Reflexive science, on the other hand, takes context and situation as its points of departure. It thrives on context and seeks to reduce the effects of power—domination, silencing, objectification, and normalization. Reflexive science realizes itself with the elimination of power effects, with the emancipation of the lifeworld. Even as that utopian point may be receding, the extended case method measures the distance to be traveled. In highlighting the ethnographic worlds of the local, it challenges the postulated omnipotence of the global, whether it be international capital, neoliberal politics, space of flows, or mass culture. Reflexive science valorizes context, challenges reification, and thereby establishes the limits of positive methods.

TWO

The Ethnographic Revisit

Capitalism in Transition and Other Histories

Tacking back and forth through forty years of fieldwork, Clifford Geertz (1995) describes how changes in the two towns he studied, Pare in Indonesia and Sefrou in Morocco, cannot be separated from their nation-states—the one beleaguered by a succession of political contestations and the other the product of dissolving structures. These two states, in turn, cannot be separated from competing and transmogrifying world hegemonies that entangle anthropologists as well as their subjects. Just as Geertz's field sites have been reconfigured, so has the discipline of anthropology. After decades of expansion, starting in the 1950s, many more anthropologists now are swarming the globe. They come not only from Western centers but also from former colonies. Anthropologists are ever more skeptical of positive science and embrace the interpretive turn, itself pioneered by Geertz, that gives pride of place to culture as narrative and text. "When everything changes, from the small and immediate to the vast and abstract—the object of study, the world immediately around it, the student, the world immediately around him, and the wider world around them both—there seems to be no place to stand so as to locate just what has altered and how" (Geertz 1995: 2). This is the challenge of the ethnographic revisit: to disentangle movements of the external world from the researcher's own shifting involvement with that same world, all the while recognizing that the two are not independent.

With their detailed ethnographic revisits to classic sites, earlier anthropologists tended toward realism, focusing on the dynamic properties of the world they studied, whereas more recently they have increasingly veered in a constructivist direction in which the ethnographer becomes the central figure in interpreting change. They have found it hard to steer a balanced course. On the other hand, sociologist-ethnographers, grounded theorists in particular, have simply ducked the challenge altogether. Too often they remain trapped in the contemporary, riveted to and contained in their sites, from where they bracket questions of historical change, social process, wider contexts, and theoretical traditions, as well as their own relation to the people they study. While sociology in general has taken a historical turn—whether as a deprovincializing aid to social theory or as an analytical comparative history with its own mission, whether as historical demography or longitudinal survey research—ethnography has been slow to emancipate itself from the eternal present. My purpose here is to encourage and consolidate what historical interest there exists within sociology-as-ethnography, transporting it from its unconscious past into a historicized world by elaborating the notion of ethnography-as-revisit. This, in turn, lays the foundations for a reflexive ethnography.[1]

Let me define my terms. An ethnographic revisit occurs when an ethnographer undertakes participant observation, that is, studying others in their space and time, with a view to comparing his or her site with the same one studied at an earlier point in time, whether by this ethnographer or someone else. This is to be distinguished from an ethnographic reanalysis, which involves the interrogation of an already existing ethnography without any further fieldwork. Both Richard Colignon's (1996) critical reexamination and reinterpretation of Selznick's *TVA and the Grass Roots* (1949) and Franke and Kaul's (1978) reexamination of the Hawthorne studies are examples of reanalyses. A revisit must also be distinguished from an ethnographic update, which brings an earlier study up to the present but does not reengage it. August de Belmont Hollingshead's (1975) empirical account of changes in Elmtown is an update because it does not seriously engage with the original study. Herbert Gans updates *The Urban Villagers* (1982), not so much by adding new field data as by addressing new literatures on class and poverty. These are not hard and fast distinctions, but they nonetheless guide my choice of the ethnographic revisits I examine in this chapter.

There is one final but fundamental distinction—that between revisit and replication. Ethnographers perennially face the criticism that their research is not transpersonally replicable—that one ethnographer will view the field differently from another.[2] To strive for replicability is to strip ourselves of our prejudices, biases, theories, and so on before entering the field and to minimize the impact of our presence once we are in the field. Rather than dive into the pool fully clothed, we stand naked on the side. With the revisit we believe the contrary: There is no way of seeing clearly without a theoretical lens, just as there is no passive, neutral position. The revisit demands that we be self-conscious and deliberate about the theories we use and that we capitalize on the effects of our interventions. There is also, however, a second meaning of *replication* that concerns not controlling conditions of research but testing the robustness of findings. We replicate a study in order to show that the findings hold across the widest variety of cases, that—to use one of Hughes's (1958) examples—the need to deal with dirty work applies as much to physicians as janitors. Replication means searching for similarity across difference. When we revisit, however, our purpose is not to seek constancy across two encounters but to understand and explain variation, in particular to comprehend difference over time.

In short, the ethnographic revisit champions what replication strives in vain to repress. Where replication is concerned with minimizing intervention to control research conditions and with maximizing the diversity of cases to secure the constancy of findings, the purpose of the revisit is exactly the opposite: to focus on the inescapable dilemmas of participating in the world we study, on the necessity of bringing theory to the field, all with a view to developing explanations of historical change. As I will show, to place the revisit rather than replication at the center of ethnography is to reenvision ethnography's connection to social science and to the world it seeks to comprehend.

WHAT SOCIOLOGY CAN LEARN FROM ANTHROPOLOGY

Anthropologists routinely revisit their own sites and those of others, or reanalyze canonical works, while sociologist-ethnographers seldom revisit their own sites, let alone those of their forebears.

Even reanalyses are rare. Why should the two disciplines differ so dramatically? It is worth considering a number of mundane hypotheses, if only to dispel disciplinary stereotypes. The first hypothesis, as to why anthropologists are so fond of revisits, is that fieldwork has long been a tradition in their discipline, and they have accumulated, therefore, a vast stock of classic studies to revisit. Ethnography is so new to sociology that there are few worthy classic studies to revisit. This hypothesis doesn't stand up to scrutiny, though, as sociologists have been doing systematic fieldwork almost as long as anthropologists. Franz Boas began his first fieldwork among the Kwakiutl in 1886, only a little more than a decade before Du Bois worked on *The Philadelphia Negro* (1899). Bronislaw Malinowski first set out for the Trobriand Islands in 1915, and at the same time W. I. Thomas and Florian Znaniecki were collecting data for their *The Polish Peasant in Europe and America* (1918–20).

A second hypothesis might turn the analytic eye to the present. Anthropologists, having conquered the world, can now only revisit old sites (or study themselves). As in the case of archeologists, there are only so many sites to excavate. Sociologists, on the other hand, have so many unexplored sites to cultivate, even in their own backyards, that they have no need to retread the old. This second hypothesis doesn't work either, especially now that anthropologists have spread into advanced capitalism where they compete with sociologists (see, for example, Susser and Patterson 2001). Moreover, sociologists are always returning to the same places to do their ethnographies, but rarely, it would seem, to revisit. That is, generations of sociologists have studied Chicago, but never, or almost never, have they systematically compared their fieldwork with that of a predecessor.

This brings me to a third, rather bleak, hypothesis: that the early ethnographies in sociology were so poorly done, so ad hoc, that they are not worth revisiting. I hope to disabuse the reader of this idea by the time I have finished. Sociologists have been quite capable of superbly detailed ethnography, just as anthropologists can be guilty of sloppy fieldwork. Moreover, flawed fieldwork does not discourage revisits, but, as I will show, it often stimulates them.

A fourth hypothesis is that the worlds studied by the early sociologist-ethnographers have changed so dramatically that the sites are unrecognizable, whereas anthropological sites are more enduring. This too does not make sense. Sharon Hutchinson's (1996) Nuerland has been invaded, colonized, and beset by civil war since Evans-Pritchard was there in the 1930s, but that did not stop her from using Evans-Pritchard as a baseline to understand the impact of decolonization, war, Christianity, and transnational capital. Similarly, Elizabeth Colson (1971) followed the Gwembe Tonga after they had been displaced by flooding from the Kariba Dam. Sociological sites, on the other hand, are not all demolished. To be sure, urban renewal overtook Herbert Gans's (1982) West End, but William Foot Whyte's (1943) North End is still recognizable despite the changes it has sustained. The drama of change and the dissolution of old sites do become factors in revisits, but this does not distinguish the anthropologist from the ethnographer-sociologist.

If the distinction is not in the nature of the site being studied, then perhaps it lies with the observer—the anthropologist's romance with the past or the sociologist's attachment to the present. One does not have to resort to such an essentialist and unlikely psychology. One might simply argue that anthropologists invest

so much in their research site—learning the language, the practices, rituals, and so on—that they are drawn back to their own sites rather than driven to excavate new ones. But this fifth hypothesis doesn't explain the anthropologist's relish for studying other people's sites, revisiting other people's studies.

Perhaps the answer lies with the disciplinary projects of anthropology and sociology. So my sixth hypothesis is that anthropologists have been trained to study the "other" as exotic (or they came to anthropology with this in mind), and they are therefore more reflexive—more likely to ask who they are and where they came from. Sociologists, because they study the familiar (i.e., their own society), are less reflexive, less likely to think about themselves and their traditions. But here too the difference is not clear—sociologists have a trained capacity to exoticize a different world, even if they are next-door neighbors. Indeed, some would say that was their craft—making the normal abnormal and then making it normal again.

Still, in turning to the discipline for an explanation, I think one may be getting nearer to the mark. Ethnography in U.S. sociology has followed a twisted road. It began as the dominant approach in the field when the Chicago School prevailed, but with the spread of sociology and the expansion of the university, it succumbed to the twin forces of survey research and structural functionalism—what Mills called abstracted empiricism and grand theory (1959). His point, of course, was that sociology had lost touch with social reality. Even before he wrote his polemic, the Chicago School had taken up this challenge, reconstituting itself under the influence of Everett Hughes, but also of Anselm Strauss, into what Fine (1995) has called the Second Chicago School, creating an alternative to theoreticism and empiricism.

To deductive grand theory these sociologists counterposed grounded theory, discovered in the empirical data. To survey research they counterposed field research based on in situ observation of the microsocial. Here we find the great studies of Goffman, Becker, Gusfield, Gans, Davis, Freidson, and others. They reclaimed ethnography for science, an inductive science of close observation, codified in Glaser and Strauss's *The Discovery of Grounded Theory* (1967) and reaching its apotheosis in Becker's craft manual, *Tricks of the Trade* (1998).

Forced to carve out its own "scientific" niche, participant observation turned inward. To put their best positivist foot forward, participant observers (1) pretended to be neutral insiders and thus silenced the ways fieldworkers are irrevocably implicated in the world they study, (2) repressed preexisting theory as a dangerous contamination, (3) sometimes even eclipsed processual change in the search for singular descriptions of microsituations, and (4) suspended as unknowable the historical and macrocontext of the microanalysis.[3] In studying ethnographic revisits I will provide correctives along all four dimensions—thematizing the observer as participant, reconstruction of theory, internal processes, and external forces—thereby establishing the four principles of the extended case method and reflexive ethnography (see chapter 1, and Burawoy, Burton, et al. 1991; Burawoy, Blum, et al. 2000).

My criticism of sociologist-ethnographers should not be misunderstood. There is much to be studied and gleaned from the present. The long tradition of community studies, dominated by the Chicago School, has made enormous contributions to our understanding of urban life. The symbolic interactionists and the ethnomethodologists have deployed participant observation

to great advantage, sustaining this marginal technique in face of the ascendancy of quantitative research. As an embattled minority participant observers insulated themselves both from changes in the discipline and from changes in the world. Today, when historical sociology is mainstream, when grand theory is no longer so imperial, when survey research is itself increasingly concerned with longitudinal analysis, when globalization is the topic of the day, participant observation should come out from its protected corner to embrace history, context, and theory.[4] In this project sociologists have much to learn from anthropologists, from both their insights and their oversights. Anthropologists offer an inspiration but also a warning.

Within anthropology the trajectory of ethnography has been very different. Its canonical texts were ethnographic. Just as sociology returns again and again to Marx, Weber, and Durkheim, so anthropology returned to Boas, Mead, Malinowski, Evans-Pritchard, Radcliffe-Brown, and the rest—and will continue to do so as long as they define the anthropological tradition. When the very possibility of ethnography was threatened by anticolonial revolts, anthropology reverberated in shock. Acknowledging how dependent they were on forces they no longer controlled, anthropologists willy-nilly became exceedingly conscious of the world beyond their field site. They revisited (and reanalyzed) the innocent studies that were their canon and that, so often, had been conducted under the protective guardianship of colonialism—conditions that remained silent in the original studies. The isolation of the village, of the tribe, was a conjuring act that depended on the coercive presence of a colonial administration (Asad 1973). Simultaneous with this heightened historical consciousness came a questioning of the anthropological theories that emerged from these hitherto unstated conditions and a questioning of the way their texts already contained within them particular relations of colonial domination (Clifford and Marcus 1986). Thus history, theory, and context came to be deeply impressed upon the anthropologist's sensibility (Comaroff and Comaroff 1991, 1992; Mintz 1985; Vincent 1990; E. Wolf 1982).

While the anthropologist was thrown into a turbulent world order, the sociologist-ethnographer retreated into secure enclaves in both the discipline and the community. The sociologists threw up false boundaries around their sites to ward off accusations that they did not practice science, while the anthropologists forsook science as they opened the floodgates of world history. Once the former colonial subject was released from anthropological confinement and allowed to traverse the world, the trope of revisit became as natural to the practice of anthropology as it was to the movements of its subjects. The revisit is so taken for granted by the anthropologist that perhaps it takes a sociologist to exhume the significance and variety of revisits.

In the remainder of this chapter I design a framework to critically appropriate the classic revisits of anthropology and to bring sociology-as-ethnography out of its dark ages.

DISSECTING THE FOCUSED REVISIT: *MANUFACTURING CONSENT*[5]

Revisits come in different types. However, the most comprehensive is the focused revisit, which entails an intensive comparison of one's own fieldwork with an earlier ethnography of the same site, usually conducted by someone else. Like the focused interview

(Merton, Fiske, and Kendall 1956), the focused revisit takes as its point of departure an already investigated situation, but one that takes on very different meanings because of changes in historical context and the interests and perspectives of the revisitor.

The scheme of focused revisits that I develop here derives from my own serendipitous revisit to a factory studied by Donald Roy, one of the great ethnographers of the Chicago School. Roy studied Geer Company in 1944–45, and I studied that same factory thirty years later, in 1974–75, after it had become the engine division of Allied Corporation (Roy 1952a, 1952b, 1953, 1954; Burawoy 1979). Like Roy, I was employed as a machine operator. For both of us it was a source of income as well as our dissertation fieldwork. As I grew accustomed to the workplace, I was reminded of other piecework machine shops, not least Roy's classic accounts of output restriction.[6] There were the machine operators on piece rates, working at their radial drills, speed drills, mills, and lathes, while the auxiliary workers (inspectors, set-up men, crib attendants, dispatchers, truck drivers) were on hourly rates. I observed the same piecework game of "making out" (making the piece rate), and the same patterns of output restriction, namely, goldbricking (slowing down when piece rates were too difficult) or "quota restriction" (not busting rates when they were easy). In turning to Roy's dissertation (1952b) I discovered a series of remarkable coincidences that left me in no doubt that I had miraculously landed in his factory thirty years later. What made it even more exceptional was the rare quality of Roy's 546-page dissertation. If I had planned to do a revisit, I could not have chosen a better predecessor than Roy—the exhaustive detail, the brilliant use of events, his familiarity with industrial work, his rich portraits of shop-floor games.[7]

In fact, Roy's findings were so compelling that I was at a loss to know what more I could contribute. For all the talk of science, I knew that to replicate Roy's study would not earn me a doctoral degree, let alone a job. As Robert Merton confirmed long ago, in academia the real reward comes not from replication but from originality.[8] My first instinct was reactive—to denounce Roy as a myopic Chicago participant observer, interested in promoting human relations on the shop floor, who did not understand the workings of capitalism or the way state and market impressed themselves on shop-floor relations. But if external context was so important in shaping the shop floor, then one would expect changes in the state and the market to produce experiences in 1974 that were different from those of 1944. But everything seemed to be the same. Or was it?

I painstakingly examined Roy's dissertation and discovered, indeed, a series of small but significant changes in the factory. First, the old authoritarian relation between management and worker had dissipated. This change was marked by the disappearance of the "time and study men," who would clock operators' jobs when their backs were turned, in pursuit of piece rates that could be tightened. Second, if vertical tension had relaxed, horizontal conflicts had intensified. Instead of the collusion between operators and auxiliary workers that Roy described, I observed hostility and antagonism. Truck drivers, inspectors, and crib attendants were the bane of my life. As Roy and I reported our experiences, they were different, but what to make of those differences? I now consider four hypothetical explanations for our different experiences, although at the time of my study I considered only the fourth.

Observer as Participant

My first hypothesis is that Roy's experiences at Geer and mine at Allied differed because we had a different relationship to the people we studied. After all, Roy was not new to blue-collar work like I was; he was a veteran of many industries. He was accepted by his coworkers whereas I—an Englishman and a student to boot—could never be. Perhaps his blue-collar pride flared up more easily at managerial edicts; perhaps he could more effectively obtain the respect and thus the cooperation of auxiliary workers? Our divergent biographies therefore might explain our different experiences, but so might our location in the workplace. I was a miscellaneous machine operator who could roam the shop floor with ease, while Roy was stuck to his radial drill. No wonder, one might conclude, he, more than I, experienced management as authoritarian. Finally, a third set of factors might have intervened—our embodiment as racialized or gendered subjects. Although many have criticized *Manufacturing Consent* for not giving weight to race and gender, it is not obvious that either was important for explaining the discrepancies between Roy's experiences and mine, as we were both white and male. Still, in my time whiteness might have signified something very different because, unlike Roy, I was working alongside African Americans. This racial moment may have disrupted lateral relations with other workers and bound me closer to white management.

I argue that none of these factors—not biography, location, or environment—could explain the difference in our experience of work because both of us observed every other operator on the shop floor going through the same shared and common experience, regardless of their biography, location, or race. Work was organized as a collective game, and all workers evaluated others as well as themselves in terms of "making out." We all played the same game and experienced its victories and defeats in the same way—at least that was what both Roy and I gleaned from all the emotional talk around us.

Reconstructing Theory

If it was not the different relations we had to those we studied that shaped our different experiences of work, perhaps it was the theory we each brought to the factory. Undoubtedly, we came to the shop floor with different theories. Roy was a dissident within the human relations school. He argued against the findings of the Western Electric Studies, that restriction of output was the product of workers' failing to understand the rules of economic rationality. To the contrary, Roy argued, workers understood economic rationality much better than management, which was always putting obstacles in the way of their "making out"—obstacles that operators cleverly circumvented in order to meet managerial expectations without compromising their own economic interests. If rates were impossible to make, workers would signal this by slowing down. If piece rates were easy, workers would be sure not to draw attention to that ease by rate busting, lest it lead to rate cutting. Not workers but management, it turned out, was being irrational by introducing counterproductive rules that impeded the free flow of work.

Like Roy, I was a dissident but within the Marxist tradition. I tried to demonstrate that the workplace was not the locus for

the crystallization of class consciousness hostile to capitalism but was an arena for manufacturing consent. I showed how the political and ideological apparatuses of the state, so fondly theorized by Gramsci, Poulantzas, Miliband, Habermas, Althusser, and others, found their counterpart within production. On the shop floor I found the organization of class compromise and the constitution of the individual as an industrial citizen. Borrowing from Gramsci, I called this the hegemonic organization of production, or the hegemonic regime of production.

If our theories were so different, could they explain the different experiences that Roy and I had in the workplace? Certainly different theories have different empirical foci, select different data. But at least in this case theoretical differences cannot explain why I experienced more lateral conflict and Roy more vertical conflict, why he battled with time-and-study men, whereas in my time they were nowhere to be found. If theory alone were the explanation for our different accounts, then Allied Corporation would look the same as Geer Company if examined through the same theoretical lens. When I focus my theory of hegemony on Geer Company, however, I discover a more despotic workplace than Allied, one that favors coercion over consent, with fewer institutions constituting workers as individuals or binding their interests to the company. Equally, were Roy to have trained his human relations lens on Allied, he would have perceived a more participatory management culture. Whereas Geer treated workers as "yardbirds," Allied's management expanded worker rights and extended more human respect and in exchange obtained more worker cooperation. Differences remained, therefore, even as we each take our own theory to the workplace of the other.

I am not saying that theories can never explain discrepancies in observations made by two researchers, but in this case work was so tightly structured and collectively organized that our lived experiences were largely impervious to the influence of consciousness brought to the shop floor from without, including our own sociological theories.

Internal Processes

So far I have considered only constructivist explanations for the difference in our experiences—that is, explanations that focus on the relations that Roy and I had to our coworkers (whether due to biography, location, or embodiment) or explanations that focus on the theories we used to make sense of what we saw. I now turn to the realist explanations for the differences we observed—that is, explanations that consider how our accounts reflect attributes of the world being studied (rather than products of our theoretical or practical engagement with the site). Like constructivist explanations, realist explanations are also of two types: the first attributes divergence to internal processes and the second to external forces.

Is it possible to explain the shift from despotic to hegemonic regimes of production by reference to processes within the factory? Roy did observe internal processes of a cyclical character (1952b). Rules would be imposed from above to restrict informal bargaining and collusion, but over time workers would stretch and circumvent the rules until another avalanche of managerial decrees descended from on high. Could such cyclical change explain a secular change over thirty years? It is conceivable that the shift from despotism to hegemony was an artifact of our different

placement in the cycle between patterns of bureaucratic imposition and indulgence. But this explanation does not work, because I too observed a similar oscillation between intensified rules and their relaxation during my year on the shop floor. So this rules out the possibility that Roy and I were simply at different points in the cycle. Besides, the shift over thirty years cannot be reduced to the application or nonapplication of rules but also involved the introduction of completely new sets of rules regarding the bidding on jobs, grievance machinery, collective bargaining, and so on. Annual cyclical change could not explain the overall shift in the thirty years. Therefore we must turn to external factors to explain the secular shift to a hegemonic regime.

External Forces

The shift from despotism at Geer Company to hegemony at Allied Corporation is compatible with a shift reported in the industrial relations literature. The system of internal labor markets (both in terms of bidding on jobs and the system of layoffs through bumping), as well as the elaboration of grievance machinery and collective bargaining, became common features in the organized sectors of U.S. industry after World War II. These changes were consolidated by the "pattern bargaining" between trade unions and leading corporations within the major industrial sectors. I drew on the literature that documented the more corporatist industrial relations to explain what had happened on the shop floor since Roy's fieldwork. While the overall transformation of the system of state-regulated industrial relations was one factor governing the move from despotism to hegemony, the absorption of the independent Geer Company into the multinational Allied Corporation was the second factor. Allied's engine division had a guaranteed market and was thereby protected from competition—the very pressure that stimulated despotism. Here, then, were my twin explanations for the shift from despotism to hegemony: Geer Company's move from the competitive sector to the monopoly sector, and the transformation of industrial relations at the national level. Both forces originated from beyond the plant itself.

What do I mean by *external forces?* I use the term *external forces,* rather than, say, *external context,* to underline the way the environment is experienced as powers emanating from beyond the field site, shaping the site yet existing largely outside the control of the site. These forces are not fixed but are in flux. They appear and disappear in ways that are often incomprehensible and unpredictable to the participants. External context, by contrast, is a more passive, static, and inertial concept that misses the dynamism of the social order.

This brings up another question: From among the myriad potential external forces at work, how does one identify those that are most important? They cannot be determined from the perspective of participant observation alone but, in addition, require the adoption of a theoretical framework for their delimitation and conceptualization. But theory is necessary not just to grasp the forces operative beyond the site but also to conceptualize the very distinction between internal and external, local and extralocal. For example, Marxist theory directs one first to the firm and its labor process (the local or internal) and then to an environment (the extralocal or external) comprised of markets and states. The internal and the external are combined within a more general theory of the development of capitalism. In sum,

Table 3. *Potential Explanations for the Divergence between Roy's Original Ethnography and Burawoy's Revisit*

Explanations	Internal	External
Constructivist	Observer as participant (a) Biography (work experience) (b) Location (in production) (c) Embodiment (language, race, age)	Reconstructing Theory (a) Human relations (Roy) (b) Marxism (Burawoy)
Realist	Internal Processes Cyclical imposition and relaxation of rules	External Forces (a) Absorption of factory into monopoly sector (b) Secular national shift in industrial relations

theory is a sine qua non of both types of realist explanation for change between successive ethnographies of the same site.

Table 3 assembles the four hypothetical explanations for the discrepancy between Roy's dissertation and my account of the Geer/Allied shop floor. Along one dimension I distinguish between constructivist and realist explanations—the former focusing on changes in knowledge of the object (whether the result of different relations to the field or alternative theory), and the latter focusing on changes in the object of knowledge (whether these changes are the result of internal processes or external forces). The second dimension refers to the distinction between internal and external explanations of change—between relations constituted in the field and theories imported from outside, or between internal processes and external forces.

Critique and Autocritique

In claiming external forces as the explanation for the discrepancy in our accounts, I am not saying that the other three dimensions are unimportant. Far from it. The impact of those external forces—the changing state and market context of the company—could have been observed only through participant observation, could have been detected only with the aid of some theoretical framework, and could have had their actual effects only through the mediation of social processes within the workplace. My approach here, however, is very different from the Chicago School's, exemplified by Roy's (1980) review of *Manufacturing Consent.* Roy was curiously uninterested in explaining changes and continuities in the organization of work or in placing our labor processes in their respective economic and political contexts or in evaluating how our respective theoretical frameworks shed different light on what had happened during those thirty years. For Roy our two studies merely showed that there are different ways to "skin a worker." He evinced no interest in the factors that might explain why "skinning" took one form earlier and another form later.[9]

If there are limitations to Roy's Chicago method, there are also limitations to my use of the Manchester method.[10] Even though I still believe that external forces offer the most accurate explanation for the discrepancies between our accounts, in hindsight the way I conceptualized markets and states was deeply problematic.[11] I was guilty of reifying external forces as natural and eternal, overlooking that they are themselves the product of unfolding social processes. Here I was indeed shortsighted. Markets and states do change. Indeed, soon after I left Allied in

1974 the hegemonic regime came under assault from the globalization of markets (which in fact led to the disintegration of Allied) and the Reagan state's offensive against trade unions. In forging class compromise and individualizing workers, the hegemonic regime made those very same workers vulnerable to such offensives from without. If I had been *more* attentive to Marxist theory, I would have recognized that states and markets change. More than that, I would have noticed that the hegemonic regime had sowed the seeds of its own destruction by disempowering the workers whose consent it organized. The hegemonic regime that I saw as the culmination of industrial relations in advanced capitalism was actually on the verge of disappearing.

The problem was not with the choice of external forces as the explanation of change from Geer to Allied but my failure to take sufficiently seriously the other three elements in table 3. I should have deployed theoretical reconstruction to recognize internal processes (elsewhere within the economy or state) that might have produced those external forces. Furthermore, had I problematized my own embodied participation at Allied, I might have appreciated the peculiarities of manufacturing that were being replaced by ascendant varieties of newly gendered and racialized labor processes. The lesson here is that revisits demand that ethnographers consider all four elements set out in table 3.

From Elements to Types of Focused Revisits

The four elements in table 3 define reflexive ethnography, that is, an approach to participant observation that recognizes that we are part of the world we study. Reflexive ethnography presumes

Table 4. *Typology and Examples of Classic (Focused) Revisits*

Explanations	Internal	External
Constructivist	Type 1: Refutation	Type 2: Reconstruction
	(a) Freeman (1983) revisits Mead (1928)	(a) Weiner (1976) revisits Malinowski (1922)
	(b) Boelen (1992) revisits Whyte (1943)	(b) Lewis (1951) revisits Redfield (1930)
Realist	Type 3: Empiricism	Type 4: Structuralism
	(a) Lynd and Lynd (1937) revisit Lynd and Lynd (1929)	(a) Hutchinson (1996) revisits Evans-Pritchard (1940)
	(b) Caplow et al. (1982) revisit Lynd and Lynd (1929)	(b) Moore and Vaughan (1994) revisit Richards (1939)

an "external" real world, but it is one that we can know only through our constructed relation to it. There is no transcendence of this dilemma—realist and constructivist approaches provide each other's corrective.[12] Following Bourdieu, I believe that interrogating one's relation to the world one studies is not an obstacle but a necessary condition for understanding and explanation.[13] In particular, as ethnographers we enter only part of the world we study. That is, we face human limitations on what we can examine through participant observation, which makes the distinction between internal and external inescapable. Once again, by cross-classifying these two dimensions, we get four ways of explaining the discrepancy between an original study and its revisit. It so happens that actual focused revisits tend to emphasize one or another of these four explanations, giving rise to the four types shown in table 4.

Not only do focused revisits tend to fall into one of four types but each type assumes a quite distinctive modal character.

Type 1 revisits focus on the relations between observer and participant, and they tend to be refutational. That is to say, the successor uses the revisit to refute the claims of the predecessor, for example, Derek Freeman's (1983) denunciation of Margaret Mead's *Coming of Age in Samoa* (1928) and Marianne Boelen's (1992) vilification of William Foot Whyte's *Street Corner Society* (1943).

Type 2 revisits focus on theoretical differences, and they tend to be reconstructive. That is to say, the successor uses the revisit to reconstruct the theory of the predecessor, for example, Annette Weiner's (1976) feminist reconstruction of Bronislaw Malinowski's *Argonauts of the Western Pacific* (1922) and Oscar Lewis's (1951) historicist reconstruction of the Robert Redfield's *Tepoztlan: A Mexican Village* (1930).

Type 3 revisits focus on internal processes, and they tend to be empiricist. That is, the successor tends to describe rather than explain changes over time. Such is Robert Lynd and Helen Lynd's (1937) revisit to their own first study, *Middletown: A Study in Modern American Culture* (1929) and the subsequent revisit to Middletown by Theodore Caplow and his colleagues (Caplow and Bahr 1979; Caplow and Chadwick 1979; Caplow et al. 1982; Bahr, Caplow, and Chadwick 1983).

Type 4 revisits focus on external forces, and they tend to be structuralist. That is, they rely on a configuration of external forces to explain the discrepancy between the two studies. Here my two main examples are Sharon Hutchinson's (1996) revisit to Evans-Pritchard's *The Nuer* (1940) and Henrietta Moore and Megan Vaughan's (1994) revisit to Audrey Richards's *Land, Labour and Diet in Northern Rhodesia* (1939).

FOCUSED REVISITS OF A CONSTRUCTIVIST KIND

The distinguishing assumption of the constructivist revisit is that the site being studied at two points in time does not itself change, but rather it is the different relation of the ethnographer to the site (type 1) or the different theory that the ethnographer brings to the site (type 2) that accounts for the discrepancy in observations. Our knowledge of the site but not the site itself changes, in the first instance through refutation and in the second instance through reconstruction. We call these revisits constructivist because they depend upon the involvement or perspective of the ethnographer, that is, upon his or her agency.

Type 1: Refutation

Perhaps the most famous case of refutation is Derek Freeman's (1983) revisit to Margaret Mead's (1928) study of Samoan female adolescents. In her iconic *Coming of Age in Samoa* Mead claimed that Samoans had an easy, placid transition to adulthood, marked by a relaxed and free sexuality, so different from the anxious, tension-filled, guilt-ridden, and rebellious adolescence found in the United States. Based on multiple sources—accounts of missionaries and explorers, archives, and his own fieldwork in 1940, 1965, 1968, and 1981—Freeman claimed Samoans were a proud, vindictive, punitive, and competitive people. Far from easygoing, they were defiant individuals; far from placid, they were often more bellicose; far from their celebrated sexual liberation, Samoans prized virginity—among them, adultery excited rage, and rape was common. Samoan adolescents, Freeman claimed, were as delinquent as those in the West.

How could Mead have been so wrong? Freeman had a long list of indictments. Mead knew little about Samoa before she arrived; she never mastered the language; she focused narrowly on adolescents without studying the wider society; her fieldwork was short, lasting only three months out of the nine months she spent on Samoa; she lived with expatriates rather than with her informants; she relied on self-reporting of the teenage girls, who later declared that they were just teasing her. Mead was naive, inexperienced, unprepared, and finally hoaxed.[14] Worse still, and here we see how theory enters the picture, Freeman accused Mead of dogmatic defense of the cultural research program of her supervisor, Franz Boas. By showing that the trauma of adolescence was not universal, Mead was lending support to the importance of culture as opposed to biology. But the evidence, said Freeman, did not sustain her claims.

This attack on a foundational classic of cultural anthropology reverberated through the discipline.[15] Social and cultural anthropologists regrouped largely in defense of Mead. While recognizing potential flaws in her fieldwork, and tendentious interpretations of her own field notes, they turned the spotlight back on Freeman. Refutation inspired refutation. Critics found his citations of sources opportunistic, they wondered how he (a middle-aged white man) and his wife might have been more successful in discovering the sex lives of female adolescents than the twenty-three-year-old Mead. They accused him of relying on informants who had their own axe to grind, making him appear either more gullible than Mead or simply cynical. They complained that he said little about his own relations to the people he studied, except that he knew the language better than Mead. They were skeptical of his claim that being made an honorary chief meant that Samoans trusted him more than Mead. His critics considered him to have been gripped by a pathological refutational frenzy that lasted from his first fieldwork until he died in 2001.

Freeman brought further vituperation upon himself by refusing to offer an alternative theory of adolescence, biological or other, that would explain the data that he had mobilized against Mead. He followed Karl Popper, to whom he dedicated his 1983 book, but only halfway. Popper (1963) insisted that refutations be accompanied by bold conjectures, but that would have required Freeman to move to a type 2 revisit—theory reconstruction. Other anthropologists have come up with such reconstructions, partial resolutions of the controversy. Thus Bradd Shore (1983) argued that Samoan character was ambiguous, displaying Mead-type features in some situations and Freeman-like features in others. He proposed a richer theory of Samoan ethos than did either Mead or Freeman.

Others have tried to resolve the contradiction in a realist manner, proposing that Mead and Freeman were studying different Samoans. In refuting Mead, Freeman was forced to homogenize all Samoa. He did not distinguish the Samoa colonized by the Dutch from the Samoa colonized by the United States. Data collected from anywhere in Samoa between 1830 and 1987 were grist for his refutational mill. Yet even Mead herself recognized major changes that overtook Samoa during this period and suggested that the period of her fieldwork was especially harmonious. Weiner (1983) argued that Samoan character varied with the influence of missions. In the area studied by Freeman, competition among several denominations led Samoans to be more defiant than in Mead's Manu'a, where there

was only a single mission. Such real differences between the communities, Weiner claimed, went a long way to reconciling the divergent accounts. We are here moving in the direction of realist revisits.

In short, Freeman's obsessive focus on refutation, based on the distorting relations of ethnographer to the field, occluded both the reconstruction of theory and historical change as strategies to reconcile predecessor and successor studies. The same narrow refutational focus can be found in Marianne Boelen's (1992) revisit to William Foot Whyte's *Street Corner Society* (1943). Based on a series of short visits to Cornerville in the 1970s and 1980s, Boelen accused Whyte of all manner of sins—from not knowing Italian, ignoring family, not understanding Italian village life, and poor ethics to defending flawed Chicago School theories of gangs. Unlike Mead, who died five years before Freeman's book was published, Whyte was still alive to rebut Boelen's accusations (Orlandella 1992; Whyte 1992). In Whyte's account his Italian was better than the gang members', he did not consider the family or the Italian village as immediately relevant to street corner society, his ethical stances were clear and beyond reproach, and, finally, his theory of the slum, far from embracing Chicago's disorganization theories, was their refutation. Like Freeman, Boelen was fixated on refutation without proffering her own theory or considering the possibility of historical change between the time of Whyte's study and her own observations.

Boelen's critique of sociology's iconic ethnography barely rippled the disciplinary waters, in part because ethnography is more marginal in sociology than in anthropology and in part because the critique was poorly executed. Even if Boelen had approached her revisit with Freeman's seriousness, she would have had to confront a sociological establishment mobilized to defend its archetypal ethnography. As a graduate student, and female to boot, she would have been at a severe disadvantage. As Freeman discovered, it is always an uphill task to refute an entrenched study that has become a pillar of the discipline and, in Mead's case, a monument to America's cultural self-understanding. One might say that Freeman had to develop a pathological commitment to refutation if he were to make any headway. In the business of refutation the balance of power usually favors the predecessor, especially if he or she is alive to undermine or discredit the refuting successor.[16] The evidence brought to bear in the refutation must be either especially compelling or resonant with alternative or emergent disciplinary powers. Rather than cutting giants down to size or trampling them to the ground, it is often easier to stand on their shoulders, which is the strategy of the next set of revisits—the reconstruction of theory.

Type 2: Reconstruction

We have seen how some refutational revisitors, not content to highlight the distorting effects of poorly conducted fieldwork, also claimed that their predecessors imported arbitrary theory at the behest of an influential teacher or as a devotee of a favored school of thought. In these examples the revisitors failed, however, to put up their own alternative theory. They pursued the destruction of theory but not its reconstruction. It is reconstruction that distinguishes type 2 revisits.

One cannot be surprised that feminist theory is at the forefront of theoretical reconstruction of the classic ethnographies. There

have been feminist reanalyses of canonical works, such as Gough's (1971) famous reconstruction of Evans-Pritchard's (1940) work on the Nuer. The classic feminist focused revisit, however, is Annette Weiner's (1976) revisit to Bronislaw Malinowski's study of the Trobriand Islanders (1922). Malinowski did his fieldwork between 1915 and 1918, and Weiner did hers in a neighboring village in 1971 and 1972. Although by no means the first to revisit this sacred site, Weiner's study is a dramatic reconstruction from the perspective of Trobriand women. Where Malinowski focused on the rituals and ceremonies around the exchange of yams, Weiner dwelt on "mortuary ceremonies," conducted by women after the death of a kinsman, when the kin of the diseased exchange bundles of specially prepared banana leaves and skirts (also made out of banana leaves). While men work in the yam gardens, women labor over their bundles. These two objects of exchange represent different spheres of power: control of the intergenerational transfer of property in the case of men and control of ancestral identity in the case of women. Thus the rituals of death similarly divide into two types: those concerned with reestablishing intergenerational linkages through the distribution of property and those concerned with repairing one's "dala" identity, or ancestry, by distributing bundles of banana leaves. Women monopolize a power domain of their own, immortality in cosmic time, while they share control of the material world with men in historical time.

Weiner committed herself to repositioning women in Trobriand society and, by extension, in all societies. Theretofore anthropologists had reduced gender to kinship or had seen women as powerless objects, exchanged by men (Levi-Strauss 1969). In taking the perspective of these supposed objects (i.e., in subjectifying their experiences), Weiner showed them to wield significant power, institutionalized in material practices and elaborate rituals. Her revisit therefore served to reconstruct a classic study by offering a more complete, deeper understanding of the power relations between men and women. While Weiner may have been inspired to develop her reinterpretation by virtue of being a woman and living with women, these were not sufficient conditions for her gender analysis; we know this from the women anthropologists who preceded her. The turn to her particular understanding of gender was shaped by feminism. Rather than impugn Malinowski's fieldwork as limited by his focus on men and a myriad of other foibles that could be gleaned from his diaries, she attended to its theoretical limitations.

At the same time Weiner's study is curiously ahistorical in that she made no attempt to consider what changes might have taken place in the fifty-five years that had elapsed between her study and Malinowski's. Determining change might have been difficult for Weiner, as Malinowski had paid so little attention to mortuary rituals. It would have required her to first reconstruct Malinowski's account of the Trobriand Islanders as they were in 1915—a daunting task, but one that, as I will show, some type 4 revisits have attempted.

Still, in some type 2 revisits, in particular, Oscar Lewis's (1951) classic revisit to Robert Redfield's (1930) Tepoztlan, the successor reconstructs the theory of history used by the predecessor. Redfield studied Tepoztlan in 1926, and Lewis studied the village seventeen years later, in 1943, ostensibly to discover what had changed. But he became much less interested in studying the change in Tepoztlan than in taking Redfield to task for his portrait of an integrated, homogeneous, isolated, and smoothly functioning village, glossing over "violence, disruption, cruelty, disease, suffering and maladjustment" (Lewis 1951: 428–29). Lewis stressed the individualism of the villagers, their political schisms, their lack of cooperation, the struggles between the landed and landless, and conflicts among villages in the area. Instead of upholding Redfield's isolation of Tepoztlan, Lewis situated the village in a web of wider political and economic forces and traced features of Tepoztlan to the Mexican Revolution.

How did Lewis explain the differences between his account and Redfield's? First, he ruled out historical change during the seventeen years as sufficient to explain their discrepant portraits of Tepoztlan. Rather, Lewis criticized Redfield's folk-urban continuum—the theory that historical change can be measured as movement from folk to urban forms. While Lewis did grant some validity to Redfield's theory—communities do become more secular and individualized over time—he held the folk-urban continuum responsible for Redfield's sentimental portrait of Tepoztlan. Lewis's criticisms were multiple: The idea of a folk-urban continuum creates a false separation of town and country and an illusory isolation of the village; it overlooks the internal dynamics and diversity of villages; and, most important, it ignores the impact of broader historical changes. Also, Redfield substitutes position on a continuum from rural to urban for the study of real historical change. Thus in the final analysis Lewis attributed Redfield's romanticization of Tepoztlan to his myopic theory of history.[17]

For Lewis to stop here would leave his revisit as type 1, but he advances to Type 2 by providing his own broadly Marxist theory of social change. He situated Tepoztlan within an array of historically specific external influences, such as new roads and improved transportation, commerce, land reform, new technology, and the expansion of schooling. Like Weiner, Lewis did not use Redfield's study as a baseline to assess social change. Lewis thought that Redfield's ethnography was based on a misguided theory of history, which he, Lewis, replaced with his own context-dependent understanding of history.

The story does not end here. In *The Little Community* (1960: 132–48) Redfield subsequently offered a reanalysis of Lewis's focused revisit. He agreed with Lewis: historical change cannot explain the discrepancy between their two portraits of Tepoztlan. But Redfield denied the relevance of the folk-urban continuum because he hadn't even developed the theory at the time he wrote *Tepoztlan*. Instead he attributed their differences to the question each posed: "The hidden question behind my book is, 'What do these people enjoy?' The hidden question behind Dr. Lewis' book is, 'What do these people suffer from?'" (Redfield 1960: 136). And, Redfield continued, this is how it should be—we need multiple and complementary perspectives on the same site. Each has its own truth.[18] We are back to a type 2 reanalysis. But this misses Lewis's point—that questions derive from theories, and some theories are superior to others. Even if the folk-urban continuum did not spring fully formed from Tepoztlan, its embryo was already there in the early study, casting its spell as an inadequate synchronic theory of social change.[19]

When Lewis claimed some theories have a better grasp of social change than others, he was undoubtedly heading in a realist direction. Today we find anthropologists taking a constructivist turn, locking themselves into type 2 revisits that rule out

explanatory history altogether as either impossible or dangerous. In the late 1980s James Ferguson revisited the Zambian Copperbelt, about thirty years after the famous studies of the Manchester School (Ferguson 1999). In his account of deindustrialization, retrenchment, and return migration to the rural areas—the result of plummeting copper prices, International Monetary Fund–sponsored structural adjustment, and a raging AIDS epidemic—Ferguson discredited the Mancunians' teleology of urbanization and industrialization as a mythology of development (see, for example, Gluckman 1961b). Rather than subscribing to a theory of underdevelopment and decline, however, Ferguson refused any theory of history for fear of generating a new mythology. Although there are realist moments to his ethnography, and the data he offers could be reinterpreted through a realist lens, Ferguson replaced Manchester School teleology with an antitheory that disengaged from any causal account of social change. In other words, his revisit went beyond pure refutation to theory reconstruction (type 2), but the new theory is the apotheosis of constructivism, explicitly repudiating the realist endeavor. Constructivism, brought to a head, now topples over.

FOCUSED REVISITS OF A REALIST KIND

To the simpleminded realist focused revisits are designed specifically to study historical change. We have seen, however, that revisits may never mention history or mention it only to discount it. Constructivist revisits pretend there is no change, and the differences between predecessor and successor accounts are the result of the ethnographers' participation in the field site or of the theory they bring to the site. The revisits I now consider start from the opposite assumption—that discrepant accounts are the result of changes in the world, but, as I will show, they are often modified by considering the effects of the ethnographer's participation and theory. The constructivist perspective brings a needed note of realism to the realist revisit by insisting that we cannot know the external world without having a relationship with it. In what follows, constructivism disturbs rather than dismisses, corrects rather than discounts, deepens rather than dislodges the realist revisit.

I divide realist revisits into two types: type 3 revisits, which give primary attention to internal processes, and type 4 revisits, which give more weight to external forces. This is a hard distinction to sustain, especially when the time span between studies is long. Only if the revisit is an empirical description, cataloging changes in a community's economy, social structure, culture, and so on, can a purely internal focus be sustained. I therefore call these revisits empiricist. As soon as the focus shifts to explaining social change, the ethnographer is almost inevitably driven to consider forces beyond the field site.[20] Even the most brilliant ethnographers have failed in their endeavors to reduce historical change to an internal dynamics. Thus Edmund Leach's account of the oscillation between egalitarian *gumlao* and hierarchical *gumsa* organization in Highland Burma and Fredrik Barth's account of the cyclical movement of concentration and dispersal of land-ownership among the Swat Pathans have both come under trenchant criticism for ignoring wider forces.[21] Revisits that thematize the configuration of external forces, whether economic, political, or cultural, I call structuralist revisits. But the emphasis on external forces should

not come at the expense of the examination of internal processes. The mark of the best structuralist revisits is their attention to the way internal processes mediate the effect of external forces.

Sustaining the distinction between internal and external compels us to problematize it but without relinquishing it. Just as type 1 refutational revisits by themselves are unsatisfactory and require incorporation into type 2 revisits of reconstruction, so type 3 revisits that dwell on internal processes are equally unsatisfactory by themselves, requiring incorporation within type 4 revisits that thematize external forces.

Type 3: Empiricism

A compelling empiricist revisit is hard to find, but Robert Lynd and Helen Lynd's (1937) revisit to their own study of Middletown is at least a partial case. Insofar as they described Middletown's change between 1925 and 1935, they confined their attention to the community, but as soon as they ventured into explanation, they were driven to explore forces beyond the community. Without so much as recognizing it, they reconstructed the theory they had used in the first study—a reconstruction that can be traced to their own biographies and their changed relation to Middletown. In other words, their revisit, ostensibly an investigation of internal processes, bleeds in all directions into type 1 and 2 constructivist explanations as well as type 4 structuralist explanations.

The first Middletown study (Lynd and Lynd 1929), which I call Middletown I, was most unusual for its time in focusing on social change. Taking their baseline year as 1890, the Lynds reconstructed the intervening thirty-five years from diaries, newspapers, and oral histories.[22] To capture a total picture of Middletown they adopted a scheme used by the anthropologist W. H. R. Rivers that divided community life into six domains: making a living, making a home, training the young, organizing leisure, practicing religion, and engaging in community activities. The Lynds argued that the long arm of the job increasingly shaped all other domains. The expansion of industry entailed deskilling, monotonous work, unemployment, and declining chances of upward mobility. Employment lost its intrinsic rewards, and money became the arbiter of consumption. The exigencies of industrial production led to new patterns of leisure (organized around the automobile, in particular) and of homemaking (with new gadgets and fewer servants), as well as the rise of advertising (in newspapers, which expanded their circulation). The pace of change was greatest in the economy, which set the rhythm for the other domains—leisure, education, and home underwent major changes, while religion and government changed more slowly.

In all realms the Lynds discerned the profound effects of class. The previous thirty-five years had witnessed, so they claimed, a growing division between a working class that manipulated physical objects and a business class that manipulated human beings (stretching all the way from the lowest clerical workers to the highest corporate executive). They discovered a growing class divide in access to housing, schooling, welfare, and medical services; in patterns of the domestic division of labor, leisure, reading, and religious practices; and in influence over government, media, and public opinion. The business class controlled ideology, promoting progress, laissez faire, civic loyalty, and patriotism, while the working class

became ever more atomized, bereft of an alternative symbolic universe.

If we should congratulate the Lynds on adopting a historical perspective, we should also be cautious in endorsing their study's content, especially after the historian Stephan Thernstrom (1964) demolished a similar retrospective history found in Warner and Low's (1947) study of Yankee City. This is all the more reason to focus on the Lynds' revisit to Middletown in 1935, *Middletown in Transition,* which I call Middletown II.

Robert Lynd returned to Middletown with a team of five graduate students but without Helen Lynd. The team set about examining the same six arenas of life that structured the first book. With the depression the dominance of the economy had become even stronger, but the Lynds were struck by continuity rather than discontinuity, in particular, by Middletowners' reassertion of old values, customs, and practices in opposition to change emanating from outside. They documented the emergence and consolidation of big business as a controlling force in the city; the expansion and then contraction of unions as big business fought to maintain the open shop in Middletown; the stranglehold of big business on government and the press; the growth and centralization of relief for the unemployed; adaptation of the family as women gained employment and men lost prestige; expansion of education; stratification of leisure patterns; and the continuity of religious practices that provided consolation and security.

So much for the Lynds' empiricist account. But there is a second register, an explanation of the changes, interwoven with the description. Capitalist competition and crises of overproduction produced the disappearance of small businesses, making the power of big business all the more visible; uncertain employment for the working class, which was living from hand to mouth; diminished opportunities for upward mobility as rungs on the economic ladder disappeared; resulting in a more transparent class system. The two-class model had to be replaced by six classes. Already one can discern a change in the Lynds' theoretical system: In Middletown I change came about internally through increases in the division of labor; in Middletown II change was produced by the dynamics of capitalism bound by an ineluctable logic of competition, overproduction, and polarization. The influence of Marxism is clear but unremarked. Market forces were absorbing Middletown into greater America; the federal government was delivering relief, supporting trade unions, and funding public works, while from distant places came radio transmissions, syndicated newspaper columns, and standardized education. Middletown was being swept up in a maelstrom beyond its control and comprehension.

The Lynds could not confine themselves to internal processes, but how conscious were they of the shift in their own theoretical perspective? Two long and strikingly anomalous chapters in Middletown II have no parallels in Middletown I. The first anomalous chapter is devoted to Family X, which dominated the local economy, government, the press, charity, trade unions, and education. Yet Family X was barely mentioned in Middletown I, although its power, even then, must have been apparent to all. The second anomalous chapter is titled the "Middletown Spirit"; it examines the ruling-class ideology and the possibilities of a counterideology based on working-class consciousness. If Middletown I was a study of culture as social relations, Middletown II became a study of culture as masking and reproducing relations of power.

Different theoretical perspectives select different empirical foci: instead of the inordinately long chapter on religion we find one on the hegemony of Family X.[23] It's not just that Middletown had changed—the Lynds, or at least Robert Lynd, had modified their theoretical framework.

But why? Did the refocused theory simply mirror changes in the world? In other words, does the world simply stamp itself onto the sociologist who faithfully reports change? That was the Lynds' position in 1925 when they described themselves as simply recording "observed phenomena" with no attempt to "prove any thesis" (1929: 4, 6). The intellectual ambience of Middletown II was entirely different. Robert Lynd started out by declaring that research without a viewpoint is impossible and that his viewpoint was at odds with that of the people he studied. In those ten intervening years Lynd had become persuaded that laissez-faire capitalism was unworkable, that planning was necessary, and that trade unions should be supported. He had begun to participate in the New Deal as a member of the National Recovery Administration's Consumers Advisory Board, and he had been influenced by what he regarded as the successes of Soviet planning (see M. Smith 1994). As we know from Robert Lynd's *Knowledge for What* (1939), he took up an ever more hostile posture toward capitalism. In ten years he had come a long way from the declared empiricism in Middletown I, and his revisit was shaped by his own transformation as much as by Middletown's, by his adoption of a theory of capitalism that thematized the power of forces beyond Middletown and patterns of domination within Middletown. In short, there's more than a whiff of type 1, 2, and 4 revisits in this ostensibly type 3 revisit to Middletown.

If the Lynds were never the empiricists they originally claimed to be, the second revisit (Middletown III), conducted between 1976 and 1978 by Theodore Caplow and his collaborators, did attempt a purely empirical description of changes within Middletown. While the researchers did spend time—serially—in Middletown, their results were largely based on the replication of two surveys that the Lynds administered in 1924—one of housewives and the other of adolescents. Leaving aside changes in the meaning of questions or the differential bias in the samples themselves, Caplow and his collaborators concluded that values had not changed much over fifty years and that the lifestyles of the working class and the business class had converged (Bahr, Caplow, and Chadwick 1983; Caplow and Bahr 1979; Caplow and Chadwick 1979). In their best-known volume, *Middletown Families,* Caplow and colleagues (1982) noted that despite changes in the economy, state, and mass media, the family maintained its integrity as a Middletown institution.

Caplow and colleagues (1982) debunked the idea that the American family was in decline, but they were not interested in explaining its persistence—how and why it persisted alongside changes in other domains. Nor were they interested in explaining the significant changes in the family that they did observe, namely, increased solidarity, smaller generation gaps, and closer marital communication. Such a task might have led them to examine the relations between family and other spheres or to investigate the impact of forces beyond the community. In choosing to focus on replicating the Lynds' Middletown I surveys, Caplow and colleagues necessarily overlooked questions of class domination at the center of Middletown II and, in particular, the power of Family X.[24] Indeed, lurking behind their

empiricism was a set of choices—choices made by default but choices nonetheless: techniques of investigation that define the researcher's relation to the community, values that determine what not to study, theories to be refuted and reconstructed.[25]

Caplow and his collaborators shed further light on the distinction between replication and revisit, for their return to Middletown was indeed a replication that attempted to control the relation of observer to participant. That is, they asked the same questions under the simulated conditions of a parallel sample of the population—all for the purpose of isolating and measuring changes in beliefs, lifestyle, and so on.[26] The trouble is, of course, as in the natural sciences, one never knows to what extent responses to a survey reflect something real that can be used to test a hypothesis or to what extent they are a construction of the survey instrument (Collins 1985; Collins and Pinch 1993). The focused revisit makes no pretense to control all conditions and confronts these questions of realism and constructivism head on. There is a second sense, however, in which the replication studies of Middletown III are limited, and that is in their failure to explain what has or has not changed. That would mean reconstructing rather than refuting theories, and of course, it would entail going beyond Middletown itself. This brings us, conveniently and finally, to type 4 revisits.

Type 4: Structuralism

Parallel to the Lynds' return to Middletown is Raymond Firth's classic revisit to Tikopia, an isolated and small Polynesian island that he first studied in 1928–29 and to which he returned in 1952 (Firth 1936, 1959). Like the Lynds in their revisit to Middletown, Firth was not about to deconstruct or reconstruct his own original study. Rather, he took it as a baseline from which to assess social change during the twenty-four years that had elapsed between the two studies. Having constructed Tikopia as an isolated and self-sustaining entity, the impulse to social change came primarily from without. Indeed, Firth arrived just after a rare hurricane—an external force if ever there was one—had devastated the island, causing widespread famine. As a counterpart to the depression that hit Middletown, the hurricane became Firth's test of the resilience of the social order, a test that for the most part was met. But Firth was more concerned to discern long-term tendencies, independent of the hurricane and the famine it provoked. He emphasized Tikopian society's selective incorporation of changes emanating from without—labor migration to other islands, the expansion of commerce and a money economy, the influx of Western commodities, the expansion of Christian missions, the intrusion of colonial rule. In the face of these irreversible forces of so-called modernization, the Tikopian social order still retained its integrity. Its lineage system attenuated but didn't disappear, gift exchange and barter held money at bay, and residence and kinship patterns were less ritualized, but the principles remained despite pressure on land, and the chiefs' power was less ceremonial but also strengthened as it became the basis of colonial rule. In short, an array of unexplicated, unexplored external forces had their effects but were mediated by the social processes of a homogeneous Tikopian society.

More recent structuralist revisits problematize Firth's assumptions. They examine the contingency of external forces as well as the deep schisms these forces induce within societies. They think more deeply about the implication of the original ethnographers' living in the world they study and even the impact of their presence on the world that is revisited.[27] Sharon Hutchinson (1996) and Henrietta Moore and Megan Vaughan (1994) replace Firth's homogeneous society undergoing modernization with societies beset by domination, contestation, and indeterminacy. These revisits reflect the profoundly different theoretical lenses that the ethnographers bring to their fieldwork.

Hutchinson's revisit is the most comprehensive attempt to study what has happened to the Nuer of the southern Sudan—those isolated, independent, cattle-minded warriors immortalized by Evans-Pritchard in his classic studies of the 1930s (1940, 1951, 1956). Hutchinson did her first fieldwork in 1980–83, just before the outbreak of the second civil war between the "African" South and the "Arab" North. She returned to Nuerland in 1990, while it was still in the midst of the devastating war. Hutchinson took Evans-Pritchard's accounts of the Nuer as her baseline point of reference and asked what had changed during sixty years of colonialism, with the succession of a national government in Khartoum (northern Sudan), and then two civil wars. Her questions were entirely different from those of Evans-Pritchard. Where he was interested in the functional unity of the Nuer community, viewing it as an isolated order, insulated from colonialism, wars, droughts, and diseases, Hutchinson focused on the latter. Where he looked for the peace in the feud, the integrative effects of human animosities and ritual slayings, she focused on discord and antagonism in order to understand the transformation of the Nuer community.

Instead of reconstructing Evans-Pritchard's original studies, relocating them in their world-historical context, Hutchinson deployed the clever methodological device of comparing two

Nuer communities—one in the western Nuer territory that more closely approximated Evans-Pritchard's enclosed world and another in the eastern Nuer territory that had been more firmly integrated into wider economic, political, and cultural fields. Administered by the Sudanese People's Liberation Army (SPLA), the western community became a bastion of resistance to Islamicization from the north. Still, even there, despite being swept into war, markets, and states, the Nuer managed to maintain their cattle-based society. Exchanging cattle, especially as bridewealth, continued to cement the Nuer, but this was possible only by regulating and marginalizing the role of money. As the Nuer say, "Money does not have blood." It cannot re-create complex kin relations, precisely because it is a universal medium of exchange. Instead of commodifying cattle, the Nuer "cattle-ified" money. As in the case of bridewealth, so in the case of bloodwealth, cattle continue to be means of payment. In Nuer feuds cattle were forfeited as compensation for slaying one's enemy. When guns replaced spears or when the Nuer began killing those they did not know, they retained bloodwealth but only where it concerned the integrity of the local community.

Change may have taken place within the terms of the old order, but nonetheless it was intensely contested. As war accelerated Nuer integration into wider economic, political, and social structures, Nuer youth exploited new opportunities for mobility through education. An emergent class of educated Nuer men—bull-boys, as they were called—threatened the existing order by refusing scarring marks of initiation (scarification). Initiation lies at the heart of Nuer society, tying men to cattle wealth and women to human procreation. Thus the newly educated classes were at the center of controversy. Equally, cattle sacrifice was

contested as communities became poorer, as Western medicines became more effective in the face of illness and disease, and as the spreading Christianity sought to desacralize cattle. The SPLA promoted Christianity both to unite the different southern factions in waging war against the north and as a world religion to contest Islam in an international theater. Finally, the discovery of oil and the building of the Jonglei Canal (which could ruin the southern Sudan environmentally) increased the stakes and thus the intensity of war. Indeed, southern Sudan became a maelstrom of global and local forces.

Rather than reifying and freezing external forces, Hutchinson endowed them with their own historicity, following their unexpected twists and turns but also recognizing the appearance of new forces as old ones receded. Uncertainty came not only from without but also from within Nuerland, where social processes had a profound openness—a cacophony of disputing voices opened the future to multiple possibilities. Unstable compromises were struck between money and cattle, Nuer religion and Christianity, prophets and evangelists, guns and spears, all with different and unstable outcomes in different areas. The radical indeterminacy of both external forces and internal processes had a realism of terrifying proportions.

For all its indeterminacy Hutchinson's revisit is realist to the core. She does not try to deconstruct or reconstruct Evans-Pritchard's account. The next revisit, however, does precisely that—it problematizes the original study much as Freeman did to Mead and Weiner did to Malinowski. In *Land, Labour and Diet in Northern Rhodesia* (1939), another of anthropology's African classics, Audrey Richards postulated the breakdown of Bemba society as its men migrated to the mines of southern

Africa in the 1930s. She attributed her postulated breakdown to the slash-and-burn agriculture (*citimene* system), which could not survive the absence of able-bodied men to cut down the trees. Henrietta Moore and Diane Vaughan (1994) returned to the Northern Province of Zambia (Northern Rhodesia) in the 1980s only to discover that the *citimene* system was still alive, if not well. Why was Richards so wrong and yet so widely believed?

Moore and Vaughan's first task was to reexamine *Land, Labour and Diet* in the light of the data Richards herself compiled and then in the light of data gathered by subsequent anthropologists, including themselves. Moore and Vaughan discovered that Bemba women were more resourceful than Richards had acknowledged—they cultivated relish on their own land and found all sorts of ways to cajole men into cutting down trees. This was Richards's sin of omission—she overlooked the significance of female labor and its power of adaptation. Her second sin was one of commission; namely, she endorsed the obsession of both Bemba chiefs and colonial administrators with the *citimene* system, an obsession that stemmed from the way the Bemba used shifting cultivation to elude the control of their overlords, whether that control was to extract taxes or enforce tribal obligations. So it was said by Bemba chiefs and colonial administrators alike—*citimene* was responsible for the decay of society. Richards not only reproduced the reigning interpretation but gave ammunition to successive administrations, which wished to stamp out *citimene*. *Land, Labour and Diet* was forged in a particular configuration of social forces and extent knowledge, and it then contributed to their reproduction. As a particular account of Bemba history it also became part of that history.

The conventional wisdom that Richards propagated—that Bemba society was in a state of breakdown—was deployed by colonial and postcolonial administrations to justify their attempts to transform Bemba agriculture. Even as late as the 1980s the Zambian government's agrarian reforms assumed that *citimene* was moribund. It responded to the Zambian copper industry's steep decline by encouraging miners to return "home" (i.e., to rural areas), where they were offered incentives to begin farming hybrid maize. Moore and Vaughan show how it was this return of men (not their absence) that led to impoverishment as the farmers now demanded enormous amounts of female labor, delivered at the expense of subsistence agriculture and domestic tasks. In particular, this compulsory labor caused women to wean their children prematurely, leading to higher infant mortality. It was not the cash economy, *citimene,* or male absenteeism that threatened Bemba livelihood, as Richards and conventional wisdom had it, but the regulation of female labor by male workers returning from the Copperbelt.

This is a most complex revisit. On one hand, Moore and Vaughan did to Richards precisely what the Lynds did not do to themselves and Hutchinson did not do to Evans-Pritchard—namely, to locate the original study in the social context of its production, recognizing its contribution to the history that the successor study uncovers, drawing out the link between power and knowledge. On the other hand, unlike Freeman, who also proposed ways in which Mead shaped the world she described, Moore and Vaughan did not sacrifice history. They were still able to offer an account of the transformation of Bemba agriculture from the 1930s, taking their reconstruction of Richards's classic study as their point of departure. But here is the final paradox:

Moore and Vaughan did not consider the ways that their own analysis might have been one-sided, governed by specific feminist and Foucauldian assumptions, and thereby contributed to discourses that would shape the Bemba world of future revisits. While they located Richards in the world she produced, they did not locate themselves in their own relation to the Bemba. Indeed, they write all too little about their own fieldwork, their own interaction with the Bemba. In restoring Richards to history, ironically, Moore and Vaughan placed themselves outside history.

Moore and Vaughan did not take the final step toward grounding themselves because they did not engage in any self-conscious theorizing. They had no theory to help them step outside themselves. As in the indeterminacy of outcomes in Nuerland, the openness of the future stems from a refusal of theorization, beyond orienting propositions about gender, power, and knowledge. Both these revisits contrast vividly with my own structuralist study in which I viewed the hegemonic organization of work as the "end of history" and had no conception of reversal or alternative paths. Where I froze external forces to produce a structural overdetermination, Hutchinson and Moore and Vaughan left external forces in the hands of the gods to produce a structural undetermination. My error was the opposite of theirs, but the source was the same—an ignorance of the processes behind the external forces. I did not examine the processes behind state transformation or market globalization; Hutchinson did not study the strategies of war in the Sudan or the World Bank's development schemes; Moore and Vaughan did not attempt an analysis of the declining copper industry or the Zambian state's strategies of rural development. The revisits to the Nuer and the Bemba reversed the determinism of their

predecessors, whether it was the static functionalism of the one or imminent breakdown of the other. These anthropologists' aversion to explanatory theory led to an empiricism without limits, just as my failure to take Marxist theory sufficiently seriously led me to reification without possibilities. In all cases the problem was the undertheorization of external forces. We need to deploy our theories to grasp the limits of the possible and the possibilities within limits.

REVISIT AS THE ETHNOGRAPHIC TROPE

I am now in a position to extend the analysis of the focused revisit to other dimensions of ethnography. But first to recap: The focused revisit entails a focused dialogue between the studies of the successor and predecessor. From this dialogue I have elucidated four explanations for the divergence of accounts of the "same" site at two points in time. I distinguished revisits based on whether they were constructivist (i.e., focused on the advance—refutation or reconstruction—of "knowledge of the object") or whether they were realist (i.e., focused on historical change in the "object of knowledge").

In the constructivist class I distinguished type 1 from type 2 revisits. Type 1 revisits focus on a claimed distortion in the original study brought about by the relation of ethnographer to the people being studied. These revisits aim to show how misguided the first study was, thereby discrediting it without substituting an alternative interpretation. The peculiarity here is refutation without reconstruction. The type 2 revisit focuses on the theory brought to bear by the original ethnographer and replaces it with an alternative theory. In neither case is the revisit itself exploited for its insight into historical change, which is the focus of types 3 and 4. Type 3 revisits concentrate on internal processes of change. Such a confinement proves possible only in so far as there is no attempt to explain change, that is, only if we limit ourselves to describing it. Finally, type 4 revisits admit external forces into the framework of explanation. Here ignorance of those external forces—their appearance, and disappearance, and their dynamics—leads either to structural determinacy or, more usually, to historical indeterminacy, to which even the effects of the original study may contribute.

I have argued that the nine revisits discussed here tend to fall into, rather than across, the four types. This suggests that the dimensions I used to define the four types have a certain robustness with respect to the actual practice of focused revisiting. Still, the distinctions are far from watertight. Take the more imposing distinction between constructivism and realism. While constructivist revisits seem to be able to suspend historical change, that is precisely their shortcoming. On the other hand, I have shown how realist revisits continually face constructivist challenges, underlining the dilemmas of participating in a world while externalizing and objectifying it. If there is bleeding across the constructivist-realist dimension, the boundary between internal and external is a veritable river of blood. Refutation easily leads to reconstruction and empiricism to structuralism. However fluid and permeable the line between internal and external may be, the distinction itself is nonetheless necessary. First, theorizing cannot be reduced to the ethnographer's relation to the field. Theorizing cannot begin tabula rasa with every new fieldwork—it's not possible for ethnographers to strip themselves of their prejudices. Even if it were possible, researchers

wouldn't get far as a scientific collectivity if they insisted always on returning to ground zero—they necessarily come to the field bearing theory. Simply put, the mutually enhancing dialogue between participant observation and theory reconstruction depends on the relatively autonomous logics of each. Second, everything cannot be a topic of study: An ethnographer must distinguish the arena of participant observation from what lies beyond that arena. The necessity of the demarcation between internal and external is therefore practical—ethnographers are part of the world they study but *only* part of it—but it is represented and justified in terms of the theories they deploy.

In short, reflexive ethnography recognizes two dilemmas: There is a world outside ourselves (realist moment), but ethnographers can know it only through their relation to it (constructivist moment); and ethnographers are part of that world (internal moment) but *only* part of it (external moment)., There is no way to transcend these dilemmas, and so reflexive ethnography must consider all four moments, even if in the final analysis it concentrates on only one or two. The practitioners of other sociological methods have no reason to gloat—the same dilemmas also apply to them; they are just less glaring and less invasive. Reflexive ethnography clarifies and anticipates the methodological challenges facing all social science. Ethnographers can say to their scientific detractors: "De te fabula narratur!" (The story applies to you).

Now that I have demonstrated the principles of reflexive ethnography at work in the focused revisit, which is still rather esoteric for sociologists, can these principles be applied to other aspects of fieldwork? Can ethnography be conceptualized more broadly through the lens of the "revisit"? In addition to the focused revisit I delineate five other types of revisit—rolling, serial, heuristic, archeological, and valedictory. Here my intent is to show how sociologists have begun to deploy these in their ethnographies, thereby gesturing to, and even embracing, history, context, and theory.

Fieldwork: The Rolling Revisit

I begin with the mundane routines of fieldwork, the elementary form of ethnography. Conventionally, fieldwork is regarded as a succession of discrete periods of observation that accumulate in field notes, later to be coded, sorted, and analyzed when all the data are in. Every visit to the field is unconnected to previous and subsequent ones, so in the final analysis visits are aggregated as though they were independent events. In the reflexive view of fieldwork, on the other hand, visits to the field are a succession of experimental trials, each intervention separated from the next one, to be sure, but each in conversation with the previous ones. In this conception fieldwork is a rolling revisit. Every entry into the field is followed not just by writing about what happened but also by an analysis in which questions are posed, hypotheses are formulated, and theory is elaborated—all to be checked out during successive visits. In this rendition field notes are a continuous dialogue between observation and theory.

In his appendix to *Street Corner Society* (1955) William Foot Whyte describes the detached process of accumulating data, writing everything down, and sorting it into folders, but he also writes of the conversation between theory and data. Thus he writes of the influence that the anthropologist Conrad

Arensberg had in encouraging Whyte's focus on social interaction among particular individuals and how that interaction reflected the social structures in which they were embedded. Arensberg provided the theoretical frame that Whyte was to elaborate so famously. Accordingly, Whyte's field notes became filled with detailed events and conversations between particular individuals. His epiphany came when he discovered the link between performance at bowling and position within the gang and later when he related mental illness (e.g., Doc's dizzy spells) to the disruption of customary roles. He carried out experiments in the field to test his theories. Thus he cured Long John of his nightmares by restoring him to his former place in the gang. Once Whyte realized what his project was about—after eighteen months in the field he was forced to write a report to renew his grant—his field notes did indeed become more like a dialogue of theory and data. It would have happened much earlier if he had subscribed to, rather than stumbled upon, the idea of the rolling revisit.

While field notes are a running dialogue between observation and theory, fieldwork is a running interaction between ethnographer and participant. It involves a self-conscious recognition of the way embodiment, location, and biography affect the ethnographer's relations to the people studied and thus how those relations influence what is observed and the data that are collected. Whyte was only too aware of the significance of his ethnicity, his large physical size, and his relative youth, as well as his upper-middle-class background and his connection to Harvard, for making and sustaining contact with the various groups in Cornerville. His relation to the community changed with his status, when, for example, his new wife came to live with him. But it also altered as his interests shifted from gangs to racketeering and politics. Throughout, he was strategic in how he positioned himself within the community, acting as secretary of the Italian Community Club, becoming part of local election campaigns (one of which led him into illegal repeat voting), and even organizing a demonstration at the mayor's office. By his own admission he began his research as a nonparticipating observer and ended as a nonobserving participant.

These, then, are constructivist moments in the field. They focus on the way knowledge of the field changes, as though the field itself remains unchanged. The assumption of a fixed site is a useful but ultimately problematical fiction. Fields have dynamics of their own that often erupt with outside interventions. Again, Whyte was far ahead of his time in focusing on the dynamics of the field itself. By studying the rise and fall of the Norton Gang, its relation to the Italian Community Club, the evolution of political campaigns, and the continuing struggles for control of gambling houses, Whyte was able to tease out the relations between individuals and social structures and among social structures themselves. Human behavior and the groups to which individuals belong could only be understood, Whyte averred, through analyzing their change through time. Largely a function of internal dynamics and life trajectories of individuals, these changes were also affected by such external events as election campaigns and police raids. Whyte's extensions to macrostructures and history were limited, but he definitely pointed to the wider world in which the gangs were embedded.

Reflexive fieldwork, in short, calls attention to realist as well as constructivist moments. It demands that the field be understood as always in flux, so that the rolling revisit records the

processual dynamics of the site itself. But, more than that, the rolling revisit demands attention to disruptions of the field from outside, which shift its character and take it off in new directions. Still, remember that this field-in-flux can be grasped only through theoretical lenses and through the ethnographer's interactions with those she or he studies.

Long-term Field Research: The Serial Revisit

George Foster and colleagues have advanced the idea of long-term field research in which ethnographers, either as individuals or as a team, revisit a field site regularly over many years (they arbitrarily say more than ten) with a view to understanding historical change and continuity.[28] Their collection of cases of long-term field research ranges from Louise Lamphere's (1979) overview of the dense thicket of Navajo ethnographies to Evon Vogt's (1979) account of the Harvard Chiapas Project (1957–75).

A subspecies of this long-term research is what I call the serial revisit, in which the same ethnographer conducts separated stints of fieldwork at the same site over a number of years. This is how Elizabeth Colson (1989) describes her own multiple revisits to the Gwembe Tonga of Northern Rhodesia since her first research there in 1956. She and her colleague, Thayer Scudder, followed the resettlement of the Tonga after the completion of the Kariba Dam in 1959 and subsequently studied how the Tonga fared under the postcolonial dispensation (Scudder and Colson 1979). They noted how their relations with the Tonga shifted as their concern for the fate of the Tonga intensified but also as they and their informants aged. At the same time Colson and Scudder's theoretical framework shifted from a focus on kinship and ritual to the absorption of the Tonga into a national and regional political economy and from there to the broadest analysis of resettlement patterns and refugee problems in a global context. All four dimensions of reflexive ethnography were at work as this project evolved over three decades.

Most serial revisits within sociology are unashamedly realist. Thus between 1975 and 1989 Elijah Anderson studied uneven urban development in Philadelphia within what he called Village-Northton (Anderson 1990). With the exodus of manufacturing from the surrounding area, one side, namely, the middle-class Village, became gentrified and whiter, while the other side, lower-class Northton, became poorer and blacker. Anderson described changing patterns of social control and etiquette on the streets, the replacement of the "old heads" by the young drug dealers, changing sex codes, and spillover effects from one community to the other. Sudhir Venkatesh, whose work is more historically self-conscious, studied the Robert Taylor Homes in Chicago during a ten-year period, plotting the rise and fall of the modern ghetto (Venkatesh 2000). He tied changing modes of community control (the rise of gangs, of informal economy, and of mothers' groups) to rising unemployment and the withdrawal of state services (especially the withdrawal of police and the destruction of public housing).

Not all serial revisits exploit the temporal extension of fieldwork to study social change. Quite to the contrary, they are often used to extract what does not change. Ruth Horowitz (1983) studied youth gangs in a poor Mexican American neighborhood of Chicago for three years, 1971 to 1974. Then she returned in 1977 to follow their paths into the labor market and to discover how the gangs had sustained themselves. Reaffirming the clash of community culture and the wider individualism of U.S. society, she emphasized stasis rather than change. Martin Sanchez Jankowski, who was even more determined to focus on the constant, studied thirty-seven gangs in three cities for ten years (Jankowski 1991). Stints of fieldwork were undertaken and data collected as though they were independent observations at a fixed site. He focused on their common organizational form and their community embeddedness; he was not interested in how the gangs changed over time or varied between cities or how their ties to the political and economic contexts shifted over time. He deployed his long-term field research to reveal the stabilizing effects of another constant—the defiant individualism of gang members. He dwelled on what stayed the same, despite change and through change.

Although technically a serial revisit, Jankowski's goal was replication in both the constructivist and realist senses. As an unobtrusive participant observer, he sought to establish replicable conditions of research, inducing theory from his neutral observations. At the same time he decentered the study of change, whether through internal processes or external forces, in favor of replicating the same result across space and time—the wider the range of cases, the more convincing the result. One might say, paradoxically, he mobilized reflexivity in pursuit of replication.

Although Jankowski made reference to other studies of gangs, it was not to suggest that time and place explained their different conclusions. He could, for example, have drawn on Whyte's (1943) parallel gang study with similar findings to ask what had changed during the intervening forty years. That, however, would have turned Jankowski's study into a "heuristic" revisit, the antithesis of replication.

Framing the Present: The Heuristic Revisit

The rolling and serial revisits return ethnographers to the familiarity of their own field sites. In these revisits memory plays an enormous but rarely theorized role (Mayer 1989). Rolling and serial revisits contrast with the next two types of revisit in which ethnographers compare their own fieldwork with someone else's research, documentation, or study. The first is the heuristic revisit, which appeals to another study—not always strictly ethnographic and not necessarily of the same site but of an analogous site—that frames the questions posed, provides the concepts to be adopted, or offers a parallel and comparative account.

Most heuristic revisits in sociology, like the serial revisits, have a strong realist bent. Thus Mary Pattillo-McCoy (1999) used Frazier's *Black Bourgeoisie* (1957) and Drake and Cayton's *Black Metropolis* (1945) to frame her ethnographic account of the social, economic, and geographical proximity of black middle-class life to the south Chicago ghetto. Mitchell Duneier's (1999) study of street vendors in Greenwich Village goes back forty years to Jane Jacobs's *Death and Life of Great American Cities* (1961), recovering her analysis of the same area and, in particular, the role of public characters. Following Jacobs's example, Duneier regarded the street vendors as "public characters" who, contrary to stereotype, stabilize community relations. With Jacobs's work as his baseline, Duneier considered the broad changes in Greenwich Village—the rising inequality, cultural difference, and crime—and how it came to be a home for the

homeless. He traced the vendors to their previous location in Pennsylvania Station and uncovered the political forces that led to their eviction. He practiced what he called the extended place method—realist method par excellence—which attempts to remove all traces of constructivism by striving for an objective record of the behaviors of his subjects and by renouncing theoretical reconstruction in favor of induction.[29]

My final example of a heuristic revisit adopts a more constructivist perspective. Leslie Salzinger (2003) used Patricia Fernandez-Kelly's (1983) pioneering study of women as inexpensive and malleable labor in the Mexican maquiladora industry to frame her own study of the same industry twenty years later. Where Fernandez-Kelly saw only one gender regime, Salzinger discovered a multiplicity, reflecting the expansion of the industry and its changing market context. Stressing indeterminacy of outcomes and reflecting twenty years of feminist thought, Salzinger also made a theoretical turn. Her analysis of production focused on the poststructuralist subjectivity rather than on the political economy of gender regimes. History moves on, but so does theory. Their trajectories are intertwined.

Digging Up the Past: The Archeological Revisit

If the heuristic revisit moves forward in time, from the earlier study to the later one that it frames, the archeological revisit moves backward in time to excavate the historical terrain that gives rise or gives meaning to the ethnographic present. If not strictly a revisit—since there is no reference study known ahead of time—it is a common technique for giving historical depth to ethnography.[30] In the archeological revisit multiple sources of data are used, whether retrospective interviews, published accounts, or archival documents. One could simply triangulate and aggregate all the historical data from different sources as though they measured a singular and fixed reality. This, however, would violate the rules of reflexivity, which demand disaggregating data to reflect their relations of production, namely, relations between observers and participants, and the theories that observers (journalists, officials, witnesses) deploy.

A number of recent sociological studies turn on archeological revisits. Pierrette Hondagneu-Sotelo (1994) explored the historical antecedents of the gendered streams of immigration from Mexico to the United States. To give specificity to the revelations of her fieldwork in a community in northern California, she was led back in time to distinguish immigrants who came before the end of the bracero program in 1965 (the program that channeled single, male migrants into the agricultural fields of California) from those who came after its end. Through oral histories Hondagneu-Sotelo was able to trace the consequences of original migration patterns for the domestic division of labor. Similarly, Rhonda Levine (2001) produced an unexpected ethnography of German cattle dealers in New York State, refugees from Nazi persecution. To understand their participation in the transformation of New York's dairy industry, she uncovered details of their lives in rural Germany before they left. Like Hondagneu-Sotelo, Levine traced the connection between the community of origin and the community of settlement.

Lynne Haney (2002) conducted an ethnography of the social effects of cutbacks in Hungary's postsocialist welfare. To understand the reaction of the poor women she studied, Haney had to reexamine the socialist welfare state, distinguishing the maternalist welfare regime of reform communism from the societal welfare of the early post–World War II period. She turned to archives and oral histories to reconstruct the past, disclosing a novel periodization of state socialism and its aftermath.

It is no accident that so many of the ethnographies of the market and of democratic transitions become archeological revisits, excavating the socialist antecedents of the postsocialist order (Burawoy and Verdery 1999; Kligman 1998; Lampland 1995; Woodruff 1999). As in the case of the postcolonial transition, ethnographers have looked to the character of the previous regime for the source of disappointed expectations. The archeological revisit, however, is not unidirectional, because of necessity the ethnographer tacks back and forth between the past she or he uncovers and the present he or she interprets, rendering all sorts of new insights into both.

The archeological revisit can be used to connect the present to the past, but it may also be used to compare the present with the past. Thus Haney revised our understanding of socialist welfare by stressing its extensiveness and its flexibility. Similarly, Steven Lopez (2003) participated in labor-organizing campaigns in Pittsburgh. He asked why such campaigns were successful in one historical conjuncture but not in another. To understand the conditions of this differential success, Lopez reconstructed an earlier point in time for each campaign from interviews, archives, legal reports, and newspapers. He disentangled how obstacles to organizing were overcome (or not) as a function of both the new context and the cumulative effects of previous campaigns.

In the sometimes desperate search for historical data, the ethnographer is easily tempted to repress data's constructed character. Thus, as I alluded to earlier, historians such as

Stephan Thernstrom have been critical of how community ethnographers reduce history to the mythologies of their participants. With theoretical lenses to guide their investigations, however, ethnographers become sensitive to the constructed nature of historical narrative. Indeed, they are able to exploit its constructedness.

Reporting Back: The Valedictory Revisit

My last type of revisit is what I call the valedictory revisit, when the ethnographer returns to the subjects, armed with the results of the study, whether in draft or published form. The purpose is not to undertake another in-depth ethnography but rather to ascertain the subjects' responses to the reported research and perhaps to discover what has changed since the last visit. Assuming the subjects can be engaged, this is the moment of judgment, when previous relations are reassessed, theory is put to the test, and accounts are reevaluated. It can be traumatic for both sides, and for this reason it is all too rare.

William Foot Whyte (1955) undertook valedictory revisits to Cornerville to find out what, if anything, *Street Corner Society* had meant to the gang members. Doc, his chief informant, showed some ambivalence and embarrassment about the central role he played in the book; Chick was more upset by the way he was portrayed; and Sam Franco was inspired to do fieldwork himself. Whyte was not led to any reassessment of the study itself, for he had had a relatively smooth ride as compared, for example, with Nancy Scheper-Hughes (2001). She was drummed out of her Irish village, An Clochan, when she returned twenty-five years after her original fieldwork.

The inhabitants still remembered her. Many had not forgiven her for her portrait of their weak and vulnerable community. The hostile reception prompted her to rethink the argument in a new prologue and epilogue to her book. It was also an occasion to reflect on changes that had occurred during the intervening period—the impact of Ireland's integration into the European Union, the expansion of the tourist industry, and continued out-migration. In her case rejection by the participants led her to qualify her original interpretations but also propelled her to write an account of historical change. Her valedictory revisit borders on a focused revisit, covering all four principles of reflexivity.

Frequently, the subjects of an ethnography are simply not interested in what the ethnographer has to say until it comes to the attention of adversarial forces. Consider Diane Vaughan's (1996) historical ethnography—itself an archeological revisit that retraced the steps that led up to the *Challenger* disaster of 1986. Contesting the conventional story of human error and individual blame, she uncovered an alternative history of the National Aeronautics and Space Administration (NASA) as it descended incrementally into bad judgment and normalized design flaws. She located the cause of the disaster in the type of technology, organizational culture, and external context. Published ten years after the *Challenger* disaster, her study received much publicity but not a peep from NASA, the object of her investigation. There was no valedictory revisit to NASA until *Columbia* crashed on February 1, 2003, whereupon her *Challenger* study revisited her, and with a vengeance (Vaughan 2006). Her original diagnoses of the problem at NASA found a new lease on life among journalists, engineers, and other experts, prompting her to investigate the *Columbia* disaster. Her comparison of the two disasters figured prominently in a report of the Columbia Accident Investigation Board. Her valedictory revisit turned into a focused revisit that confirmed her earlier conclusions, much to NASA's chagrin. As in the case of Audrey Richards, which I noted earlier, ethnographies have their own history of effects—ignored at one moment, invoked at another—drawn in by the play of external forces.

It is often said that handing a finished product to the subject is the responsibility of the ethnographer. That may be so, but the valedictory revisit also serves a scientific function. This final engagement with the people one studies, confronting them with one's conclusions, deepens both constructivist and realist insights into the world we study. It may be traumatic—for both the participant and the observer—but through pain the cause of reflexive ethnography advances.

WHAT ANTHROPOLOGY CAN LEARN FROM SOCIOLOGY

The postcolonial world has driven anthropologists back to their early historical and macroperspectives, which they lost in the era of professionalization. As I have tried to argue here, in their inception these moves beyond fieldwork in time and beyond the field site in space were invariably positive. Now, however, these moves often take a self-defeating turn. As anthropologists release their subjects from conceptual confinement in their villages, the anthropologists mimic their subjects' migratory circuits. Bouncing from site to site, anthropologists easily substitute anecdotes and vignettes for serious fieldwork, reproducing the

cultural syncretism and hybridity of the peoples they observe (Hannerz 1996).

As they join their subjects in the external world, anthropologists have also all too easily lost sight of the partiality of their participation in the world they study. They begin to believe they are the world they study or that the world revolves around them. Ruth Behar's (1993) six-year dialogue with her single subject, Esperanza, fascinating though it is, brackets all concern with theoretical issues and thus fails to grapple with change in Mexican society. Behar's view of reflexivity reduces everything to the mutual orbiting of participant and observer. It dispenses with the distinction between internal and external: in the constructivist dimension, where anthropological theory is reduced to the discourse of the participant, and in the realist dimension, where there is nothing beyond "multisited" ethnography. Furthermore, the very distinction between realism and constructivism folds into an autocentric relation of ethnographer to the world.

Clifford Geertz, whose recounting of the quandary of the changing anthropologist in a changing world introduced this chapter, similarly fails to address the dilemmas of revisits, dissolving his reflections into a virtuoso display of literary images. In his hands ethnography becomes a mesmeric play of texts upon texts, narratives within narratives (Geertz 1995). By the end of its cultural turn anthropology has lost its distinctive identity, having decentered its techniques of fieldwork, sacrificed the idea of intensively studying a site, abandoned its theoretical traditions, and forsaken its pursuit of causal explanation. Theory and history evaporate in a welter of discourse. Anyone with literary ambition can now assume the anthropological mantle, making the disrupted discipline vulnerable to cavalier invasion by natives and impostors. Once a social science, anthropology aspires to become an appendage of the humanities. Although this is only one tendency within anthropology, it is significant and ascendant—a warning to ethnographer-sociologists as they emerge from their own wilderness.

As the examples in this chapter have shown, ethnographer-sociologists are following anthropologists out of seclusion—more cautiously but more surely. As I have said, within sociology ethnography has had to wrestle with a positivist legacy that was also reductionist—a tradition that reduced the external to the internal (theory induced from observation, context suspended to insulate the microsituation) at the same time that it privileged realism over constructivism (the world is purely external to us). As anthropologists veer toward the center of the universe looking out, ethnographer-sociologists are coming from the margins and looking in. Ethnographer-sociologists may be latecomers to history and theory, but therein lies their advantage. For as they leave their guarded corner they are disciplined by the vibrancy of sociology's comparative history and theoretical traditions. This dialogue within sociology, and with social science more broadly, will help the ethnographer-sociologist retain a balance between constructivism and realism. Such, indeed, are the benefits of backwardness. The ethnographer-anthropologist, on the other hand, has no such disciplinary protection and, unless new alliances are forged, faces the onrushing world alone.

The divergent orbits of ethnography in sociology and anthropology reflect the histories of our disciplines, but they are also responses to the era in which we live. The spatially

bounded site, unconnected to other sites, is a fiction of the past that is no longer sustainable. Under these circumstances what does it mean to undertake a revisit, especially a focused revisit? What is there to revisit when sites are evanescent, when all that's solid melts into air? How, for example, might I revisit Allied today—thirty years after my first encounter—if I cannot find it where I left it? One possibility, all too popular, is to simply study myself. I could trace my own research trajectory from Chicago to communist Hungary to postcommunist Russia, reflecting on the world-historical shifts since the mid-1970s. Moving beyond such solipsism, I might follow my workmates, as Jay MacLeod (1995) did with his two gangs. We might call this a biographically based revisit.[31] Or I could study the homeless recyclers who now, hypothetically, inhabit the vacant lot that used to be Allied. We might call this a place-based revisit. Or I could go off to South Korea where, again hypothetically, Allied's new engine division can be found. We can call this an institution-based revisit. These different types of revisit might all coincide if we were studying the same enclosed village or the old company town, but with globalization they diverge into three profoundly different projects. The only way of connecting them is to look upon each as a product of the same broad historical process, examining, for example, the implications of the shift in the United States from an industrial to a service economy. This could interconnect biographies of workers and their children, the redeployment of place, and the fleeing of capital to other countries.

But we can no longer stop at the national level. Today the recomposition of everyday life is also the product of transnational or supranational processes. A comprehensive revisit might involve following individual biographies, institutional trajectories, and the reconstitution of place, locating them all in regional, national, and also global transformation. Katherine Verdery (2003) conducted such a complex of nested revisits in her ethnography of decollectivization in Aurel Vlaicu—a Transylvanian village she studied under communism and then again during the postcommunist period. She followed individual kin members, specific groups (insiders and outsiders), the village land restitution committee, and different economic organizations (state farms, cooperatives, and individual production), all in relation to the transformation of property relations, which itself makes sense only within the local political economy, the national law of privatization, the conditionalities of the World Bank and the International Monetary Fund, and the global spread of market fundamentalism. With so many parts of the world dissolving, reconfiguring, and recomposing under the pressure of their global connections, ethnographic revisits with a global reach become irresistible. The more irresistible is the global revisit, however, the more necessary is theory to track and make sense of all the moving parts.

Privatization and market transition push ethnography to global extensions, which require not only theoretical frameworks for their interpretation but also historical depth. The only way to make sense of global forces, connections, and imaginations is to examine them over time. In other words, global ethnographies require focused, heuristic, serial, and, especially, archeological revisits to excavate their historical terrains (Burawoy, Blum, et al. 2000). Approaching a global ethnography of Allied today would require resituating the

company of 1973–74 in its global market, in the global connections between the engine division and other divisions, in the global imagination of its workers and managers—before I could undertake a parallel investigation. This is how June Nash (2001) turned a focused revisit into a global ethnography of the Zapatista movement. Every summer between 1988 and 1993 she returned to Chiapas—the site of her own 1957 study—with a team of students. While acknowledging the shortcomings of the descriptive anthropology extant in the 1950s, namely, the tendency to insulate communities from their determining context, she nonetheless partially recuperated that insulation as a political struggle to defend autonomy. In the early 1990s such defensive maneuvers were no longer effective. In the face of the North American Free Trade Agreement, the rollback of land reform through privatization, the erosion of subsistence agriculture, the attrition of state welfare, and the violation of human rights, Chiapas autonomy could no longer be defended by withdrawal and insulation. It required aggressive political organization and the development of an indigenous movement of national focus and global reach. Nash demonstrated that without history to ground it and theory to orient it, global ethnography is lost.

The time is nigh for the sociologist-ethnographer to come out of hiding and join the rest of sociology in novel explorations of history and theory.[32] We should not forget that Marx, Weber, and Durkheim grounded their history, as well as their theory, in an ethnographic imagination, whether of the factories of nineteenth-century England, the religious bases of economic behavior, or the rites and beliefs of small-scale societies. Michel Foucault founded his originality in a virtual ethnography of prisons and asylums. Simone de Beauvoir and her daughters set out from the privatized experiences of women, while Pierre Bourdieu launched his metatheory from the villages of Algeria. Thus not only does reflexive ethnography require the infusion of both theory and history, but theory and historical understanding will be immeasurably advanced by the conceptualization and practice of ethnography as revisit.

THREE

Two Methods in Search of Revolution

Trotsky versus Skocpol

> If methodological work—and this is naturally its intention—can at some point serve the practice of the historian directly, it is indeed by enabling him once and for all to escape the danger of being imposed upon by a philosophically embellished dilettantism.
>
> Max Weber, *The Methodology of the Social Sciences*

Sociology has founded its scientific credentials on imitating the method of the physical sciences as understood by philosophers. Regulative principles such as Mill's "canons of induction," Hempel's "deductive-nomological explanation," or Popper's falsificationism are laid down as *the* scientific method. However, these principles evolved more from philosophical speculation than from careful empirical examination of the "hard sciences" from which they derived their legitimacy. Indeed, when philosophers turned to history and the actual practice of science, they found their principles violated. New understandings of science