TWO

Reconstructing Social Theories

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

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

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

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

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

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

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

We, therefore, agree with Karl Popper's critique of induction and verification.[7] We also follow Popper's own logic of scientific discovery in emphasizing processes of conjecture and refutation. Where we depart from him is that we use counterinstances to reconstruct rather than reject theory. That is to say, instead of proving a theory by corroboration or forsaking a theory because it faces falsification, our preferred approach is to *improve* theories by turning anomalies into exemplars. In a sense we take Popper to his logical conclusion. Instead of abandoning theory when it faces refutation, we try to "refute the refutation" by making our theory stronger.[8]

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

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

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

In pursuing theory reconstruction, on the other hand, we conduct a running exchange between field notes and the analysis that follows them. The conjectures of yesterday's analysis are refuted by today's observations and then reconstructed in tomorrow's analysis. But there is a second running exchange, that between analysis and existing theory, in which the latter is reconstructed on the basis of emergent anomalies. Analysis, therefore, is a continual process, mediating between field data and existing theory.

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

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

"NEW SOCIAL MOVEMENTS"

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

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

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

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

But at this point he halted. Because of his highly unusual experiences in the field, Gamson was uncomfortable with turning his extension into a generalization: that what new social movements shared, what made them "new," was their response to new forms of invisible domination. With only one case to go on, he did not feel able to define the scope of his reconstructed theory of NSMs.

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

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

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

Why should the peace movement adopt such different and unconventional strategies for achieving its goals? Like Gamson, Schiffman turned to the literature on new social movements, but she problematized it in a slightly different way. Where he saw new social movements as a response to the invisibility of domination and strategies of normalization, she saw them as responding to the penetration of the state, particularly the nuclear state, into private realms. In this view, NSMs define themselves as carving out arenas of civil society autonomous from the state. But how does this explain the very different strategies of BAPT and BW? This silence in the literature became her puzzle.

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

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

REORGANIZING PRODUCTION IN A SERVICE ECONOMY

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

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

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

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

Her analysis led her to spend more time examining the workplace. In the second semester she undertook intensive interviews with welfare workers about their work and how it had changed over the last two decades. O'Connor's anticipations were based on the radical labor-community alliances that sprang up in the early 1970s. Why was O'Connor correct then but wrong now? What had happened to the organization of work? Burton traced the history of the division of case workers into SWs and EWs. She discovered that, following the early struggles, the welfare system began carving up each client into a set of distinct problems and distributing these to different workers. By fragmenting welfare work and by fragmenting clients, the system effectively undermined any solidarity. The state's strategy of divide and rule, budget cuts, and union busting took the wind from the sails of labor radicalism. Thus, Burton's observations of grievance meetings led her to enter into a dialogue with and eventually reconstruct O'Connor's theory so as to accommodate and interpret the historically changing forces shaping public sector unions.

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

After circling around this complex of problems, Ferguson looked for a theoretical point of entry—a key that would unlock what she was observing. She turned to the longevity of Wholly Grains, then thirteen years old. Certainly the literature suggested that cooperatives should be ephemeral organizations, unable to withstand the inevitable internal tendencies toward bureaucratization or external pressures to place profit before collective decision making. So how had Wholly Grains survived? Without doubt these subversive pressures existed; indeed, they led to a never-ending succession of crises. Ferguson examined three such crises that threatened the collective: the use of white flour (which would compromise the political commitment to healthy food), the development of efficiency norms, and the abdication of production coordinators.

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

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

NEW IMMIGRANTS

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

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

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

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

Through the lens of "work" the two co-ops took on a new significance. At first it seemed that one catered to newer immigrants who had few skills, didn't speak English, and were paid particularly low wages, while at the other co-op women had generally been in the United States longer, constituted a more developed occupational community, and even organized English lessons and training sessions for their members. But social characteristics of the women couldn't explain the differences between the two co-ops. Instead Salzinger focused on the limited economic opportunities available to immigrants from Central America. If women were largely confined to domestic work, then upward mobility could be assured most effectively by professionalizing the occupation through credentialing and establishing an occupational community.

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

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

With interests similar to Salzinger's, Shiori Ui set out to examine the place of women among Cambodian refugees. She spent some time in Bay Area cities trying to gain access to families but without much success. Leaders of the Cambodian community told her that women play no significant role. Pursuing a contact from her Khmer language course, she visited the Cambodian community in Stockton where, much to her surprise, she found a group of women leaders. A tension developed between her original interest in women as family members and her discovery of an influential group of women leaders. For a time her research pursued both tracks simultaneously, until she saw a link between them.

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

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

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

FROM CLASSROOM TO COMMUNITY

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

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

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

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

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

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

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

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

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

RESEARCHING THE RESEARCHERS

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

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

At this point Fox was more interested in the expression of disaffection than its content. She wondered whether the ethnographic project spontaneously generated such dissident responses. On further investigation she found dissidence to be an important but minority response of outreach workers. Those who stayed with the project turned out to be of two types: professionals who endorsed the means and the goals of the agency but were "realistic" in their assessment of what was possible, and cynics for whom it was a badly needed job. Having developed a typology of outreach workers—crusaders, dissidents, professionals, and opportunists—Fox then tried to explain their adoption of a particular orientation in terms of their prior connection to the community, whether they had been insiders or outsiders. But she couldn't get this to work out.

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

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

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

Whereas Fox converged on Gouldner's critique of sociology from the side of sociologists in the field, Charles Kurzman approached Gouldner from the study of sociologists in their own setting. He came to our seminar armed with three ready-made theories of knowledge to be tested and explored. Would his model work for a bunch of ethnographers? He was going to observe the class to study which "knowledge claims" were successful and why. His first analysis focused on three competing theories: knowledge claims might be successful because they appealed to evidence, to commonly accepted paradigms, or to interests. Each approach shed some light on what had taken place in the seminar, but, he subsequently argued, they missed the fluidity of the seminar.

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

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

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

It is one thing to place those we study, particularly if we neither depend on them nor identify with them, in the context of their determination, denying them their autonomy. It is quite another matter to do the same to ourselves. Kurzman was trapped in what Gouldner calls "methodological dualism": "others" are dupes of social forces but "we" are rational agentic beings.[20] His study problematizes the extended case method for, according to Kurzman's theory, to the extent that the participant observer becomes an insider, sensitive to every whim of those being studied, there will be considerable pressure to give the participants full agency and to repress the way they are constrained by external forces. Of course, in some situations participants regard themselves as victimized by external forces beyond their control. Had Kurzman studied a course that students universally disliked but were compelled to take, perhaps he might have been happier with the extended case method!

CONCLUSION

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

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

We should, therefore, begin to think about different ways of improving theories through their reconstruction. With respect to the sort of field work represented in this volume, I want to suggest two alternative strategies. First, one can decide what one wants to study, immerse oneself in empirical work, and then search for theories that are inadequate because they ignore salient issues or lead to false anticipations or have latent theoretical ambiguities or contradictions that are revealed by the data. In each case the data call for an improvement of the existing theory. Presumably, there will be a number of theories for which the data would be anomalous, and so we choose the one that is closest to our interests. Alternatively, one can commit oneself to a given theory rather than a specific empirical phenomenon. If it is an important theory (and why else choose it?), its anomalies are already well known, and these would suggest possible empirical foci for research. Our task is to improve the theory by introducing auxiliary hypotheses that will turn anomalies into exemplars.

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

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