geography, anthropology as well as sociology, but not the economic sciences or political sciences that have become ever more asocial or antisocial. In the postcommunist era Marxism and sociology become collaborators in the defense of the social. Indeed, we can go further and declare that sociology itself is fast becoming a real utopia, providing a concrete imagination for an alternative world incompatible with capitalism. As such we need a reflexive ethnography to propagate the sociological imagination, the prophetic glue that can bind real utopias together while holding at bay the destructive forces of market and state. Sociology, if it is to survive, may have no alternative but to go public.

NOTES

INTRODUCTION

1. Outside sociology Victor Turner and Raymond Smith knew all about the Manchester School, and, just as I was leaving the University of Chicago, John Comaroff and Jean Comaroff arrived.

CHAPTER ONE

Acknowledgments: This chapter was twenty years in the making. Earlier versions are unpublished and barely recognizable. Two people in particular helped me bring this endeavor to a close. Erik Wright plied me with dozens of pages of intense argumentation to the effect that there can be only one model of science, while Peter Evans insisted that I persist despite all opposition. And opposition there was plenty, from hostile receptions in talks to dismissive reviews from journal referees. My ideas took shape in heated courses on participant observation and while working with Berkeley graduate students on two books, *Ethnography Unbound* and *Global Ethnography*. Teresa Gowan, Leslie Salzinger, Maren Klawiter, and Amy Schalet were intent on

holding me accountable for what I said, while Raka Ray, Jennifer Pierce, Charles Ragin, Michael Goldman, Raewyn Connell, Nora Schaeffer, and especially Linda Blum provided more gentle stimuli over the years. My greatest debt is to Jaap van Velsen, my first sociology teacher, who, as an anthropologist, exemplified the extended case method, although he'd recoil in horror at the formalization to which I have subjected it. Finally, Craig Calhoun braved the opposition to first steer this into print when he was editor of *Sociological Theory*.

1. For an overview of the Manchester School's approach to anthropology, see Kingsley Garbett (1970). For commentary on the Manchester Method by its leading figure see Max Gluckman (1958, 1961a, 1961b, 1964). Major expositions of the extended case method include Clyde Mitchell (1956, 1983); A.L. Epstein (1958); and van Velsen (1960, 1964, 1967). Andrew Abbott (2007) has written a fascinating and erudite critique of my appropriation of the extended case method, rightly distinguishing it from the original Manchester version.

2. On racism and labor markets see Liebow (1967) and Bourgois (1995). On urban political regimes see Whyte (1943), Susser (1982), and Haney (1996).

3. Dorothy Smith (1987). On family ethnographies see Stacey (1990), DeVault (1991), and Hondagneu-Sotelo (1994). Dorothy Smith's paradigm-breaking "sociology of women," originally written in 1977, begins by debunking abstract, decontextualized, and universalistic sociology as the ideology of ruling men and turns to the concrete lived experience of women as its point of departure. The microstructures of everyday life, which women direct, become the foundation and invisible premise for macrostructures controlled by men. This looks like the extended case method, but whereas Smith justifies it on the ground of the "standpoint of women," I ground it in an alternative conception of science. In this regard I am closer to Sandra Harding (1986, 1990), who works the terrain between androcentric science and postmodern dismissal of science. Rather than surrender science to men, Harding calls for a successor science. In her

subsequent writings Smith turns this opening rupture with mainstream sociology into a methodological universal. Thus her institutional ethnography, what Smith (2007) calls a sociology for people, pursues the link between local and extralocal by accentuating lived experience, relations of ruling, and their mediation through texts. She dismisses all other approaches to ethnography and ignores preexisting sociology, including feminist sociologies since the 1970s, except those that are done by her students, pretending to start afresh with every problem she tackles and thereby reproducing what we already know. She identifies problematic aspects of the extended case method, namely, the reification of external forces and the arbitrary invocation of theory, issues I take up in this chapter, but she has difficulty identifying any problematic aspects in her own methodology. All methodologies, like all theories, are limited, and they develop by openly recognizing and engaging with those limitations.

4. See A.L. Epstein (1958) for a description of the system at Luanshya Mine in the 1950s.

5. In chapter 2 I describe another study (Burawoy 1979), this time of a factory in South Chicago. Here I found myself in the same plant that had been studied by another sociologist thirty years earlier. I could have tried to show why his theory of "output restriction" was wrong, but instead I used it as a baseline from which to extend my own study back into history.

6. Just how difficult is it to control context effects can be seen in ethnographically sensitive survey research. In order to reduce "interview effects" survey research matches the race of interviewer and interviewee, but this can exaggerate respondent effects and field effects. Sanders (1995) shows that the wider racial field invades the interview so much that some black respondents imputed whiteness to their black telephone interviewers. Moreover, those blacks who identified their interviewer as white adopted more conciliatory attitudes. In their pen experiment Bischoping and Schuman (1992) show that the divergent polling results before the Nicaraguan election of 1991 were the result of the respondents' perception that the polling organization

was partisan. Bischoping and Schuman (1992) conclude that this was an artifact of the polarized situation in Nicaragua, but exactly how that field affected the responses remained unclear.

7. Stinchcombe (1980). In regard to social situations I am appealing to a methodological situationism (Knorr-Cetina 1981; Cicourel 1964) to replace a methodological individualism. Survey researchers might try to build social situation in as a variable, examining, for example, how a person's race is affected by situation, but that is very different from methodological situationism in which the situation rather than the individual is the unit of analysis. Thus Cicourel (1982) raises the problem of "referentiality"—what can we know about a given situation from a conversation that takes place in another situation?

8. See Clark and Schober (1992). In an inventive move Sniderman and Piazza (1993) try to build dialogue into their surveys by presenting respondents with predetermined counterarguments. For example, respondents are first asked whether they approve of government support for blacks. If respondents approve of spending increases, then they are asked whether they would feel the same way if blacks were singled out for special treatment. If, on the other hand, respondents do not approve of more spending, they are asked if they would feel the same if this meant that blacks would continue to be poorer than whites. The data show that 44 percent of whites were "talked out" of their original position. In the case of affirmative action only 20 percent changed their minds in the face of counterarguments. It is not clear why there should be such changes, whether Sniderman and Piazza are tapping context-specific attitudes, whether attitudes of whites toward race are pliable and superficial, or whether this is simply an artifact of the interview situation itself in which the respondent flows with an expected answer. Whatever else, these changes in responses suggest the importance of studying the interview itself as a social situation.

9. This distinction can be extended to the natural sciences. There are philosophers of the natural sciences, such as Michael Polanyi (1958), who refuse the separation of subject and object. His theory of personal knowledge gives centrality to the natural scientist who makes contact

with and dwells in "nature." Similarly, Evelyn Fox Keller (1983, 1985) makes the case that natural scientists, like social scientists, may also be part of the world they study, that they have a human relation to the objects under investigation. In her feminist view what is distinctive is not the objects of science but the gendered way we approach them. Finally, from a realist standpoint Roy Bhaskar (1979) insists on intervention and experiment as central to both the natural and social sciences. The distinction between reflexive and positive science does not have ontological foundations; it does not depend on the nature of the world being studied. The distinction between the two models lies not in its object (human as opposed to nonhuman) but in the relation of scientist to object.

10. In other words, I follow Abbott (1992, 1997) and Somers and Gibson (1994) in distinguishing the "narrative" of social process from the causality of social forces, but where they want to replace the second with the first, I insist on retaining a place for social forces as methodological expedient and experiential reality, framing and confining social processes.

11. Anthony Giddens (1984) has made structuration the leitmotif of his work. He seeks to transcend the dualism of subject and object, agency and structure, micro and macro by substituting the notion of duality in which practices simultaneously reproduce the conditions that enable them. He stresses how structure facilitates rather than constrains action, much as language allows speech. In the end, intuitive notions of structure evaporate and we are left with a voluntarist vision that emphasizes the control we exercise over our worlds. I return to a more conventional notion of structuration in which "structure," or "social forces," really do confine what is possible, although they are themselves continually reconfigured. What he understands as structuration is closer to what I call process, but even here I will give more centrality to structures of micropower that are beyond the control of individuals.

12. A substantial body of philosophy of science, informed by historical exploration of the growth of knowledge, argues that science

moves forward through the absorption of anomalies within paradigms (Kuhn 1962) or research programs (Lakatos 1978), as well as through competition among paradigms or research programs.

13. Rebecca Emigh (1997) has made the critical distinction between "deviant case" analysis, in which the outliers increase the generalizability of our theory, and "negative cases" analysis, which increases the "empirical content" of theory, what I have called theory reconstruction.

14. Again, Anthony Giddens (1992) has made much of this interchange between academic and lay theory, arguing that sociology appears not to advance because its discoveries become conventional wisdom. The reflexivity of social theory, he argues, is one of the distinctive features of modernity.

15. My position here is not unlike John van Maanen's three "tales of the field" (1988)—realist tales that privilege the participant, confessional tales that privilege the observer, and impressionist tales that accent the interaction of the two. The last, which is the one he favors, is parallel to the interventionist approach that I am advocating here.

16. Giddens (1984) and Sewell (1991). Still, I am closer to Bourdieu and Foucault than Giddens and Sewell, who have little to say about how power enters into the constitution of the conditions of our existence.

17. There is a large literature here starting from Rosabeth Kanter's (1977) analysis of organization processes to Ruth Milkman's (1987) analysis of the forces shaping the position of the gender line to Linda Blum's (1991) class analysis of the contending forces of affirmative action and comparable worth (parallel to the two meanings of *African advancement*).

18. Starting from tensions within Weber's analysis of bureaucracy and refusing Weber's monolithic characterization, Gouldner (1954) develops three types of bureaucracy—mock, representative, and punishment centered. In so doing Gouldner brackets the context of his gypsum plant and misses the historical specificity of his ideal types. The extended case method would have tried to locate the plant in its political, economic, and geographical context. See Burawoy (1982).

19. James Clifford's (1988, chap. 2) study of the French anthropologist Marcel Griaule highlights the strategies of power, the panoptic techniques of surveillance that the outsider uses in documenting the recalcitrant colonized. Ethnography depends on an unabashed power struggle between observer and participant. Clifford contrasts this with Griaule's subsequent initiation into Dogon life by one of its chiefs. Griaule becomes the interpreter of "authentic" Dogon culture, an ambassador who would defend their interests in a colonial world. From willful resister and liar the informant becomes colleague and teacher. But in neither case is there joint symmetrical construction of an ethnographic portrait. Power suffuses both dramaturgies.

20. The colonial encounter provides especially vivid examples of this close link between knowledge and power. See, for example, Mitchell (1988) and Stoler (1995).

21. See, for example, Alain Touraine's "action sociology," which insists on social scientists' working together with participants in a social movement (Touraine 1983, 1988) or "participatory action research" that designs the coproduction of knowledge to contest deep-rooted power inequities.

22. For a nuanced survey and evaluation of different approaches to "qualitative methods" that inclines toward postmodern approaches but without being dogmatic, see Denzin and Lincoln (1994).

23. Elsewhere I have elaborated the distinction between the extended case method and grounded theory (Burawoy, Burton, et al. 1991, chap. 13). A contemporary exemplar of grounded theory is Martin Sanchez Jankowski's (1991) *Islands in the Street*—a ten-year study of thirty-seven urban gangs in three metropolises. It is a remarkable, sustained commitment to positivism. Jankowski constitutes himself as ethnographer and outsider. He tries to minimize his own involvement, although this could never be complete if he was to survive. In seeking general claims across the three cities about gang organization, business activities, patterns of violence, as well as relations to the community, to the criminal justice system, local politicians, and to the media, he has to standardize his evidence and his categories, leading to thin rather

than thick description, correlations rather than processes. In making the cases comparable, he brackets the geographical and historical context—both the importance of the specific urban context and changes during the ten-year period of the study. He homogenizes space and time. In building up his theory from the ground, he systematically codes and classifies all the evidence. He tends to reject (or sometimes endorse) other theories but rarely enters into sustained dialogue with them.

24. Feminists have also explored this clinical or dialogical approach to interviewing. See, for example, Oakley (1981) and De Vault (1990).

25. Even the very best of methodological texts compound these different levels. Charles Ragin's (1987) comparison of "variable" analysis and "case study," while overlapping with some of the distinctions between survey research and the extended case method, assumes there to be a single model of science, one that we all share and therefore not requiring explication.

26. Burgess (1927: 114) writes: "The case-study method was first introduced into social science as a handmaiden to statistics." He was referring to such early sociologists as LePlay, who used monographic studies to prepare the basis for greater statistical studies. But, Burgess continues, there is nothing inherently unscientific about the case study "provided that it involves classification, perception of relationships, and description of sequences" (117). He, of course, sees these as two techniques for getting at the truth and not two methods corresponding to two visions of social science.

CHAPTER TWO

This chapter was launched in a dissertation seminar where it received spirited criticism from Bill Hayes, Linus Huang, Rachel Sherman, and Michelle Williams. On the road it picked up comments and suggestions from many, including Julia Adams, Philip Bock, Patricia Clough, Mitchell Duneier, Steve Epstein, Jim Ferguson, Maria Patricia Fernandez-Kelly, Marion Fourcade-Gourinchas, Herb Gans,

Tom Gieryn, Teresa Gowan, Richard Grinker, Lynne Haney, Gillian Hart, Mike Hout, Jennifer Johnson-Hanks, Gail Kligman, Louise Lamphere, Steve Lopez, Ruth Milkman, Sabina Neem, Sherry Ortner, Mary Pattillo, Melvin Pollner, Leslie Salzinger, Ida Susser, Joan Vincent, Loic Wacquant, Ron Weitzer, and Erik Wright. I also thank the four reviewers from the *American Sociological Review,* in particular Diane Vaughan, whose inspired commentary led to major revisions, and Chas Camic, whose persistent critical interventions kept my argument on an even keel. This venture was made possible by a year at the academy's Arcadia, the Russell Sage Foundation, to which revisits are rightly, but sadly, barred.

1. As I will show in chapter 4, reflexive ethnography can also be developed through synchronic comparisons—comparing two factories, communities, schools, and so on—in different spatial contexts, as well as through the diachronic comparisons of the temporal revisit that form the basis of this chapter.

2. Or, even worse, the same ethnographer will have divergent interpretations of the "same" event. Thus Van Maanen (1998) describes his fieldwork among police on patrol successively as a realist tale that strives for the "native point of view," as a confessional tale that is preoccupied with the fieldworker's own experiences, and as an impressionistic (from the art school of impressionism) tale that brings the fieldworker and subject into a dynamic relationship. Margery Wolf (1992) similarly presents her fieldwork on shamans in Taiwan in three different ways: as field notes, fictional account, and professional article. While recognizing the importance of experimental writing and the contributions of the postmodern criticism of ethnography, Wolf ends up defending the professional article with its rules of evidence and interpretation. Such polyphony calls for a vocabulary and framework beyond "replication."

3. Abbott (1999, chap. 7) argues that the Chicago School ethnographies were "historical" in that they were concerned with process. In my view Chicago ethnographies were largely bereft of process, let alone history. If process or history entered the Chicago School, it was

in the form of the general cyclical theories of social change associated with Robert Park.

4. At least in one area sociologists practicing participant observation have embraced history, theory, and context. Science studies began as a reaction to grand Mertonian claims about the normative foundations of scientific knowledge. It turned to the daily practice of laboratory life (Latour and Woolgar 1979)—a resolutely microanalysis drawing on strains of ethnomethodology. These laboratory studies then relocated themselves in the wider context that shaped science and its history but without losing their ethnographic foundations. See S. Epstein (1996), Fujimura (1996), and Latour (1988).

5. *Manufacturing Consent* (Burawoy 1979) is the published version of my Chicago doctoral dissertation.

6. Roy (1952a, 1953, 1954). I was familiar with a number of other studies of piece rates that showed similar patterns of "output restriction" (see, especially, Lupton 1963).

7. According to Chapoulie (1996: 17), Everett Hughes considered Roy's dissertation "one of the best he had supervised."

8. Merton (1957). When a finding is controversial, replication might pay off. A case in point was the heated and seemingly everlasting debate between pluralist and elite perspectives on community power. Hunter (1953), whose reputational study (an ethnography of sorts) of Atlanta in 1950 led the charge for the elite perspective, revisited Atlanta in the early 1970s to confirm his original finding (Hunter 1980). The very different conditions he found in Atlanta (emergence of a black elite, expansion of the downtown, importance of information technology, etc.) made the replication all the more persuasive. The more diverse the conditions under which a finding holds, the more robust it becomes. Because the conditions of Roy's ethnography and mine were so similar, replication was less interesting than was the explanation of small changes.

9. Similarly, Howard Becker (1998: 89) reduces my revisit to studying the "same problem" under "new conditions." In so doing he misses the distinctiveness of my extended case method. First, I didn't study the same problem but the opposite problem. That is, he ignores my inversion of Roy's theoretical framework (from the human relations question—why people don't work harder—to the Marxist question—why people work so hard). Second, he misses the historical focus of the study, namely, my attempt to explain changes on the shop floor between 1944 and 1974. Therefore, third, he overlooks my examination of external forces as the source of such change. The problem with both Roy and Becker is not their critique of *Manufacturing Consent* but their anodyne assimilation of the study to a methodology it opposes: the methodology of Hughes (1971), thematized by Becker (1998), that searches inductively for what is common to the most disparate of cases rather than explaining divergences. To be sure, there are insights to be gleaned from showing the similarity between janitors and physicians, but there is also much to be gained by examining why medical and janitorial services have each changed over time or why each varies from place to place.

10. I am here referring to the Manchester School of social anthropology and its extended case method (see chapter 1 and Van Velsen 1967).

11. There have been numerous criticisms of *Manufacturing Consent,* most recently in a symposium edited by Gottfried (2001). These and other criticisms include excellent reanalyses, but few bear directly on my revisit to Geer.

12. Abbott (2001, chap. 3) has written a delightful account of how constructionism and realism reproduce each other. Each is incomplete without the other; each corrects the other.

13. Bourdieu (1990). It should be clear that, like Bourdieu and Wacquant (1992) or Morawska (1997), I do not reduce reflexive ethnography to the relationship between observer and informant (as Rabinow [1977] and Behar [1993] do in their accounts). First, a reflexive ethnography is reflexive in the sense of recognizing not only the relation we have to those we study but also the relation we have to a body of theory we share with other scholars. Second, a reflexive ethnography is ethnographic in the sense that it seeks to comprehend an external world both in terms of the social processes we observe and the external forces we discern.

14. This strategy of indicting one's adversaries by stressing their extrascientific motivation or their nonscientific practices is not confined to the social sciences. In *Opening Pandora's Box* Gilbert and Mulkay (1984) show how biochemists, entangled in disputes about "the truth," deploy two types of discourse: an empiricist discourse that deals in "the facts" and a contingent discourse that attributes errors to noncognitive (social, political, and personal) interests. Scientists apply the empiricist discourse to themselves and the contingent discourse to their opponents. We find the same double standards in type 1 revisits. The revisitor's research is beyond reproach, while the predecessor's research is marred by flawed fieldwork, by biases resulting from biography, location, or embodiment. In these cases of refutation, as for the scientists studied by Gilbert and Mulkay (1984), revisitors exempt themselves from such biases or inadequacies in their own fieldwork—but the grounds for such exemption are more presumed than demonstrated. Critics easily turn the tables on the revisitor by playing the same game and revealing his or her biases.

15. Journals devoted special sections (*American Anthropologist* [1983]: 908–47; *Current Anthropology* [2000]: 609–22) or even whole issues (*Canberra Anthropology* vol. 6, nos. 1, 2 [1983]; *Journal of Youth and Adolescence* vol. 29, no. 5 [2000]) to the controversy. A number of books have appeared (Caton 1990; Freeman 1999; Holmes 1987; Orans 1996), a documentary film was made (Heimans 1988), and a fictionalized play was produced of this high drama in the academy (Williamson 1996).

16. In her comments as a reviewer, Diane Vaughan made this point. So did Richard Grinker in private remarks to the author. Grinker (1994, 2000) revisited the Central African sites of the famous and controversial anthropologist Colin Turnbull.

17. Vincent (1990, chap. 4) situates Lewis's critique of Redfield in much broader moves toward historical analysis that preoccupied postwar anthropology in both England and the United States. I also note, parenthetically, that Redfield was deeply influenced by the Chicago School of urban ethnography, at that time dominated by Robert Park and Ernest Burgess. Indeed, Redfield married Park's daughter and started out studying Mexicans in Chicago under the direction of Burgess. So one cannot be surprised by his ahistorical, acontextual approach to historical change.

18. In a further revisit to Tepoztlan in 1970 Bock (1980) focuses on the continuing potency of the symbolic life, generating yet another type 2 revisit, reconstructing the interpretations of both Redfield and Lewis.

19. In 1948 Redfield (1950) actually conducted his own revisit to a village he had studied seventeen years earlier. *A Village That Chose Progress* reads like a Durkheimian fairy story of a community moving along the folk-urban continuum or, as Redfield puts it, taking "the road to the light" (1950: 153)—and the light is Chicago. This is an unreal realist revisit of type 3.

20. Even apparently robust internal explanations of social change, such as Michels's "iron law of oligarchy" ([1910] 1962), have been subject to punishing criticism for bracketing historical context. Schorske (1955), for example, showed how the bureaucratic tendencies of the German Social Democratic Party, the empirical basis of the iron law of oligarchy, were a function of a range of forces emanating from the wider political field. Coming closer to ethnography, I (Burawoy 1982) inveighed against Gouldner's (1954) classic case study of the dynamics of industrial bureaucracy for bracketing the external economic context of his gypsum plant.

21. Leach (1954), Barth (1959). David Nugent's (1982) reanalysis of *The Political Systems of Highland Burma* showed that changes in the region were a product of political instability in neighboring China, changing patterns of long-distance opium trade, and contestation between British and Burmese armies as much as they were a product of internal processes. Before Nugent, Talal Asad (1972) had shown the limitations of Barth's Hobbesian model of equilibrium politics by refocusing on class dynamics, in particular, the secular concentration of land-ownership and how this was shaped by colonial forces beyond the immediate region.

22. Later in this chapter I call this type of historical digging an archeological revisit.

23. Another explanation of Lynd and Lynd's (1937) focus on Family X is that Robert Lynd was criticized by residents for omitting it from Middletown I. Bahr (1982) goes so far as to imply that Lynd drew his ideas about the importance of Family X from a term paper written by a resident of Middletown, Lynn Perrigo, that was critical of the first Middletown study. Merton (1980) wrote a letter to Bahr, questioning his insinuations of plagiarism and suggesting alternative reasons for Lynd's change of focus in Middletown II. (See also Caccamo 2000, chap. 4.)

24. The original surveys in Middletown I were not replicated by Robert Lynd in Middletown II, in part, I suspect, because of the absence of Helen Lynd.

25. As Robert Lynd himself wrote: "The current emphasis in social science upon techniques and precise empirical data is a healthy one; but, as already noted, skillful collection, organization, and manipulation of data are worth no more than the problem to the solution of which they are addressed. If the problem is wizened, the data are but footnotes to the insignificant" (1939: 202). Mark Smith (1984) reviewed the Middletown III studies as betraying Robert Lynd's project of critical sociology. Caplow (1984) responded that he and his colleagues were just good social scientists, examining hypotheses put forward by the Lynds and describing the complex social changes since 1924.

26. In the extensive literature on replication, of particular interest is Bahr, Caplow, and Chadwick's (1983) discussion of the problems of replication with respect to their own Middletown III studies.

27. Macdonald (2000) writes about the effects of Firth himself on her own revisit to Tikopia. The Tikopians would cite Firth back to her as the authentic interpretation of their society, and they treated her as his daughter. The chiefs in particular embraced the portrait that Firth had painted of a proud and independent people, captured in the title to his first book, *We, the Tikopia* (Firth 1936).

28. Foster et al. (1979). See Phelps, Furstenberg, and Colby (2002) for a parallel collection of studies that engages some strikingly similar methodological issues in quantitative longitudinal research.

29. Needless to say, Duneier's engagement with Jacobs is already a form of theoretical reconstruction—an externally imposed lens.

30. The archeological revisit goes back to Thomas and Znaniecki (1918–20), who used letters written to Polish immigrants in Chicago to construct the social structure and malaise of the sending communities.

31. Or, since most have retired, perhaps I should study the occupations of their children in a generationally based revisit? This is what Sennett implicitly does when he moves from his account of blue-collar workers in *Hidden Injuries of Class* (Sennett and Cobb 1972) to studying the new service workers in the *Corrosion of Character* (Sennett 1998).

32. As we know from Adams, Clemens, and Orloff (2005), who have compiled a wonderful compendium of novel explorations, comparative historians are ready to embrace ethnography, so ethnographers can only gain from taking their exploration in comparative and historical directions.

CHAPTER THREE

Acknowledgments: I wrote the first, crude version of this chapter in the fall of 1985 for my dissertation seminar. Soon Kyoung Cho, Linda Blum, Vedat Milor, Gay Seidman, Louise Jezierski, and Brian Powers greeted it with bewilderment, dismay, and even horror. Had their adviser gone mad? After that I moderated the argument many times under the influence of their comments as well as those of Vicki Bonnell, Carol Hatch, Elizabeth Nichols, Michael Liu, Charles Tilly, Ira Katznelson, Arthur Stinchcombe, Jerry Karabel, Adam Przeworski, Wally Goldfrank, Wolfgang Schluchter, Erik Wright, Alan Sica, Kathleen Schwartzman, Reinhard Bendix, Julia Adams, Ron Aminzade, Barbara Laslett, Bill Sewell, Perry Anderson, Rick Biernacki, Rebecca Scott, Bill Rosenberg, and Jeffrey Alexander. I should also like to pay tribute to the patience of Bill Form, the editor of the *American Sociological Review*, and his battalion of six referees who, over a period of two years, instigated two major revisions and more than sixty pages of written exchange. Although in the end our differences proved too great to bridge, I believe the essay has benefited substantially from their objections. The essay was eventually published in *Theory and Society* in 1989. Of the two anonymous referees I should particularly like to thank the one who provided a superb set of criticisms of my handling of Skocpol, forcing me to revise the argument once more. Finally, the issues I address here were central to the methodology course (before it was banished) that tainted four consecutive cohorts of graduate students who entered the Berkeley Department of Sociology between 1984 and 1987. It was with those students that I explored the meanings of science and, specifically, social science. I am grateful to all the people I have mentioned for pointing out major flaws in the chapter, forcing me to revise, clarify, and elaborate its claims.

1. It is important to emphasize that those who have criticized the use of the "scientific method" to study the social world have assumed and adopted outdated positivist conceptions of science. The irony is that the alternative interpretive approaches to sociology, proposed by humanists with an antiscientific bent, often turn out to be similar to the historical understandings of science as found in, for example, Polanyi (1958), Kuhn (1962), Toulmin (1972), Feyerabend (1975), and Lakatos (1978). In this chapter I follow Lakatos's methodology of research programs not because it best fits Trotsky's approach but because it most satisfactorily explains the growth of scientific knowledge.

2. It is particularly strange, therefore, to find Skocpol vilifying Marxism along with other theories because it "theorize[s] on the basis of a voluntarist image of how revolution happens . . . focus[es] primarily or exclusively upon *intra*national conflicts and processes of modernization . . . analytically collapse[s] state and society or they reduce political and state actions to representations of socioeconomic forces and interests" (Skocpol 1979: 14). Inexplicably, we hear nothing of Trotsky—neither his view of history as a dramatic script in which actors can interpret only their assigned parts, nor his theory of the combined and uneven development of capitalism on a world scale, nor

even his obsessive interest in the autonomy of the state. Indeed, throughout the whole of her book she refers to him twice and then only in passing: in connection with his remark that 1905 was a dress rehearsal for 1917 and when describing the organization of the Red Army (1979: 94, 217). There is no reference to his theory of the Russian Revolution or indeed to his writings on the French Revolution or his prophetic commentaries on the Chinese Revolution.

3. Lakatos (1978: 8–101). Apart from Lakatos's own work, three volumes apply and discuss his ideas: Howson (1976); Cohen, Feyerabend, and Wartofsky (1976); and Radnitzsky and Andersson (1978).

4. Eli Zahar (1978), for example, addresses this problem directly by trying to show that the methodology of scientific research programs represents the best available reconstruction of the intuitive methodology in cases of major scientific advance.

5. Just as this chapter is not concerned with social revolutions per se, it is also not intended as a defense of Marxism. It is a discussion of two methodologies that are not necessarily tied to any particular theoretical framework. Accordingly, I link Trotsky's theories to the methodology of research programs and not to the methodological prescriptions of Marx. The methodology of research programs has also informed reconstructions of "structural functionalism." See, for example, Jeffrey Alexander (1982, 1983, and 1987). These reconstructions do not strictly follow Lakatos as they give little weight to the discovery and corroboration of new facts, and indeed one could say it was a good example of a degenerating research program.

6. Critics have complained that I deal with only a single example of each methodology and therefore I have not demonstrated my claims about the consequences of adopting different methodologies. Undoubtedly, my argument would be more persuasive if other cases were included. Even if space were not a problem, finding suitable cases is not so easy. To isolate the effects of methodology, each case should be, as far as possible, methodologically pure and postulate comparable theories. Such were the reasons that led me to Skocpol and Trotsky. Although these examples are not perfect, better cases might be hard to find.

7. Skocpol (1979: 6). Charles Ragin and David Zaret (1983: 746) claim that Weber's method of genetic explanation, which seeks particular historical trajectories, is "no less evident in work by Bendix and Skocpol." Contrary to her own conception of what she is doing, they claim that her adoption of Mill's methods is *not* in pursuit of generic explanation characteristic of statistical analysis. In what follows I show she does attempt to mimic statistical strategies of comparison, and with the adverse consequences Ragin and Zaret anticipate. Nevertheless, their assessment reflects a real tension in Skocpol's book. I follow Elizabeth Nichols's (1986) identification of the latent genetic, or "conjunctural," analysis behind Skocpol's reduction of all revolutions to peasant revolt and international pressure on the state. Skocpol explains these two factors as emergent from a constellation of forces particular to each revolution, a mode of explanation that is at odds with Mill's canons of induction. Her rejoinder to Nichols (Skocpol 1986) refuses to recognize Weber's distinction, following Rickert, between the generalizing and the particularizing cultural sciences. She misunderstands the critique as accusing her of misapplying Mill's canons, when Nichols was pointing to the coexistence of a different method. She seems so caught up in a linear causality in which every factor must make the same causal contribution to each revolution that she is blind to her own subterranean use of a different notion of causality. Skocpol (1973) treats Barrington Moore's *Social Origins of Dictatorship and Democracy* in a similar way, forcing it into the mold of generic (generalizing) explanation, when much of his analysis seeks genetic (particular) explanations for modernization.

8. While it is true that John Stuart Mill (1888) did advocate, with important qualifications, the method of induction, or what he calls the experimental or chemical method for the natural sciences, he explicitly repudiates its applicability to the social sciences. In the study of society where "the causes of every social phenomenon . . . are infinitely numerous" (1888: 612), one cannot assume that one effect has always the same causes, so that revolutions, for example, may be caused by different factors in different countries. The method of difference is

of even less value, according to Mill. One must find cases in which two societies are alike in every respect except the one that we are trying to isolate as a causal factor. "But the supposition that two such instances can be met with is manifestly absurd" (610). Skocpol's resolute application of the two canons justifies Mill's skepticism. The point is not that Skocpol failed to execute the method of induction properly but rather the method is, as Mill well knew, "completely out of the question" in the social sciences. Skocpol, of course, is quite aware of these flaws—the impossibility of so controlling the variables as to execute the method of difference, that the units being compared are rarely if ever independent and that induction cannot be a substitute for theory (Skocpol 1979: 38–39). Yet she still clings to this as the best approach. While not "without its difficulties and limitations," nevertheless, "provided it is not mechanically applied, it can prompt both theoretical extensions and reformulations, on the one hand, and new ways of looking at concrete historical cases, on the other" (40).

9. Charles Tilly (1984: 105–15) argues that Skocpol pays too much attention to the method of agreement and not enough attention to the method of difference. He suggests looking at variations within those societies that experienced revolutions, both regional differences at the time of its outbreak and why revolution did not occur at earlier periods. Elsewhere, however, Tilly (1976: 159) notes that revolution is a state of a whole society and cannot be explained by comparison of its parts. As to comparing different moments in a society's history, it is notoriously difficult to explain a nonevent. There are, as Mill warns, just too many variables to control for. The problem lies not with Skocpol's failure to use the method of difference but with the method of induction itself, which underestimates the importance of earlier theory and takes the facts as given. Very different is the method of *The Vendee,* where Tilly is sensitive to the social construction and tendentiousness of historical facts as well as to the necessity of proceeding deductively from a theory, in his case a theory of urbanization. When a researcher collects his or her own data with a view to careful comparative analysis, the need to grapple with the illusive, complex,

and uncertain character of "facts" compels a much stronger dependency upon earlier theorizing. Even more important, Tilly seeks to reconstruct theory based on an anomaly—a counterrevolutionary movement in revolutionary France—rather than discover theory inductively.

10. Skocpol (1979:4). She also writes: "Social Revolutions are rapid, basic transformations of a society's state and class structures." One has to ask whether the Chinese Revolution of 1911 fits this definition, given that the transformation is completed only in 1949. Skocpol herself refers to the period 1911 to 1949 as a revolutionary interregnum (1979: 80, 148). How rapid is "rapid"?

11. Here again Mill (1888: 612) himself warns against the method of agreement: "From the mere fact, therefore, of our having been able to eliminate some circumstances, we can by no means infer that this circumstance was not instrumental to the effect in some of the very instances from which we have eliminated it. We can conclude that the effect is sometimes produced without it; but not that, when present, it does not contribute its share." Even in the discussion of the natural sciences Mill (book 3, chap. 10) sensitizes us to the problem of plurality of causes, that the method of agreement assumes "that there is only one assemblage of conditions from which the given effect could result" (1888: 311).

12. Carl Hempel's deductive-nomological model recodifies Humean causality of "constant conjunction" by insisting that the connection between antecedent conditions and outcomes has to be explained by universal "covering laws." Hempel would argue that Skocpol does not distinguish between antecedent conditions and "laws": "A related error occurs in singling out one of several important groups of factors which would have to be stated in the initial conditions, and then claiming that the phenomenon in question is 'determined' by that one group of factors and thus can be explained in terms of it" (Hempel 1965: 239). That his model is in fact rarely carried out in historical analysis Hempel attributes to the complexity of historical laws, while Popper (1957) contrarily argues it is often their triviality that leads to their omissions. Be that as it may, Skocpol seems to share

Hempel's distrust of invoking causal mechanisms as a defining feature of explanation. For a general critique of the shortcomings of such empiricism, see, for example, Richard Miller (1987, pt. 1).

13. In her rejoinder to Sewell's review Skocpol (1985a: 86–87) writes: "Few aspects of *States and Social Revolutions* have been more misunderstood than its call for a 'nonvoluntarist,' 'structuralist' approach to explaining social revolutions. . . . For the point is simply that no single acting group, whether a class or an ideological vanguard, deliberately shapes the complex and multiply determined conflicts that bring about revolutionary crises and outcomes." But which serious scholar argues that the intentional action of a single actor is a sufficient cause for revolution? Here Skocpol criticizes theories no one holds and holds theories no one criticizes. The actual claim she pursues in her book is more interesting. There she denies that the intention of a collective actor to make a revolution is necessary for its outbreak. However, this is not empirically examined, let alone justified, and is linked, I argue, to the character of her causal analysis.

14. Stinchcombe (1983: 12–15, 247–50) makes the same criticism of Skocpol for leaving out the microfoundations of revolutionary process but does not attribute this to her method.

15. Although Mill does not regard facts as problematical, he does recognize their underdetermination of explanation: "Accordingly, most thinkers of any degree of sobriety allow that an hypothesis of this kind is not to be received as probably true because it accounts for all the known phenomena, since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses; while there are probably many others which are equally possible, but which, for want of anything analogous in our experience, our minds are unfitted to conceive. But it seems to be thought that an hypothesis of the sort in question is entitled to a more favorable reception, if, besides accounting for all the facts previously known, it has led to the anticipation and prediction of others which experience afterwards verified" (Mill 1888: 356).

16. As Karl Popper (1963: 50) has pointed out, there is a latent affinity between induction and dogmatism: "For the dogmatic attitude is

clearly related to the tendency to *verify* our laws and schema by seeking to apply them and to confirm them, even to the point of neglecting refutations, whereas the critical attitude is one of readiness to change them—to test them; to refute them; to *falsify* them, if possible. This suggests that we may identify the critical attitude with the scientific attitude, and the dogmatic attitude with the one which we have described as pseudo-scientific."

17. Subsequently, Skocpol (1985b) did examine the capacity of states, and with it her hostility to research traditions takes a new twist. On the one hand neo-Marxist theories of the state are severed from their Marxist roots, locating them in the academic debates in the United States of the 1960s and 1970s. As Paul Cammack (n.d.) suggests, this is a curious move for one so committed to historical analysis. On the other hand, in the very act of rejecting research programs tout court she launches her own, calling on Weber and Hintze as potential forefathers of the "state centric" perspective. But even here she vacillates between a strong thesis in which state dynamics are the central force in history and a weak thesis that argues simply that the state cannot be left out of account. There continues to be a strong inductivist commitment to confirmation, to purging her theories of counterexamples even at the cost of their explanatory power. Thus, when confronted with anomalies, instead of specifying and reconstructing her strong thesis, she abandons it for her weak thesis, which is trivially true. See Erik Wright (1986).

18. Similar arguments have been made against classical anthropological studies in Clifford and Marcus (1986). Introductory remarks or reflections on fieldwork are separated from the "real" science of anthropology. On further examination those remarks and reflections prove to be constitutive of, not separate from, the main text. Thus Renato Rosaldo shows how the results of Evans-Pritchard's study of the Nuer were influenced by the context of colonial domination and civil war, just as Le Roy Ladurie's account of Montaillou represses the effect of relying on data gathered in an inquisition (Clifford and Marcus 1986: 77–97). Both bracket the domination that makes knowledge possible.

Clifford argues that anthropological texts have multiple "registers"—a manifest voice of science alongside a latent voice in search of an essential, uncontaminated, natural world, what he calls the pastoral mode (98–121). Ethnography is an allegory with ethical or political messages for advanced industrial societies. For example, Derek Freeman's critique of Margaret Mead's controlled experiment in the field makes her account of the Samoans look less like science than a moral and practical lesson for the American people (see chapter 2).

19. Peter Beilharz (1987) has argued that Trotsky, far from deducing the direction of history, imposes a telos on history—the inevitability of socialism and the view that in the final analysis history must be on the side of the working class. Beilharz seeks to discover in Trotsky's early writings the seeds of his later unimaginative defense of Marxism. All that he finds there is Trotsky's use of generative metaphors of birth and death, disease and health, seed and fruit, and the idea of history as theater in which actors can interpret only scripts handed to them. But Trotsky's writings cannot be reduced to metaphor or to his eschatology. How one reaches socialism, with what means and when, is not given but the subject of his investigations, his innovations, his prophecies, as well as his struggles. In projecting back into Trotsky's early writings the most dogmatic formulations in his later writings, Beilharz is committing the same generative sin of which he accuses Trotsky. In so doing he marginalizes Trotsky's important contributions to Marxism.

20. Lakatos (1978: 48). Lakatos himself considered Marxism to be a degenerate research program, a claim he made from within the context of Soviet Marxism. To be sure, this would become a degenerate branch of the Marxist tradition, but the branch must not be mistaken for the whole tree. See Burawoy (1990b).

21. Trotsky (1969: 52). After the revolution of 1917, and particularly after Lenin's death in 1924, Trotsky, like the other Bolsheviks, would seek out parallels with the French Revolution. Unwillingly, Trotsky would come to the conclusion that the bureaucratization of the revolution could be seen as a Soviet Thermidor and that Stalin had become the Soviet Bonaparte. But while Trotsky saw the process of

bureaucratization as similar, he nevertheless regarded the outcomes as well as the causes of the French and Russian revolutions as different. See Trotsky ([1936]1972, chap. 5), Knei-Paz (1978: 392–94), Deutscher (1959: 311–14, 342–47, and 457–64), and Deutscher (1963: 313–18).

22. This is a capsule summary of chapters 1 and 2 of *Results and Prospects* and of the same argument presented in more detail in chapter 1 and appendix 1 of volume 1 of *The History of the Russian Revolution.*

23. Stinchcombe (1978, chap. 2). I do not endorse Stinchcombe's (1978) claim that Trotsky's insights have nothing to do with his Marxism, that good theory comes from inspired interrogation of the facts. "Deep analogies" don't spring tabula rasa, under the influence of genius, from "the facts." Trotsky's commitment to Marxism and his need to revise it led him to delve into the molecular processes of revolution. Fortunately, Stinchcombe's (1978: 65–66) theoretical sense gets the better of his empiricist polemic when he recognizes that Trotsky's account of the Russian Revolution can be understood only in the light of his earlier theory of combined and uneven development. In fact, his empiricist polemic is confined to the opening and concluding chapters and does not obscure his fascinating reconstructions in between. Stinchcombe's comparison of Tocqueville and Trotsky does indeed illuminate the construction of causal processes out of historical events, but it does not demonstrate the irrelevance of the intellectual traditions in which each is embedded. As Charles Tilly (1981, chap. 1) has underlined, historical analysis as immaculate conception is a myth.

24. This is also how John Roemer (1986: 192) sees the project of analytical Marxism: "What Marxists must provide are explanations of *mechanisms,* at the micro-level, for the phenomena they claim come about for teleological reasons." In a similar vein what Jon Elster (1985: 5) finds of lasting importance in Marx is the use of methodological individualism: "[T]he doctrine that all social phenomena—their structure and their change—are in principle explicable in ways that only involve individuals—their properties, their goals, their beliefs and their actions." In drawing on the rational choice models of neoclassical economics, they

move toward a mythological rather than a methodological individualism (Burawoy 1986, 1995). If they are serious about their microfoundations, they would do better to study Trotsky's *History of the Russian Revolution* than Walras.

25. The same can be said of "Trotskyism," itself very much divided by what it inherits from Trotsky. On the one hand C. L. R. James and Raya Dunayevskaya return to Trotsky's early hostility to Bolshevism and his spontaneitist faith in the revolutionary spirit of the working class while characterizing the Soviet Union as state capitalism. On the other hand, Ernest Mandel and Isaac Deutscher embrace a more top-down view of history as well as a more optimistic assessment of the Soviet Union as a degenerate workers' state. See Beilharz (1987, pt. 2).

26. Although "facts" are themselves theoretical constructs of sense data, what Feyerabend (1975) calls natural interpretations, they have greater stability than the theories created to explain them. That is to say, they have an obduracy—if for no other reason than by convention, as in Popper's basic statements—that allows them to act as falsifications of explanatory theories.

CHAPTER FOUR

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1. This section draws on the analysis in Burawoy (1980).

2. This section draws on analysis previously reported in Burawoy (1985).

3. I would also throw the "colonial despotism" of southern Africa into the mix, but it is not essential to the story I tell here.

4. This section draws on research previously reported in part 1 of Burawoy and Lukács (1992).

5. This section draws on research previously reported in part 2 of Burawoy and Lukács (1992).

6. This section is based on research previously reported in Burawoy and Hendley (1992).

7. This section draws on research previously reported in Burawoy and Krotov (1992, 1993) and Burawoy (1996).

8. This section draws on the research previously reported in Burawoy, Lytkina, and Krotov (2000) and Burawoy (2001).

CONCLUSION

1. The Real Utopias Project is Wright's undertaking at the Havens Center at the University of Wisconsin. So far four books have been published, but Fong and Wright (2003) is perhaps the best representative of the project, examining the logic, the limits, and the possibilities of experiments in deepening democracy in different parts of the world. Wright (2006) has elaborated a broad theory that encompasses the broader vision of the real utopias project.

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