anything specific about PI that might account for its success. She interviewed six sets of parents, focusing on why they had enrolled their children in PI. The parents, she discovered, were not engaged in middle-class occupations, but all were providing their families with relatively comfortable and stable circumstances. They were determined to give their children the best possible education, propelling their children to PI and arranging the best possible conditions for study. It turned out that the stringent conditions for parental participation would have denied the vast majority of poor African-Americans access to PI. In locating the problem at the level of interaction among separated spheres, Gartrell presents a picture that is less deterministic than the cultural theories of Bernstein and Bourdieu.

But still she is not optimistic that the separation of spheres can be overcome for the majority of African-Americans who face poverty and deprivation.

RESEARCHING THE RESEARCHERS

In his article "Sociologist as Partisan," Alvin Gouldner subjects the work of Howard Becker, pioneer of labeling theory, to withering criticism for failing to locate deviance within its wider context.¹⁹ He accuses Becker of treating deviants as victims of middle-level bureaucrats, whereas they are better seen as political actors in a context shaped by the state. Chicago School ethnography insulated drug addicts, the homeless, and delinquents from their determining context, exhibiting an anthropological fascination with the exotic and pathological. But Gouldner takes his argument one step further and asks what lies behind this approach. He comes up with the bleak hypothesis that behind the veil of "underdog sympathies" lies a sociologist trying to advance a career by establishing lucrative connections to the welfare state. In answer to the question, "Whose side are we on?" he replies that we are on our own side. Gouldner's extended case study of the sociology of deviance informs both Kathryn Fox's study of ethnographers doing outreach work among injection drug users and Charles Kurzman's study of the participant observers in our own seminar.

When the semester began Fox had already begun her study of an agency that was tackling the AIDS epidemic by bringing condoms and bleach to injection drug users. Intervention was designed according to the "ethnographic model" of bringing the agency to the streets and defined in opposition to the "treatment model," which expected drug users to come to clinics. In addition to distributing supplies, outreach workers were to gather data on the culture of injection drug users and then use the data as a means of helping them cope with the AIDS epidemic. Fox went onto the streets with the outreach workers, who were themselves ideally from the community, often former drug addicts or retired prostitutes. She began as an enthusiastic partisan of this ethnographic model, but her enthusiasm was thrown into doubt by what she saw, crystallized by the revulsion of one of the outreach workers. In a fit of rage, he condemned the project directors as poverty pimps, extracting information from the drug users for their own private ends.

At this point Fox was more interested in the expression of disaffection than its content. She wondered whether the ethnographic project spontaneously generated such dissident responses. On further investigation she found dissidence to be an important but minority response

of outreach workers. Those who stayed with the project turned out to be of two types: professionals who endorsed the means and the goals of the agency but were "realistic" in their assessment of what was possible, and cynics for whom it was a badly needed job. Having developed a typology of outreach workers—crusaders, dissidents, professionals, and opportunists—Fox then tried to explain their adoption of a particular orientation in terms of their prior connection to the community, whether they had been insiders or outsiders. But she couldn't get this to work out.

Disillusioned with the ethnographic model and demoralized by her field experiences, Fox turned her study toward the agency itself. Its success depended on the commitment of outreach workers, she argued, yet this commitment was systematically eroded in two ways. First, the expansion of the agency created a confidence gap between staff and outreach worker, as staff became more concerned with garnering funds than with the mission of bringing help to a beleaguered population. Second, working under such difficult circumstances gradually ate away at the commitment of the outreach workers, and crusaders and professionals became cynics.

Fox continued her research in the second semester. Earlier, she had been reluctant to shift her focus from the outreach workers toward the wider context within which the agency had to operate. However, when she began to pay more attention to discussions among the directors, she saw how the state severely constrained their room to maneuver. On the one hand the agency had to continually garner funds from a federal institute on the basis of its research component, while on the other hand a promising means of AIDS prevention—needle exchange—was illegal. Fox moved from an analysis of immediate relations between injection drug users and outreach workers to the external forces that limited the effectiveness of such ethnographic intervention.

In her conclusion, Fox draws attention to these limitations but still defends the ethnographic model as against the treatment model, the strategic comparison that frames her study. The virtue of the ethnographic model, at least potentially, lies in its holistic regard for drug users on their own terms and in their own community.

Whereas Fox converged on Gouldner's critique of sociology from the side of sociologists in the field, Charles Kurzman approached Gouldner from the study of sociologists in their own setting. He came to our seminar armed with three ready-made theories of knowledge to be tested and explored. Would his model work for a bunch of ethnographers? He was going to observe the class to study which "knowledge claims" were successful and why. His first analysis focused on three competing theories: knowledge claims might be successful because they

appealed to evidence, to commonly accepted paradigms, or to interests. Each approach shed some light on what had taken place in the seminar, but, he subsequently argued, they missed the fluidity of the seminar.

This led him to reanalyze his data and offer two conclusions. First, he found that we endorsed explanations couched in broad social and political frameworks and were hostile to psychological explanations. The proclaimed hostility to social workers or to medical models supported the view that we invoked norms in accordance with our interests as sociologists. Second, he found that we recognized the views of some participants while silencing those of others. Operating according to norms of identification with and distance from the participants, we, as he put it, gave credence to the positive but not to the negative deviants. Moreover, among the seminar participants there was an unstated consensus as to who were the positive deviants: generally those who resisted subjugation to political and economic systems.

Rather than straightforwardly adjudicate among his original theories, Kurzman had translated them into terms relevant to his observations. What struck him was the contradictory character of the norms to which we appealed. Instead of there being a single interest, which we defended as sociologists, there were a multiplicity of interests: our interests vis-à-vis those we studied, other sociologists, and even other social scientists. We are indeed on our own side, but that meant different things in different situations.

Kurzman stopped short of extending out, of locating the specific interests of the participants in the seminar in their wider context. He imposed strict boundaries on his research, devoting himself to the analysis of the seminar itself. He explicitly did not want to draw on information that he gleaned from private conversations outside the seminar, or by accompanying people to their sites. This would have betrayed his classmates, who were more important to him than the extended case method. In his own terms, he saw the members of the seminar as "positive" deviants, as people with agency of their own. Not surprisingly, therefore, he did not seek external forces that might reduce them to the effects of structures. He even went so far as to deny the importance of internal structure or hierarchy.

It is one thing to place those we study, particularly if we neither depend on them nor identify with them, in the context of their determination, denying them their autonomy. It is quite another matter to do the same to ourselves. Kurzman was trapped in what Gouldner calls "methodological dualism": "others" are dupes of social forces but "we" are rational agentic beings.²⁰ His study problematizes the extended case method for, according to Kurzman's theory, to the extent that the participant observer becomes an insider, sensitive to every whim of those

being studied, there will be considerable pressure to give the participants full agency and to repress the way they are constrained by external forces. Of course, in some situations participants regard themselves as victimized by external forces beyond their control. Had Kurzman studied a course that students universally disliked but were compelled to take, perhaps he might have been happier with the extended case method!

CONCLUSION

In order to pursue the approach we have advocated in this chapter, the ethnographer must identify existing social theories to reconstruct. Where do such theories come from? First and most obviously, all of us have social theories that inform the way we organize and pursue our lives. Participant observers are particularly aware of lay theories, or commonsense knowledge, and this can always provide a point of departure for reconstruction. Moreover, there is a circular movement in which social science built on the reconstruction of common sense feeds back and transforms that common sense. Our newspapers and media are full of watered-down social science, which in part explains why social science doesn't appear to grow. The new theory of today becomes the conventional wisdom of tomorrow.

Nevertheless it is reasonable to argue that the last fifty years have witnessed the growth of the body of academic theory, whether in the form of deductive grand theory, middle-range theory, or the empirical generalizations of grounded theory. The generation of theory from the ground up was perhaps imperative at the beginning of the sociological enterprise, but with the proliferation of theories reconstruction becomes ever more urgent. Rather than always starting from scratch and developing new theories, we should try to consolidate and develop what we have already produced.

We should, therefore, begin to think about different ways of improving theories through their reconstruction. With respect to the sort of field work represented in this volume, I want to suggest two alternative strategies. First, one can decide what one wants to study, immerse oneself in empirical work, and then search for theories that are inadequate because they ignore salient issues or lead to false anticipations or have latent theoretical ambiguities or contradictions that are revealed by the data. In each case the data call for an improvement of the existing theory. Presumably, there will be a number of theories for which the data would be anomalous, and so we choose the one that is closest to our interests. Alternatively, one can commit oneself to a given theory rather than a specific empirical phenomenon. If it is an impor-

tant theory (and why else choose it?), its anomalies are already well known, and these would suggest possible empirical foci for research. Our task is to improve the theory by introducing auxiliary hypotheses that will turn anomalies into exemplars. IX

Participant observers are more likely to find the first strategy attractive. For, if we approach a field site to examine the anomalies of a particular theory, we often find that the data don't address those anomalies, particularly if we take seriously the self-understanding of the participants. It is then difficult to decamp to another site and begin the field work anew. For pragmatic reasons we should, therefore, adopt a flexible approach and be prepared to shop around for appropriate theories. But there are also intellectual reasons for adopting the first strategy. Participant observers often either start out with a commitment to those they study or acquire this commitment as they prolong their stay in the field. This responsiveness to the participant is often at odds with strong prior commitments to a particular theory.

Yet there is still something to be said for the second strategy, of locating oneself within a particular theoretical tradition. Whereas the first strategy may lead to the improvement of weak theories, the second strategy is more likely to foster the improvement of powerful theories that are attractive by virtue of their power. There are intellectual gains and satisfactions to participating in and contributing to an established theoretical tradition even if it constrains the sorts of anomalies one seeks to normalize. When the preeminent dialogue is between participant and observer, shopping around for appropriate theories to reconstruct is more likely. When social scientists are more interested in a dialogue among themselves than with their subjects, they are more likely to have prior commitments to established theoretical perspectives. But in neither strategy does theory emerge spontaneously from the data. To be sure it must "fit" the data, but, as Kurzman shows, this still leaves ample scope for selection in the light of the values and interests we hold.

The Extended Case Method

Michael Burawoy

This book treats methodology neither narrowly as the science of technique nor broadly as a branch of theory. Indeed, for us methodology provides the link between technique and theory. It explores ways of utilizing technique to advance theory. If technique is concerned with the instruments and strategies of data collection, then methodology is concerned with the reciprocal relationship between data and theory.

The studies of this book utilize a single methodology, that of the extended case method, for (re)constructing theory out of data collected through participant observation.¹ However, the extended case method is only one methodological response to the two traditional criticisms of participant observation: first, that it is incapable of generalization and therefore not a true science and, second, that it is inherently "micro" and ahistorical and therefore not true sociology. In this chapter I want to clarify the distinctiveness of the extended case method by comparing its response to these criticisms with the responses of other methods, in particular ethnomethodology, the interpretive case method, and grounded theory. Thus, in the first part of this chapter I move from technique to method. In the second part I move from method to theory. There I argue that by focusing on the "macro" determinations of everyday life, the extended case method is also the most appropriate way of using participant observation to (re)construct theories of advanced capitalism.

FROM TECHNIQUE TO METHOD

According to convention, the technique of participant observation suffers from two fundamental problems. The first is the problem of sig-

nificance. Single case studies may provide very interesting results, but they provide no measure of their generalizability. Moreover, singularity is compounded by the observer's potential disruption of the situation under examination. The case study is, therefore, so it is said, inherently particular until its results are tested in a sample of cases carefully selected from a population. The second criticism refers to the *level of analysis*. The study of face-to-face interaction, of the social situation, is said to be inherently micro and ahistorical. By its very definition the technique of participant observation is confined to the short term and to limited geographical space. In what follows I try to explicate the extended case method's response to these criticisms by comparing it with three other responses—ethnomethodology, the interpretive case method, and grounded theory.

Ethnomethodologists deny the relevance of the two charges. They argue that social situations of face-to-face interaction must be understood first as a unique product of the competencies, reflexivities, and assumptions of reciprocity among participants. In this perspective the abstractions necessary to make comparisons and thus generalizations across social situations have no "objective" standing. They are themselves constructed from the social situation of the inside participant or the outside observer. Jack Douglas, for example, shows how the official suicide rates that Durkheim used to establish the existence of a "macro" order of external constraining forces cannot be compared, because they take on very different meanings according to the circumstances of their collection.² Aaron Cicourel shows how the concept of delinquent is socially constructed by the juvenile justice system: by police officers and probation officers working under organizational and client pressures and with particular theories of delinquency. If the use of general concepts to apply across social situations is sociologically suspect, so too is the postulation of an external level of reality.³ In the view of ethnomethodology the macro world is not a real world but a construction of participants enabling them to negotiate and uphold face-to-face interaction. There is, therefore, only one sociology and that is the microsociology of the unique social situation, what Knorr-Cetina calls "methodological situationism."⁴ The task of ethnomethodology, therefore, is to elaborate the cognitive accomplishments that make social interaction possible. These accomplishments include the social construction of abstractions that constitute both general and macro phenomena.

Like ethnomethodology, the interpretive case method denies the premises of conventional criticisms. Here too macro and micro, general and particular, are collapsed, but in the other direction. The micro is viewed as an expression of the macro, the particular an expression of the general. It is as if the whole lodges itself in each part in the form

of a genetic code, which has to be uncovered through a process of hermeneutic interpretation. Clifford Geertz is one of the supreme artists of this *interpretive case method*. In his celebrated study of the Balinese cockfight, for example, Geertz presents the ritual cockfight as a "paradigmatic event" that displays the social organization of Balinese society and the Balinese sense of self.⁵ Through an elaborate system of betting and gaming rules, the warring of cocks brings together and orders villages, kin-groups, lineages, and castes into a status hierarchy. Cocks are the symbolic expression of their owners' masculinity, standing, and prestige, while the cockfight itself simulates the social matrix of Balinese society. The Balinese cockfight holds a key to the wider society in which it is embedded: "Its function, if you want to call it that, is interpretive: it is a Balinese reading of Balinese experience, a story they tell themselves about themselves."⁶ Geertz describes his method as follows: "Hopping back and forth between the whole conceived through the parts that actualize it and the parts conceived through the whole that motivates them, we seek to turn them, by a sort of intellectual perpetual motion, into explications of one another."⁷ The *interpretive case method* denies the premises of the conventional criticisms by claiming that, properly understood, the *micro* and *particular* are simultaneously *macro* and *general*.

TABLE 1 Responses to the Criticisms of Participant Observation

| | | Significance of Social Situation | |
|-----------------------------------|-------|----------------------------------|--------------------------|
| | | Particular | General |
| Orienting Level of Analysis | Micro | Ethnomethodology | Grounded theory |
| | Macro | Extended case method | Interpretive case method |

In table 1 I have summarized the four different responses to the criticisms that participant observation is inherently micro, ahistorical, and particular. By their reductionism, ethnomethodology and the interpretive case method both reject the *terms* of the twin criticisms. Ethnomethodology makes a virtue of necessity and *reduces sociology to the micro and particular*, whereas the *interpretive case method fuses the micro and the macro*, the particular and the general, into a single expressive totality. I use them here to shed light on the distinctiveness of grounded theory and the extended case method, both of which accept that micro and macro are discrete and causally related levels of reality

and that generalizations can be derived from the comparison of particular social situations.

As we shall see, each of these two preferred methods addresses one of the criticisms at the expense of the other. On the one hand, the extended case method, by explicating the link between micro and macro, constitutes the social situation in terms of the *particular* external forces that shape it. It faces the problem of generalization. On the other hand, grounded theory, by pursuing generalizations across social situations, obscures the specific contextual determinations of the social situation. It faces the problem of the link between micro and macro. Nevertheless, I propose to show that each method can in principle deal with both criticisms: grounded theory can build up the macro from its micro generalizations, and the extended case method can give rise to generalizations through reconstructing theory.

The Rise of Microsociology

Grounded theory works with a particular image of science in which theories are induced from data. Its principles were already laid out in Park and Burgess's classic *Introduction to the Science of Sociology* (1921): "As soon as historians seek to take events out of their historical setting, that is to say, out of their time and space relations, in order to compare them and classify them; as soon as historians begin to emphasize the typical and representative rather than the unique character of events, history ceases to be history and becomes sociology."⁸ But Park imposed his naturalistic sociology and the laws of human ecology on the field work of his students. He slotted their studies of the "natural areas" of Chicago into a preordained framework. Only later would field workers be encouraged to construct their own theories from the ground up.

Florian Znaniecki would be the first to theorize the importance of using field work to generate abstract principles or even laws. He called this process analytical induction: "[C]ertain particular objects are determined by intensive study, and the problem is to define the logical classes which they represent. No definition of the class precedes . . . the selection of data to be studied as representatives of this class. The analysis of the data is all done before any general formulations; and if well done, there is nothing more of importance to be learned about the class which these data represent by any subsequent investigation of more data of the same class."⁹

Everett Hughes, one of the most influential teachers of participant observation at Chicago, also advocated this approach. He, for example, compared dissimilar occupations: the physician and the janitor, the real estate agent and the jazz musician, in order to draw conclusions of general validity about the nature of occupations:

The comparative student of man's work learns about doctors by studying plumbers; and about prostitutes by studying psychiatrists. . . . [W]e seek for common themes in human work. . . . I believe that in the study of work, as in that of other human activities and institutions, progress is apt to be commensurate with our ability to draw a wide range of pertinent cases into view. The wider the range, the more we need a fundamental frame of reference.¹⁰

Later, Glaser and Strauss codified these methods in their pathbreaking book, *The Discovery of Grounded Theory*, which has become one of the definitive texts for field workers in sociology.¹¹ They sought to demonstrate that qualitative analysis was neither an anachronism nor doomed as foreplay to the real sociological act. Field work need be no less theoretical than the "grand theory" of structural functionalism and no less scientific than the "abstracted empiricism" of verification.

Glaser and Strauss were at pains to show that theory development was not the prerogative of a few leaders at elite establishments who construct deductive systems of thought based on a priori assumptions and concepts removed from reality. Nor should sociologists be condemned to the slavish task of verifying the theories of the anointed. "[The grand theorists] played 'theoretical capitalist' to the mass of 'proletariat' testers, by training young sociologists to test their teachers' work but *not* to imitate it."¹² Grounded theory is a populist sociology, a way in which all of us can turn our data into the very best scientific theory. By accumulating judicious comparisons constructed from qualitative data, even the participant observer can begin to develop concepts of general applicability to diverse settings.

Grounded theory's claim to science lies in its ardent pursuit of generalizations, induced from comparisons across social situations. But in making those comparisons grounded theory represses the specificity of each situation. Thus Glaser and Strauss examine how physicians, families, nurses, and patients deal with terminal illness in different "awareness contexts," related to who knows what and with what certainty.¹³ They develop general laws about the way in which the social loss of a dying patient (loss to family and to occupation) affects the behavior and attitudes of nurses and doctors. In moving from this substantive theory to a more formal theory, Glaser and Strauss propose the more general law: the higher the status of a client, the better the care he or she will receive from experts.¹⁴

Ethnomethodologists, by contrast, prefer to plumb the depths of the single "awareness context" for processes that underlie stable interaction. Typical is Garfinkel's celebrated study of Agnes, whose attempt to pass from boy to girl highlights all the "seen but unnoticed" labors involved in managing sexual identity.¹⁵ More explicitly hostile to the comparative approach of grounded theory is Gilbert and Mulkey's anal-

ysis of scientific discourse.¹⁶ Rather than extracting from their interview material some underlying reality, some truth of how science "really" works, they highlight the discrepant discourses of scientists as products of specific social contexts. They show how scientists create two discourses to cope with disagreement: an empiricist discourse in which facts reflect a real world and a contingent discourse in which noncognitive factors account for error. Whereas sociologists have tended to reduce science to the contingent repertoire and philosophers have reduced it to the empiricist repertoire, neither give serious attention to the world of the scientist as a patterning of a *multiplicity* of discourses. In the words of Zimmerman and Pollner, grounded theory uses discourse as a "resource" from which to derive generalization whereas ethnomethodology regards discourse as a "topic" in its own right, something that is produced in specific contexts.¹⁷

Methodological situationism leads ethnomethodologists in two directions. One tendency is toward extreme relativism, in which there is no real world but only a multiplicity of situationally specific perspectives. The other tendency seeks invariant properties that make social interaction possible, whether these be linguistic communication, procedures for accomplishing understanding, or the cognitive basis of all meaning production.¹⁸ In contrast to grounded theory, which seeks generalizations across social situations, ethnomethodology, if it makes any claims beyond the particular, seeks *universals* that underlie all social situations. As we shall see, the extended case method also adopts a situational analysis but avoids the pitfalls of relativism and universalism by seeing the situation as shaped from above rather than constructed from below.

The Anthropological Turn to the Macro

Grounded theory defended the scientificity and theoretical relevance of microsociology against the rising hegemony of structural functionalism. Symbolic interaction criticized the ideas of system and structure for missing the creativeness of actors in social situations. "Instead of accounting for the activity of the organization and its parts in terms of organizational principles or system principles, [symbolic interactionism] seeks explanation in the way in which participants define, interpret, and meet the situations at their respective points."¹⁹ Ethnomethodology, on the other hand, turned the problem of order from Parsons's institutionalization and internalization of norms to the prior cognitive accomplishments of everyday life that make normative consensus at all possible.²⁰ This movement from macrosociology to microsociology was also reflected in actual research. Through their field work, urban and industrial sociologists were busy discovering *small-scale communities*—the urban village and the informal work group—that were holding back the forces of commodification, atomization, anomie, and alienation

trumpeted by classical sociology as defining the modern condition. Within institutions such as prisons, asylums, the military, hospitals, and so on, microsociology was uncovering an unsuspected world of the inmate.

At the same time that participant observation was being increasingly identified with microsociology in the United States, the opposite move was occurring within anthropology. With the expansion of anticolonial struggles and with industrialization disrupting and connecting the furthest corners of the globe, anthropologists could no longer pretend that their villages were isolated and timeless. The problem became even more acute when they left their villages and ventured into urban areas, where it was impossible to impose boundaries on face-to-face interaction.²¹

One of the first anthropological attempts to come to terms with "macro" forces was Max Gluckman's exploration of the social structure of South Africa in his analysis of a bridge-opening ceremony in Zululand in 1940.²² In attendance were representatives from both sides of the color bar: on the one side the Chief Native Commissioner, the magistrates, the chief surgeon, missionaries, traders, and recruiting agents for the mines, and on the other side the Zulu king, local chiefs, the men who had built the bridge, clerks, African police, and armed warriors. Analyzing who said and did what, when, where, and to whom, Gluckman was able to represent the ceremony as South African society in microcosm. Based on interrogation of the social situation and on his prior knowledge, he showed how the opening of the bridge contained the key factors with which to construct an understanding of South African society. Presumably had he looked at a bridge opening in the Transkei, or a mine dance in Johannesburg, he would have come to similar conclusions about the nature of South African pluralism. Society was, so to speak, composed of cells each encoded with the same structure, reflecting the essential character of the totality in which they existed. It did not matter which cell one looked at; the purpose was to arrive at features that were generalizable to society as a whole.

This was in effect an application of the interpretive case method. Gluckman was interested in the way the macro was present in the micro situation and less concerned with their mutual determination as two different levels of reality. Subsequently Gluckman and, more particularly, his students developed the *extended case method*, opening up villages and urban situations to wider political and economic forces associated with colonialism.²³ Whereas in the original study of the bridge opening, Gluckman had regarded the social situation as an *expression* of the wider society, many of his followers viewed the village, the strike, the tribal association as *shaped by* external forces.

Jaap van Velsen elaborated the extended case method in his reflections on his own studies of kinship relations among the Lakeside Tonga of Malawi (then Nyasaland).²⁴ When he asked the Tonga to describe their kinship system, they told him that they were a matrilineal and matrilocal tribe. A traditional anthropologist would have then diagrammed the beautifully symmetrical kinship patterns and called it a day. Van Velsen, however, was not satisfied with just studying norms and began collecting data on actual marriage patterns of the Tonga. He discovered that 40 percent failed to conform to the normative ideal. Whereas a structural anthropologist might have dismissed these as unimportant exceptions, Van Velsen regarded them as the key to understanding the changes taking place in Tonga society. Many of the able-bodied men in the villages were absent for long periods while they worked on the South African mines, and this led to new sources of wealth, changes in the sexual division of labor, and alternative marriage patterns that competed with the ideal. Accordingly, he did not view kinship norms as internalized and then executed, but as an arena of struggle for the realization of competing interests.²⁵

In making problematic the exceptional or deviant cases, Van Velsen is driven outside the field situation to the broader economic and political forces impinging on the Tonga. Looked at through the lens of colonialism and industrialization, the social situation becomes a completely different object, one threaded by patterns of power in which kinship norms become the terrain of struggle. The movement outward compels a reconceptualization inward—from self-equilibration and cohesion to domination and resistance.

The differences between the interpretive and extended case methods become clearer on reexamination of Geertz's Balinese cockfight. Whereas Geertz regards it as a "paradigmatic event" that displays the social organization of Balinese society, the extended case method would examine the specificity of the cockfight—how it varies from place to place, how it has changed over time—as a vehicle for comprehending the forces shaping Balinese society. Geertz gives us a few clues. For example, he tells us that most of the fights are organized by petty merchants near to and on the occasion of markets; "trade has followed the cock for centuries in rural Bali, and the sport has been one of the main agencies of the island's monetization."²⁶ Second, the Javanese government and the Dutch colonial government before it made the cockfight illegal, which Geertz attributes to their modernizing ideology.²⁷ But from a different point of view the cockfight can be seen as a ritual of resistance to colonial and then Javanese domination. The economic and political forces come together when Geertz explains that the cockfight is the way in which the Balinese raise money for public purposes, in this particular case for a new school that the government had denied them.

We see how the extended case method leads directly to an analysis of domination and resistance, obscured in Geertz's interpretation. We can come to similar conclusions by contrasting the way Van Velsen problematizes exceptions to norms and the way Geertz sweeps them aside. Thus, on the one hand, Geertz says that the cockfight is a *paradigmatic* event that simulates the Balinese status hierarchy, but on the other hand he says it is *exceptional* in excluding women since Bali is a "rather unisex society."²⁸ Problematizing rather than dismissing this anomaly, one might conclude that the centrality of the cockfight testifies to the importance of male domination despite and indeed perhaps because of appearances to the contrary. It is a fraternal organization that constructs male solidarity and through its association with the cash nexus cements the material, political, and cultural subordination of women. It is a text written by men for men.

Geertz denigrates as reductive and relegates to footnotes what is constitutive of the extended case method, namely the specific historical context that shaped both the cockfight and the domination it produced. In Geertz's own favored metaphor of text he fails to examine who wrote the text and for whom, and how it was received.²⁹ The metaphor of text is not innocent. One can read and interpret a text without knowing its author or audience. Texts may appear to be autonomous, but nevertheless they have to be created, and they do produce effects.

The extended case method looks for specific macro determination in the micro world, but how does it measure up to the criticism of generalizability? It seeks generalization through reconstructing existing generalizations, that is, the reconstruction of existing theory. In focusing on the deviant marriage patterns among the Tonga, Van Velsen developed a "post-structuralist" theory of kinship that emphasized strategic action in the pursuit of interests rather than the execution of norms.³⁰ In examining the source of the deviations, Van Velsen rejected the idea that return migration from the towns was due to the strength of primordial ties to a traditional way of life. Instead he showed how oscillating migration was shaped by the policies and institutions of the colonial administration and the South African mining industry. The colonial administration in Nyasaland needed a tax base and the mines needed cheap labor—both objectives were assured by reproducing a system of return migration. For their part the Tonga worked within these political and economic constraints, using entry into wage labor to further their own material interests.³¹ By reconstructing existing theories of migration in this way, Van Velsen was able to generalize from his single case study.³²

The Extended Case Method and Grounded Theory

We have seen how both ethnomethodology and the interpretive case method deny the terms of our two criticisms, by reducing all sociology either to the micro and the particular or to the macro and the general. We are now in a position to directly compare the two methods that *do* problematize the relationship between the particular and the general. Table 2 summarizes the differences between the two strategies, which are discussed below.

TABLE 2 Comparison of the Extended Case Method and Grounded Theory

| | Extended Case Method | Grounded Theory |
|-----------------------------|---|---|
| Mode of generalization | Reconstructing existing theory | Discovering new theory |
| Explanation | Genetic | Generic |
| Comparison | Similar phenomena with a view to explaining differences | Unlike phenomena with a view to discovering similarities |
| The meaning of significance | Societal | Statistical |
| Nature of totality | Uniqueness is located in a context external to itself, which elucidates society | Abstraction from space and time in a setting, which facilitates generalization to population of cases |
| Object of analysis | Situation | Variables |
| Causality | Indivisible connectedness of elements | Linear relationship between variables |
| Micro-macro | Macro foundations of a microsociology | Micro foundations of a macrosociology |
| Social change | Social movements | Social engineering |

As I described in some detail in chapter 2 and above, the extended case method derives generalizations by constituting the social situation as anomalous with regard to some preexisting theory (that is, an existing body of generalizations), which is then reconstructed. Grounded theory, on the other hand, discovers generalizations by abstracting from time and place.

Grounded theory's inductive strategy leads to *generic* explanations, which take the form of invariant laws, such as "all organization tends toward oligarchy." The extended case method constructs *genetic* explanations, that is, explanations of particular outcomes. An example would

be Weber's analysis of the historically specific constellation of forces, including the motivational component provided by the Protestant ethic, which gave rise to Western bourgeois capitalism. A generic strategy looks for similarities among disparate cases, whereas the genetic strategy focuses on differences between similar cases. The goal of the first is to seek abstract laws or formal theory, whereas the goal of the second is historically specific causality.³³

Our two types of explanation work with very different understandings of what we might mean by "significance." In the generic mode we seek out what different social situations have in common, and generalization is based on the likelihood that all similar situations have similar attributes. Here significance refers to *statistical significance*, generalization from a sample to a population. In the genetic mode the significance of a case relates to what it tells us about the world in which it is embedded. What must be true about the social context or historical past for our case to have assumed the character we have observed? Here significance refers to *societal significance*. The importance of the single case lies in what it tells us about society as a whole rather than about the population of similar cases.

In grounded theory society provides "natural" settings for the discovery of recurrent patterns of social behavior. Thus, Robert Park referred to the city as a "laboratory" where, because of rapid change and social diversity, the laws of human nature become readily visible.³⁴ These laws are not specific to the city but reveal themselves most clearly there. On the other hand, in the extended case method, the city is not an arena where laws are played out but a constellation of institutions located in time and space that shape domination and resistance.

The two approaches also adopt different conceptions of causality. Generalizing across disparate social situations involves not only abstraction from time and space but also the simplification of the social situation itself. Where causal patterns are observed across situations, they tend to be of a simple, linear form such as x causes y , what Herbert Blumer and more recently Charles Ragin call "variable analysis."³⁵ In constituting a social situation as unique, the extended case method pays attention to its complexity, its depth, its thickness. Causality then becomes multiplex, involving an "individual" (i.e., undividable) connectedness of elements, tying the social situation to its context of determination.

Glaser and Strauss move from substantive to formal theory, making their generalizations among micro situations ever more abstract. They always remain at the same level of society. Others use microsociology as building blocks for a macrosociology. The simplest way is to argue, as Randall Collins has, that with the exception of space, time, and num-

bers there are no true "macro-variables."³⁶ The macro is then the aggregation and repetition of many micro interactions. The purpose of sociology is to arrive at "generalized explanatory principles, organized into models of the underlying processes that generate the social world."³⁷ Collins thus reduces "macro-phenomena" to the only real experiential reality, the micro situation with its rules of interaction.

Can one derive the properties of the macro world from the micro level laws? Such attempts to establish the micro foundations of a macrosociology ride on both the intended and unintended consequences of the social interaction order. Logically this realm should be the province of symbolic interactionism, which regards social structure as an emergent process. In practice symbolic interactionism, like grounded theory, either ignores or takes social structure as given. Instead the project has been taken over by game theory and economic models, which claim to link the macro to the micro on the basis on methodological individualism. Sensitivity to the social situation, to the symbolically constructed lifeworld, is abandoned in favor of normative assumptions about the rationality of actors.³⁸ The extended case method takes the opposite approach and seeks to uncover the macro foundations of a microsociology. It takes the social situation as the point of empirical examination and works with given general concepts and laws about states, economies, legal orders, and the like to understand how those micro situations are shaped by wider structures.

Finally, we turn to the vision of social change that emerges from the two methods. Glaser and Strauss self-consciously aim to develop theories that will enhance the control participants exercise over their situations.³⁹ They distinguish "access variables," which give participants access to "controllable variables," whose manipulation would affect the situation. Grounded theory, for example, should allow nurses to better cope with different levels of awareness of terminal illness just as it gives doctors an idea of the consequences of disclosure.

In advocating social engineering, grounded theory suppresses two related factors. First, it does not consider the dimension of power within the micro context; how, for example, doctors exercise power over nurses and how both exercise power over patients. Second, in focusing on variables that can be manipulated within the immediate situation, it represses the broader macro forces that both limit change and create domination in the micro sphere. Whereas grounded theory might examine the way AIDS patients can be more "effectively" handled in hospitals, the extended case method would examine the way the state has failed to take AIDS seriously, has held back the development of public policy, and has restricted experimentation with new drugs.

Once one highlights systemic forces and the way they create and sustain patterns of domination in the micro situation, the application of social theory turns to building social movements. As Alain Touraine has argued, it is not a matter of applying the knowledge of the expert but of the observer joining the participants in a joint movement of analysis and action.⁴⁰

FROM METHOD TO THEORY

In order to distinguish the extended case method from other methods of using participant observation to advance theory, we had to adopt the general categories of "micro" and "macro." The considerable divergence between the methods requires categories that are so abstract and ahistorical that they are of little use in examining the appropriateness of a given method for studying particular phenomena. We have to respecify the meaning of micro and macro to appreciate the relevance of the extended case method for the study of power and resistance in the modern metropolis.

We may take as our point of departure Alain Touraine's idea of a postindustrial society in which not only the *means* but also the *ends* of production are transformed. Postindustrial society acts on itself and determines its own goals. It is a "programmed" society that rejects the old teleological models of history as evolution and progress in order to embrace "historicity," that is, our ability to create social relations through participation in social movements. "Society used to be in history; now history is within societies, and they have the capacity to choose their organization, their values, and their processes of change without having to legitimate these choices by making them conform to natural or historical laws."⁴¹

The studies in the volume exemplify the production rather than the consumption of social relations. ACT UP and the antinuclear movement challenge the goals of society: they challenge how society uses its resources, how it legitimates inequality, and how it imposes alien needs. The state fragments and bureaucratizes the administration of social benefits to prevent alliances between worker and client for a more just and rational welfare order. Workers at Wholly Grains oppose the degradation of work with the ideal of self-management in which producers decide how and what they produce. New immigrants show astonishing imagination in articulating alternative visions, as Central American domestic workers create new meaning in their jobs and Cambodian women define the needs of their community. In African-American communities parents band together to counter the denial of public ed-

ucation to their children. Even junior high school students take the classroom into their own hands and challenge the teacher's authority to teach. Social research itself becomes self-conscious about its purposes. An outreach program for drug users employs a sociologist to monitor its activities, and a sociologist uncovers the hidden agendas of his fellow students.

But where does this self-consciousness come from? Where are the new meanings produced and where are they lodged? Jürgen Habermas, drawing on Schutz's concept of the lifeworld, considers their source to be social arenas integrated through mutual understanding and collective will formation such as the workplace, family, community, and school.⁴² New meaning arises from communicative labors whose comprehension requires actual or virtual participation. To gain access to the lifeworld, scientists have to enter into dialogue with participants. They have to become real or imaginary participant observers. Like Touraine, Habermas must privilege participant observation as a technique of social research.

But participant observers differ from participants precisely in their status as observers, which gives them insights into the limits of communicative action and the sources of its distortion, that is, how the system world denies freedom and autonomy in the lifeworld. As observers who also stand outside the lifeworlds they study, scientists can gain insight into the properties of the system world, which integrates the intended and unintended consequences of instrumental action into relatively autonomous institutions. Indeed, these can be understood only from the standpoint of the observer.

From this perspective the scientific and hermeneutic dimensions of social research assume an ontological foundation. They correspond to the different types of social action that integrate the system and lifeworld respectively. Thus those who would stress science over hermeneutics—the objective over the subjective—risk stressing the supremacy of system over lifeworld. Such analyses, whether critical or complacent, have a tendency to degenerate into pessimistic overestimations of the power of the welfare state, the capitalist economy, or “the system.” Too often, the system is seen as all-determining, so that forms of resistance such as innovation, negotiation, and rebellion are not taken seriously. Max Weber, for instance, waxed pessimistic about the limits of rationality and the bureaucratization of the modern world. His vision of the “iron cage” that imprisoned capitalist society would seem to deny the possibility of autonomous spaces for resistance. In stressing reification, instrumental reason, one-dimensionality, the critical theory of Lukács and the Frankfurt School relegates resistance to an ever-diminishing “rebellion of the marginal.”⁴³

Habermas is just as critical of those who give primacy to the hermeneutic moment, to intersubjective understanding, such as symbolic interactionists and ethnomethodologists. In reducing society to the lifeworld, they erroneously assume the autonomy of actors, the independence of culture, and the transparency of communication.⁴⁴ They ignore distortions brought about by the economic and political systems, particularly through the incursion of the universal media of exchange—money and power.

In Habermas's view the distinctiveness of contemporary society is the “uncoupling” of the lifeworld from the system world. Whereas the old Marxian models of society bound class struggle to the unfolding contradictions of capitalism viewed as an economic system, Habermas, like Touraine, sees a double differentiation, on the one hand between the economic and political systems and on the other between the lifeworld and the system world.⁴⁵ Once separated, the relationship between system world and lifeworld is one of invasion and resistance.

Although Habermas establishes the theoretical basis for the domination of the lifeworld by the system, he fails to elaborate the sociological basis of their actual interaction.⁴⁶ Instead he substitutes a generalized fear that the “system” will overextend itself and “colonize the lifeworld.” Colonization, he argues, not only undermines communicative action but also saps the lifeworld of the motivational and critical energies necessary for the survival of the system itself. While Habermas does see new social movements as the reaction of the lifeworld against the system, his analysis of resistance is no more concrete than the process of colonization itself.

As the studies in this volume show, the interplay between system and lifeworld, between domination and response, is dynamic and varied. However, it is difficult to systematize the various forms of interplay. Forms of resistance are more easily disentangled than are corresponding forms of domination, principally because the system is diffuse in its operation and less visible in a world that spotlights the subject of domination and hides the deployment of power. Five distinct modalities of resistance, however, are revealed by the studies in this volume.

Colonization. In some contexts the system so fragments and individualizes the lifeworld that resistance is impossible or ineffective. This is the outlying category that gives Habermas's work its critical edge. It is represented in this volume by Alice Burton's analysis of welfare workers. She shows how the state's reorganization of welfare agencies divides workers from one another and from their clients. Welfare clients are atomized into administrable segments so that they, along with the social workers and eligibility workers, can be more effectively surveilled. Even in this extreme case workers strike or work to rule, but their rebellion

is ineffective. Kathryn Fox's study of outreach work among injection drug users underlines what we might call "neocolonization of the lifeworld." Here direct surveillance is less important a constraint than financial dependence on the state and laws prohibiting needle exchange. By directly or indirectly destroying the autonomy of these lifeworlds, whether of the welfare client or the drug user, the state systematically erodes the basis of its own legitimacy and effectiveness.

Negotiation within limits. In other contexts the system leaves room for maneuver within institutions while strengthening boundaries between institutions. Leslie Hurst's study shows that such a separation of spheres can in fact facilitate what she calls a negotiated order within spheres. Mr. Henry has legitimate control only over his pupils' minds, while their bodies and souls are outside his control. Students take advantage of this to contest his authority; without support from school or family he has no recourse. Shiori Ui also describes the two-edged character of the operation of state and economy, which limits freedom and autonomy while creating opportunities. Cambodian women are able to exploit their status as political refugees to establish themselves in enclaves and prepare passages into the wider society. In keeping itself at a greater distance and permitting limited terrains of maneuver, the state successfully contains resistance and even turns it to its advantage.

Creating alternatives. Within a fragmented lifeworld people can sometimes go beyond a negotiated order and carve out spheres of self-organization. Although powerless to reshape the boundaries of the system, they can at least defend and reconstitute their lives within those boundaries. Self-organization can occur even within the system as, for example, in the case of Ann Arnett Ferguson's producer cooperative. Although continually threatened by pressures of the marketplace, Wholly Grains manages to survive one crisis after another by drawing on a reservoir of workers' commitment to collective organizations as well as on resources supplied by other collectives. In themselves cooperatives do not challenge the limits within which they operate—they have to compete with other enterprises for both labor and consumers. Yet they do plant the idea that alternatives are possible and thereby question the legitimacy of the boundaries set by the system. Similarly, the tutorial program studied by Nadine Gartrell challenges the pessimism about African-American education. The bringing together of spheres that are normally insulated, particularly family and school, gives students a better chance of realizing their aspirations.

Reshaping limits. Leslie Salzinger shows how workers, organizing in defense of their material interests, can actually reshape the limits of the system. Central American women, faced with slender labor market opportunities, opt for domestic work because it gives them autonomy and

flexibility. Individual upward mobility is not feasible, and conditions of employment rule out conventional forms of union organizing. Instead they carve out an occupational community to establish conditions and standards of work and a credentialing system. In so doing they actually construct the demand for their own services, creating a separate tier of domestic workers for richer employers. As they negotiate through the labor market, they also reshape its contours.

Collective protest. For Habermas the "new social movements," with their concerns for issues that transcend class, such as peace, environment, and civil rights, exemplify collective resistance to the encroachment of the lifeworld by the system. Josepha Schiffman's study of dual tendencies in the peace movement and Joshua Gamson's study of gay and AIDS activism can be seen in the same light. Both movements are wary about participation within the system and devote their most original efforts to confronting it or holding it at bay. The peculiarities of their strategies of organization and protest do not derive simply from their antistatist stance but from the difficulties of dealing with a form of domination that is at once ubiquitous and invisible. Before they can begin to think about asserting control over the state and the economy, they have to make what is invisible visible. *rebruci*

The preceding typology of forms of resistance—from capitulation to the creation of alternative organizations, from negotiation within limits to the negotiation of limits, from anarchic outbursts to self-conscious collective protest—demonstrates the varied interplay between system and lifeworld, showing that the lifeworld is not an inert body but a source of continual contestation. But the struggle is an unequal one. We should not overestimate or romanticize the capacity of the lifeworld to fight back. The forms of resistance are constrained and continually challenged by new and more effective forms of domination. Still, resistance there is. We have tried to document its diverse forms, its sources, and its limitations. *