Critical Sociology: A Dialogue Between Two Sciences*

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It is as a sociologist of work that I embark upon an exploration of the discipline of sociology, in both its immanent tendencies and its potentialities. I draw inspiration from Arthur Stinchcombe's (1959) classic essay which dissolves the unity of Max Weber's ideal type of bureaucracy into a *bureaucratic* mode and a *craft* mode. In the first, procedure guarantees the quality of the product. In the second the craftworker is guided by an evolving vision of the final product. Conception and execution are brought together in a spiral of mutual affirmation.

In the belief that we cannot be an exception to our own theories, I turn Stinchcombe's distinction back on sociology, dividing *sociological work* into bureaucratic and craft forms. Bureaucratic sociology works with a detailed, planned division of labor, governed by procedures that insulate scientists from their objects of investigation. Craft sociology is guided by a cognitive map, or theory, which shapes even as it is reshaped by interaction of participant and observer.

Practices require justification. Like any other social science, sociology justifies its practices with treatises on methodology, what I will call *models of science*. Bureaucratic sociology appeals to *positive* models of science, whose hallmarks are separation, distance, and detachment. Craft sociology, on the other hand, calls on *reflexive* models of science whose leitmotifs are connection, proximity and dialogue. I say reflexive *science* deliberately since so often the connotation of craft is the opposite—an ineffable, creative act more akin to artistic expression. My first goal, therefore, is to explicate *reflexive science as an ideal typical counterpart to positive science.*

We can pursue the parallels between sociology and industry further. As in Stinchcombe's analysis of the construction trades, so in sociology craft work is no hangover from the past, doomed to extinction in the face of an ascendant bureaucratic ethos. Rather, each theoretical practice is recreated to fit specific conditions, interests, and needs of the world that we engage and seek to transform. Each science with its corresponding methods has its own niches. No longer can we or should we aspire to any scientific unity. Indeed, my second goal is to argue that sociology's distinction—its disciplinary calling in the information age—lies in rejecting scientific monotheism in favor of *a duality of scientific models that portends a mutually enriching, reciprocal engagement of positive and reflexive science.*

The third parallel between industry and sociology invokes the gap between principles and practice. If industrial and organizational sociology has demonstrated anything it is that context subverts bureaucracy from below, while power dilutes craft production from above. As in industry so in science: Between principles and practice, between model and method, lies an irremovable hiatus produced by *context* in the case of positive science and by *power* in the case of reflexive science. These barriers to perfection inspire each science to scale greater heights. Even as impediments to transparency, these limits to pure science shed as much light on our social world as does the scientific product itself. They offer a critique of science, but one that neither renounces science as an illusory exercise nor denounces it as a strategy of power. Rather, as we shall see, critique of society remains lodged in the very practice of science. Thus, my third goal: *to show how the limitations of science expose the limitations of the world.*

Positive Science And Survey Research

In Comte's original use, *positivism* was intended to replace metaphysical thinking with the search for empirically founded laws of society. Sociology was the last of the disciplines to enter the kingdom of positivism; from there, armed with superior moral insight, it would rule over the unruly, creating order and progress out of chaos. Comte's nineteenth-century positivism has been divested of its self-aggrandizing reflexivity and tailored to twentieth-century professionalism.

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Its project is now the humble one of accurate representation—the mirroring of an external world. It presumes that scientists are incommensurable with, irreducible to, and unaffected by the objects they seek to represent. In short, scientists are presumed to be outside the world they study.

I call this stripped-down version of positivism simply *positive science*, usefully encoded in Jack Katz's (1983) four regulatory principles, the 4Rs. First, we must avoid *reactivity*: disturbing the world being studied for fear of introducing bias, creating the very world we seek to comprehend. Second, by standing aside from that world, we face the problem of *reliability*: how to systematically select from the mass of data it presents. Third, once we obtain results we must be assured that they do not reflect idiosyncrasies of the observer: They must be *replicable* by another observer. Finally, the data we gather cannot be idiosyncratic to the world we study: They must be *representative* of the population of data in order for the induction or testing of theory to be valid.

Survey research, organized along bureaucratic lines with a hierarchical and detailed division of labor, is most committed to putting these principles into practice and is therefore most revealing of their inherent limitations. The 4Rs are executed, but always imperfectly, through the 4Ss. First, the survey interview aspires to be an external stimulus, designed to evoke an unambiguous response. But this *stimulus-response* is undermined by the context of the interview itself, the age, gender, and race of the interviewer, the place of the interview, and the order of the questions. Second, to assure reliability, the interview schedule is *standardized*. But one can never standardize respondents who inevitably bring to the interview diverse understandings of the very questions and their embedded concepts. Third, replicability presumes that external conditions can be *stabilized* from one time and place to another; but that, too, is impossible because of external field effects that ensnare the interview. Finally, representativeness is assured by selecting a *sample* of individuals and inferring characteristics of the population. This presumes that the individual is the appropriate unit of analysis and that situation effects can be controlled for. Here, of course, the challenge of symbolic interaction and ethnomethodology is profound because they have long insisted that situations of face-to-face interaction are not simply interference, distorting underlying individual attributes, but constitute individuals together with their responses. The problem then becomes to define a population of situations, and then to devise a way of selecting a representative sample.

In pursuing the 4Rs with the 4Ss, survey researchers, therefore, face four *context effects*: interview, respondent, field, and situation effects. There's nothing new here. Today survey researchers, like Howard Schuman and his collaborators, deploy a refined ethnographic sensibility to decipher the influence of the interviewer's race and gender on responses, to minimize ambiguities in the meanings of questions through focus groups, and to recognize the impact of wider social forces on the interview itself. Some have even tried to program "dialogue" into the standardized interview. The more sophisticated survey research becomes, the more aware we are of the limits to positive science and the more clearly we can see an alternative, reflexive science rising on the horizon. So long as we live in localities, so long as lifeworlds retain some autonomy, context will remain and survey research, for all its interpretive ingenuity, will have to make the best of a bad job in realizing positive principles.

While survey research tries to minimize or measure the gap between practice and principle, science skeptics respond by throwing out principles and extolling practice as its own justification. Anything goes! They celebrate context at the expense of science. Hermeneutic philosophers, cultural anthropologists, historians of consciousness and even wayward sociologists—from Rorty to Geertz, from Winch to Bauman, from Feyerabend to Latour—proclaim the inviolability of local knowledge. The best one can do, they say, is to *interpret* one culture to another, or promote *conversations* between incommensurable worlds. Despairing of making intelligible contact with others, some end up contemplating only themselves. The denunciation of social science as "objectivism" (the standpoint of the outsider) turns into the embrace of "subjectivism" (the standpoint of the participant). Instead of replacing one one-sided standpoint with another, I propose an alternative model of social science that takes context as point of departure but not point of conclusion.

Reflexive Science

One does not have to endorse Comte's megalomania or his vision of sociologists as the high priests of society to recognize that we *are* part of the world we study. In the lineage from C. Wright Mills (1959) to Alvin Gouldner (1970) to Pierre Bourdieu and Loïc Wacquant (1992), reflexive sociology—far from being antagonistic to science—enhances its development. However, there is a vagueness to these notions of reflexive sociology—once one goes beyond the idea that the theories we develop for others should, in principle, be applicable to ourselves. At the risk of violating some preconceptions, I give more precision to a reflexive model of science. It may be idiosyncratic, but it is not an arbitrary codification. It stems from the contextual impediments to positive methodology.

First, if the proscription against reactivity proves impossible, then one can make a virtue of necessity, comprehending others through virtual or real participation in their lives. Reflexive science, therefore, takes as its presupposition an *intersubjectivity* we develop with those we study. Second, reliability is thwarted by respondent effects. We can standardize questions but not the respondent, who is a composite of attitudes, interpretations, and theories acquired in multiple situations. To comprehend actors, therefore, we have to enter their life worlds, their time and space, studying their situations as part of a social *process*. Third, replicability is confounded by field effects that make it impossible to isolate the interview situation from its wider context. Therefore, reflexive science enunciates the idea of *structuration*, which implies a reciprocal but asymmetrical constitution of local processes and extralocal forces—forces that can be economic, political, or cultural, and more or less systemic. Finally, instead of inferring or testing theories by examining a sample drawn from a population, we use the empirical to elaborate, revise, or *reconstruct* preexisting theory, whether academic or folk. Because we begin with theory a single case is quite sufficient for a progressive reconstruction. In short, reflexive sociology connects what positive sociology compartmentalizes: participant and observer, situation and knowledge, lifeworld and system, ideology and theory.

What makes for good sociology in positive science is adherence to a set of procedures, which leads to ever more accurate representations of the world. We call this *procedural objectivity*. Here the empirical world is the hard foundation for generating or toppling, inducing or falsifying our theory. For reflexive science, on the other hand, objectivity cannot be reduced to procedures, because scientist and subject, observer and participant, interviewer and interviewee cannot be isolated. Here "objectivity" requires recognition of one's biases, one's paradigm commitments, not to separate them from science but to constitute them as the premise of science. "Dwelling in" theory provides the foundation for intersubjectivity, process, and structuration as well as its own reconstruction. Such *embedded objectivity* calls for improved theory—not "true" theory but less false theory.

This begs two questions. First, what distinguishes improved or "progressive" from "degenerate" expansions of theory? Following Imre Lakatos (1978), we demand not only that reconstructions be consistent with what we already know, but that they absorb anomalies with parsimony and present novel angles of vision. Finally, reconstructions should lead to surprising predictions, some of which are corroborated.

The second obvious question is: With which theory do we begin? We can focus on an empirical phenomenon that commands our interest, searching for theories for which it is an anomaly—theories we consider worth developing, elaborating, revising, or reconstructing. Alternatively, we can commit ourselves to a theory or, more ambitiously, to a research program—Marxist, Weberian, rational choice, functionalist, feminist—that directs us to anomalies that compel the program's development. Whichever approach we take, the goal is no longer to confirm but to discover anomalies, no longer accurate representation but *the growth of knowledge*.

In this vision, each discipline becomes a hierarchically organized field of competing, overlapping, clashing, and mutually constituting theories. The trajectory of each theory is the product of its own internal logic, the external world it seeks to comprehend and equally the political discourse with other theories.

The Extended Case Method

Just as survey research uses interviews to emulate positive science, so the extended case method uses participant observation in striving toward reflexive science. The method was originally claimed by anthropologists of the Manchester School—Max Gluckman (1958, 1961), Clyde Mitchell (1956), Jaap van Velsen (1967), and others—who sought to go beyond their African villages to the forces of colonialism and industrialization. Where before anthropologists relied on informants to construct the fixed set of norms that directed behavior in supposedly isolated communities, now they depended on their own direct observations of events, dramas, ceremonies, and rituals to study how their subjects drew on and manipulated norms in the pursuit of interests. The study of strategic action led to analyses of social processes, located in a field of wider forces. Their historicization and de-exoticization of the colonial encounter was a decisive move toward a reflexive anthropology which subsequently underwent more radical transformations as ethnographers problematized themselves as well as those they studied.

In sociology, ethnography has also moved beyond thick description to incorporate historical and political sensibilities, gesturing toward the extended case method but stopping short of an alternative model of science. Contemporary examples abound: Ida Susser's (1982) and Philippe Bourgois's (1995) studies of street society, Robert Thomas's (1985) and Ching Kwan Lee's (1997) studies of the workplace, Pierrette Hondagneu-Sotelo's (1994) and Carol Stack's (1996) studies of migration, Judith Stacey's (1990) and Marjorie Devault's (1991) studies of the family, Paul Willis's (1977) and Annette Lareau's (1989) studies of schooling, Rick Fantasia's (1988) and Raka Ray's (1998) studies of social movements, Laura Enríquez's (1991) and Lourdes Benería and Martha Roldán's (1987) studies of underdevelopment, Linda Blum's (1991) and Vicki Smith's (1990) studies of organizational dynamics, Wendy Espeland's (1998) and Lynne Haney's (1996) studies of the state, Joan Fujimura's (1996) and Steve Epstein's (1996) studies of science, and many more. Here I simply offer a scientific basis for what they might be up to, one that liberates them from inappropriate criticism and establishes alternative standards for research.

In its aspiration to reflexive science, the extended case method undertakes four extensions, corresponding to intersubjectivity, process, structuration, and theory reconstruction. The first extension makes the observer a participant, experiencing the world of the Other; the second extends observations over time and space, allowing us to interpret those experiences as process; the third extends from the local to the extralocal, historicizing our interpretation of process as shaped by forces; and the fourth extends theory, making the previous three extensions possible and connecting us to communities of theorists. We can understand these four extensions by examining their self-created limitations. The extended case method is no less flawed than survey research, only here the challenge is not from context effects but from the *multiple dimensions of power*.

When we participate in the life of others, we find ourselves enmeshed in networks of *domination*, distorting communication and restricting what we can discover. We can try to diminish relations of domination between ourselves and our subjects through collaborative projects, but in so doing we intensify the domination that already exists in the field. Indeed, such "action research" often aims to clarify and exacerbate, and ultimately contest preexisting hierarchies of power. It can never be fully successful until it also tackles three other dimensions of power.

In reducing observations made over time and space to a social process that connects events, we force a complex field into a reified frame. We inevitably privilege some actors and silence others as we analyze how relations and differences are produced and reproduced in a microphysics of power. *Silencing* is part and parcel of the sociological reduction that is only underlined by siding with some underdogs rather than others. This is a call not to abandon science for a babel of voices but to listen for challenges to the analysis of process, for opportunities for reconstruction.

To study the field of forces that imposes itself on the microprocesses from without, we have to *objectify* those forces, making them appear natural and permanent. Objectification is essential to the extended case method, but we should always be ready to recognize its limits by acknowledging social forces as having their own history, and that they are themselves the effects of invisible but real social processes (as, for example, in Dorothy Smith's [1987, 1990] "institutional ethnography"). Objectification is punctured when the same macroforces promote very different microprocesses or when these in turn congeal into social movements that challenge any external determination. However necessary to the method, objectification endows forces with a power they do not necessarily possess.

Finally, extending theory through digesting anomalies simultaneously threatens to *normalize* the empirical world we study, naturalizing what is, homogenizing difference, domesticating resistance, stigmatizing tradition. We easily forget that theory is also a circulating *discourse* that brings social reality into being, framing what we can see and express, constituting subjects as particular types of objects—"Oriental," "Native," "Homeless"—often the better to control them. Therefore, we should embrace anomalies not only as a means to advance theory but also as a powerful negativity that challenges normalization. Theory extension should be a process without end. It should consecrate rather than obscure rebellious reality.

Between the principles of reflexive science and their practice lie these four dimensions of power—domination, silencing, objectification, and normalization. This is not to say that positive science escapes these dimensions of power. To the contrary, they are the usually unexamined and unproblematized presuppositions of procedural objectivity. Positive and reflexive science are like Siamese twins: The imperfections of each become the foundation of the other. For positive science, power is its precondition and context its impediment; for reflexive science, context is its precondition and power its impediment.

I can see postmodernists rubbing their hands in glee. From inescapable context effects they concluded the impossibility of science, and now they take the admission of unavoidable power as demonstration of the danger of science. To abandon science, however, rather than reshaping and reappropriating it, would leave us helpless before the ravages of modernity. *We are therefore prepared to live with an imperfect science because it accentuates, rather than pretends to escape, precisely what is so "dangerous" about the world.* Just as survey researchers try to reduce or control context effects, practitioners of the extended case method focus on the limitations to human freedom. In the reflexive mode the pursuit of science becomes the critique of society. Far from being antithetical, as is usually assumed, science and critique form an indissoluble unity.

Technique, Method, And Model

The labor process around a spinning jenny is very different according to whether it operates in feudalism or capitalism. Likewise in sociology, *techniques* are deployed differently, embodied in different *methods* according to the *model of science* they follow. As long as there is only one model of science, all three levels—technique, method, and model—can be collapsed into one. But with two models, scientific rationale and methodological choice can no longer hide behind techniques—participant observation, interviewing, archival research, laboratory experiments, demographic techniques, and so on. Here I focus on the use of interviewing and participant observation.

Consider the interview. The technique takes subjects out of their usual setting and subjects them to inquisition in the space and time of the interviewer. In survey research it is deployed "positively," but it can also be deployed reflexively. Thus, as Nancy Chodorow (forthcoming) has shown, in psychoanalysis the "interview" is self-consciously intersubjective, highlights process through space and time, and locates the individual in historical and social milieus. Each of the three forgoing elements is embedded in the reconstitution of theory. The psychoanalytic encounter is an example of what we may call the *clinical method*: the reflexive interview which highlights dialogue and narrative, empowering the interviewee.

As a technique, participant observation is the obverse of the interview. Instead of subjects' being organized by the scientist, the observer joins participants in their space and time. Participant observation is deployed reflexively in the extended case method, but it can also aspire to positive principles: observing from the outside, developing sophisticated apparatuses of coding, bracketing context to assure the conditions of replication, and inducing theory by seeking commonality across internal diversity. This method is elaborated by Glaser and Strauss (1967) as *grounded theory*, but it originated in the heyday of the Chicago School when Robert Park defended ethnography as detached and objective, and admonished surveys as partisan instruments of muckraking reform. A similar "objectivism" infused the now discredited mission of classical anthropology—immersion in the colonial world from an olympian position that was textually invisible and politically protected.

The positive tradition in ethnography has been carried forward today in such exemplary studies as Martín Sánchez Jankowski's (1991) research on gangs and David Snow and Leon Anderson's (1993) research on the homeless. To understand the difference between grounded theory and the extended case method, compare Arlie Hochschild's (1989) and Majorie Devault's (1991) studies of the "second shift." Hochschild organizes her material case by case to show how an unequal gender division of domestic labor prevails regardless of social and economic context. By contrast, Devault's more extended case approach analyzes domestic work in its wider context, tracing variations in gender practices to the effects of class structure. Furthermore, Devault critically engages theories embedded in public discourses about caring, whereas Hochschild stands aside to derive her conclusions inductively.

In short, as each technique reaches out to different models of science it becomes implanted in different methods. Both interviewing and participant observation can follow either positive or reflexive principles. We can find the same methodological division in *historical sociology*. Take, for example, two theorists of classical revolution: Skocpol (1979) and Trotsky ([1906] 1969, [1930] 1977). The first emulates positive science while the second exemplifies reflexive science. Thus, Skocpol stands outside history, standardizes each revolution to a single form, freezes the context of revolution over three centuries, and induces her explanatory theory by discovering the factors present and missing in successful and failed revolutions. Trotsky, by contrast, stands at the center of history, reconstructing it as he participates in it. He sees revolutions as processes rather than singular, homogeneous events. He locates revolutions in a changing international context—the combined and uneven development of capitalism—which both shapes and is shaped by revolutions. Finally, he reconstructs Marxism to accommodate the anomaly that the first socialist revolution took place in a relatively backward country, and then anticipates its denouement. Of course, Skocpol and Trotsky represent polar types. I confess not all historical sociologists fit so neatly into this scheme! As in any analytic scheme, we must recognize all sorts of incomplete and hybrid forms.

Reflexive science defines itself not only in opposition to positive science but also against the rejection of science altogether. Thus, we can learn much from *feminist methods* which, in grappling with power, have often trodden the road to poststructuralism. First, disturbed by the dynamics of domination between participant and observer, interviewer and interviewee, feminism has sometimes recoiled from systematic research. Second, sensitive to silencing, it has dissolved into "postfeminism" before the proliferating differences of class, race, and ethnicity. Feminism has difficulty retaining the primacy of gender because it refuses or fails to develop totalizing theories. This creates problems for its third aspiration—to locate everyday life within its macrodeterminations. When it has not withdrawn from such objectification, feminism has often borrowed from Marxism's institutional analysis. Finally, the deconstruction of theory or ideology from a feminist standpoint (with a view to consciousness raising) often has taken precedence over research programs in fear of reifying gender. In short, having left behind the critique of positive science, feminism turns between reflexive science and the poststructural rejection of science. Therein lies its tension, its excitement, and its plethora of methodological spaces.

Sociology In The Information Age

A reflexive science must reflect on the conditions of its own existence. Does the academic discipline we inhabit or the world we seek to comprehend promote or undermine reflexive science? To answer this question, we need theory that allows us to locate sociology in its wider context. The distinction from which we began, between bureaucratic and craft administration, fails to encompass our immersion in the postindustrial order. We might more usefully turn to Manuel Castells' (1996, 1997, 1998) three-volume sociology of the information age, depicting a world of unprecedented concentrations and disparities of power—corporate elites threaded together on global networks, disconnected from the "black holes" of marginalized populations, dispossessed of all but their identity.

Curiously, Castells steadfastly refuses to consider the implications for sociology—or even the implications *of* sociology—in this emergent world of "real virtuality" and "spatial flows." I say "curiously" because with the information society, for the first time knowledge is simultaneously raw material, process, and product of production. Knowledge acts upon knowledge to produce new knowledge. It is ever more difficult for sociologists to restrict themselves to observation, since we are now the co-authors of the very spectacle we claim to study, a society that poses new threats to human existence even as it creates a host of new opportunities for human emancipation.

We have entered what Anthony Giddens (1992) and Ulrich Beck (1992) call "reflexive modernization," when society confronts itself as its own product, when expert knowledge is ever more indispensable as its effects become more uncertain—more necessary but less sufficient. The "secondary" consequences of shock therapy, workfare, or busing, as of nuclear power, chemicals, canals, or deforestation, can overshadow their intended effects. Reflexive modernization calls for a science that is subject to internal self-regulation as well as to external control by affected populations, a science that cuts across dangerous hyperspecialization by dwelling upon contexts in relation to their environment, and above all a science that inspects its own "consequentiality" and fallibility. Never have we been more in need of reflexive science!

We need to liberate sociology, therefore, not from the metaphysical pathos of juggernaut bureaucracy but from its cramped methodology; and *not* by demanding any exclusive or utopian adherence to positive or reflexive methods but by holding them in tension, by interweaving them, by playing them off against each other—in short, by recognizing their interdependence. Thus, survey research is inevitably shot through with interpretive moves from questionnaire design to interviewer improvisation to analysis. Take Erik Wright's comparative surveys, which showed workers in the United States claiming more control over their work environment than workers in the Soviet Union. Flying in the face of all my ethnography, I reinterpreted his finding as follows: Soviet workers exercise considerable control in order to combat shortages but for the same reason experience their workplaces as "out of control," whereas American workers possess limited control but, through institutionalized individualism, experience freedom. The extended case method reinterprets the findings of survey research by revealing the links between structural constraints and their subjective apprehension.

In the same way, the extended case method relies on survey research to explore the forces impinging on social processes. It is impossible for case extenders to collect all the data for their research. They simply have to accept survey and demographic data, even as they recognize how mercurial these can be. The impact of more durable, systemic forces, such as markets or state formations, can be captured effectively through survey research.

At this point two caveats are important. First, even though methods combine, each using the other as a subordinate partner, one should be careful not to merge or confuse criteria of evaluation. It is opportunistic to censure the extended case method for failing to live up to the 4Rs when it strives toward other goals. Equally, ethnographers should respect survey research efforts to close or measure the gap created by context effects. *Each method dances to its own tune—listen to the tune before evaluating the dance.*

Second, we may combine methods, but that does not translate into choosing methods according to the problem in hand. For example, one might argue that the study of objective forces calls for positive methods, subjective processes for reflexive methods. That would be to put the cart before the horse. Our commitments to positive or reflexive science occur before and indeed govern the choice and definition of problems. We may change the topics of investigation, the questions we pose, even the theoretical frameworks we use, but we rarely change our reflexive or positive convictions. Even if the science model is buried in our unconscious, it is no less powerful for being subliminal. Methodological orientations are well entrenched in our scientific "habitus," guiding our imagination, framing our academic quotidian, defining our sociological persona.

If the two models of science and their methods define different but overlapping and mutually engaged sociological worlds, are there social tendencies that favor one or the other? Survey research overcomes its flaws by reducing context effects. James Coleman (1986) once averred that survey research owed its hegemony to the rise of mass society and, one might add, to the dissolution of the local on a global scale. Bureaucratic social science itself can contribute to such tendencies—for example, as it becomes inscribed in "developmental apparatuses" that constitute the Third World, proliferating rather than eradicating sites of global control. If survey research colludes with power, the extended case method realizes itself against power, through local rebellions against the global, through the flourishing of context. Postcolonial theory endorses this portrait, arguing that the present period is finally giving voice to the marginal, in which the colonized vanquish the colonizers, challenging them on their very doorsteps.

It is not the object of knowledge itself—the balance of local and global, context and power, lifeworld and system—that determines the ascendancy of reflexive or positive science, but the way this "object" world is reproduced within our discipline, the way it constitutes the field of theoretical practice that we call sociology. Thus, inasmuch as information technology becomes the prime mover of social change, the disciplinary world of sociology comes to resemble the wider society of which it is a part. Both inside and outside, bureaucratic hierarchies are replaced by horizontal networks, so that survey researchers become what Robert Reich (1991) calls *symbolic analysts* connected through computer nodes strung out across the globe. At the same time, the extended case ethnographers redefine their craft to follow what Alain Touraine (1988) calls *action research*, forging a close identity with their subjects, promoting movements of self-actualization. Our methods become increasingly tied to the polarized world we study—survey research linked to the contextless world of spatial flows and the extended case method to the powerless and excluded. Applying the extended case method to our own practices underlines the way our methods bear the imprint of the world we seek to comprehend.

Conclusion

Since the heyday of Parsonsian structural functionalism our discipline has turned into a fragmented field. I am proposing not the resurrection of any theoretical unity but rather the embrace of a methodological duality. Replenishing reflexive traditions is important in its own right, but it will also challenge the streams of positive methodology, thereby giving our discipline a singular place among the social sciences. First, sociology can provide a bridge between the resurgent positivism in economics and political science and the interpretive turn within anthropology, history, and geography. The dialogue between positive and reflexive sociology turns the manichean divide between the warriors of science and the iconoclasts of reason into a productive engagement. Second, armed with its two sciences, sociology can tackle the pressing antinomies of our time: participant and observer, the audible and the silenced, the local and the global, lifeworld and system, discourse and theory. Third, the practice of imperfect science becomes the critique of an imperfect society. Positive science reveals the power of context, while reflexive science uncovers the context of power. We recognize the limits on freedom not through the arbitrary juxtaposition of essence and existence or utopia and reality but in the everyday practice of reflexive science.

However, as long as positive methods exercise an uncontested hegemony within sociology they force reflexive methods either to defend themselves on alien grounds or to turn against science altogether. Censuring reflexive methodology as "Other," condemning it for violating the 4Rs, consolidates the power of positive science, thereby masking its own flaws, restricting its own problematization, closing its eyes to the intellectual ferment in neighboring disciplines, causing sociology to lag behind the world it creates, and turning it into a backwater among the social sciences. The way forward, therefore, lies in articulating and defending a counterhegemonic vision, an alternative model of science that forces positive methods to confront the challenge of their own imperfections, awarding reflexive methods a space for their own expansion, a chance to reconnect sociology to currents in other fields, and a basis from which to respond creatively and critically to the troubles and (dis)illusions of the epoch.

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