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Qualitative Methods

Their History in Sociology and Anthropology

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MODERN sociology has taken as its mission the analysis and understanding of the patterned conduct and social processes of society, and of the bases in values and attitudes on which individual and collective participation in social life rests. It is presupposed that, to carry out the tasks associated with this mission, the sociologist has the following:

1. The ability to perceive and contextualize the world of his or her own experience as well as the capacity to project a metaempirical conceptualization onto those contexts of life and social institutions with which he or she has not had direct experience. The sociologist requires a sensitivity to and a curiosity about both what is visible and what is not visible to immediate perception—and sufficient self-understanding to make possible an empathy with the roles and values of others.
2. The ability to detach him- or herself from the particular values and special interests of organized groups in order that he or she may gain a level of understanding that does not rest on a priori commitments. For every

individual and group, ideologies and faiths define the distinction between good and evil and lead to such nonsociological but conventional orientations as are involved in everyday judging and decision making. The sociologist's task in ethnography is not only to be a part of such thoughts and actions but also to understand them at a higher level of conceptualization.

3. A sufficient degree of social and personal distance from prevailing norms and values to be able to analyze them objectively. Usually, the ability to engage in self-objectification is sufficient to produce the quality of orientation necessary for an individual to be an ethnographic sociologist or anthropologist.

Qualitative ethnographic social research, then, entails an attitude of detachment toward society that permits the sociologist to observe the conduct of self and others, to understand the mechanisms of social processes, and to comprehend and explain why both actors and processes are as they are. The existence of this sociological attitude is presupposed in any meaningful discussion of methods appropriate to ethnographic investigation (see

Adler, Adler, & Fontana, 1991; Hammersley, 1992).

Sociology and anthropology are disciplines that, born out of concern to understand the "other," are nevertheless also committed to an understanding of the self. If, following the tenets of symbolic interactionism, we grant that the other can be understood only as part of a relationship with the self, we may suggest a different approach to ethnography and the use of qualitative methods, one that conceives the observer as possessing a self-identity that by definition is re-created in its relationship with the observed—the other, whether in another culture or that of the observer.

In its entirety, the research task requires both the act of observation and the act of communicating the analysis of these observations to others (for works describing how this is accomplished, see Johnson, 1975; Schatzman & Strauss, 1973; see also Pratt, 1986). The relationships that arise between these processes are not only the determinants of the character of the final research product, but also the arena of sociological methods least tractable to conventionalized understanding. The data gathering process can never be described in its totality because these "tales of the field" are themselves part of an ongoing social process that in its minute-by-minute and day-to-day experience defies recapitulation. To take as one's objective the making of a total description of the method of gathering data would shift the frame of ethnological reference, in effect substituting the means for the end. Such a substitution occurs when exactitude in reporting research methods takes priority over the solution to substantive sociological problems.

In fact, a description of a particular method of research usually takes place as a retrospective account, that is, a report written after the research has been completed. This all-too-often unacknowledged fact illustrates the part of the research process wherein the acts of observation are temporally separated from the description of how they were accomplished. Such essays in methodology are reconstructions of ethnographic reality; they take what was experienced originally and shrink it into a set of images that, although purporting to be a description of the actual method of research, exemplify a textbook ideal.

The point may be clarified through a comparison of the world of a supposedly "scientific" sociologist with that of such artists as painters, novelists, composers, poets, dancers, or chess masters. Viewing a painting, listening to music, reading a novel, reciting a poem, watching a chess game, or attending to the performance of a ballerina, one experiences a finished production, the "front region," as Goffman (1959, p. 107) puts it. The method seems to be inherent in the finished form (Goffman, 1949, pp. 48-77). More appropri-

ately, we might say that the method—of composing, writing, painting, performing, or whatever—is an intrinsic part of the creator's craftsmanship, without which the creation could not be made. If the artist were to be asked, "How did you do it? Tell me your method," his or her answer would require an act of *ex post facto* reconstruction: the method of describing the method. However, the original production would still retain its primordial integrity; that cannot be changed, whatever conclusions are to be drawn from later discussions about how it was accomplished. Speaking of sociological methods, Robert Nisbet (1977) recalls:

While I was engaged in exploration of some of the sources of modern sociology [it occurred to me] that none of the great themes which have provided continuing challenge and also theoretical foundation for sociologists during the last century was ever reached through anything resembling what we are to-day fond of identifying as "scientific method." I mean the kind of method, replete with appeals to statistical analysis, problem design, hypothesis, verification, replication, and theory construction, that we find described in textbooks and courses on methodology, (p. 3)

From Nisbet's pointed observation we may conclude that the method-in-use for the production of a finished sociological study is unique to that study and can be neither described nor replicated as it actually occurred. That societal investigators may choose to use different kinds of material as their data—documents for the historian, quantified reports for the demographer, or direct perception of a portion of society for the ethnographer—does not alter the fact that social scientists are observers. As observers of the world they also participate in it; therefore, they make their observations within a mediated framework, that is, a framework of symbols and cultural meanings given to them by those aspects of their life histories that they bring to the observational setting. Lurking behind each method of research is the personal equation supplied to the setting by the individual observer (Clifford, 1986). In this fundamental sense all research methods are at bottom qualitative and are, for that matter, equally objective; the use of quantitative data or mathematical procedures does not eliminate the intersubjective element that underlies social research. Objectivity resides not in a method, *per se*, but in the framing of the research problem and the willingness of the researchers to pursue that problem wherever the data and their hunches may lead (Vidich, 1955; see also Fontana, 1980; Goffman, 1974).¹ If, in this sense, all research is qualitative—because the observer is at the center of the research process—does this mean that the find-

ings produced by the method are no more than the peculiar reality of each observer (Atkinson, 1990)?

One simple answer is that we judge for ourselves on the standard of whether the work communicates or "says" something to us—that is, does it connect with our reality?² Does it provide us with insights that help to organize our own observations? Does it resonate with our image of the world? Or does it provide such a powerful incursion on the latter that we feel compelled to re-examine what we have long supposed to be true about our life world?

Or, put another way, if the method used is not the issue, by what standards are we able to judge the worth of sociological research (Gellner, 1979)? Each is free to judge the work of others and to accept it or reject it if it does not communicate something meaningful about the world; and what is meaningful for one person is not necessarily meaningful for another.

In the present and for the foreseeable future, the virtually worldwide disintegration of common values and a deconstruction of consensus-based societies evoke recognition of the fact that there exist many competing realities, and this fact poses problems not previously encountered by sociology. In effect, this situation sets up a condition wherein the number of possible theoretical perspectives from which the world, or any part of it, may be viewed sociologically is conditioned only by the number of extant scientific worldviews. As for the potential subjects of investigation, their outlooks are limited only by the many religious faiths, occupational and professional ideologies, and other *Weltanschauungen* that arise to guide or upset their lives. At the time of this writing, a new outlook on epistemology has come to the fore. It disprivileges all received discourses and makes discourse itself a topic of the sociology of knowledge.³

The history of qualitative research suggests that this has not always been the case (Douglas, 1974). In the past, the research problems for many investigators were given to them by their commitment to or against a religious faith or an ethnic creed, or by their identification with or opposition to specific national goals or socioeconomic programs. In the historical account of the use of qualitative methods that follows, we shall show that their use has been occasioned by more than the perspective of the individual observer, but also that the domain assumptions that once guided qualitative research have lost much of their force. However, the faiths, creeds, and hopes that had given focus to the work of our predecessors have not disappeared altogether from the sociologist's mental maps (Luhmann, 1986). Rather, they remain as a less-than-conscious background, the all-too-familiar furniture of the sociological mind. Milan Kundera (1988) has pointed to a central

issue in our present dilemma in *The Art of the Novel*: "But if God is gone and man is no longer the master, then who is the master? The planet is moving through the void without any master. There it is, the unbearable lightness of being" (p. 41).

Throughout all of the eras during which social science made use of observational methods, researchers have entered into their studies with problems implicitly and, in some cases, explicitly defined by hopes and faiths. Focusing on the substance of these problems and their ideational adumbrations, we shall confine our discussion of this history to the qualitative methods used by anthropologists and sociologists in ethnographic research, that is, the direct observation of the social realities by the individual observer. Our history proceeds along a continuum that begins with the first encounters of early ethnographers with the New World and ends with the practical and theoretical problems facing the work of our contemporaries.

Early Ethnography: The Discovery of the Other

Ethnos, a Greek term, denotes a people, a race or cultural group (A. D. Smith, 1989, pp. 13-18). When *ethno* as a prefix is combined with *graphic* to form the term *ethnographic*, the reference is to the subdiscipline known as descriptive anthropology—in its broadest sense, the science devoted to describing ways of life of humankind. *Ethnography*, then, refers to a social scientific description of a people and the cultural basis of their peoplehood (Peacock, 1986). Both descriptive anthropology and ethnography are thought to be atheoretical, to be concerned solely with description. However, the observations of the ethnographer are always guided by world images that determine which data are salient and which are not: An act of attention to one rather than another object reveals one dimension of the observer's value commitment, as well as his or her value-laden interests.

Early ethnography grew out of the interests of Westerners in the origins of culture and civilization and in the assumption that contemporary "primitive" peoples, those thought by Westerners to be less civilized than themselves, were, in effect, living replicas of the "great chain of being" that linked the Occident to its prehistoric beginnings (Hodgen, 1964, pp. 386-432). Such a mode of ethnography arose in the fifteenth and sixteenth centuries as a result of fundamental problems that had grown out of Columbus's and later explorers' voyages to the Western hemisphere, the so-called New World, and to the island cultures of the South Seas.

The discovery of human beings living in non-Occidental environments evoked previously unimagined cosmological difficulties for European intellectuals, who felt it necessary to integrate the new fact into the canon of received knowledge and understanding.⁴ Because the Bible, especially the book of Genesis, was taken to be the only valid source on which to rely for an understanding of the history of geography and processes of creation, and because it placed the origin of humankind in the Garden of Eden—located somewhere in what is today called the Middle East—all human beings were held to be descended from the first pair, and, later, in accordance with flood ethnography (Numbers, 1992) from the descendants of Noah and his family, the only survivors of a worldwide deluge. Linking Columbus's encounter with what we now know as the Taino, Arawak, and Carib (Keegan, 1992; Rouse, 1992) peoples in the New World to the biblical account proved to be difficult. Specifically, the existence of others outside the Christian brotherhood revealed by his "discovery" posed this question: How had the ancestors of these beings reached the Americas in pre-Columbian times? Any thesis that they had not migrated from Eurasia or Africa was held to be heresy and a claim that humankind might have arisen from more than one creative act by God.

In general, the racial and cultural diversity of peoples throughout the globe presented post-Renaissance Europeans with the problem of how to account for the origins, histories, and development of a multiplicity of races, cultures, and civilizations (see Baker, 1974; Barkan, 1992; Trinkhaus & Shipman, 1993). Not only was it necessary for the cosmologist to account for the disconcerting existence of the "other,"⁵ but such a scholar was obliged to explain how and why such differences in the moral values of Europeans and these "others" had arisen. In effect, such a profusion of values, cultures, and ways of life challenged the monopolistic claim on legitimacy and truth of the doctrines of Christianity. Such practices as infanticide, cannibalism, human sacrifice, and what at first appeared as promiscuity reopened the problem of contradictions among cultural values and the inquiry into how these contradictions might be both explained and resolved (Oakes, 1938).

These issues of value conflicts were conflated with practical questions about the recruitment, organization, and justification for the division of labor in the Spanish settlements in the Americas, and these confusions are to be found in the debates of Bartolome de Las Casas with Juan Gines de Sepulveda at the Council of Valladolid. Sepulveda, "who used Aristotle's doctrine of natural slavery in order to legitimize Spanish behavior against the Indians" (Hosle, 1992, p. 238) in effect won the day against Las Casas, who insisted

that the peoples we now call Native Americans were "full fellow human beings, possessing valid traditions, dignity and rights" (Marty, 1992, p. xiii). Today, despite or perhaps because of the new recognition of cultural diversity, the tension between universalistic and relativistic values remains an unresolved conundrum for the Western ethnographer (Hosle, 1992).⁶ In practice, it becomes this question: By which values are observations to be guided? The choices seem to be either the values of the ethnographer or the values of the observed—that is, in modern parlance, either the *etic* or the *emic* (Pike, 1967; for an excellent discussion, see Harre, 1980, pp. 135-137). Herein lies a deeper and more fundamental problem: How is it possible to understand the other when the other's values are not one's own? This problem arises to plague ethnography at a time when Western Christian values are no longer a surety of truth and, hence, no longer the benchmark from which self-confidently valid observations can be made.

Colonial Mentalities and the Persistence of the Other

Before the professionalization of ethnography, descriptions and evaluations of the races and cultures of the world were provided by Western missionaries, explorers, buccaneers, and colonial administrators. Their reports, found in church, national, and local archives throughout the world and, for the most part, not known to contemporary ethnologists, were written from the perspective of, or by the representatives of, a conquering civilization, confident in its mission to civilize the world (for pertinent discussion of this issue, see Ginsburg, 1991, 1993). Some of the seventeenth-, eighteenth-, and nineteenth-century explorers, missionaries, and administrators have provided thick descriptions of those practices of the "primitives" made salient to the observer by his Christian value perspective.⁷ For societies studied by these observers (see, for example, Degerando, 1800/1969), the author's ethnographic report is a reversed mirror image of his own ethnocultural ideal. That these early ethnographies reveal as much about the West as about their objects of study may explain why they have not been recovered and reanalyzed by contemporary anthropologists: Present-day ethnographers hope to separate themselves from the history of Western conquest and reject the earlier ethnographies as hopelessly biased (see "Symposium on Qualitative Methods," 1993). Recently they have begun to take seriously the accounts the natives have given of their Western "discoverers" and to "decenter" or

"disprivilege" the reports presented by the latter (Abeyesekere, 1992; Salmond, 1991; Todorov, 1984).

A rich resource, through which one can discern the effects that this early ethnographic literature had on the subjugation of these peoples, is to be found in the works of latter-day colonial administrators (e.g., Olivier, 1911/1970). Ethnology arose out of the reports written by administrators of the long-maintained seaborne empires of the Spanish, English, French, and Dutch (Maunier, 1949). These empires provided opportunities for amateur and, later, professional ethnologists not only to examine hosts of "native" cultures,⁸ but also to administer the conditions of life affecting the "cultural advancement" of peoples over whom their metropole exercised domination (Gray, 1911/1970, pp. 79-85). In respect to the seaborne empires, European interest was often confined to exploiting the labor power of the natives, utilizing their territory for extractive industry and/or establishing it in terms of the strategic military advantage it provided them in their struggles against imperialist rivals (for some representative examples, see Aldrich, 1990; Boxer, 1965; Duffy, 1968; Gullick, 1956; Suret-Canale, 1988a, 1988b). Hence the anthropology that developed under colonial administrators tended toward disinterest in the acculturation of the natives and encouragement for the culturally preservative effects of indirect rule. Their approach came to be called pluralistic development (M. G. Smith, 1965). Colonial pluralism left the natives more or less under the authority of their own indigenous leaders so long as these leaders could be co-opted in support of the limited interests of the colonial administration (Lugard, 1922/1965). This tendency led to the creation of a market economy at the center of colonial society (Boeke, 1946; Furnivall, 1956) surrounded by a variety of local culture groups (Boeke, 1948), some of whose members were drawn willy-nilly into the market economy and suffered the effects of marginalized identity (Sachs, 1947).

Ethnographers who conducted their field studies in colonized areas were divided with respect to their attitudes toward cultural and/or political nationalism and self-determination. A few became champions of ethnocultural liberation and anticolonial revolt. Some respected the autonomy of the traditional culture and opposed any tendency among natives in revolt against colonialism to seek further modernization of their lifestyles. The latter, some of whom were Marxists, admired the anticolonial movement but were concerned to see that the natives remained precapitalist. Some of these might have imagined that precapitalist natives would practice some form of primitive communism (see Diamond, 1963, 1972) as described by Friedrich Engels (1884) in *The Origins*

of the Family, Private Property and the State. Engels, in fact, had derived his idea of primitive communism from Lewis Henry Morgan's (1877/1964) *Ancient Society*, an original study in the Comtean ethnohistorical tradition of American aborigines that conceived of the latter as "ancestors" to the ancient Greeks (for a recent critique, see Kuper, 1988). Others, no longer concerned to prove that "mother-right" preceded "father-right" by presenting ethnographic accounts of Melanesians, Tasmanians, Bantus, or Dayaks (for a fine example, see Hartland, 1921/1969), turned their attention to acculturation, and, unsure of how long the process might take and how well the formerly colonized subjects would take to Occidental norms, reinvented "the doctrine of survivals" (Hodgen, 1936) to account for elements of the natives' culture that persisted (see, e.g., Herskovitz, 1958, 1966), or marveled at how well some native peoples had traded "new lives for old" (Mead, 1956/1975). These diverse value and ideological orientations are pervasive in the work of early professional ethnologists and provided anthropology the grounding for most of its theoretical debates.

The "Evolution" of Culture and Society: Comte and the Comparative Method

Even before the professionalization of anthropology engulfed the discipline, the enlightened ethnographer had abandoned any attitude that might be associated with that of a merciless conqueror and replaced it with that of an avatar of beneficent evolutionary progress. Value conflicts arising within anthropology from the history of colonialism, and with the moral relativism associated with them were, in part, replaced by theories of social evolution. The application of Darwinian and Spencerian principles to the understanding of how societies and cultures of the world have developed over eons freed the ethnographer from the problems presented by moral relativism; it permitted the assertion that there existed a spatiotemporal hierarchy of values. These values were represented synchronically in the varieties of cultures to be found in the world, but might be classified diachronically according to the theory of developmental advance.

This new approach to comprehending how the lifeways of the Occident related to those of the others had first been formally proposed by Auguste Comte and was soon designated the "comparative method" (Bock, 1948, pp. 11-36). According to Comte and his followers (see Lenzer, 1975), the study of the evolution of culture and civilization

would postulate three stages of culture and would hold fast to the idea that the peoples and cultures of the world are arrangeable diachronically, forming "a great chain of being" (Lovejoy, 1936/1960). Moreover, these stages are interpretable as orderly links in that chain, marking the epochs that occurred as human societies moved from conditions of primitive culture to those of modern civilization. By using technological as well as social indicators, ethnographers could discover where a particular people belonged on the "chain" and thus give that people a definite place in the evolution of culture. (For a recent discussion and critique of Comte as a theorist of history and evolution, see R. Brown, 1984, pp. 187-212.) The seemingly inconvenient fact that all of these different cultures coexisted in time—that is, the time in which the ethnographer conducted his or her field study—was disposed of by applying the theory of "uneven evolution," that is, the assertion, in the guise of an epistemological assumption, that all cultures except that of Western Europe had suffered some form of arrested development (Sanderson, 1990; Sarana, 1975). In this way, and in the absence of documentary historical materials, ethnographers could utilize their on-the-spot field studies to contribute to the construction of the prehistory of civilization and at the same time put forth a genealogy of morals. Following Comte, this diachrony of civilizational development was usually characterized as having three progressive and irreversible stages: savagery, barbarism, and civilization. The peoples assigned to each of these stages corresponded to a color-culture hierarchical diachrony and fitted the ethnocentric bias of the Occident (Nisbet, 1972).

In the nineteenth century, Comte had formalized this mode of thinking for both anthropologists and sociologists by designating as epochs of moral growth (Comte's terms) three stages that, he averred, occurred in the development of religion. The ethnologists' adaptation of Comte's comparative method to their own efforts provided them with a set of a priori assumptions on the cultures of "primitives"—assumptions that vitiated the need to grant respect to these cultures in their own terms—that is, from the perspective of those who are its participants (for a countervailing perspective, see Hill-Lubin, 1992). The imposition of a preconceived Eurocentric developmental framework made the work of the ethnographer much simpler;¹⁰ the task became that of a classifier of cultural traits in transition, or in arrest. Ultimately, this approach was institutionalized in the Human Relations Area Files (HRAF) housed at Yale University, which became the depository for an anthropological data bank and the resource for a vast project dedicated to the classification and cross-classification of virtually all the extant ethnographic literature—in the drawers of the

HRAF any and all items of culture found a secure classificatory niche (Murdock, 1949/1965). A Yale-produced handbook of categories provided the ethnographer with guidelines to direct his or her observations and provided the basis for the classification of these and other collections of cultural traits.¹¹ The trait data in the Yale cross-cultural files represent ethnography in a form disembodied from that of a lived social world in which actors still exist. They are a voluminous collection of disparate cultural items that represent the antithesis of the ethnographic method.

Twentieth-Century Ethnography: Comteanism and the Cold War

Two twentieth-century developments have undermined both the various "colonial" anthropological perspectives and evolutionary schemes. Within 30 years of the termination of World War II, the several decolonization movements in Africa and Asia succeeded in ending the direct forms of Western global colonialism. As part of the same movements, an anticolonial assault on Western ethnocentrism led to a critical attack on the idea of "the primitive" and on the entire train of ethnological thought that went with it (Montagu, 1968). In effect, by the 1960s anthropologists had begun not only to run out of "primitive" societies to study but also to abandon the evolutionary epistemology that had justified their very existence in the first place.

A new term, *underdeveloped*, tended to replace *primitive*. The colonial powers and their supporters became defendants in an academic prosecution of those who were responsible for the underdevelopment of the newly designated "Third World" and who had neglected to recognize the integrity of "black culture" and that of other peoples of color in the United States (see Willis, 1972).¹² Ethnologists discovered that their basic orientation was under attack. Insofar as that orientation had led them or their much-respected predecessors to cooperate with imperial governments in the suppression and exploitation of natives, or with the American military and its "pacification" programs in Vietnam, anthropologists began to suffer from the effects of a collective and interdisciplinary guilt complex (see Nader, 1972).¹³

Changes in what appeared to be the direction of world history led anthropologists to retool their approach to ethnography. Because, by definition, there were few, if any, primitives available for study, and because the spokespersons for the newly designated Third World of "underdeveloped" countries often held anthropologists to have contributed to the latter condition, access to tribal socie-

ties became more difficult than it had been. As opportunities for fieldwork shrank, recourse was had to the study of linguistics, to the data banks of the Yale files, or to the discovery of the ethnographic possibilities for anthropological examinations of American society. Anthropology had come full circle, having moved back to a study of its own society, the point of departure—as well as the benchmark—for its investigation of more "primitive" cultures. Linguistics and data banks lend themselves to the study of texts, as does the study of Western society, with its rich literary and historical archives. These tendencies opened ethnography to the modernist and, later, the postmodernist approaches to the study of exotic peoples and to the investigation of alien culture bearers residing within industrial societies of the Occident.

However, even as anthropology was convulsed by decolonization movements and constrained by restricted access to its traditional fieldwork sites, the Cold War gave to sociology an opportunity to revive Comte's and Spencer's variants of evolutionary doctrine in modernist form and to combine them with a secular theodicy harking back to America's Puritan beginnings.

Talcott Parsons's (1966, 1971) two-volume study of the development of society restored the Calvinist-Puritan imagery, applying the latter to those "others" not yet included in the Christian brotherhood of the Occident. Written during the decades of the U.S. global contest with the Soviet Union, it arranged selected nations and societies in a schema according to which the United States was said to have arrived at the highest stage of societal development; other peoples, cultures, and civilizations were presumed to be moving in the direction plotted by America, "the first new nation" (Lipset, 1979; for a critique, see Lyman, 1975), or to be suffering from an arrest of advancement that prevented them from doing so. That developmental scheme held to the idea that economic progress was inherent in industrialization and that nation building coincided with capitalism, the gradual extension of democratization, and the orderly provision of individual rights. Despite the pointed criticisms of the comparative method that would continue to be offered by the school of sociohistorical thought associated with Frederick J. Teggart (1941) and his followers (Bock, 1952, 1956, 1963, 1974; Hodgen, 1974; Nisbet, 1969, 1986; for a critical discussion of this school, see Lyman, 1978; see also Kuper, 1988), a Comtean outlook survived within sociology in the work of Talcott Parsons and his macro-sociological epigoni.

Social scientific literature during the Cold War included such titles as Robert Heilbroner's *The Great Ascent*, A. F. K. Organski's *The Stages of Political Development*, and W. W. Rostow's *The Stages of Economic Growth*. The American politi-

cal economy and a democratic social order replaced earlier images of the ultimate stage of cultural evolution. Changes in the rest of the nations of the world that seemed to herald movement toward adoption of an American social, political, and economic institutional structure became the standard by which social scientists could measure the "advance" of humankind. This standard provided the analyst-ethnographer with a new measure for evaluating the "progress" of the "other" (which, after 1947, included the peoples and cultures of the Soviet Union as well as those of the "underdeveloped" world). The matter reached epiphany in the early 1990s, when students and scholars of the cosmological, moral, economic, and military problems faced by claimants of the right to spread a benevolent variant of Christianized Western civilization throughout the world began to rejoice over the collapse of communism, the disintegration of the Soviet Union, and the decomposition of its allies and alliances in Eastern Europe (Gwertzman & Kaufman, 1992). But for some there arose a new apprehension: worry over whether these events signaled the very end of history itself (see Fukuyama, 1992).¹⁴

The end of the Cold War and the deconstruction of the Soviet Union revived nationalist and ethnic claims in almost every part of the world. In such a newly decentered world, cultural pluralism has become a new watchword, especially for all those who hope to distinguish themselves from ethnonational "others." The dilemmas once posed by cultural relativism have been replaced by the issues arising out of the supposed certainties of primordial descent. Ethnographers now find themselves caught in the cross fire of incommensurable but competing values.

The Ethnography of the American Indian: An Indigenous "Other"

In the United States, the Calvinist variant of the Protestant errand into the wilderness began with the arrival of the Puritans in New England. Convinced of their own righteousness and of their this-worldly mission to bring to fruition God's kingdom on the "new continent," the Puritans initially set out to include the so-called Indians in their covenant of faith. But, having misjudged both the Indians' pliability and their resistance to an alien worldview, the Puritans did not succeed in their attempt (Galloway, 1991, pp. 57-90; A. T. Vaughan, 1965). Nevertheless, they continued their missionary endeavors throughout the nineteenth and twentieth centuries (Coleman, 1985; Keller, 1983; Milner & O'Neil, 1985). American political

and jurisprudential policy toward the Indian, as well as the ethnographic work on the cultures of Native Americans, derive from this failure and shape its results. As one consequence, the several tribes of North American aborigines would remain outside the ethnographic, moral, and cultural pale of both European immigrant enclaves and settled white American communities.

From the seventeenth through the nineteenth centuries—that is, during the period of westward expansion across the American continent—ethnographic reports on Indian cultures were written from the perspective of the Euro-American conqueror and his missionary allies (Bowden, 1981). Even more than the once-enslaved Africans and their American-born descendants, the Indians have remained in a special kind of "otherness." One salient social indicator of this fact is their confinement to reservations of the mind as well as the body. In the conventional academic curriculum, the study of Native Americans is a part of the cultural anthropology of "primitive" peoples, whereas that of European and Asian immigrants and American blacks is an institutionalized feature of sociology courses on "minorities" and "race and ethnic relations."

In the United States a shift in ethnographic perspective from that written by missionaries and military conquerors to that composed exclusively by anthropologists arose with the establishment of the ethnology section of the Smithsonian Institution (Hinsley, 1981). However, ethnographies of various Indian "tribes" had been written earlier by ethnologists in service to the Bureau of Indian Affairs (BIA) (Bieder, 1989; two representative examples of pre-Smithsonian Amerindian ethnography are found in McKenney & Hall, 1836/1972; Schoolcraft, 1851/1975). In addition to being "problem peoples" for those theorists who wished to explain Indian origins in America and to construct their ancestry in terms consistent with the creation and flood myths of the Bible, the presence of the Indians within the borders of the United States posed still another problem: their anomalous status in law (R. A. Williams, 1990). Politically, the Indian "tribes" regarded themselves as separate sovereign nations and, for a period, were dealt with as such by the colonial powers and the U.S. government. However, in 1831, their legal status was redesignated in a Supreme Court case, *Cherokee Nation v. Georgia* (1831). In his decision, Chief Justice Marshall declared the Indians to occupy a unique status in law. They form, he said, "a domestic dependent nation." As such, he went on, they fell into a special "ward" relationship to the federal government. The latter had already established the Bureau of Indian Affairs to deal with them. Within the confines and constraints of this decision, the BIA administered the affairs of the Indian. From

the special brand of anthropology that it fostered, American ethnography developed its peculiar outlook on Native Americans.¹⁵

The BIA and later the Smithsonian Institution employed ethnographers to staff the various reservation agencies and to study the ways of the Indians. The focus of study for this contingent of observers was not the possible conversion of Indians, but rather the depiction of their cultures—ceremonies recorded, kinship systems mapped, technology described, artifacts collected—all carried out from a secular and administrative point of view.¹⁶ The theoretical underpinning of the BIA's perspective was the civilized/primitive dichotomy that had already designated Indians as preliterate. In effect, the tribal lands and reservation habitats of these "domestic, dependent nationals" became a living anthropological museum from which ethnologists could glean descriptions of the early stages of primitive life. In those parts of the country where Indians lived in large numbers—especially the Southwest¹⁷—and where archaeological artifacts were numerous, the Comtean evolutionary perspective was used to trace the ancestry of existing tribes back to an origin that might be found by paleontological efforts. From the beginning, however, the Southwest would also be the setting where debates—over how ethnography was to be carried out, and what purpose it ought to serve—would break out and divide anthropologists not only from missionaries and from federal agents, but from one another (Dale, 1949/1984; Dockstader, 1985).

The life world of "the primitive" was thought to be a window through which the prehistoric past could be seen, described, and understood. At its most global representation, this attitude had been given the imprimatur of ethnological science at the St. Louis World's Fair in 1904, when a scientifically minded missionary, Samuel Phillips Verner, allowed Ota Benga, a pygmy from the Belgian Congo, to be put on display as a living specimen of primitivism. A year later, Ota Benga was exhibited at the Monkey House of the Bronx Zoo (Bradford & Blume, 1992). In 1911, the American anthropologist Alfred Kroeber took possession of Ishi, the last surviving member of the Yahi tribe, and placed him in the Museum of Anthropology at the University of California. In the two years before his death, Ishi dwelled in the museum and, like Ota Benga before him, became, in effect, a living artifact, a primitive on display, one to be viewed by the civilized in a manner comparable to their perspective on the presentation of Indians in American museum dioramas (see Kroeber, 1962, 1965; for contemporary accounts in newspapers and other media, see Heizer & Kroeber, 1979).

Although U.S. Indian policy established both the programs and the perspectives under which

most ethnographers worked, its orthodoxy was not accepted by all of the early field-workers. Among these heterodoxical ethnologists, perhaps the most important was Frank Hamilton Gushing (1857-1900), who became a Zuni shaman and a war chief while working as an ethnologist for the Smithsonian Institution (see Gushing, 1920/1974, 1979, 1901/1988, 1990; see also Culin, 1922/1967).¹⁸ Gushing's case stands out because, though he was an active participant in Zuni life, he continued to be a professional ethnographer who tried to describe both Zuni culture and the Zuni worldview from an indigenous perspective. Moreover, Gushing joined with R. S. Culin in proposing the heterodoxical thesis that America was the cradle of Asia, that is, that in pre-Columbian times the ancestors of the Zuni had migrated to Asia and contributed significantly to the development of Chinese, Japanese, Korean, and other Asiatic civilizations that in turn had been diffused over the centuries into Africa and Europe (Lyman, 1979, 1982a, 1982b).

Without attempting to become a native himself, Paul Radin (1883-1959) devoted a lifetime to the ethnographic study of the Winnebago Indians (see Radin, 1927, 1927/1957a, 1937/1957b, 1920/1963, 1933/1966, 1953/1971b, 1923/1973, 1956/1976).¹⁹ Maintaining that an inner view of an alien culture could be accomplished only through a deep learning of its language and symbol system, Radin documented the myths, rituals, and poetry in Winnebago and, in his reports, provided English translations of these materials. Taking Gushing's and Radin's works as a standard for Amerindian ethnography, their perspective could be used to reinterpret the works of earlier ethnographers; they might enable future field investigators to comprehend the cultural boundedness of American Indian ethnography and at the same time provide the point of departure for a critical sociology of ethnological knowledge (Vidich, 1966). But, in addition, their work recognizes both the historicity of preliterate cultures and the problems attendant upon understanding the world of the other from the other's point of view. In this, as in the work of Thucydides and in the Weberian conception of a sociology of understanding (*verstehende* sociology), Gushing and Radin transcended the problem of value incommensurability.

The Ethnography of the Civic Other: The Ghetto, the Natural Area, and the Small Town

The Calvinist mission to save and/or include the Indian found its later counterpart in a mission to bring to the urban ghetto communities of blacks

and Asian and European immigrants the moral and communitarian values of Protestantism. That these immigrants had carried their Catholic, Judaic, or Buddhist religious cultures to the United States and that the lifestyles of the recently emancipated blacks did not accord with those of the white citizens of the United States were causes for concern among representatives of the older settled groups, who feared for the future integrity of America's Protestant civilization (Contosta, 1980, pp. 121-144; Hartmann, 1948/1967; Jones, 1992, pp. 49-166). Initially, efforts to include these groups focused on Protestant efforts to preach and practice a "social gospel" that found its institutionalization in the settlement houses that came to dot the urban landscape of immigrant and ghetto enclaves (Holden, 1922/1970; Woods & Kennedy, 1922/1990).

About three decades after the Civil War, when it became clear that the sheer number and cultural variety of the new urban inhabitants had become too great to be treated by individual efforts, recourse was had to the statistical survey. It would provide a way to determine how many inhabitants from each denomination, nationality, and race there were in any one place, and to describe each group's respective problems of adjustment (C. A. Chambers, 1971; Cohen, 1981; McClymer, 1980). In this manner, the "other" was transformed into a statistical aggregate and reported in a tabular census of exotic lifestyles. These quantified reports, sponsored in the first years by various churches in eastern cities of the United States, were the forerunners of the corporate-sponsored surveys of immigrants and Negroes and of the massive government-sponsored surveys of European, Asian, Mexican, and other immigrant laborers in 1911 (Immigration Commission, 1911/1970). The church surveys and their corporate and sociological successors were designed to facilitate the "moral reform" and social adjustment of newcomer and ghetto populations. What is now known as qualitative research in sociology had its origins in this Christian mission (see Greek, 1978, 1992).

It was out of such a movement to incorporate the alien elements within the consensual community that the first qualitative community study was carried out. W. E. B. Du Bois's (1899/1967) *The Philadelphia Negro*, a survey of that city's seventh ward, was supported by Susan B. Wharton, a leader of the University of Pennsylvania's college settlement. To Wharton, Du Bois, and their colleagues, the "collection and analysis of social facts were as much a religious as a scientific activity offered as a form of prayer for the redemption of dark-skinned people" (Vidich & Lyman, 1985, p. 128). This study, which included 5,000 interviews conducted by Du Bois, aimed not only at description, but also at the uplift of Philadelphia's Negro population by the Quaker

community that surrounded it. The tone of noblesse oblige that inspires the final pages of Du Bois's book are a stark reminder of the paternalistic benevolence underlying this first ethnographic study of a community.

Church- and corporate-sponsored survey methods continued to dominate social research until the early 1920s (see Burgess, 1916), when Helen and Robert Lynd began their study of Middletown. Robert Lynd, a newly ordained Protestant minister, was selected by the Council of Churches, then concerned about the moral state of Christian communities in industrial America, to examine the lifeways of what was thought to be a typical American community. Rather suddenly catapulted into the position of a two-person research team, the Lynds consulted the anthropologist Clark Wissler (1870-1947), then on the staff of the American Museum of Natural History,²⁰ for advice on how to conduct such a survey and how to report it once the data had been gathered. Wissler provided them with what was then known as the cultural inventory, a list of standard categories used by anthropologists to organize field data (see Wissler, 1923, chaps. 5, 7). Those categories—getting a living, making a home, training the young, using leisure, engaging in religious practices, engaging in community activities—became the organizing principle of Lynd and Lynd's (1929/1956) book and provided them with a set of cues for their investigation. Although the Middletown study was designed to provide its church sponsors with information that might be used to set church policy, the Lynds approached the Middletown community in the manner of social anthropologists. As Wissler (1929/1956) states in his foreword to the published volume of the study, "To most people anthropology is a mass of curious information about savages, and this is so far true, in that most of its observations are on the less civilized. . . . The authors of this volume have approached an American community as an anthropologist does a primitive tribe" (p. vi). In Middletown, the "other" of the anthropologist found its way into American sociological practice and purpose. Moreover, from the point of view of the policy makers in the central church bureaucracy, he who had once been assumed to be the civic "brother" had to all intents and purposes become the "other," an ordinary inhabitant of Muncie, Indiana.

Shortly after the publication of *Middletown* in 1929, the Great Depression set in. Soon, the Lynds were commissioned to do a restudy of Muncie. Published in 1937 as *Middletown in Transition: A Study in Cultural Conflicts*, this investigation reflected not only changes in the town, but also a transformation in the outlook of its two ethnographers. During the early years of the Depression, Robert Lynd, a church progressive, had begun to look to the Soviet Union for answers to the glaring

contradictions of capitalism that seemed to have manifested themselves so alarmingly in Depression-ridden America. This new political orientation was reflected in both what the Lynds observed and how they reported it. Where the first volume had made no mention of the Ball family's domination of what was a virtual "company town," or of the family's philanthropic sponsorship of Ball State University and the local library and hospital, or its control over the banks, *Middletown in Transition* included a chapter titled "The X Family: A Pattern of Business-Class Control," and an appendix titled "Middletown's Banking Institutions in Boom and Depression." Responding to what they believed to be the utter failure of America's laissez-faire, free market economy, the Lynds abandoned the ethnographic categories they had used in *Middletown*. Choosing instead to employ categories and conceptualizations derived from their own recently acquired Marxist outlook, they shifted the sociological focus from religious to political values.

Middletown in Transition would become a standard and much-praised work of sociological ethnography for the next half century. At Columbia University, where Robert Lynd taught generations of students, explicit Christian values and rhetoric were replaced by those of an ethically inclined political radicalism. With the radicalization of many Columbia-trained youths (as well as of their fellow students at City College, many of whom would later become prominent sociologists), variants of Marxism would provide a counterperspective to that of the anthropologically oriented ethnographic observer of American communities. Ironically, however, Middletown's second restudy, conducted by a team of non-Marxist sociologists nearly 50 years after *Middletown in Transition* was published, returned the focus to the significance of kinship and family that had characterized the early anthropological perspective, combining it with the kind of concern for Protestant religiosity that had been the stock-in-trade of the earlier American sociological orientation (Caplow, Bahr, Chadwick, Hill, & Williamson, 1982, 1983).

Even before the Lynds' original study, ethnography as a method of research had become identified with the University of Chicago's Department of Sociology. The first generation of Chicago sociologists, led by Albion W. Small, supposed that the discipline they professed had pledged itself to reassert America's destiny—the nation that would be "the city upon a hill." America would become a unified Christian brotherhood, committed to a covenant through which the right and proper values would be shared by all (Vidich & Lyman, 1985, p. 179). Small sought a sociological means to impress the values and morals of Protestantism upon the inhabitants of the newer

ethnic, racial, and religious ghettos then forming in Chicago. However, this explicitly Christian attitude—in service to which the University of Chicago had been brought into existence by John D. Rockefeller in 1892—did not survive at Chicago. It was discarded after Robert E. Park, Ernest W. Burgess, W. I. Thomas, and Louis Wirth had become the guiding professoriat of Chicago's sociology, and after Park's son-in-law, Robert Redfield, had become an important figure in that university's anthropology program. Park's secular conceptualization of the "natural area" replaced the Christian locus of the unchurched in the city, while, at the same time, and in contradistinction to Park's point of view, Redfield's formulation of the morally uplifting "little community" introduced a counterimage to that of the metropolis then emerging in Chicago.

Park (1925/1967) conceived the city to be a social laboratory containing a diversity and heterogeneity of peoples, lifestyles, and competing and contrasting worldviews. To Park, for a city to be composed of others, ghettoized or otherwise, was intrinsic to its nature. Under his and Ernest W. Burgess's direction or inspiration, a set of ethnographic studies emerged focusing on singular descriptions of one or another aspect of human life that was to be found in the city. Frequently, these studies examined urban groups whose ways of life were below or outside the purview of the respectable middle classes. In addition to providing descriptions of the myriad and frequently incompatible values by which these groups lived, these ethnographies moved away from the missionary endeavor that had characterized earlier studies. Instead, Park and his colleagues occupied themselves with documenting the various forms of civil otherhood that they perceived to be emerging in the city (see Burgess & Bogue, 1967).

Central to Park's vision of the city was its architectonic as a municipal circumscription of a number of "natural areas," forming a mosaic of minor communities, each strikingly different from the other, but each more or less typical of its kind. Park (1952a) observed, "Every American city has its slums; its ghettos; its immigrant colonies, regions which maintain more or less alien and exotic cultures. Nearly every large city has its bohemia and hobohemia, where life is freer, more adventurous and lonely than it is elsewhere. These are called natural areas of the city" (p. 196). For more than three decades, urban ethnography in Chicago's sociology department focused on describing such "natural areas" as the Jewish ghetto (Wirth, 1928/1956), Little Italy (Nelli, 1970), Polonia (Lopata, 1967; Thomas & Znaniecki, 1958, pp. 1511-1646), Little Germany (Park, 1922/1971), Chinatown (Lee, 1978; Siu, 1987; Wu, 1926), Bronzeville and Harlem (Drake & Cayton, 1962; Frazier, 1931, 1937a, 1937b), the gold coast

and the slum (Zorbaugh, 1929), hobo jungles (N. Anderson, 1923/1961), single-room occupants of furnished rooms (Zorbaugh, 1968), enclaves of cultural and social dissidents (Ware, 1935/1965),²¹ the urban ecology of gangdom (Thrasher, 1927/1963), and the urban areas that housed the suicidal (Cavan, 1928/1965), the drug addicted (Dai, 1937/1970), and the mentally disturbed (Paris & Dunham, 1939/1965), and on the social and economic dynamics of real estate transactions and the human and metropolitical effects arising out of the occupational interests of realtors as they interfaced with the state of the economy (Hughes, 1928; McCluer, 1928; Schietinger, 1967). Park's (1952b, 1952c) orientation was that of Montesquieu; he emphasized the freedom that the city afforded to those who would partake of the "romance" and "magic" of its sociocultural multiverse.

Some of Park's students, on the other hand, following up an idea developed by Louis Wirth (1938), all too often took to contrasting its forms of liberty in thought and action—that is, its encouragement of "segmented" personalities and role-specific conduct and its fostering of impersonality, secondary relationships, and a blase attitude (see Roper, 1935, abstracted in Burgess & Bogue, 1967, pp. 231-244)—with what they alleged was the sense of personal security—that is, the gratification that came from conformity to custom, the comfort that arose out of familiar face-to-face contacts, the wholesomeness of whole personalities, and the companionability of primary relationships—to be found among the people who dwelt in rural, ethnoracially homogeneous small towns (see Bender, 1978, pp. 3-27; Redfield & Singer, 1973; see also M. P. Smith, 1979). For those who idealized the "folk society," and who conflated it with concomitant idealizations of the "little community," "primitive" primordialism, pastoral peace, and the small town, the impending urbanization of the countryside—heralded by the building of highways (Dansereau, 1961; McKenzie, 1968), the well-documented trend of young people departing to the city (for early documentation of this phenomenon, see Weber, 1899/1967), and the intrusion of the automobile (Bailey, 1988; Rae, 1965), the telephone (Ball, 1968; de SolaPoole, 1981), and the radio (Gist & Halbert, 1947, pp. 128, 505-507) on rural folkways—was a portent not merely of change but of irredeemable tragedy (see Blake, 1990; Gusfield, 1975; Lingeman, 1980; Tinder, 1980). On the other hand, for those ethnographers who concluded on the basis of their own field experiences that the processes as well as the anomalies of America's inequitable class structure had already found their way into and become deeply embedded within the language and customs of the nation's small towns, there was an equally portentous observation: America's Jeffersonian ideals

were professed but not practiced in the very communities that had been alleged to be their secure repository. As August B. Hollingshead (1949/1961) would point out on the basis of his ethnographic study of "Elmtown's youth": "The . . . American class system is extra-legal . . . [but] society has other dimensions than those recognized in law It is the culture which makes men face toward the facts of the class system and away from the ideals of the American creed" (pp. 448, 453).

Ethnographic studies that followed in this tradition were guided by a nostalgia for nineteenth-century small-town values, an American past that no longer existed, but during the heyday of which—so it was supposed—there had existed a society in which all had been brothers and sisters.

However, neither the civil otherhood conceived by Park nor the classless brotherhood sought by Hollingshead could account for American society's resistance to the incorporation of blacks. It was to address this point that E. Franklin Frazier (1894-1962) would stress the "otherhood" of the American Negro. Building on the teachings of both Park and Du Bois, Frazier began his sociological studies in Chicago with an analysis of the various lifeways within the black ghetto. In the process, he discovered both the ghetto's separateness and its isolation from the larger social and political economy. In his later evaluation of the rise of the "black bourgeoisie" (1957a) he saw it as a tragic, although perhaps inevitable, outcome of the limited economic and social mobility available to the black middle classes. Based on his observations of largely university-based black middle classes, Frazier presented their lifestyle as an emulation of the lifestyle of the white middle classes: as such, his monograph on the subject should be regarded as much as a study of the white bourgeoisie as of the black. Frazier's ethnographic studies were based on almost a lifetime of observation, not only of this specific class, but also of African American ghetto dwellers in Harlem and Chicago, of black families in the rural South and the urban North, and of Negro youths caught up in the problems of their socioeconomic situation (see Frazier, 1925, 1957b, 1963, 1939/1966, 1940/1967, 1968). Frazier's work stands apart, not only because it points to the exclusion of blacks from both the American ideal of brotherhood and the then-emerging civic otherhood, but also because its research orientation drew on the life histories of his subjects and on his own experience.

The importance of personal experience in ethnographic description and interpretation is implicit in all of Frazier's work. His methodology and chosen research sites are comparable to those employed by a very different kind of ethnographer—Thorstein Veblen. In such studies of American university ghettos as *The Higher Learning in America: A Memorandum on the Conduct of Uni-*

versities by Businessmen, Veblen (1918/1965) drew on his own experiences at the University of Chicago, Stanford University, and the University of Missouri, three sites that provided the raw materials for his highly organized and prescient examination of the bureaucratic transformations then occurring in American universities.²² Frazier's and Veblen's oeuvres are, in effect, examples of qualitative research based on data acquired over the course of rich and varied life experiences. In these studies it is impossible to disentangle the method of study from either the theory employed or the person employing it. Such a method would appear to be the ultimate desideratum of ethnographic research.

The ethnographic orientation at the University of Chicago was given a new twist by William Foote Whyte. Whyte made what was designed to be formal research into part of his life experience and called it "participant observation." The Chicago Sociology Department provided Whyte with an opportunity to report, in *Street Corner Society* (1943a, 1955, 1981), his findings about Italian Americans residing in the North End of Boston. That work, initially motivated by a sense of moral responsibility to uplift the slum-dwelling masses, has become the exemplar of the techniques appropriate to participant observation research: Whyte lived in the Italian neighborhood and in many but not all ways became one of the "Cornerville" boys.²³ Although he presents his findings about Cornerville descriptively, Whyte's theoretical stance remains implicit. The book has an enigmatic quality, because Whyte presents his data from the perspective of his relationships with his subjects. That is, Whyte is as much a researcher as he is a subject in his own book; the other had become the brother of Italian ghetto dwellers.

Anthropology at the University of Chicago was also informed by a qualitative orientation. Until 1929, anthropology and ethnology at that university had been subsumed under "historical sociology" in a department called the Department of Social Science and Anthropology. Anthropological and ethnological studies were at first directed by Frederick A. Starr, formerly head of ethnology at the American Museum of Natural History (Diner, 1975). Starr became a Japanophile after his first trip to Japan, while he was on assignment to bring a few of the Ainu people to be displayed, like Ota Benga, at the St. Louis World's Fair in 1904 (Statler, 1983, pp. 237-255). A separate Department of Anthropology was established in 1929, but, unlike Starr's, it reflected the orientation developed by the sociologists W. I. Thomas and Ellsworth Paris (see Paris, 1970, p. 16). One year before the advent of the new department, Robert Redfield presented his dissertation, *A Plan for the Study of Tepoztlan, Mexico* (1928). Borrowing from Tonnies's (1887/1957) dichotomous para-

digm, *gemeinschaft-gesellschaft*, and drawing upon VonWiese's and Becker's (1950/1962, 1932/1974) sacred-secular continuum, Redfield asserted the virtues of "the folk culture" and what he would later call "the little community" (Redfield, 1962, pp. H3-144; see also Redfield, 1930, 1941, 1960, 1950/1962b; Redfield & Rojas, 1934/1962a).

Regarding the metropolis as a congeries of unhappy and unfulfilled others, Redfield stood opposed to the values associated with urban life and industrial civilization. He extolled the lifestyles of those nonindustrial peoples and small communities that had resisted incorporation into the globally emerging metropolitan world. In his final essay, written in 1958, the year of his death, describing an imaginary conversation with a man from outer space, Redfield (1963) abjured the condition of mutually assured destruction that characterized the Cold War, despaired of halting the march of technocentric progress, conflated the pastoral with the premodern, and concluded by lamenting the rise of noncommunal life in the metropolitan city. Redfield's orientation, Rousseauian in its ethos, would provide a generation of anthropologists with a rustic outlook—a post-missionary attitude that sought to preserve and protect the lifeways of the primitive. His was the antiurban variant of Puritanism, a point of view that held small-scale, face-to-face communities to be superior to all others. To those ethnologists who followed in the ideological footsteps of Redfield, these communal values seemed representative of primordial humanity.²⁴

A counterimage to that of ethnography's romance with small-town, communitarian and primordial values of primitivism was offered in 1958 when Arthur J. Vidich and Joseph Bensman published their ethnographic account of "Springdale," a rural community in Upstate New York.²⁵ As their title forewarned, this was a "small town in mass society."²⁶ Its situation, moreover, was typical of other American towns. Springdale's much-vaunted localism, its claims to societal, economic, and political autonomy, were illusions of a bygone era. Their "central concern," the authors observed in their introduction to a revised edition released 10 years after the original publication of their monograph, "was with the processes by which the small town (and indirectly all segments of American society) are continuously and increasingly drawn into the central machinery, processes and dynamics of the total society" (Vidich & Bensman, 1968, p. xi).

In so presenting their findings, Vidich and Bensman reversed the direction and exploded what was left of the mythology attendant upon the *gemeinschaft-gesellschaft* (Parsons, 1937/1949, 1973) and folk-urban continua in American sociological thought (Duncan, 1957; Firey, Loomis, & Beegle, 1950; Miner, 1952). Although the theo-

retical significance of their study was often neglected in the wake of the controversy that arose over its publication and the charge that they had not done enough to conceal the identities of the town's leading citizens (Vidich & Bensman, 1968, pp. 397-476), their concluding observations—namely, that there had occurred a middle-class revolution in America, that the rise and predominance of the new middle classes had altered the character and culture of both the cities and towns of America, and that "governmental, business, religious and educational super-bureaucracies far distant from the rural town formulate policies to which the rural world can respond only with resentment" (p. 323; see also Bensman & Vidich, 1987)—challenged the older paradigms guiding field research on community life.

By 1963, Roland L. Warren would take note of what he called "the 'great change' in American communities" and point out how a developing division of labor, the increasing differentiation of interests and associations, the growing systemic relations to the larger society, a transfer of local functions to profit enterprises and to state and federal governments, urbanization and suburbanization, and the shifts in values that were both cause and consequences of these changes had been accompanied by a "corresponding decline in community cohesion and autonomy" (see Warren, 1972, pp. 53-94). In effect, community ethnography would not only have to adjust to the encroachment of the city and the suburb on the town, but also enlarge its outlook to embrace the effects of the state and the national political economy on the towns and villages of the Third World as well as of the United States (see, e.g., the ethnographies collected in Toland, 1993; see also Marcus, 1986). ("The point is," Maurice Stein [1964, p. 230] observed in his reflection on nearly six decades of American community studies, "that both the student of the slum and of the suburb [and, he might have added, the small town] require some sort of total picture of the evolution of American communities and of emerging constellations and converging problems"; p. 230. Had the practitioners of American community studies taken their point of departure from Otto von Gierke's, 1868/1990, or Friedrich Ratzel's, 1876/1988, orientations, they might have been more critical of the "Rousseauian" variant of Tonnies's outlook from the beginning of their research. [See McKinney, 1957])²⁷

The Ethnography of Assimilation: The Other Remains an Other

A breakdown in another fundamental paradigm affected the ethnographic study of ethnic and racial minorities. Until the 1960s, much of the

sociological outlook on race and ethnic relations had focused on the processes and progress of assimilation, acculturation, and amalgamation among America's multiverse of peoples. Guided by the cluster of ideas and notions surrounding the ideology of the "melting pot," as well as by the prediction of the eventual assimilation of everyone that accompanied the widely held understanding of Robert E. Park's theory of the racial cycle, ethnographers of America's many minority groups at first sought to chart each people's location on a continuum that began with "contact," passed consecutively through stages of "competition and conflict" and "accommodation," and eventually culminated in "assimilation" (for critical evaluations of Park's cycle, see Lyman, 1972, 1990b, 1992b). Although by 1937 Park had come to despair of his earlier assertion that the cycle was progressive and irreversible (see Park, 1937/1969b), his students and followers would not give up their quest for a pattern and process that promised to bring an ultimate and beneficent end to interracial relations and their attendant problems.

When the ethnic histories of particular peoples in the United States seemed to defy the unidirectional movement entailed in Park's projected sequence—for example, when Etzioni's (1959) re-study of the Jewish ghetto showed little evidence that either religion or custom would be obliterated, even after many years of settlement in America; when Lee's (1960) discovery that Chinatowns and their old world-centered institutions persisted despite a decline in Sinophobic prejudices; when Woods's (1972) careful depiction of how 10 generations of settlement in America had failed to erode either the traditions or the ethnoracial identity of a marginalized people, the Letoyant Creoles of Louisiana (see also Woods, 1956); and, more generally, when Kramer (1970) had documented the many variations in minority community adaptation in America—there arose a cacophony of voices lamenting the failure of assimilation and calling for a resurgence of WASP hegemony (Brookhiser, 1991, 1993), or expressing grave apprehension about America's ethnocultural future (Christopher, 1989; Schlesinger, 1991; Schrag, 1973).

Even before popularizers and publicists announced the coming of an era in which there would be a "decline of the WASP" (Schrag, 1970) and a rise of the "unmeltable ethnics" (Novak, 1972), some sociologists had begun to reexamine their assumptions about ethnicity in America and to rethink their own and their predecessors' findings on the matter. In 1952, Nathan Glazer caused Marcus Lee Hansen's (1938/1952) hitherto overlooked work on the "law of third generation return" to be republished,²⁸ sparking a renewed interest in documenting whether, how, and to what extent the grandchildren of immigrants re-

tained, reintroduced, rediscovered, or invented the customs of their old-world forebears in modern America (Kivisto & Blanck, 1990). Stanford M. Lyman (1974, 1986) combined participant observation with documentary and historical analyses to show that the solidarity and persistence over time of territorially based Chinatowns was related in great measure to persistent intracommunity conflict and to the web of traditional group affiliations that engendered both loyalty and altercation. Kramer and Leventman (1961) provided a picture of conflict resolution among three generations of American Jews who had retained many but not all aspects of their ethnoreligious traditions despite, or perhaps because of, the fact that the third generation had become "children of the gilded ghetto." Richard Alba (1985, 1989, 1990) reopened the questions of whether and how European ethnic survival had occurred in the United States, pointing to the several dimensions of its presentation, representation, and disintegration, and carrying out, once more, a study of Italian Americans, a group often chosen by sociologists for ethnographic studies seeking to support, oppose, modify, or reformulate the original assimilation thesis (see, e.g., Covello, 1967; Cans, 1962; Garbaccia, 1984; Landesco, 1968; Lopreato, 1970; Tricarico, 1984; Whyte, 1943a, 1943b).

The reconsideration of assimilation theory in general and Park's race relations cycle in particular produced a methodological critique so telling that it cast doubt on the substance of that hypothesis. In 1950, Seymour Martin Lipset observed that "by their very nature, hypotheses about the inevitability of cycles, whether they be cycles of race relations or the rise and fall of civilization, are not testable at all" (p. 479). Earlier, some ethnographers of racial minority groups in America had attempted to construct lengthier or alternative cycles that would be able to accommodate the findings of their field investigations. Bogardus's (1930, 1940; Ross & Bogardus, 1940) three distinctive cycles for California's diversified Japanese communities and Masuoka's (1946) warning that three generations would be required for the acculturation of Japanese in America and that the third generation would still be victims of "a genuine race problem" evidence the growing disappointment with assimilation's promise. Others, including W. O. Brown (1934), Clarence E. Click (1955), Stanley Lieberman (1961), and Graham C. Kinloch (1974, pp. 205-209) came to conclusions similar to that of Park's 1937 reformulation—namely, that assimilation was but one possible outcome of sustained interracial contact, and that isolation, subordination, nationalist or nativist movements, and secession ought also to be considered.

Those seeking to rescue the discredited determinism of Park's original cycle from its empirically minded critics turned to policy proposals or

hortatory appeals in its behalf. Wirth (1945) urged the adoption of programs that would alleviate the frustration experienced by members of minority groups who had been repeatedly rebuffed in their attempts to be incorporated within a democratic America; Lee (1960, pp. 429-430) converted her uncritical adherence to Park's prophecy into a plaintive plea that Chinese ghetto dwellers live up to it—that is, that they assimilate themselves as rapidly as possible (see also Lyman, 1961-1962, 1963). Still others resolved the ontological and epistemological problems in Park's cycle by treating it as a "logical" rather than "empirical" perspective. Frazier (1953) suggested that, rather than occurring chronologically, the stages in the theory might be spatiotemporally coexistent: "They represent logical steps in a systematic sociological analysis of the subject." Shibutani and Kwan (1965), after examining the many studies of integrative and disintegrative social processes in racial and ethnic communities, concurred, holding that although there were many exceptions to its validity as a descriptive theory, Park's stages provided a "useful way of ordering data on the manner in which immigrants become incorporated into an already-established society" (see pp. 116-135). Geschwender (1978) went further, holding that Park's race relations cycle was "an abstract model of an 'ideal type' sequence which might develop" (p. 25).

In 1918, Edward Byron Reuter had defined America's race issue as "the problem of arriving at and maintaining mutually satisfactory working relations between members of two nonassimilable groups which occupy the same territory" (Reuter, 1918/1969, p. 18). After a half century of sociological studies had seemed to demonstrate that virtually none of the racial or ethnic groups had traversed the cyclical pathway to complete assimilation, America's race problem seemed not only to be immense, but also to have defied as well as defined the basic problematic of sociological theory. Such, at any rate, was the position taken by the ethnological anthropologist Brewton Berry (1963), whose field investigations would eventually include studies of various peoples in Latin America as well as several communities of previously unabsorbed racial hybrids in the United States (see also Lyman, 1964). Having shown that none of the proposed cycles of race relations could claim universal validity on the basis of available evidence, Berry and Tischler (1978) observed, "Some scholars . . . question the existence of any universal pattern, and incline rather to the belief that so numerous and so various are the components that enter into race relations that each situation is unique, and [that] the making of generalizations is a hazardous procedure" (p. 156). Berry's thesis, though not necessarily intended in this direction, set the tone for the subsequent

plethora of ethnographies that offered little in the way of theoretical advancements but much more of the detail of everyday life among minorities and other human groups.

During the two decades after 1970, ethnological studies of African American, Amerindian, Mexican American, and Asian peoples also cast considerable doubt on whether, when, and to whose benefit the much-vaunted process of ethnocultural meltdown in America would occur. Ethnographies and linguistic studies of black enclaves, North and South, slave and free, suggested that the tools employed in earlier community analyses had not been honed sufficiently for sociologists to be able to discern the cultural styles and social practices that set African American life apart from that of other segments of the society (see, e.g., Abrahams, 1964, 1970, 1992; E. Anderson, 1978; Bigham, 1987; Blassingame, 1979; Duneier, 1992; Evans & Lee, 1990; Joyner, 1984; Liebow, 1967; for an overview, see Blackwell, 1991). Other critics observed that sociological studies of the "American dilemma" had paid insufficient attention to politics, civil rights, and history (Boxhill, 1992; Button, 1989; Jackson, 1991; Lyman, 1972; V. J. Williams, 1989). Anthropological studies of the culture-preserving and supposedly isolated Native American nations and tribes had to give way in the face of a rising ethnoracial consciousness (Cornell, 1988; Martin, 1987; Sando, 1992), selective demands for the return of Amerindian museum holdings (Berlo, 1992; Clifford, 1990; Messenger, 1991; Milson, 1991-1992; "A Museum Is Set," 1993), Indian recourse to American courts in quest of redress and treaty rights (see T. L. Anderson, 1992; Jaimes, 1992), and political alliances and the tracing of ethnohistorical descent that would connect Amerindians with Hispanics, African Americans, and Jews (Forbes, 1973, 1988; Gutierrez, 1991; Tobias, 1990; Vigil, 1980). Mexican American studies moved from early historical institutional studies through ethnographies of farmworkers, and in the 1980s became part of the new postmodernist revolution.²⁹ To the Amerasian peoples conventionally treated by ethnographic sociologists—namely, the Chinese and Japanese—were added more recent arrivals, including Koreans, Thais, Vietnamese, Cambodians, Laotians, and the Hmong (see, e.g., Chan, 1991; Hune et al., 1991; Knoll, 1982; Nomura et al., 1989; Okihiro et al., 1988; Takaki, 1989). And, as in the instance of Mexican American ethnographers, a shift in issues and methods is beginning to emerge—moving away from debates about whether and how to measure assimilation and acculturation and toward such postmodern topics as the character, content, and implications of racial discourse about Asians in America (e.g., K. J. Anderson, 1991; Okihiro, 1988). As East Indians, Burmese, Oceanians, Malaysians, and

other peoples of what used to be called "the Orient" began to claim common cause with the earlier-established Asian groups (Espiritu, 1992; Ignacio, 1976; Mangiafico, 1988), but insisted on each people's sociocultural and historical integrity, as well as the right of each to choose its own path within U.S. society, it became clear that the trend toward ethnographic postmodernism would continue (see, e.g., Hune et al., 1991; Leonard, 1992).

In 1980, Harvard University Press issued its mammoth *Harvard Encyclopedia of American Ethnic Groups* (Thernstrom, 1980), a work that includes not only separate entries for "Africans" and "Afro-Americans" but also individual essays devoted to each of 173 different tribes of American Indians and reports on each of the Asian peoples coming to the United States from virtually all the lands east of Suez. Harold J. Abrahamson's entry, "Assimilation and Pluralism," in effect announces American sociology's awakening not only from its dream of the eventual assimilation of every people in the country, but also from its conflation of assimilation with Americanization: "American society . . . is revealed as a composite not only of many ethnic backgrounds but also of many different ethnic responses. . . . There is no one single response or adaptation. The variety of styles in pluralism and assimilation suggest that ethnicity is as complex as life itself (p. 160; see also Gleason, 1980; Novak, 1980; Walzer, 1980).

For the moment, pluralism had won its way onto paradigmatic center stage.³⁰ But even that orientation did not exhaust the possibilities or dispose of the problems arising out of the presence of diverse races and peoples in America. In 1993, together with Rita Jalali, Seymour Martin Lipset, who had criticized Park's formulation of an inevitable cycle leading to assimilation four decades earlier, observed that "race and ethnicity provide the most striking example of a general failure among experts to anticipate social developments in varying types of societies" (Jalali & Lipset, 1992-1993, p. 585). Moreover, the celebration of pluralism that now prevails in social thought obscures recognition of a fundamental problem: the self-restraint to be placed upon the competitive claims put forward by each ethnic and racial group.

Ethnography Now: The Postmodern Challenge

Historically, the ethnographic method has been used by both anthropologists and sociologists. The guiding frameworks for those who have used

this method in the past have all but been abandoned by contemporary ethnographers. The social-historical transformations of society and consciousness in the modern world have undermined the theoretical and value foundations of the older ethnography.

With the present abandonment of virtually every facet of what might now be recognized as the interlocked, secular, eschatological legacies of Comte, Tonnies, Wissler, Redfield, Park, and Parsons—that is, the recognition that the "comparative method" and the anthropology of primitivism is inherently flawed by both its Eurocentric bias and its methodological inadequacies; the determination that the *gemeinschaft* of the little community has been subverted by the overwhelming force of the national political economy of the *gesellschaft*; the discovery that assimilation is not inevitable; and the realization that ethnic sodalities and the ghettos persist over long periods of time (sometimes combining deeply embedded internal disharmonies with an outward display of sociocultural solidarity, other times existing as "ghost nations," or as hollow shells of claimed ethnocultural distinctiveness masking an acculturation that has already eroded whatever elementary forms of existence gave primordial validity to that claim, or, finally, as semiarticulated assertions of a peoplehood that has moved through and "beyond the melting pot" without having been fully dissolved in its fiery cauldron)—ethnography and ethnology could emerge on their own terms.³¹

No longer would ethnography have to serve the interests of a theory of progress that pointed toward the breakup of every ethnos. No longer would ethnology have to describe the pastoral peacefulness, proclaim the moral superiority, or document the psychic security supposed to be found in the villages of the nonliterate, the folk societies of non-Western peoples, the little communities of the woods and forests, the small towns of America, or the urban ethnic enclaves of U.S. or world metropolises. No longer would ethnography have to chart the exact position of each traditional and ascriptively based status group as it moved down the socioculturally determined pathway that would eventually take it into a mass, class, or civil society, and recompose it in the process.

Liberated from these conceptual and theoretical constraints, ethnography and ethnology are, for the first time as it were, in a position to act out their own versions of the revolution of "life" against "the forms of life"—a cultural revolution of the twentieth century that Simmel (1968) foresaw as both imminent and tragic. Just as Simmel predicted that the cultural revolutionaries that he saw emerging in pre-World War I Europe would oppose both marriage and prostitution on the

grounds that each was a form of the erotic and that they wished to emancipate the erotic from all forms of itself, so the new ethnographers proclaim themselves to be self-liberated from the weight of historical consciousness, relieved of the anxiety of influence (see Bloom, 1979),³² and, in effect, content to become witnesses to and reporters of the myriad scenes in the quixotic world that has emerged out of the ruins of both religion and secular social theory (see Kundera, 1988).

The proclamation of ethnography as a self-defining orientation and practice in sociology and anthropology and the importation of the postmodernist outlook into it took place recently, irregularly, and in somewhat disorderly moves. Aleksandr Solzhenitsyn (1993) once pointed out that "no new work of art comes into existence (whether consciously or unconsciously) without an organic link to what was created earlier" (p. 3). Such also remains the case in social science, as will be shown with the new developments in sociological and anthropological ethnography.

One beginning of the emancipatory movement in ethnographic methodology is to be found in Peter Manning's seminal essay, "Analytic Induction" (1982/1991). Seeking to set ethnography on an even firmer foundation of the symbolic interactionist perspective and hoping to reinforce its connections to the classical period of the "Chicago school," Manning sought first to warn any practitioners of the sociological enterprise against employing any "concepts and theories developed to deal with the problems of such other disciplines as behavioristic psychology, economics, medicine, or the natural or physical sciences." He identified analytic induction as a procedure derivable from George Herbert Mead's and Florian Znaniecki's writings on scientific method, and he observed that it had been employed with greater or lesser precision by such classical Chicago ethnographers as Thomas and Znaniecki, and, later, by Robert Cooley Angell, Alfred Lindesmith, and Donald Cressey. Distinguishable from deductive, historical-documentary, and statistical approaches, analytic induction was "a nonexperimental qualitative sociological method that employs an exhaustive examination of cases in order to prove universal, causal generalizations." The case method was to be the critical foundation of a revitalized qualitative sociology.

The claim to universality of the causal generalizations is—in the example offered by Manning as exemplary of the method³³—the weakest, for it is derived from the examination of a single case studied in light of a preformulated hypothesis that might be reformulated if the hypothesis does not fit the facts. And "practical certainty" of the (reformulated) hypothesis is obtained "after a small number of cases has been examined." Discovery of a single negative case is held to disprove the

hypothesis and to require its reformulation. After "certainty" has been attained, "for purposes of proof, cases outside the area circumscribed by the definition are examined to determine whether or not the final hypothesis applies to them." If it does, it is implied, there is something wrong with the hypothesis, for "scientific generalizations consist of descriptions of conditions which are always present when the phenomenon is present but which are never present when the phenomenon is absent." The two keys to the entire procedure, Manning points out, are the definition of the phenomenon under investigation and the formulation of the tentative hypothesis. Ultimately, however, as Manning concedes, despite its aim, analytic induction does not live up to the scientific demand that its theories "understand, predict, and control events." After a careful and thoroughgoing critique of the procedure he has chosen over its methodological competitors, Manning asserts, "Analytic induction is not a means of prediction; it does not clearly establish causality; and it probably cannot endure a principled examination of its claims to [be] making universal statements." Indeed, Manning goes further, pointing out that, "according to the most demanding ideal standards of the discipline, analytic induction as a distinctive, philosophical, methodological perspective is less powerful than either enumerative induction or axiomatic-modelling methods." Manning's essay seems about to eject a method intrinsic to ethnography from the scientific community.

Manning's frank appraisal of the weaknesses of analytic induction is "drawn from a positivistic, deductive model of the scientific endeavor, a model seizing on a selected group of concerns." The proponents of that model seek to set the terms and limits of the social sciences according to its criteria. In fact, though few American scholars seem to know much about either the long history or the irresolution of debates over epistemological matters in the social sciences, the very issues of those debates are central to the questions the positivists are raising (see, in this regard, Rorty, 1982, pp. 191-210).

In his defense of analytic induction, Manning invokes an unacknowledged earlier critique by Sorokin (1965), namely, "that what is taken to be [appropriate] methodology at a given time is subject to fads, fashions, and foibles." Manning goes on to credit analytic induction with being a "viable source of data and concepts" and with helping investigators to sort out "the particulars of a given event [and to distinguish them from] those things that are general and theoretical." Erving Goffman, surely a sociological practitioner whose methodological orientation is akin to but not the same as analytic induction, goes even further, however. Opposing, in a defense of his own brand of ethnographic sociology, both system building

and enumerative induction, in 1961 he wrote, "At present, if sociological concepts are to be treated with affection, each must be traced back to where it best applies, followed from there wherever it seems to lead, and pressed to disclose the rest of its family. Better, perhaps, different coats to clothe the children well than a single splendid tent in which they all shiver" (p. xiv). A decade later, Goffman (1971) dismissed the scientific claims of positivistic sociologists altogether: "A sort of sympathetic magic seems to be involved, the assumption being that if you go through the motions attributable to science then science will result. But it hasn't" (p. xvi).

With the waning of interest in, support for, or faith in the older purposes for doing ethnology, by the 1970s there had also arisen a concomitant discontent with the epistemological claims as well as the latent or secretive political usages—(see Diamond, 1992; Horowitz, 1967)—of the mainstream perspectives of both sociology—(see Vidich, Lyman, & Goldfarb, 1981)—and anthropology—(e.g., Clifford & Marcus, 1986; Fox, 1991; Manganaro, 1990). An outlook that could be used to carry out research projects and at the same time to treat the very resources of each discipline as a topic to be investigated critically was needed. Postmodernism appeared and seemed to fill that need.

Toward the end of his essay, Manning hints at the issue that would explode on the pages of almost every effort to come to terms with postwar and post-Cold War America: "In an age of existentialism, self-construction is as much a part of sociological method as theory construction." What he would later perceive as a reason for developing a formalistic and semiotic approach to doing fieldwork (Manning, 1987, pp. 7-24, 66-72) was that each construction would come to be seen as inextricably bound up with the other and that each would be said to provide a distorted mirror image of both the body (Cornwell, 1992; Featherstone, Hepworth, & Turner, 1991; Feher, 1989; Sheets-Johnstone, 1990, pp. 112-133; 1992) and the self (Kotarba & Fontana, 1987; Krieger, 1991; Zaner, 1981), of both one's *Umwelt* and the world of the other (the concept of *Umwelt* is developed by Gurwitsch, 1966). But for those who accepted the critique but rejected neoformalism as a technique for ethnography, there opened up a new field of investigation—representation. Hence some of the best postmodern ethnography has focused on the media that give imagery to real life (Bhabha, 1990b; Early, 1993; Oilman, 1991; Minh-ha, 1991). Justification for turning from the fields of lived experience to what is represented as such is the assumption that the former is itself perceived holographically, calling for the thematization of representation as a problem in the construction of

"persuasive fictions" (Baudrillard, 1988a, pp. 27-106; Norris, 1990).

The postmodern ethnographer takes Simmel's tragedy of culture to be a *fait accompli*: It is not possible at the present time to emancipate free-floating life from all of its constraining forms (Strathern, 1990). The postmodern sociologist-ethnographer and his or her subjects are situated in a world suspended between illusory memories of a lost innocence and millennial dreams of a Utopia unlikely to be realized. From such a position, not only is the standpoint of the investigator problematic (Lemert, 1992; Weinstein & Weinstein, 1991), but also that of the people to be investigated. Each person has in effect been "touched by the mass media, by alienation, by the economy, by the new family and child-care systems, by the unceasing technologizing of the social world, and by the threat of nuclear annihilation" (Denzin, 1989, p. 139). And, if the anthropologist-ethnographer is to proceed in accordance with the postmodern perspective, he or she must, on the one hand, become less fearful about "going primitive" (Torgovnick, 1990) and, on the other, contend with the claim that Eurocentric imagery has attended virtually all previous reports from the "primitive" world (Beverly, 1992; Bhabha, 1990a; Dirlik, 1987; Turner, 1992; West, 1992). For these ethnographers, Helmut Kuzmics (1988) observes, "The claim that the 'evolutionary gradualism' of the theory of civilization renders it incapable of explaining the simultaneous appearance of civilization (in a narrower sense than is presupposed by the highest values of the Enlightenment) and 'barbarism' still needs to be confronted more thoroughly" (p. 161).

As analytic induction advocates propose, let us begin with a definition of the new outlook—the postmodern. Charlene Spretnak (1991), a critic of much of the postmodernism she surveys, provides one that is comprehensive and useful:

A sense of detachment, displacement, and shallow engagement dominates deconstructive-postmodern aesthetics because groundlessness is the only constant recognized by this sensibility. The world is considered to be a repressive labyrinth of "social production," a construction of pseudoselves who are pushed and pulled by cultural dynamics and subtly diffused "regimes of power." Values and ethics are deemed arbitrary, as is "history," which is viewed by deconstructive postmodernists as one group or another's self-serving selection of facts. Rejecting all "metanarratives," or supposedly universal representations of reality, deconstructive postmodernists insist that the making of every aspect of human existence is culturally created and determined in particular, localized circumstances about which no generalizations can be

made. Even particularized meaning, however, is regarded as relative and temporary, (pp. 13-14)

Spretnak's definition permits us to see how the postmodern ethnographer proceeds. The postmodernist ethnographer enters into a world from which he or she is methodologically required to have become detached and displaced. Such an ethnographer is in effect reconstituted as Simmel's (1950) "stranger" (see also Frisby, 1992) and Park's (1929/1969a) and Stonequist's (1937/1961) "marginalized" person (see also Wood, 1934/1969, pp. 245-284). Like those ideal-typical ethnographers-in-spite-of-themselves, this social scientist begins work as a self-defined newcomer to the habitat and life world of his or her subjects (see Agar, 1980; Georges & Jones, 1980; D. Rose, 1989). He or she is a citizen-scholar (Saxton, 1993) as well as a participant observer (Vidich, 1955). Older traditions and aims of ethnography, including especially the quest for valid generalizations and substantive conclusions, are temporarily set aside in behalf of securing "thick descriptions" (Geertz, 1973) that will in turn make possible "thick interpretations"—joining ethnography to both biography and lived experience (Denzin, 1989, pp. 32-34). History is banished from the ethnographic enterprise except when and to the effect that local folk histories enter into the vocabularies of motive and conduct employed by the subjects.³⁴ Because crossing the postmodern divide (Borgmann, 1992; I. Chambers, 1990) requires one to abandon all established and preconceived values, theories, perspectives, preferences, and prejudices as resources for ethnographic study, the ethnographer must bracket these, treating them as if they are arbitrary and contingent rather than hegemonic and guiding (Rosenau, 1992, pp. 25-76). Hence the postmodernist ethnographer takes seriously the aim of such deconstructionists as Derrida (e.g., 1976, 1981), Lyotard (e.g., 1989), and Baudrillard (e.g., 1981, 1983, 1988b), namely, to disprivilege all received texts and established discourses in behalf of an all-encompassing critical skepticism about knowledge. In so doing, the ethnographer displaces and deconstructs his or her own place on the hierarchy of statuses that all too often disguise their invidious character as dichotomies (see Bendix & Berger, 1959; for a postmodern analysis of a dichotomy, see Lyman, 1992a). To all of these, instead, is given contingency—the contingencies of language, of selfhood, and of community (Rorty, 1989; C. Taylor,

text.³⁵ The ethnographic enterprise is to be conceived as a task undertaken all too often by an unacculturated stranger who is guided by whatever the uneasy mix of poetry and politics gives to his or her efforts to comprehend an alien culture. Above all, an ethnography is now to be regarded as a piece of writing—as such, it cannot be said either to present or to represent what the older and newly discredited ideology of former ethnography claimed for itself: an unmodified and unfiltered record of immediate experience and an accurate portrait of the culture of the "other."

The postmodern critique has engendered something of a crisis among present-day anthropologists. As in the response to other crises, a new self-and-other consciousness has come to the fore, and the imperatives of reflexivity have shifted attention onto the literary, political, and historical features of ethnography as well as onto career imperatives, all of which have hitherto been overlooked. Engaging themselves with these issues, such disciplinary leaders as Clifford Geertz, Mary Douglas, Claude Levi-Strauss, and the late Victor Turner have blurred the old distinction between art and science and challenged the very basis of the claim to exacting rigor, unblinking truth telling, and unbiased reporting that marked the boundary separating one from the other.

Rereading the works in the classical ethnographic canon has now become a critical task of the highest importance. A new form of structuralist method must be devised if we are to dig beneath the works and uncover both their hidden truths and their limiting blinders. That canon is now to be seen as a product of the age of Occidental colonialism and to have been methodologically constrained by the metropole ideologies and literary conventions that gave voice and quality to them. Yet these ethnographies are not to be relegated to the historical dustbin of a rejectable epoch of disciplinary childhood by today's and tomorrow's anthropologists. Rather, in consideration of the fact that few of the latter will follow career trajectories like those of Malinowski or Powdermaker—that is, either spending decades of their lives in residence with a nonliterate Oceanic people or moving from the ethnographic task of observing at close range a group of South Africans to another, living among blacks in a segregated Mississippi town, and then to still another, closely examining how the Hollywood film industry became a "dream factory,"—the ethnologist of the present age and the immediate future is likely to do but one ethnography—a dissertation that stakes his or her claim to the title of ethnologist and to the perquisites of an academic life spent largely away from the field. Moreover, career considerations are not the only element affecting ethnology. The "field" itself has become

For anthropologists, the new forms for ethnography begin with a recognition of their irreducible limitation: the very presentation of ethnographic information in a monograph is a "text" and therefore subject to the entire critical apparatus that the postmodern perspective brings to bear on any

constricted by the march of decolonization and the modernization that has overtaken once "primitive" peoples. For these reasons, rereading old ethnographies becomes a vicarious way to experience the original ways of the discipline, whereas criticizing them provides the ethnologist with a way to distance him- or herself from modernist foibles. Except for the dissertation ethnography and for those anthropologists who choose to move in on the turf of the equally postmodern sociological ethnographers of urban and industrial settings, the ethnographic task of anthropology may become one devoted to reading texts and writing critiques. The "field" may be located in one's library or one's study.

Given the postmodern ethnographers' epistemological stance and disprivileged social status, two fundamental problems for the sociological version of the new ethnography are its relationship to social change and social action, and the applicable scope of its representations of reality.

The first problem has been posed as well as answered by Michael Burawoy et al. (1992) in their conception of "ethnography unbound" and the role of the "extended case method." They direct the ethnographer toward the macropolitical, economic, and historical contexts in which directly observed events occur, and perceive in the latter fundamental issues of domination and resistance (see also Feagin, Orum, & Sjoberg, 1991). Norman Denzin (1989), a leader of postmodern approaches to ethnography, approaches the generality issue in two distinct though related ways. His advice to ethnographers is that they first immerse themselves in the lives of their subjects and, after achieving a deep understanding of these through rigorous effort, produce a contextualized reproduction and interpretation of the stories told by the subjects. Ultimately, an ethnographic report will present an integrated synthesis of experience and theory. The "final interpretive theory is multivoiced and dialogical. It builds on native interpretations and in fact simply articulates what is implicit in those interpretations" (p. 120). Denzin's strategic move out of the epistemological cul-de-sac presented by such daunting observations as Berry's specific skepticism about the possibility of making valid generalizations in an ethnographically pluralist society, or by the growing skepticism about the kind and quality of results that sociologists' adherence to positivistic and natural science models will engender (T. R. Vaughan, 1993, p. 120), is to take the onset of the postmodern condition as the very occasion for presenting a new kind of ethnography. He encourages, in effect, an ethnographic attitude of engagement with a world that is ontologically absurd but always meaningful to those who live in it (see Lyman & Scott, 1989). Thus he concludes his methodological treatise by claiming that the

world has now entered its Fourth Epoch (following Antiquity, the Middle Ages, and the Modern Age), and that this latest epoch is in fact the "postmodern period" (Denzin, 1989, p. 138). The ethnographic method appropriate to this period, Denzin goes on, is one that is dedicated "to understanding how this historical moment universalizes itself in the lives of interesting individuals" (p. 189). Method and substance are joined in the common recognition that everyone shares in the same world and responds to it somehow. The study of the common condition and the uncovering of the uncommon response become the warp and woof of the fragile but not threadbare sociological skein of the postmodern era.

The postmodern is a cultural form as well as an era of history. As the former, like all the forms noted by Simmel, it invites and evokes its counteracting and rebellious tendencies. It too, then, is likely to suffer the penultimate tragedy of culture—the inability to emancipate life from all of its forms (Weinstein & Weinstein, 1990). However, in this era, the sociologist-ethnographer will not merely observe that history; he or she will participate in its everlasting quest for freedom, and be a partner in and a reporter on "the pains, the agonies, the emotional experiences, the small and large victories, the traumas, the fears, the anxieties, the dreams, fantasies and the hopes" of the lives of the peoples. These constitute this era's ethnographies—true tales of the field (Van Maanen, 1988).

The methods of ethnography have become highly refined and diverse, and the reasons for doing ethnography have multiplied. No longer linked to the values that had guided and focused the work of earlier ethnographers, the new ethnography ranges over a vastly expanded subject matter, limited only by the varieties of experience in modern life; the points of view from which ethnographic observations may be made are as great as the choices of lifestyles available in modern society. It is our hope that the technological refinement of the ethnographic method will find its vindication in the discovery of new sets of problems that lead to a greater understanding of the modern world.

Although it is true that at some level all research is a uniquely individual enterprise—not part of a sacrosanct body of accumulating knowledge—it is also true that it is always guided by values that are not unique to the investigator: We are all creatures of our own social and cultural pasts. However, in order to be meaningful to others, the uniqueness of our own research experience gains significance when it is related to the theories of our predecessors and the research of our contemporaries. Social and cultural understanding can be found by ethnographers only if

they are aware of the sources of the ideas that motivate them and are willing to confront them—with all that such a confrontation entails.

Notes

1. For a discussion of the fundamental similarities between so-called quantitative and qualitative methods, see Vidich and Bensman (1968, chap. 13).
2. Here we merely gloss a serious problem in the philosophy and epistemology of the social sciences and present one possible approach to it. Some of the issues are discussed and debated in such recent works as those by C. W. Smith (1979), Rabinow and Sullivan (1979), G. Morgan (1983), Fiske and Schweder (1986), Hare and Blumberg (1988), Ashmore (1989), Minnich (1990), Bohman (1991), Sadri (1992, pp. 3-32, 105-142), and Harr6(1984).
3. Many of the issues raised by this new outlook are treated in the essays collected in A. Rose (1988).
4. The following draws on Lyman (1990a).
5. This orientation differs from that used by Thucydides (1972) in *History of the Peloponnesian War*. His observations were made from the perspective of a participant who detached himself from the norms of both warring sides while never making explicit his own values. His book has confounded legions of scholars who have attempted to find his underlying themes, not understanding that the work is replete with ambiguities that do not lend themselves to a single viewpoint. For various perspectives on Thucydides' work, see Kitto (1991, pp. 136-152), Kluckhohn (1961, pp. 4, 34-35, 55, 64-66), Humphreys (1978, pp. 94, 131, 143, 227-232, 300-307), and Grant (1992, pp. 5, 45, 148-149).
6. When discussing the crimes committed by the Spaniards against the Indians, Hosle (1992) states: "It is certainly not easy to answer the following question: Were the priests who accompanied the conquistadors also responsible, even if they condemned the violence committed, insofar as their presence in a certain sense legitimized the enterprise? It is impossible to deny that by their mere presence they contributed to Christianity appearing as an extremely hypocritical religion, which spoke of universal love and nevertheless was the religion of brutal criminals. Yet it is clear that without the missionaries' presence even more cruelties would have been committed. Hypocrisy at least acknowledges in theory certain norms, and by so doing gives the oppressed the possibility to claim certain rights. Open brutality may be more sincere, but sincerity is not the only value. Sincere brutality generates nothing positive; hypocrisy, on the other side, bears in itself the force which can overcome it" (p. 236). If it does anything, Hosle's defense of Christianity reveals the difficulty still remaining in debates over universalistic as opposed to relativistic values and leaves wide open any resolution of the problem. See also Lippy, Choquette, and Poole (1992). For further history and discussion of the de Las Casas-Sepulveda dispute and its implications for ethnohistory and ethnology of the Americas, see Hanke (1949/1965, 1959/1970, 1974).
7. A fine example is the ethnographic study by Bishop Robert Henry Codrington (1891) titled *The Melanesians*. Codrington's study provided the sole source for Yale University anthropologist Loomis Havemeyer's (1929) chapter on the Melanesians (pp. 141-160). See Codrington (1974) for an excerpt from *The Melanesians* titled "Mana." See also the critical discussion in Kuper (1988, pp. 152-170).
8. A good example that also illustrates the anthropologists' despair over the disastrous effects of missionary endeavor on native life and culture is to be found in the last published work of William Hale R. Rivers (1922/1974).
9. Thus if the reader wishes to peruse one well-known exposition of "primitive" culture, George Peter Murdock's (1934) *Our Primitive Contemporaries*, as an example of one aspect of the "comparative method," he or she will discover therein ethnographies of 18 peoples who occupy time and space coincident to that of the author, arranged in terms of geography, but—with the term *primitive* as the descriptive adjective in use throughout—making the title of the book historically (that is, diachronically) oxymoronic. For a thoughtful critique, see Bock (1966).
10. Two exceptions to this mode of ethnocentric expression are worthy of note: William Graham Sumner (1840-1910), who coined the term *ethnocentrism*, seemed also to suggest that the failure of either Congress or the courts to do anything to halt the lynching of Negroes in the South signaled something less than that nation's rise to perfected civilization that other ethnologists were willing to credit to America and to other republics of the Occident: "It is unseemly that anyone should be burned at the stake in a modern civilized state" (Sumner, 1906/1940, p. 471; see also Sumner, 1905/1969). Thorstein Veblen (1857-1929) used such categories as "savagery" and "barbarism" tongue-in-cheek, often treating the moral codes and pecuniary values of the peoples so labeled as superior to those of the peoples adhering to the Protestant ethic or the spirit of capitalism, and disputing the claims of Aryan superiority so much in vogue in his day (see Veblen, 1899/1959, 1914/1990, 1919/1961a, 1919/1961b; see also A. K. Davis, 1980; Diggins, 1978; Tilman, 1991).
11. The Human Relations Area Files were reproduced, marketed, and distributed to anthropology departments in other universities. This not only added an element of standardization and uniformity to culture studies, but also made it possible for the analyst of ethnography to forgo a trip to the field. That this approach is still in vogue is illustrated by two researches by the Harvard sociologist Orlando Patterson (1982). Patterson relies on Murdock's "World Sample" of 61 slaveholding societies (out of a total of 186 societies), which are arranged geographically, but rearranges them temporally to make them serve a developmentalist thesis

that seeks to uncover the variations in as well as the functional origins of slavery. On the basis of this method, it is not surprising to find that in the sequel to his study Patterson (1991) believes he can show that "the Tupinamba, the ancient Greeks and Romans, and the southerners of the United States, *so markedly different in time, place, and levels of sociocultural development*, nonetheless reveal the remarkable tenacity of this culture-character complex" (p. 15; emphasis added).

12. For the conceptualization of a sector of the world's peoples as belonging to the Third World, as well as for the conceptualization of "developed" and "undeveloped" or "underdeveloped" societies, see Worsley (1964, 1984).

13. That capitalism had contributed to underdevelopment in both the European overseas empires and America's homegrown "ghetto colonialism" became an assumption and even an article of faith that could shape the perspective of posttraditional ethnography (see Blauner, 1972; Marable, 1983; see also Hechter, 1975).

14. For a historical view on eschatological, millennial, sacred, and secular "end-times" theories, as well as other modes of chronologizing events, see Paolo Rossi (1987).

15. It should be noted that American ethnography up to the beginnings of World War II focused almost exclusively on American Indians and the aboriginal inhabitants of American colonies. Anthropologists' interests in the high cultures of Central and South America were archaeologically oriented and were designed both to fill in the "prehistoric record" and to fill museums. Some ethnographic work was carried out in the U.S.-controlled Pacific Islands (in association with the Bernice P. Bishop Museum in Hawaii). Margaret Mead worked on American Samoa and is one of the earliest of the nonmissionaries to ethnograph a Pacific Island. Her work, aimed in part at criticizing the Puritanical sexual mores of America, overstated the actual situation in Oceania and eventually led to a counterstatement (see Freeman, 1983; Holmes, 1987; Mead, 1928/1960a, 1930/1960b, 1949/1960c, 1935/1960d).

16. This was the same perspective used by anthropologists who administered the Japanese relocation centers during World War II and who had had some of their training on the reservation. For accounts by those anthropologists who moved from Amerindian to Japanese American incarceration ethnography and administration, see Leighton (1945), Wax (1971), Spicer, Hansen, Luomala, and Opler (1969), and Myer (1971). For a spirited critique, see Drinnon (1987).

17. For some representative ethnographies of the southwestern Amerindian peoples, see Schwatka (1893/1977), Nordenskiöld (1893/1979), McGee (1899/1971), Goddard (1913/1976), White (1933/1974), Spier (1933/1978), and Kluckhohn (1944). See also Eggan (1966, pp. 112-141).

18. A recent ethnography of the Zuni by Tedlock (1992) both reflects upon and critically appraises Cushing's work among that tribe.

19. Radin (1935/1970, 1936/1971a) also did fieldwork among the Italians and Chinese of San Francisco.

20. Clark Wissler (1940/1966a, 1938/1966b) established his credentials on the basis of a lifetime in service to ethnohistorical and ethnographic study of the United States.

21. Although not carried out at the University of Chicago, this study bears the stamp of that school's approach.

22. In that report, he was the first to see the new role of the university president as an administrative "Captain of Erudition," the beginnings of university public relations designed to protect the image of learning, and the business foundations in real estate and fund-raising (endowments) of the university system in the United States.

23. In 1992, when new questions were raised about the ethnocultural and ethical aspects of Whyte's study of "Cornerville," a symposium reviewed the matter extensively (see "*Street Corner Society Revisited*," 1992).

24. A social variant of Redfield's perspective found its way into some of the urban community, ethnic enclave, and small-town studies of America that were conducted or supervised by anthropologists or Chicago sociologists (see Hannerz, 1980; Lyon, 1987; Suttles, 1972, pp. 3-20). (A revival of ecological studies rooted in the idea that the uses of space are socially constructed was begun with the publication of Lyman & Scott, 1967; see also Ericksen, 1980.) As early as 1914, M. C. Elmer, a promising graduate student at the University of Chicago, had written a Ph.D. dissertation on social surveys in urban communities that reflected the shift from the church to the "scientific" survey tradition in both the social gospel movement and the discipline of sociology; seven years later, Raleigh Webster Stone (1921) in effect signaled that the transition to a newer orientation was well under way when he offered *The Origin of the Survey Movement* as his Ph.D. dissertation at Chicago. In 1933, Albert Bailie Blumenthal submitted *A Sociological Study of a Small Town* as his doctoral dissertation at the same university (Paris, 1970, pp. 135-140). However, the central thrust of ethnological studies in Chicago's sociology department after Robert E. Park had joined its faculty concerned community and subcommunity organization within the city (see, e.g., N. Anderson, 1959), and, for some, how the *gemeinschaft* could be reconstituted in the metropolis (see Fishman, 1977; Quandt, 1970).

25. That ethnographies of small towns and large cities adopted an approach more or less consistent with the macropolitical-economic orientation emphasized by Vidich and Bensman is evidenced in works by P. Davis (1982), Wallace (1987), Arsenault (1988), Campbell (1992), Moorhouse (1988), and Reid (1992).

26. Earlier, Vidich (1952, 1980) had contributed to the reconsideration of anthropological approaches to so-called primitive societies, reconceiving such studies as requiring an orientation that focused on the effects of global colonialism and its rivalries on the structure