2 A HISTORY OF QUALITATIVE INQUIRY IN SOCIAL AND EDUCATIONAL RESEARCH[1]

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Qualitative inquiry seeks to discover and to describe narratively what particular people do in their everyday lives and what their actions mean to them. It identifies meaning-relevant *kinds* of things in the world—kinds of people, kinds of actions, kinds of beliefs and interests—focusing on differences in forms of things that make a difference for meaning. (From Latin, *qualitas* refers to a primary focus on the qualities, the features, of entities—to distinctions in kind—while the contrasting term, *quantitas*, refers to a primary focus on differences in amount.) The qualitative researcher first asks, "What are the kinds of things (material and symbolic) to which people in this setting orient as they conduct everyday life?" The quantitative researcher first asks, "How many instances of a certain kind are there here?" In these terms, quantitative inquiry can be seen as always being preceded by foundational qualitative inquiry, and in social research, quantitative analysis goes haywire when it tries to shortcut the qualitative foundations of such research—it then ends up counting the wrong kinds of things in its attempts to answer the questions it is asking.

This chapter will consider major phases in the development of qualitative inquiry. Because of the scale of published studies using qualitative methods, the citations of literature present illustrative examples of work in each successive phase of qualitative inquiry's development rather than an exhaustive review of literature in any particular phase. I have referred the reader at various points to additional literature reviews and historical accounts of qualitative methods, and at the outset, I want to acknowledge the comprehensive historical chapter by Arthur Vidich and Stanford Lyman (1994, pp. 23–59), which was published in the first edition of this *Handbook*. Our discussion here takes a somewhat different perspective concerning the crisis in authority that has developed in qualitative inquiry over the past 40 years.

This chapter is organized both chronologically and thematically. It considers relationships evolving over time between six foundational "footings" for qualitative research: (1) disciplinary perspectives in social science, particularly in sociology and anthropology; (2) the participant-observational fieldworker as an observer/author; (3) the people who are observed during the fieldwork; (4) the rhetorical and substantive content of the qualitative research report as a text; (5) the audiences to which such texts have been addressed; and (6) the underlying worldview of research—ontology, epistemology, and purposes. The character and legitimacy of each of these "footings" have been debated over the entire course of qualitative social inquiry's development, and these debates have increased in intensity in the recent past.

ORIGINS OF QUALITATIVE RESEARCH

In the ancient world, there were precursors to qualitative social inquiry. Herodotus, a Greek scholar writing in the 5th century B.C.E., had interests that were cross-cultural as well as historical. Writing in the 2nd century C.E., the Greek skeptical philosopher Sextus Empiricus conducted a cross-cultural survey of morality, showing that what was considered right in one society

was considered wrong in others. Both he and Herodotus worked from the accounts of travelers, which provided the primary basis for comparative knowledge about human lifeways until the late 19th century. Knowledge of nature also was reported descriptively, as in the physics of Aristotle and the medicine of Galen.

Descriptive reporting of everyday social practices flourished again in the Renaissance and Baroque eras in the publication of "how to do it books" such as Baldassar Castiglione's *The Book of the Courtier* and the writing of Thoinot Arbeau (*Orchésographie*) on courtly dancing, of Johann Comenius (*Didactica Magna*) on pedagogy, of Isaak Walton (*The Compleat Angler*) on fishing, and of John Playford (*The Division Viol*) on how to improvise in playing the viola da gamba. The treatises on dancing and music especially were descriptive accounts of very particular practices—step-by-step description at molecular grain size. Narrative descriptive reports were also written in broader terms, such as the accounts of the situation of Native Americans under early Spanish colonial rule in Latin America, written by Bartolomeo de las Casas in the 16th century, and the 17th-century reports French Jesuits submitted to superiors regarding their missionary work in North America (*Relations*). A tension between scope and specificity of description remains in contemporary qualitative inquiry and reporting.

Simultaneously with the 17th-century writing on everyday practices, the quantitative physics of Galileo Galilei and Isaac Newton was being established. As the Enlightenment developed, quantitatively based inquiry became the standard for physical science. The search was for general laws that would apply uniformly throughout the physical world and for causal relations that would obtain universally. This became a worldview, assuming not only a "realist" ontology—that the physical world existed apart from humans' awareness and conceptions of it—but also an assumption that its processes were so consistent and stable that clear discovery of cause and clear prediction would be possible. The British moral philosopher Hume was skeptical that causes could be observed directly, but he maintained that they could be inferred from regular association—constant conjunction—between events (i.e., A can be considered to cause B when the two events always occur together, and A always precedes B in time) (Hume, 2007, Book I, Part 3, Section 14, p. 170). It follows that the job of the "scientist" is to tabulate instances of regular association between events.

Could there be an equivalent to this in the study of social life—a "social physics"—in which social processes were monitored by means of frequency tabulation, and generalizations about social processes could be derived from the analysis of frequency data? In England, William Petty's *Political Arithmetic* was one such attempt, published in 1690. In France and Germany, the term *statistics* began to be used to refer to quantitative information collected for purposes of the state—information about finance, population, disease, and mortality. Some of the French Enlightenment philosophers of the 18th century saw the possibility that social processes could be mathematically modeled and that theories of the state and of political economy could be formulated and empirically verified in ways that would parallel physics, chemistry, and astronomy.

As time went on, a change of focus occurred in published narrative descriptive accounts of daily practices. In the 16th and 17th centuries, the activities of the leisured classes were described, while the lower classes were portrayed patronizingly at the edges of the action, as greedy, lascivious, and deceitful, albeit clever. (A late example can be found in the portrayal of the lusty, pragmatic countrymen and women in Picander's libretto for J. S. Bach's *Peasant Cantata*, written and performed in 1742.) By the end of the 18th century, the everyday lives of servants and rustics were being portrayed in a more sympathetic way. Pierre Beaumarchais's play, *The Marriage of Figaro,* is an example. Written in 1778, it was initially banned in both Paris and Vienna on the grounds that by valorizing its servant characters and satirizing its aristocratic characters, it

was dangerously subversive and incited insubordination. By the early 19th century, the Brothers Grimm were collecting the tales of German peasants, and documentation of folklore and folklife of commoners became a general practice.

By the mid-19th century, attempts were being made to define foundations for the systematic conduct of social inquiry. A fundamental disagreement developed over what kind of a "science" the study of society should be. Should such inquiry be modeled after the physical sciences, as Enlightenment philosophers had hoped? That is a worldview that Auguste Comte (1822/2001) presumed as he developed a science of society he would come to call *sociology;* his contemporary, Adolphe Quetelet (1835/2010), advocated the use of statistics to accomplish what he labeled outright as a "social physics." Early anthropologists with foundational interests in social and cultural evolution also aimed their inquiry toward generalization (e.g., L. H. Morgan, 1877; Tylor, 1871); they saw the comparative study of humans as aiming for general knowledge, in their case, an understanding of processes of change across time in physical and cultural ways of being human—of universal stages of development from barbarism to contemporary (European) civilization—comparative study that came to be called *ethnology.* Like Comte, they saw the purposes of social inquiry as the discovery of causal laws that applied to all cases, laws akin to those of physics and chemistry.

In contrast, the German social philosopher Wilhelm Dilthey (1883/1989) advocated an approach that differed from that of natural sciences (which he called *Naturwissenschaften*). He advocated conducting social inquiry as *Geisteswissenschaften*—literally, "sciences of the spirit" and more freely translated as "human sciences" or, better, "human studies." Such inquiry was common to both the humanities and what we would now call the social sciences. It focused on the particulars of meaning and action taken in everyday life. The purpose of inquiry in the human sciences was understanding (*verstehen*) rather than proof or prediction. Dilthey's ideas—an alternative worldview for social inquiry—influenced younger scholars (e.g., Max Weber and Georg Simmel in sociology and early phenomenologists in philosophy such as Edmund Husserl and Martin Heidegger). His ideas became even more influential in the mid-20th-century "hermeneutical turn" taken by philosophers such as Hans-Georg Gadamer and Jürgen Habermas and by anthropologists such as Ernest Gellner and Clifford Geertz.

The Emergence of Ethnography

In the last quarter of the 19th century, anthropologists began to use the term *ethnography* for descriptive accounts of the lifeways of particular local sets of people who lived in colonial situations around the world. These accounts, it was claimed, were more accurate and comprehensive than the reports of travelers and colonial administrators. In an attempt to improve the information quality and comprehensiveness of description in travelers' accounts, as well as to support the fieldwork of scholars in the emerging field of anthropology, the British Society for the Advancement of Science published in 1874 a manual to guide data collection in observation and interviewing, titled *Notes and Queries on Anthropology for the Use of Travelers and Residents in Uncivilized Lands* (available at http://www.archive.org/details/notesandqueries00readgoog). The editorial committee for the 1874 edition of *Notes and Queries* included George Lane-Fox Pitt-Rivers, Edward Tylor, and Francis Galton, the latter being one of the founders of modern statistics. The *Notes and Queries* manual continued to be reissued in further editions by the Royal Anthropological Society, with the sixth and last edition appearing in 1951.

At 6½ by 4 inches, the book could be carried to field settings in a large pocket, such as that of a bush jacket or suit coat. Rulers in both inches and centimeters are stamped on the edge of

the cover to allow the observer to readily measure objects encountered in the field. The volume contains a broad range of questions and observation topics for what later became the distinct branches of physical anthropology and social/cultural anthropology: Topics include anatomical and medical observations, clothing, navigation, food, religion, laws, and "contact with civilized races," among others. The goal was an accurate collection of facts and a comprehensive description of the whole way of life of those who were being studied.

This encyclopedic approach to fieldwork and information collection characterized late 19th-century qualitative research, for example, the early fieldwork of Franz Boas on the northwest coast of North America and the two expeditions to the Torres Straits in Oceania led by Alfred Haddon. The second Haddon expedition involved fieldworkers who would teach the next generation of British anthropologists—for example, W. H. R. Rivers and C. G. Seligman, with whom A. R. Radcliffe Brown and B. Malinowski later studied. (For further discussion of the early history of field methods in anthropology, see Urry, 1984, pp. 33–61.)

This kind of data collection and reporting in overseas settings was called *ethnography,* combining two Greek words: *graphein,* the verb for "to write," and *ethnoi,* a plural noun for "the nations—the others." For the ancient Greeks, the *ethnoi* were people who were not Greek—Thracians, Persians, Egyptians, and so on—contrasting with *Ellenoi* or Hellenes, as us versus them. The Greeks were more than a little xenophobic, so that *ethnoi* carries pejorative implications. In the Greek translation of the Hebrew scriptures, *ethnoi* was the translation for the Hebrew term for "them"—*goyim*—which is not a compliment. Given its etymology and its initial use in the 19th century for descriptive accounts of non-Western people, the best definition for ethnography is "writing about other people."

Perhaps the first monograph of the kind that would become modern realist ethnography was *The Philadelphia Negro,* by W. E. B. Du Bois (1899). His study of a particular African American census tract combined demographic data, area maps, recent community history, surveys of local institutions and community groups, and some descriptive accounts of the conduct of daily life in the neighborhood. His purpose was to make visible the lives—and the orderliness in those lives—of people who had been heretofore invisible and voiceless in the discourses of middle-class white society and academia. A similar purpose and descriptive approach, combining demography and health statistics with narrative accounts, was taken in the reports of working-class life in East London by Charles Booth (1891), whose collaborators included Sidney and Beatrice Webb. Even more emphasis on narrative description was found in *How the Other Half Lives*, an account of the everyday life of immigrants on the lower East Side of New York City, written by the journalist Jacob Riis (1890) and illustrated with photographs. All of these authors—and especially Booth and Du Bois—aimed for factual accuracy and holistic scope. Moreover, these authors were social reformers—Booth and the Webbs within the Fabian Socialist movement in England, Riis as a founder of "muckraking" journalism and popular sociology, and Du Bois as an academic sociologist who turned increasingly to activism, becoming a leader of the early 20th-century African American civil rights movement. Beyond description for its own sake, their purpose was to advocate for and to inform social change.

None of these early practitioners claimed to be describing everyday life from the points of view of those who lived it. They were outsider observers. Du Bois, although an African American, grew up in a small New England town, not Philadelphia, and he had a Harvard education. Booth and the Webbs were upper middle class, and so was Riis. They intended to provide accurate descriptions of "facts" about behavior, presented as self-evidently accurate and "objective," but not about their functional significance in use, or as Clifford Geertz (1973) said, what distinguishes an eye blink from a wink (p. 6). To use terms that developed later in linguistics and

metaphorically applied to ethnography, their descriptions were *etic* rather than *emic* in content and epistemological status.

Adding Point of View

Portraying social action (as wink) rather than behavior (as eye blink)—that is, describing the conduct of everyday life in ways that make contact with the subjective orientations and meaning perspectives of those whose conduct is being reported—is the fundamental shift in interpretive (hermeneutical) stance within ethnography that Bronislaw Malinowski claimed to have accomplished a generation later. In his groundbreaking monograph, *Argonauts of the Western Pacific* (Malinowski, 1922), he said that ethnographic description should not only be holistic and factually accurate but also aim "to grasp the native's point of view, his relation to life, his vision of his world" (p. 25).

During World War I, Malinowski, a Pole who had studied anthropology in England, was interned by British colonial authorities during his fieldwork in the Trobriand Islands of Melanesia because they were concerned that, as a subject of the Austro-Hungarian Empire, he might be a spy. He was not allowed to return home until the war had ended. Malinowski later made a virtue of necessity and claimed that his 4 years of enforced fieldwork and knowledge of the local language enabled him to write a report that encompassed the system of everyday life in its entirety and accurately represented nuances of local meaning in its daily conduct. After Malinowski, this became a hallmark of ethnography in anthropology—reporting that included the meaning perspectives of those whose daily actions were being described.

Interpretively oriented (i.e., hermeneutic) realist ethnography presumed that *local meaning* is causal in social life and that local meaning varies fundamentally (albeit sometimes subtly) from one local setting to another. One way this manifested in anthropology was through cultural relativism—a position that Franz Boas had taken before Malinowski. By the late 1920s, anthropologists were presuming that because human societies were very different culturally, careful ethnographic case study documentation was necessary before valid ethnological comparison could take place—the previous armchair speculations of scholars such as Edward Tylor and Lewis Henry Morgan were seen as having been premature.

What is implied in the overall emphasis on the distinctive differences in local meaning from one setting to another is a presumption that stands in sharp contrast to a basic presumption in natural science. There one assumes a fundamental *uniformity of nature* in the physical universe. For example, one can assume that a unit measurement of heat, or of force, or a particular chemical element is the same entity in Mexico City and Tokyo as it is in London—and also on the face of the sun and in a far distant galaxy. The presumption of uniformity of natural elements and processes permitted the statement of general laws of nature in physics, chemistry, and astronomy and, to a lesser extent, in biology. In contrast, a human science focus on locally constructed meaning and its variability in construction presumes, in effect, a fundamental *nonuniformity of nature in social life.* That assumption was anathema to those who were searching for a social physics.

But qualitative social inquiry is not aiming to be a social physics. Or is it? Within anthropology, sociology, and educational research, researchers disagreed about this, even as they did ethnographic case studies in traditional and modern societies.

A basic, mainstream approach was developing in qualitative social inquiry. We can see that approach as resting on six foundational grounds or footings: the disciplinary enterprise of social science, the social scientific observer, those who are observed, the research report as a text, the

research audience to which that text is addressed, and the worldview that guides the research—ontology and epistemology. Each of these six was considered an entity whose nature was simple and whose legitimacy was self-evident. In current qualitative inquiry, the nature and the legitimacy of each of those footings have been called into question.

First, the *enterprise of social science.* By the late 19th century, sociology and anthropology were developing as new disciplines, beginning to achieve acceptance within universities. Physical sciences had made great progress since the 17th century, and social scientists were hoping for similar success.

Next, the *social scientist as observer.* His (and these were men) professional warrant for paying research attention to other humans was the social scientific enterprise in which he was engaged—that engagement gave him the right to watch other people and question them. It was assumed that he would and should be systematic and disinterestedly open-minded in the exercise of research attention. The process of looking closely and carefully at another human was seen as being no more ethically or epistemologically problematic than looking closely and carefully at a rock or a bird. Collecting specimens of human activity was justifiable because it would lead to new knowledge about social life. (Unlike the field biologist, the social scientist was not justified to kill those he studied or to capture them for later observation in a zoological museum—although some non-Western people were exhibited at world expositions, and the anthropologist Alfred Kroeber had housed a Native American, Ishi, at the anthropological museum of the University of California, Berkeley, making him available for observation and interview there—but artifact collecting and the writing of field notes were the functional equivalent of the specimen collection and analysis methods of biologists and geologists.) Moreover, research attention in social inquiry was a one-way matter—just as the field biologist dissected an animal specimen and not the other way around, it was the researcher's watching and asking that counted in social inquiry, not the attending to and questioning of the researcher by the people whose daily lives were being studied.

Those who were observed as research objects (not as subjects but as objects) were thus considered essentially passive participants in the research enterprise—patients rather than agents—there to be acted upon by observing and questioning, not there to affect the direction taken in the inquiry. Thus, in the division of labor within the process of qualitative social inquiry, a fundamental line of distinction and asymmetry was drawn between the observer and the observed, with control over the inquiry maximized for the observer and minimized for the observed.

That asymmetry extended to the process of *producing the text of a research report*, which was entirely the responsibility of the social scientist as author. Such reports were not written in collaboration with those whose lives were studied, nor were they accompanied by parallel reports produced by those who were studied (just as the finches of the Galapagos Islands had not published a report of Darwin's visit to them). In reports of the results of social inquiry by means of firsthand participant observation, the portrayal of everyday life of the people studied was done by the researcher.

The asymmetry in text production extended further to text consumption. The written report of social inquiry was addressed to *an audience consisting of people other than those who had been studied*—the community of the researcher's fellow social scientists (and, perhaps, of policy makers who might commission the research work). This audience had as its primary interests the substantive significance of the research topic and the technical quality of the conduct of the study. The success of the report (and of the author's status as a reporter) was a matter of judgment residing in the scholarly community. The research objects' existential experience of being scrutinized during the researcher's fieldwork and then described in the researcher's report was

not a primary consideration for the readers of the report or for its author. Indeed, those who had been studied were not expected to read the research report, since many were not literate. The research worldview was realist—the researcher could know the social world directly and describe it accurately.

For a time, each of these six footings had the stability of canonical authority in the "normal science" practice of qualitative inquiry. That was a period that could be called a "golden age," but with a twinge of irony in such a designation, given what we now know about the intense contestation that has developed more recently concerning each of the footings.

A "GOLDEN AGE" OF REALIST ETHNOGRAPHY

From the mid-1920s to the early 1950s, the basic approach in qualitative inquiry was realist general ethnography—at the time, it was just called *ethnography.* More recently, such work has been called *realist* because of its literary quality of "you are there" reporting, in which the narrator presents description as if it were plain fact, and *general* because it attempted a comprehensive description of a whole way of life in the particular setting that was being described—a setting (such as a village or an island or, later, an urban neighborhood or workplace within a formal organization) that was seen as being distinctly bounded. Typically, the narrator wrote in third person and did not portray themselves as being present in the scenes of daily life that were described. A slightly distanced authorial voice was intended to convey an impression of even-handedness— conveying "the native's point of view" without either overt advocacy of customary practices or explicit critique of them. (For a discussion of the stance of detachment, see Vidich & Lyman, 1994, p. 23.) Usually, the social theory perspective underlying such work was some form of functionalism, and this led authors to focus less on conflict as a driving force in society and more on the complementarity of various social institutions and processes within the local setting.

Ethnographic monographs in anthropology during this time followed the overall approach found in Bronislaw Malinowski's (1922) *Argonauts,* where he said that an adequate ethnography should report three primary bodies of evidence:

1. *The organisation of the tribe, and the anatomy of its culture* must be recorded in firm, clear outline. The method of concrete, *statistical documentation* is the means through which such an outline has to be given.

2. Within this frame the *imponderabilia of actual life*, and the *type of behaviour* must be filled in. They have to be collected through minute, detailed observations, in the form of some sort of ethnographic diary, made possible by close contact with native life.

3. A collection of ethnographic statements, characteristic narratives, typical utterances, items of folk-lore, and magical formulae has to be given as a *corpus inscriptionem*, as documents of native mentality. (p. 24)

What was studied was a certain village or region in which a named ethnic/linguistic group resided. The monograph usually began with an overall description of the physical setting (and *often of subsistence activities*). *This was followed by a chapter on an annual cycle of life,* one on a typical day, one on kinship and other aspects of "social organization," one on childrearing, and then chapters on certain features of the setting that were distinctive to it. (Thus, for example, Evans-Pritchard's [1940] monograph on a herding people, *The Nuer,* contains detailed description of the aesthetics of appreciation of color patterns in cowhide.) Narrative vignettes describing

the actions of particular people in an actual event were sometimes provided, or typical actions were described more synoptically. These vignettes and quotes from informants were linked in the text by narrating commentary. Often maps, frequency tables, and analytic charts (including kinship diagrams) were included.

Notable examples in British and American anthropology during this period include volumes by students of Franz Boas, such as Margaret Mead's (1928) semipopular account, *Coming of Age in Samoa*. Raymond Firth, a student of Malinowski, produced *We the Tikopia* (1936/2004). E. E. Evans-Pritchard, a student of Malinowski's contemporary, Alfred Radcliffe-Brown (who himself had published a monograph *The Andaman Islanders* in the same year as Malinowski's *Argonauts*, 1922), published *The Nuer* in 1940. David Holmberg (1950) published a study of the Siriono, titled *Nomads of the Longbow*. In addition to American work on Indigenous peoples of the Western Hemisphere, there were monograph series published on British colonial areas— from Australia, studies of New Guinea, Micronesia, and Melanesia, and from England, studies of East Africa, West Africa, and South Africa.

In the United States, community studies in an anthropologically ethnographic vein were encouraged by Robert Park and Ernest Burgess at the Department of Sociology of the University of Chicago. On the basis of hunches about geographic determinism in the founding and main-tenance of distinct social areas within cities, various Chicago neighborhoods were treated as if they were bounded communities, for example, Louis Wirth's (1928) study of the West Side Jewish ghetto and Harvey Warren Zorbaugh's (1929) study of contiguous working-class Italian immigrant and upper-class "mainstream American" neighborhoods on the near North Side. A tradition of community study followed in American sociology. Robert and Helen Lynd (1929, 1937) conducted a two-volume study of a small Midwestern city, Muncie, Indiana, which they called Middletown. The anthropologist W. Lloyd Warner (1941) studied Newburyport, Massachusetts; the Italian neighborhood of Boston's North End was described by William F. Whyte (1943/1955); and the anthropologists Conrad Arensberg and Solon Kimball (1940) stud-ied a rural Irish village.

The urban community studies efforts continued after World War II, with St. Clair Drake and Horace Cayton's (1945) description of the African American neighborhoods of Chicago's South Side, August Hollingshead's (1949) study of a Canadian suburb, and Herbert J. Gans's (1962) report on an Italian American neighborhood in New York, among others. Gerald Suttles (1968) revisited the "social areas" orientation of Chicago School sociology in a study of intereth-nic relations in a multiethnic neighborhood on Chicago's Near West Side, and Elijah Anderson (1992) described a multiracial West Philadelphia neighborhood in a somewhat similar vein. Some studies narrowed the scope of community studies from a whole neighborhood to a par-ticular setting within it, as in the case of bars as sites for friendship networks among African American men in the reports (e.g., Liebow, 1967). Rural sociology in America during the 1930s had also produced ethnographic accounts. (For an extensive review and listing of American community studies, see the discussion in Vidich & Lyman, 1994.)

Institutional and workplace studies began to be done ethnographically, especially in the postwar era. Labor–management relations were studied by means of participant observation (e.g., Roy, 1959). Chris Argyris published descriptive accounts of daily work in a bank depart-ment (1954a, 1954b) and of the work life of a business executive (1953). Ethnographic accounts of socialization into professions began to appear (e.g., Becker & Geer, 1961; Glaser & Strauss, 1965). Workplace accounts, as in community studies, began to focus more closely on immediate scenes of everyday social interaction, a trend that continued into the future (see, e.g., G. Fine, 1990; Vaught & Smith, 1980).

Journal-length reports of workplace studies (as well as accounts of overseas development interventions by applied anthropologists) appeared in the interdisciplinary journal *Human Organization*, which began publication under that title in 1948, sponsored by the Society for Applied Anthropology.

Ethnographic documentary film developed in the 1950s and 1960s as field recording of sound became easier, with more portable equipment—audiotape and the 16-mm camera. Boas had used silent film in the 1920s to document Kwakiutl life on the Northwest Coast of Canada, and Gregory Bateson and Mead used silent film in the late 1930s in their study of dance instruction in Bali. Robert Flaherty produced semifictional, partially staged films of Canadian Inuit in the 1920s, notably *Nanook of the North.*

The new ethnographic documentaries were shot in naturalistic field situations, using for the most part handheld cameras and microphones to move with the action. John Marshall's film, *The Hunters*, featured Kalahari Bushmen of southern Africa; Napoleon Chagnon's *The Ax Fight* and Timothy Asch's *The Feast* were filmed in the Amazon River Delta in Brazil, among the Yanomamo. John Adair and Sol Worth gave 16-mm handheld cameras to Navaho informants in a project that tried to identify differences in ways of seeing between the Navaho and Western European cinematographers. They produced film footage and a monograph on the project titled "Through Navaho Eyes" (Worth & Adair, 1972). John Collier Jr. shot extensive silent film footage showing Native American school classrooms in Alaska. He also published a book on the use of still photographs for ethnographic documentation (Collier, 1967)—a practice that Mead had pioneered a generation earlier (see Byers, 1966, 1968). The Society for Visual Anthropology, a network of ethnographic filmmakers and scholars of documentary film semiotics, was founded in 1984.

U.S. sociologists made institutionally focused documentary films during the same time period, notably the films produced in the 1960s and 1970s by Frederick Wiseman. These interpretive film essays, through the editing of footage of naturally occurring events, bridge fiction and more literal documentary depiction. They include "Titicut Follies" (1967), a portrayal of a mental hospital; "High School" (1968); "Hospital" (1970); and "Essene" (1972), a portrayal of conflict and community in a monastery (for further discussion, see Barnouw, 1993; Benson & Anderson, 2002; deBrigard, 1995; Heider, 1982; Ruby, 2000).

CRISES IN ETHNOGRAPHIC AUTHORITY

A Gathering Storm. Even in the postwar heyday of realist ethnography, some cracks in its footings were beginning to appear. In American anthropology, a bitter controversy developed over accuracy and validity of competing ethnographic descriptions of a village on the outskirts of Mexico City, Tepoztlán. Robert Redfield (1930) at the University of Chicago had published an account of everyday life in Tepoztlán. In keeping with a functionalist perspective in social theory, he characterized the community as harmonious and internally consistent, a place where people led predictable, happy lives. Beginning fieldwork in the same village 17 years after Redfield and viewing everyday life in the community through a lens of Marxist conflict theory, Oscar Lewis (1951) saw life in Tepoztlán as fraught with tension and individual villagers as tending toward continual anger, jealousy, and anxiety. In his monograph, he harshly criticized Redfield's portrayal. Two fieldworkers had gone to the "same" place and collected very different evidence. Which one was right?

Concern was developing over texts that reported the general ethnography of a whole community—those reports seemed increasingly to be hazy in terms of evidence: Description

flowed a mile wide but an inch deep. One way to address this limitation was to narrow the scope of research description and to focus on a particular setting within a larger community or institution. Another way was to become more careful in handling evidence. Within American anthropology, specialized "hyphenated" subfields of sociocultural study developed, such as cognitive anthropology, economic anthropology, anthropology of law, ethnography of communication, and interactional sociolinguistics. Studies in those subfields were often published as tightly focused journal-length articles in which evidence was presented deliberately and specifically. Careful elicitation techniques and increasing use of audio and audiovisual recording were used in attempts to get "better data." An interdisciplinary field called sociolinguistics developed across the disciplines of linguistics, anthropology, sociology, and social psychology.

In sociology first and then increasingly in anthropology, methods texts were published— becoming more explicit about methods of participant observation as another route to "better data." Notable examples are McCall and Simmons (1969), Glaser and Strauss (1967), Denzin (1970), Pelto and Pelto (1970), Hammersley and Atkinson (1983), Ellen (1984), and Sanjek (1990).

Autobiographical accounts of fieldwork also began to be published. The second edition of Whyte's *Street Corner Society* (1943/1955) and subsequent editions contained an extensive appendix in which Whyte described, in first person, his field experience. Hortense Powdermaker (1966) described her field experience in white and Black southern U.S. rural communities in the 1930s. Even earlier, Laura Bohannon had published a fictionalized memoir of fieldwork, writing a quasi-novel under the pseudonym Elenore Smith Bowen (1954) because frank revelations of ambivalence, ethical dilemmas, the intense emotionality of fieldwork, and tendencies toward self-deception were not considered proper topics of "academic" discourse at the time. Rosalie Wax (1971) candidly recalled the difficulties of her fieldwork as a white woman in Japanese internment camps during World War II. These accounts showed that actual fieldwork was not so consistently guided by detached, means-ends rationality as ethnographic monographs had sometimes suggested.

In 1967, Malinowski's Trobriand Island field diary was published posthumously. Over the next 15 years, the diary came to occupy a central place in what became a firestorm of criticism of realist general ethnography.

After World War II, the accuracy of ethnography began to draw challenges from the "natives" whose lives were portrayed in them. Thirty years after Malinowski left the Trobriands, Father Baldwin, a Roman Catholic missionary who succeeded him there, reported in a master's thesis how the "natives" had reacted to the text of *Argonauts*. Baldwin had lived on the island of Boyowa longer than Malinowski had done and learned the local language more thoroughly. To check the validity of Malinowski's portrayal of the "native's point of view," Father Baldwin translated large portions of *Argonauts* and read those texts with the Boyowans he knew, some of whom remembered Malinowski's presence among them:

> He seems to have left nothing unexplained and his explanations are enlightening, even to the people who live there. It is curious, then, that this exhaustive research, and patient, wise, and honest explanation, should leave a sense of incompleteness. But it does. I feel that his material is still not properly digested, that Malinowski would be regarded in some ways naive by the people he was studying. . . .

> I was surprised at the number of times informants helping me with checking Malinowski would bridle. Usually when a passage has been gone over more than once, they would say it was not like that. They did not quarrel with facts or explanations, but with the coloring

as it were. The sense expressed was not the sense they had of themselves or of things Boyowan. (Baldwin, n.d., pp. 17–18, as cited in M. Young, 1979, pp. 15–16)

Vine deLoria, a Native American, was more harsh in his criticism of American anthropologists, in a book evocatively titled *Custer Died for Your Sins* (1969). He characterized Amerindian studies done by American anthropologists as ethnocentric and implicitly colonialist. Sociological community studies also drew negative reactions from the "natives." Some small-town residents in rural New York were deeply offended by the monograph titled *Small Town in Mass Society* (Vidich & Bensman, 1958; see Vidich's discussion of this reaction in Vidich & Bensman, 2000, and in F. Young, 1996). They castigated the authors for inaccuracy, for taking sides in local disputes, and for violating the confidentiality of individuals (e.g., there being only one mayor, his anonymity was compromised even though his name was not used; this later became a classic example of ethical difficulties in the conduct of qualitative research and its reporting). The rise of Black Nationalism in African American communities in the late 1960s (and the reaction of African American scholars to the "blame the victim" tone of studies about inner-city families such as that of Moynihan, 1965) gave further impetus to the contention that only "insiders" could study fellow insiders in ways that would be unbiased and accurate.

This directly contradicted the traditional view that an outsider researcher, with enough time to develop close acquaintance, could accurately observe and interpret meaning, without being limited by the insider's tendency to overlook phenomena so familiar they were taken for granted and had become invisible. As the anthropologist Clyde Kluckhohn (1949) put it in a vivid metaphor, "It would hardly be fish who discovered the existence of water" (p. 11).

This was not only a matter of inaccurate conclusions—it also had to do with the power relations that obtained in the conduct of "participant observation" itself. Various feminist authors, in a distinct yet related critique of standard anthropology and sociology, pointed out that fieldworkers should attend to their own mentality/subjectivity as a perceiving *subject* trying to make sense of others' lives, especially when power relations between the observer and the observed were asymmetric. An early instantiation of these perspectives was Jean Briggs's (1970) study of her conflicting relationships with a Canadian Inuit nuclear family with whom she lived during fieldwork. Titled *Never in Anger*, her monograph reported in first person and placed her self and her reactions to her "informants" centrally in the narrative picture her monograph presented.

The notion that the researcher always sees from within (and is also blinded by) the power relationships between them and those they study was pointedly explicated in Dorothy Smith's (1974) essay "Women's Perspective as a Radical Critique of Sociology." That idea continues to evolve in feminist criticism (see, e.g., Harding, 1991; Lather, 1991) that advocates reflexivity regarding the personal standpoints, the positionality, through which the fieldworker perceives—gendered, classed, age-graded, and raced/ethnicized ways of seeing and feeling in the world, especially as these are in part mutually constructed in the interaction that takes place between the observer and observed.

George Marcus and James Clifford (Clifford, 1988; Clifford & Marcus, 1986) extended this line of criticism in the mid-1980s, a period when Malinowski became a prime target for those who considered conventional "participant observation" to be deeply flawed. With the publication of his *Diary*, Malinowski had become an easy target. The diary had unmasked power relationships that his ethnographic reporting had disguised. Thus, Malinowski's portrayal of the "native's point of view" in *Argonauts* may have had to do with the power relationships of his fieldwork. He does not mention this in his discussion of his fieldwork method; rather, he portrays himself simply (and innocently) as a detective, a Sherlock Holmes searching avidly for clues concerning native customs and character:

It is difficult to convey the feelings of intense interest and suspense with which an ethnographer enters for the first time the district that is to be the future scene of his field work. Certain salient features characteristic of the place had once riveted attention and filled him with hopes or apprehensions. The appearance of the natives, their manner, their types of behavior, may augur well or ill for the possibilities of rapid and easy research. One is on the lookout for the symptoms of deeper sociological facts. One suspects many hidden and mysterious ethnographic phenomena behind the commonplace aspect of things. Perhaps that queer looking, intelligent native is a renowned sorcerer. Perhaps between those two groups of men there exists some important rivalry or vendetta, which may throw much light on the customs and character of the people if one can only lay a hand upon it. (Malinowski, 1922, p. 51)

From the diary (Malinowski, 1967), a very different voice sounds—boredom, frustration, hostility, lust.

December 14, 1917: "When I look at women I think of their breasts and figure in terms of ERM [an Australian woman who he later married]." (pp. 151–152)

December 17, 1917: "I was fed up with the niggers and with my work." (p. 154)

December 18, 1917: "I thought about my present attitude toward ethnographic work and the natives, my dislike of them, my longing for civilization." (p. 154)

What went without mention was the asymmetry in power relationships between Malinowski and those he studied. He was the primary initiator of actions toward those around him. Years later, working with the same informants, Father Baldwin (n.d.) reported,

It was a surprise to me to find that Malinowski was mostly remembered by the natives as a champion ass at asking damn fool questions, like "You bury the seed tuber root end or sprout end down?". . . They said of him that he made of his profession a sacred cow. You had to defer though you did not see why. (p. 41, as cited in M. Young, 1979, p. 15)

In contrast, Malinowski's tone in the original monograph suggests a certain smugness and lack of self-awareness: "In fact, as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, unnecessary evil or nuisance, mitigated by donations of tobacco" (Malinowski, 1922, p. 8).

Admittedly, the alienation Malinowski revealed in the diary was not unique to him. As M. Young (1979) puts it,

It is only fair to point out that the chronic sense of alienation which permeates the diary is a common psychic experience of anthropologists in the field, and it is intensified by homesickness, nostalgia, loneliness, and sexual frustration, all of which Malinowski suffered in full measure. (p. 13)

That is humanly true, but it does not square with the popular image of the scientist—rather, it puts the professional social scientist on the same plane as the practical social actor (see Garfinkel, 1967; Latour & Woolgar, 1979; Lynch, 1993). Furthermore, it makes one distrust the dispassionate tenor of what Rosaldo (1989, p. 60) called "distanced normalizing description" in ethnographic research reporting.

Malinowski—and the overall credibility of ethnographic research reporting—was further undermined by similar criticism of Margaret Mead. Her first published study, titled *Coming of Age in Samoa* (Mead, 1928), had considered the experience of adolescence from the culturally relativist perspective of her teacher, Boas. Interviewing young Samoan girls and women, Mead concluded that their adolescent years were not emotionally turbulent and that, unlike American teenagers, they were able to engage in sexual experimentation without guilt. Her book attracted a wide popular audience and, together with subsequent popular writing, established Mead's reputation in the United States as a public intellectual. Derek Freeman (1983), an Australian anthropologist, waited until after Mead's death to publish a scathing critique of Mead's research in Samoa. He claimed that Mead had been naive in believing what her informants told her; that they had exaggerated their stories in the direction she had signaled that she wanted to hear. Subsequent consideration suggests that Mead's interpretation was correct overall (see, e.g., Shankman, 1996), but the highly authoritative style of Mead's text (and the lack of systematic presentation of evidence to support the claims she was making) left her vulnerable to the accusation that she had got her findings wrong.

Were all ethnographers self-deceived—or, worse, were many of them "just making things up"? The Redfield–Lewis controversy—two vastly different descriptions of the same group—raised an even deeper question: Do the perspective, politics, and ideology of the observer so powerfully influence what they notice and reflect on that it overdetermines the conclusions drawn? Realist general ethnography was experiencing heavy weather indeed.

One line of response to these doubts was the "better evidence" movement already discussed. Somewhat earlier, another stream of work had developed that led to participatory action research or collaborative action research. In this approach, outside researchers worked with members of a setting to effect change that was presumed to be of benefit there—for example, improvements in public health, agricultural production, the formation of cooperatives for marketing, and the organization of work in factories. Research efforts accompanied attempts at instituting change, as in the study of local community health practices and beliefs within a project aimed to prevent cholera and dysentery by providing clean water. The social psychologist Kurt Lewin (1946) was one of the pioneers of these attempts, focusing especially on labor–management relations in England. The attempts in England spread through trade union channels into Scandinavia (see Emery & Thorsrud, 1969). Another pioneer was Whyte, working in industrial settings in the United States (see Whyte et al., 1989).

Also in the period immediately before and after World War II, anthropologists were undertaking change-oriented research overseas, and as noted earlier, the Society for Applied Anthropology was founded in 1948. During the 1960s and 1970s, applied anthropologists and linguists worked in action projects in the United States and England in ethnic and racial minority communities (e.g., Gumperz et al., 1979; J. Schensul & Schensul, 1992).

One approach to justification for applied research harked back to the "better evidence" movement: Through a researcher's "involvement in the action" (S. Schensul, 1974), the accuracy and validity of evidence collection and analysis are tested in conditions of natural experimentation.

Another justification for applied research had to do with the explicit adoption of value positions by action researchers and their community partners. This is similar to the "critical" position in social research that especially took hold in the 1970s and 1980s, and as action research progressed, it combined increasingly with the various critical approaches discussed in the previous section (for elaboration, see Kemmis & McTaggart, 2005).

This aspect of action research led away from the stance of cultural relativism itself—from even the appearance of value neutrality—toward value affirmation. In research efforts to effect

social change, explicit value commitments had to be adopted if the work was to make change in specific directions. This was called critical ethnography, related to the "critical theory" perspective articulated by the Frankfurt school. Theodor Adorno and Max Horkheimer had developed a critique, based in neo-Marxist social analysis, of both capitalism and fascism. The point was to criticize whatever material or cultural influences might lead people to take actions or support actions that resulted in limiting their own life chances—that is, their collusion in their own oppression. In Marxist terms, one could say that critical theory made visible social processes that worked against the class interests of those being dominated—for example, U.S. white workers supporting an oligarchy that oppressed both them and Black workers. Culturally relativist ethnography had not called domination by that name, nor had it named suffering as an object of attention and of description. Critical ethnography claimed to do just that, and in so doing, the ethnographer stepped out of a defended position of value neutrality to one of vulnerability, shifting from distanced relations with informants to relations of solidarity. This was to engage in social inquiry as ethnography "that breaks your heart" (Behar, 1996).

The adoption of an explicit value position created a fixed fulcrum from which analytic leverage could be exerted in distinguishing between which everyday practices led to an increase or a decrease in life chances (see Bredo & Feinberg, 1982). As the critical ethnography movement developed, the focus shifted somewhat from careful explication of the value yardsticks used to judge habitual practices to claims about domination and oppression as if the inequity involved was self-evident. There was a push back from the earlier generation of scholars, who accused critical ethnographers of letting their values so drive their fieldwork that they were able to see only what they expected to see, ignoring disconfirming evidence.

As critical ethnographers identified more and more kinds of inequity, it became apparent that social criticism itself was relative depending on which dimension of superordination/ subordination was the locus for analysis. If it was economic relations, then processes of class-based oppression appeared most salient; if gender relations, then patriarchal processes of domination; if postcolonial relations, the survivals of "colonized" status; if sexual identification, then heterosexual domination. And if race became the primary fulcrum for critical social analysis—race, as distinct from, yet as linked to class, gender, colonization, or sexuality—then racial privilege and disprivilege occupied the foreground of attention, with other dimensions of inequity less prominent. Arguments over whose oppression was more heinous or more fundamental—"oppresseder than thou"—took on a sectarian character.

There was also a new relativity in the considerations of the seats of power itself, its manifestations in various aspects or domains, and the ways in which existing patterns of life (including patterns of domination) are reproduced within and across successive generations. Marxism had explained social order as a forcefield of countervailing tensions that were the result of macrosocial economic forces. Structural functionalism in anthropology and sociology had explained social order as the result of socialization of individuals, who followed systems of cultural rules. Structuralism in anthropology and linguistics had identified cultural rule systems, which appeared to operate autonomously according to inner logics that could be identified and specified by the social scientist. All these approaches treated macrosocial structures as determining factors that constrained local social actors. Poststructuralist critiques of this top-down determinism developed. One line of critique stressed the opportunistic character of the everyday practices of local social actors, who as agents made choices of conduct within sets constrained by social processes (i.e., "structures") operating at the macrosocial level (e.g., Bourdieu's [1977] critique of Lévi-Strauss's structuralism and Garfinkel's [1967] critique of Parsons's structural functionalism). Another line of critique (Foucault, 1977) showed how power could be exercised

over local social actors without physical coercion through the knowledge systems that were maintained discursively and through surveillance by secular "helping" professions—the modern successors of premodern religion—whose ideologically ratified purpose was to benefit the clients they "served" by controlling them—medicine, psychiatry, education, and modern prisons. Foucault's notion of discourse as embodied in the conventional common sense of institutions is akin to Gramsci's (1988) notion of "cultural hegemony"—again, an ideological means by which control can be exercised nonviolently through commonsense rationalization justifying the exercise of such power. Power and social structure are thus seen to be strongly influential processes, even though the influence is partial, indirect, and contested—local actors are considered agents, not simply passive rule followers, yet they are agents who must swim in rivers that have strong currents.

At the same time, historians began to look away from the accounts of the past that were produced by the powerful (rich, literate, Caucasian, male, or any combination of those traits) and began to focus more centrally on the daily life practices of people whose subaltern "unwritten" lives could fly, as it were, below the radar of history. (This was a challenge to the accounts of orthodox historians who stuck to the conventional primary source materials.) An additional line of criticism of the authoritativeness of texts, which was once taken for granted, came from postmodern scholars (e.g., Derrida, Lyotard, Deleuze) who questioned the entire Enlightenment project of authoritative academic discourse concerning human activity, whether this discourse manifested in the arts, in history, or in social science.

With roots in the early modernism of the Enlightenment, all these discourses attempted to construct "master narratives" whose credibility would be robust because they were based on reason and evidence. For the postmodernists, the rhetorical strategies that scholarly authors used to persuade readers of their text's accuracy and truthfulness could be unmasked through a textual analysis called *deconstruction*. Critical ethnography had challenged the authority of realist narrative accounts that left out explicit mention of processes of conflict and struggles over power; the postmodern line of criticism challenged the fundamental authoritativeness of texts per se. Moreover, lines of demarcation between qualitative social inquiry and scholarship in the humanities were dissolving. Approaches from literary criticism—outside the boundaries of mainstream social science—were used both in the interpretist (hermeneutic) orientation in ethnography and in the critical scrutiny of scholarly texts by means of deconstruction.

One of the ways to demystify the text of a qualitative research report is to include the author (and the author's "standpoint" perspectives) as an explicit presence in the fieldwork. The author becomes a character in the story being told—perhaps a primary one—and much or all of the text is written in first-person narration using past tense rather than the earlier ethnographic convention of present-tense narration, which to critics of realist ethnography seemed to connote timelessness—weightless social action in a gravitationless world outside history and apart from struggle. This autobiographical reporting approach came to be called *autoethnography*. Early examples of the approach have already been mentioned: the fiction of Bohannon (Bowen, 1954) and the first confessional ethnographic monograph by Jean Briggs (1970). Later examples of autoethnographic reporting include Rabinow (1977) and Kondo (1990)—see also the comprehensive discussion in Bochner and Ellis (2002).

Another approach toward alteration in the text of reports came from attempts to heighten the dramatic force of those texts, making full use of the rhetorics of performance to produce vivid kinds of narration, for example, breaking through from prose into poetry or adopting the means of "street theater," in which scripted or improvised dramatic performances were presented. Ethnographers have sometimes been invidiously called failed novelists and poets because

their monographs typically did not make for compelling reading. By analogy with performance art, the new performance ethnography sought to employ more audience-engaging means of representation (see Callier & Hill 2019; Conquergood, 1989, 2000; Conquergood & Johnson 2013; Denzin, 2003; Madison, 2006; Madison & Hamera, 2006). Examples of arts-based representation approaches are also found in the work of Richardson (2004, 2007; see also the discussions in Richardson, 1999), Bochner and Ellis (2003), Adler and Adler (2008), and the edited volume by Cahnmann-Taylor and Siegesmund (2008).

Currently, new kinds of authors appear. Indigenous research perspectives and methods are espoused and practiced by members of communities formerly studied as "others" by "outsiders" (see Brayboy & Maughan 2009; Kovach, 2010; Medin & Bang 2014; Tuhiwai Smith, 2013; Tuhiwai Smith et al., 2019; e.g., Wilson, S. 2008). Practitioner research continues to be done, often as participatory action research, and increasingly this is done by youth as researchers and authors (see Cammarota & Fine, 2008; Paris & Winn, 2013).

Qualitative inquiry has been increasingly employed in communication and discourse studies: in the interdisciplinary fields of interactional sociolinguistics and conversation analysis (Sidnell & Stivers, 2012; Tannen et al., 2015), critical discourse analysis (Fairclough, 2003), and "multimodal" studies of meaning making in "embodied" social interaction (Bezemer & Jewitt, 2010; Goodwin, 2017; Goodwin & Cekaite 2018; Streeck et al., 2011). Often these approaches use video or audio recording as a primary data source. Indeed, there is burgeoning interest in video-based studies of communication and interaction, as indicated by the establishment of new journals (e.g., *Communication and Medicine* and *Social Interaction: Video-Based Studies of Human Sociality*). (A special issue of the latter [2021, 4(2)] is titled "Researchers' Participation Roles in Video-Based Fieldwork.") In addition to visual image-based research done by professional scholars, within participatory action research, there is increasing use of visual images produced by community members themselves to document their lived experience (see the discussions of "Photovoice" in Cataleri & Minkler, 2010; Wang & Burris, 1997; see also Jones & Raymond, 2012; Milne et al., 2010). Body camera and dashboard camera recording by police produces images that can subsequently be analyzed (Raymond et al., 2022), and there is increasing research use of video footage circulated on broadcast media and the Internet, as in the recent discourse study of televised cooking shows by Tovares and Gordon (2021). (Further implications of the normalization and democratization of photography and video recording will be discussed in the next to last section of this chapter, titled "The Current Scene.")

Classic and more innovative approaches to qualitative inquiry have been extensively reviewed in the five successive SAGE handbooks on qualitative research methods edited by Denzin and Lincoln (1994, 2000, 2005, 2011, 2017).

Qualitative Inquiry in Educational Research

The authority of realist ethnography was beginning to be challenged at the very time when qualitative research approaches developed in certain fields of human services delivery, especially in education. By the 1950s, a subfield of anthropology of education was forming (Spindler, 1955, 1963). Henry (1963) published chapter-length accounts of elementary school classrooms that were highly critical of the practices used to encourage competition among students. The first book-length reports, modeled after the writing of ethnologists and anthropologists, were L. Smith and Geoffrey's (1968) *The Complexities of an Urban Classroom* and Jackson's (1968) *Life in Classrooms.* Also in 1968, the Council on Anthropology and Education was founded within the American Anthropological Association. Its newsletter developed into a journal in 1973, the

Anthropology and Education Quarterly, and for a time, this was the primary journal outlet for qualitative studies in education in the United States. Spindler became the editor of a series of overseas ethnographic studies of educational settings, published from the 1960s to the late 1980s by Holt, Rinehart, and Winston. (For recent overviews of the field of anthropology and education, see Henze [2020] and a bibliography of the field by McCarthy and Erickson [in press].)

In England, qualitative inquiry was pioneered by educational evaluation researchers with an orientation from sociology and action research. At CARE, Laurence Stenhouse formed a generation of evaluators who studied schools and classrooms by means of participant observation and who wrote narrative research reports (see, e.g., in chronological order, Walker & Adelman, 1975; Adelman, 1981; Kushner et al., 1982; Kushner, 1991; Torrance, 1995). Various sociologists also engaged in qualitative educational research. In 1977, Willis published *Learning to Labour.* See also Delamont (1984, 1989, 1992) and Walkerdine (1998). Following in the tradition of Henry and Spindler in the United States and the "new sociology of education" in England, many of these studies focused on aspects of the "hidden curriculum" of social relations and values socialization in classrooms.

Because of the "objectivist" postpositivist tenor of mainstream educational research, this early work in education anticipated to some extent the criticisms of ethnographic authority that developed in anthropology in the late 1970s and early 1980s. In defense, the early qualitative researchers in education took pains to present explicit evidence; indeed, some of them had come out of the "better data" and "hyphenated subfields" movements in anthropology or the ethnomethodological critique of mainstream work in sociology.

In the United States, qualitative approaches began to be adopted within research on subject matter instruction—initially in literacy studies (Heath, 1983) and social studies. Some of this work derived from the ethnography of communication/sociolinguistics work begun in the 1960s. As portable video equipment became available, classroom participant observation research was augmented by audiovisual recording (Erickson & Shultz, 1977/1997; McDermott et al., 1978; Mehan, 1978). A literature on classroom discourse analysis developed, involving transcriptions of recordings of speech (see Cazden, 2001). Initially focused on literacy instruction, after the mid-1980s, this approach was increasingly used in studies of "teaching for understanding" in mathematics and science that were funded by the National Science Foundation (NSF) in the United States, and that tendency has increased up to the present time. A new approach to learning theory that was informed by neo-Vygotskyan "cultural historical activity theory" in cultural psychology (see Cole et al., 1997; Lave & Wenger 1991) considered learning as changing participation within local communities of practice—moving over time from peripheral participation to more central participation. This "situated" perspective on learning invited qualitative study of learning environments inside and outside classrooms, and especially through National Science Foundation funding in the United States, it has led to the creation of a new field, "learning sciences," as an alternative to the postpositivist approaches of more conventional educational psychology. The field now has its own journal, the *Journal of the Learning Sciences.* There is also a resurgence of interest in the roles of culture in learning and learning environments. Recent examples are a magisterial collection edited by Nasir et al. (2020) titled *Handbook of the Cultural Foundations of Learning* and a special issue of the *Anthropology and Education Quarterly* titled "Revisiting the Anthropology of Learning" (Erickson & Espinoza, 2021). In addition, there has been increasing interest internationally in ethnographic studies of educational processes, especially in Latin America but also in Europe, Africa, and East Asia (see, e.g., Anderson-Levitt, 2011; Rockwell & Gomes, 2009).

Methods texts began to appear in the late 1970s and early 1980s, explaining to postpositivist audiences of educational researchers how qualitative research could be rigorous and systematic:

Guba (1978), Bogdan and Biklen (1982), and Guba and Lincoln (1985); see also J. Schensul et al. (1999). Erickson's (1986) essay on interpretive qualitative research on teaching appeared in a handbook sponsored by the American Educational Research Association, and that discussion came to be widely cited in educational research. Preceded by a meeting in 1978 at which Mead was the keynote speaker, shortly before her death, and established as an annual meeting 2 years later, the Ethnography in Education conference at the University of Pennsylvania soon became the largest gathering of qualitative educational researchers in the world, surpassed in scale only recently by the International Congress of Qualitative Inquiry at the University of Illinois, Urbana. Also in the 1980s, a movement of practitioner research in education developed in the United States, principally as teachers began to write narrative accounts of their classroom practice (see Cochran-Smith & Lytle, 1993). This was related to participatory action research with teachers and administrators (see the discussion in Erickson, 2006). Since then, there have been increasing efforts involving school students in participatory action research and in auto-ethnographic accounts that communicate their lived experience (see Cammarota & Fine, 2008; Cook-Sather, 2018; Groundwater-Smith & Mockler, 2015; Howard, 2001; Mirra et al., 2015).

As noted above, by the early 1990s, qualitative research on subject areas in both the humanities and science/mathematics had become commonplace, whereas 20 years earlier, it had been very rare. Video documentation was especially useful in the study of "hands-on" instruction in science and in the use of manipulables in teaching mathematics instruction (see Goldman et al., 2007). Increasingly, the subject matter studies—especially those supported by NSF funds—focused on the "manifest curriculum" rather uncritically. This tendency was counterbalanced by the adoption of "critical ethnography" by some educational researchers (e.g., M. Fine, 1991; Kincheloe, 1993; Lather, 1991; McLaren, 1986).

In a number of ways, qualitative inquiry in education anticipated and later ran in parallel with the shifts taking place within recent qualitative work in anthropology and sociology. From the outset of qualitative inquiry in education, its research subjects—school teachers, administrators, parents—were literate, fully able to read the research reports that were written about them, and capable of talking back to researchers using the researchers' own terms. The "gaze" of educational researchers—its potential for distorted perception and its status as an exercise of power over those observed—had been identified as problematic in qualitative educational inquiry before social science disciplinary critics such as Clifford and Marcus (1986) and Van Maanen (1988, 2006) had published on those matters. Also, action research and practitioner research—involving "insiders" in studying and reflecting on their own customary practices—had been done by educational researchers before such approaches were attempted by scholars from social science disciplines.

Today, there is a bifurcation in qualitative educational studies—with subject matter–oriented studies, on one hand, and critical or postmodern studies, on the other. In effect, this results in a split between attention to issues of manifest curriculum and hidden curriculum. Ironically, as the authority of realist ethnography was increasingly challenged within sociology and anthropology, "realist" work in applied research in education, medicine, nursing, and business came to be the most valued, as will be discussed further in the next section.

THE CURRENT SCENE

Currently, there appear to be seven major streams of qualitative inquiry: a continuation of realist ethnographic case study, a continuation of "critical" ethnography, a continuation of participatory action research, "Indigenous" studies done by "insiders" (including practitioner research in

education), autoethnography, performance ethnography, and further efforts along postmodern lines, including literary and other arts-based approaches.

At the outset of this chapter, I mentioned six foundational "footings" for qualitative inquiry, each of which has been contested across the course of the development of such inquiry: (1) disciplinary perspectives in social science, (2) the participant-observational fieldworker as an observer/author, (3) the people who are observed during the fieldwork, (4) the rhetorical and substantive content of the research report, (5) the audiences to which such reports have been addressed, and (6) research worldview—ontology, epistemology, and purposes.

As the social sciences began to develop along lines of natural science models, its social theory orientations (social evolution, then functionalism combined with cultural relativism) were seen to justify data collection and analysis as a "value-neutral" enterprise. That stance was challenged by conflict-oriented social theory, with the research enterprise redefined as social criticism. Today, the possibility of valid social critique is itself questioned by postmodern skepticism about the authoritativeness of scholarly inquiry in general, and core organizing notions taken from arts and humanities disciplines inform much new qualitative research. Sociology and anthropology are no longer the foundational "homes" for social and cultural studies.

Formerly, an "expert knowledge" model of the social scientist was seen as justifying long-term firsthand observation and interviewing—"fieldwork"—that was conducted autonomously by a researcher, an outsider to the community being studied, who operated in ways akin to those of a field biologist. Today, the adequacy and legitimacy of that researcher stance have been seriously challenged, with many researchers allying themselves as advocates (collaborators/joint authors/ editors) with the people who are studied, or researchers being members themselves of the communities whose everyday lives and meaning perspectives are being studied through qualitative inquiry. Thus, the roles of "researcher" and "researched" have been blended in recent work.

The research report was formerly considered an accurate, realistic, and comprehensive portrayal of the lifeways of those who were studied, with an underlying rhetoric of persuasion as to the realism of the account. Today, qualitative research reports are often considered partial— renderings done from within the standpoints of the life experience of the researcher. The "validity" of these accounts can be compared with that of novels and poetry—a pointing toward "truths" that are not literal; fiction may be employed as a means of illuminating interpretive points in a report.

Initially, the audiences of such reports were the author's scholarly peers—fellow social scientists—and rarely those who were studied. Today, those who are studied are expected to read the report—and they may also participate in writing it. Moreover, in practitioner research, action research, and advocacy research, research reports—presented in various media and representational genres—may also address popular audiences.

This is a story of decentering and jockeying for position as qualitative inquiry has evolved over the past 125 years. Today, there is an uneven pattern of adoption and rejection of the newer approaches in qualitative inquiry. In applied fields, such as education, medicine, and business, "realist" ethnography has gained wide acceptance, while more recently developed approaches have sometimes been adopted (especially in education) and sometimes met with skepticism or with outright rejection. In anthropology, heroic "lone ethnographer" fieldwork and reporting, after the self-valorizing model of Malinowski, has generally gone out of fashion. In sociology, the detached stance of professional researcher has also been seriously questioned, together with the realist mode of research reporting.

The differences among qualitative researchers, and between them and others who engage in social and cultural inquiry, go beyond research technique to fundamental assumptions about

the purposes and conduct of research—to worldviews that undergird differing communities of research practice and that are often adopted unreflectively by newcomers to a community of research practice. These sets of assumptions have been characterized as "paradigms" in relationships of conflict and succession (e.g., Guba, 1990; Guba & Lincoln, 2005), following the argument in philosophy of science presented by Kuhn (1962); as alternative *chronotopes* (Kamberelis & Demetriadis, 2015), a term from the literary theory of Bakhtin (1981); or as Discourse Formations, in the sense presented by Foucault (1972). The simpler term *worldview* captures the basic sense of this as a matter of ontology, the theory of being (in social inquiry, consideration of the fundamental nature of social and cultural processes). Ontology is considered in philosophy as a branch of metaphysics—and rejection of metaphysical speculation is a hallmark of the Enlightenment intellectual tradition and its descendants in logical positivism and analytic philosophy. It was thought that "science" (and social science) could do without metaphysics, but contemporary philosophy of science suggests that metaphysical assumptions—worldviews—are profoundly constitutive of scientific research approaches, shaping what inquiry makes visible and what it makes transparent—taken for granted without reflection.

These differences in ontology in social inquiry go beyond the first-level distinction between realism and idealism (the realist assumption that there is a social world independent of our knowing and the idealist/social constructionist assumption that the social world cannot be apprehended independent of the perspectives and interests—including power interests—of the researcher). Whether one is a realist or idealist, there remain questions about what the social world we are trying to study is like—is it uniform and relatively stable from one place and time to another, or is it labile and, if so, how does it vary, how often, how quickly? There are also questions of epistemology in social inquiry—assumptions about the nature of knowledge and knowing. Can we as inquirers, in the midst of all the noise in the world outside us and within us, have relatively consistent knowledge of the social world, or is that world and our apprehension of it so mutually constitutive, so confounded, that no consistency or confidence in our knowing is justifiable?

It is the latter view that was developing powerfully in the last third of the past century and the first two decades of the current century—deep "postmodern" distrust of what was seen as the overconfidence of modernism—the Enlightenment heritage. The Enlightenment project presumed an autonomous human subject (*vide* Descartes, Locke) capable of understanding the physical and social world that stands outside the subject through careful logic and empirical "scientific" inquiry. That "modernist" ontology and epistemology was fundamentally challenged in the last decades of the 20th century, especially by French "poststructuralist" philosophers, among them Foucault (1972), Derrida (1974), and Deleuze (Deleuze & Guattari, 1987). This "postmodern" turn has influenced a call for "postqualitative" and "posthumanist" inquiry (Lather & St. Pierre, 2013; St. Pierre, 2011, 2014). Increasing interest in this approach is leading to what can be called a "postqualitative turn" (see discussion in St. Pierre [2019] and the special issue of *Qualitative Inquiry* titled "Global Perspectives on the Post-Qualitative Turn in Qualitative Inquiry" [St. Pierre, 2021]). See also the discussions of "new materialism" by Coole and Frost (2010) and MacLure (2013).

In arguing for succession beyond what can be called "humanist qualitative inquiry," St. Pierre (2014, pp. 14–15) observes that an ontological implication of the deconstructive critiques of poststructuralists is that the foundational notion of the "humanist knowing subject" as an autonomous and constant individual self is an intellectual inheritance from the Enlightenment that can no longer be considered tenable. That is a point well taken, but it should be noted that an autonomous knowing subject is not something first questioned by such postmodernists as

Foucault and Deleuze. Rather, challenges to that conception appeared well before postmodernism did—for example, Boas's notion of culture deeply influencing/defining the individual, G. H. Mead's notion of the social generation of the self (and Dewey on "trans-action"), and, for that matter, Marx long ago and Althusser more recently.

Presently, the situation can be described as tension between two extremes, with a middle ground being pulled in opposite directions. In the middle is interpretive ethnography and critical ethnography, both somewhat more reflexive than in the past and both somewhat less critical of each other than in the past. (It is as if, in the presence of increasing countervailing pressure from the edges, critical ethnographers are realizing that the interpretive approach was somewhat more "critical" than had been initially realized [in that interpretation denaturalizes social processes just as does more explicitly critical inquiry], and interpretive ethnographers are coming to recognize an underlying commonality with their more explicitly "critical" colleagues.)

From one direction, the deep postmodern distrust of essentializing authoritative discourse continues, deriving in part from an antirealist ontology. If indeed the social world, the human subject, and the discourses through which we try to describe and understand them are entirely socially constructed and continually in flux, then notions of "rigor," "data," "ruling out competing interpretations," and the like are hopelessly inappropriate—vestiges of a "hard science" conception of social inquiry that is no longer credible. From another direction, there is tremendous pressure on the middle ground to become more "scientific" in a narrow sense—more rigorous, more careful with evidence, more consistent with a postpositivist ontology and epistemology that presumes that the social world consists of relatively stable entities whose interrelations, while not directly knowable, can be discovered by the use of research procedures that are similar to those employed in the physical sciences. A presumption is that all "Science"—systematic inquiry—is fundamentally the same, regardless of the domain it investigates.

This pressure toward a "hard science" approach to qualitative inquiry comes from the partial acceptance of qualitative research in applied fields with a strong tradition of what is sometimes called postpositivist inquiry (e.g., education, medicine, and business, as mentioned earlier), and in a larger sense, that pressure comes from partial acceptance in policy research more generally. As qualitative researchers seek more "relevance" and try to conduct policy-oriented research, they confront a policy discourse developed over the past 40 years whose conventional wisdom is grounded in "hard science" assumptions regarding research ontology, epistemology, and methodology. This is a "Discourse," in a Foucauldian sense, that defines the basic questions regarding social policy as those of effectiveness and efficiency (Cochrane, 1972/1989) and presumes that the provision of social services can best be achieved by "evidence-based practice." It has been described as an "audit culture" (Strathern, 2002) with now worldwide provenance in public management (Barzeley, 2001). The "gold standard" for evidence on which to base practice in service delivery has for some time been seen to be large-scale experimental field trials modeled after clinical trials in medicine in which research subjects are randomly assigned to differing conditions: "treatment" or "control" (i.e., no treatment) (Shadish et al., 2002). Despite considerable criticism that these trials often do not provide clear evidence of a causal relation between the *treatment* (conceived and operationally defined as a unitary independent variable) and its *effect* (operationally defined as a unitary dependent variable), these "randomized controlled trials" (RCTs) continue to be highly regarded as sources of evidence for determining social policy (see the extended discussion in Flyvbjerg [2006] and Torrance [2015]). Other quantitative approaches (e.g., multilevel regression analysis) and structural equations modeling also are seen as producing strong evidence of cause, in the Humean sense of regular association among entities. In attempting to enter the policy research arena, qualitative researchers experience intense

pressure to look more "scientific." One could say that "policy relevance" becomes a devil's bargain—the temptation of Faust—or a tar baby, as in the Uncle Remus story of Bre'r Rabbit—once you touch it, the more stuck to it you become.

In the same time period as the growth of "audit culture" perspectives in policy discourse, two other influences on qualitative research have developed. One is the increase of "mixed-methods" approaches—attempts to combine inferential statistical analysis with narrative description based on participant observation and interviewing (Cresswell & Plano Clark, 2011; Greene, 2007; Johnson et al., 2007; D. Morgan, 2014). The aim is to take advantage of the distinctive affordances in multiple research approaches, achieving "the best of both worlds." A criticism is that, in practice, what happens is that one approach dominates in the study at the expense of the other—often it is the quantitative "hard science" approach that dominates because that is what receives the most financial support by major funders of social research. (A colleague of mine told me of a comprehensive study of multiple inner-city schools in a major American city in which she was asked as a doctoral student research assistant to make brief visits to some of the schools and write narrative vignettes to illustrate patterns that had been discovered through large-scale statistical analyses. She said that what she was required to do felt like "drive-by ethnography.") Even in projects designed in a more even-handed way, the purposes and worldview of "hard science" and Diltheyan "human science" approaches may—for certain research topics—fit only awkwardly together or indeed be antithetical.

Another influence on qualitative research, related to but distinct from the "multimethod" movement, is that of formal "coding" of qualitative "data." Coming initially from the "constant comparison" and "grounded theory" approach to qualitative data identification and analysis developed by Anselm Strauss and his associates (see Strauss, 1987; Strauss & Corbin, 1990), computer-based schemes for data analysis have proliferated in the past 20 years. More recently, critiques of the earlier formulation have developed within the grounded theory movement— a "constructivist" critique (e.g., Charmaz, 2006) and a "situational" critique (e.g., Clarke et al., 2017). A quick Internet search for "qualitative data analysis software" produces numerous items, and the following list is not exhaustive: Ethnograph, Atlas.ti, MAXQDA, Nvivo, QDA Miner, Provalis research.com, Dedoose, Domo, and Alteryx. Some of the software is available at no cost, but many of the software packages are commercially produced and are advertised as a solution to the problems of qualitative data discovery and analysis—a process that often seems daunting at the outset, especially to beginning researchers. A criticism is that "coding" judgments (assigning function class labels to single lines of text in a corpus of field notes or in a set of interview transcripts) are asked by the software system to be done at an initial stage of review of information sources. This appears to simplify "data analysis," but it raises problems of premature typification and premature closure in the analysis process (see the discussion in Sipe & Ghiso [2004] and the critical rejoinder by Erickson [2004]). However, the software programs present the appearance of being "systematic," and that has had appeal for "hard science"—oriented researchers and for the funders of research, whose requests for proposals now often ask the proposer to identify a software program to be used in "qualitative data analysis."

That the "hard science" pressures on qualitative inquiry can be a slippery slope is illustrated by an example from educational research arguments in the United States. While realist ethnography was officially accepted as legitimately "scientific" in an influential report issued by the National Research Council (Shavelson & Towne, 2002), postmodern approaches were singled out for harsh criticism. The report took the position that science is a seamless enterprise, with social scientific inquiry being continuous in its fundamental aims and procedures with that of natural science. This position was reinforced by a statement by the primary professional society

of researchers in education, the American Educational Research Association. Quoting from the AERA website (www.AERA.net/AboutAERA/Key-Programs/Education-Research-Research-Policy/AERA-Offers-Definition):

AERA Offers Definition of Scientifically Based Research (SBR). Supported by AERA Council, July 11, 2008

The term "principles of scientific research" means the use of rigorous, systematic, and objective methodologies to obtain reliable and valid knowledge. Specifically, such research requires

development of a logical, evidence-based chain of reasoning;

methods appropriate to the questions posed;

observational or experimental designs and instruments that provide reliable and generalizable findings;

data and analysis adequate to support findings;

explication of procedures and results clearly and in detail, including specification of the population to which the findings can be generalized;

adherence to professional norms of peer review;

dissemination of findings to contribute to scientific knowledge; and

access to data for reanalysis, replication, and the opportunity to build on findings.

The statements by the NRC panel and the AERA Council claimed to provide a more broadly ecumenical definition of scientific research than that which some members of the U.S. Congress and their staffs were trying to insist on in developing criteria of eligibility for federal research funding (field-based experimental designs, with random assignment of subjects to "treatment" and "control" conditions). However, AERA's adoption of the "seamless" view of science means that many of the most recent postmodern and arts-based approaches to qualitative inquiry are declared beyond the boundaries of legitimate research. Notice key terms in the AERA statement: reliability, generalizability, replication, reanalysis. This is the language of a "hard science" worldview for social inquiry. The statements by the NRC and AERA show no awareness of an intellectual history of social and cultural research in which, across many generations of scholars, serious doubts have been raised as to the possibility that inquiry in the human sciences should be, or could be, conducted in ways that were continuous with the natural sciences.

The anthropologist Clifford Geertz (2001) warned against such a "broad umbrella" conception of science:

Using the term "science" to cover everything from string theory to psychoanalysis is not a happy idea because doing so elides the difficult fact that the ways in which we try to understand and deal with the physical world and those in which we try to understand and deal with the social one are not altogether the same. The methods of research, the aims of inquiry, and the standards of judgment all differ, and nothing but confusion, scorn, and accusation—relativism! Platonism! reductionism! verbalism!—results from failing to see this. (p. 53)

Somewhere in a middle position between "hard science" qualitative inquiry and its opposite are two related approaches—research as "phronesis" and as "critical realism." The first alternative is presented by the urban planner Bent Flyvbjerg (2001) in the edgily titled *Making Social Science Matter: How Social Inquiry Fails and How It Can Succeed Again.* The book argued for

the use of case study to address matters of value, power, and local detail, as these are pertinent to policy decision-making. What policy makers need in making decisions is not certain knowledge of causal relations that obtain generally, of the sort promised by the advocates of RCTs. Rather, what policy makers need is knowledge of the specific circumstances of the particular situation in which they find themselves. He uses as an example the planning of auto parking and pedestrian mall arrangements in the city of Aalborg, Denmark. To achieve the best traffic solution for Aalborg, one cannot make a composite of what was done in Limerick, Bruges, Genoa, Tokyo, and Minneapolis. To know what is good for Aalborg involves detailed understanding of the history, cultures, demography, economy, geography, and center city architecture of Aalborg itself. Such insight comes from a kind of knowledge that Flyvbjerg calls *phronesis*, action-oriented knowledge of a local social ecosystem (following upon Aristotle's use of the term in the Nichomachean Ethics, Book 6 [Aristotle, 1934]). *Phronesis* is the prudential knowledge of a wise city official, in contrast to *episteme*, the general, invariant, cumulative knowledge of the philosopher or mathematician (what we would now call "hard science" knowledge), or *techne*, the practical operating knowledge of a craftsman.

Another alternative comes from a "critical realist" ontology and epistemology. The ontology is realist, in that it presumes that "there are real objects that exist independently of our knowledge of their existence" (Schwandt, 2007, p. 256). The epistemology is constructivist, presuming that our knowledge of those real objects is never direct but mediated by our concepts and language—and by our practical interests—"What you see is what you intend to do about it" (an observation of the pioneering neuroscientist David Rioch, quoted in Hall, 1992, p. 233). In contrast to Hume's view of cause as a regular association between two entities, critical realism views cause as process—as events, contextually embedded. The aim of that kind of causal analysis is to discover specific causal mechanisms, coming to understand why x causes y in specific circumstances, not simply that x causes y. But the evidence and methods for identifying a process explanation of cause are different from those used to produce a variance (i.e., statistics-based) explanation (see the extended discussion in Gorski, 2013; Maxwell, 2004, 2015; Sayer, 2000).

The approach of critical realist inquiry is similar in spirit to the phronetic inquiry discussed by Flyvbjerg. Both involve understandings of evidence and method that differ from those derived from the physical sciences. But both also differ from the extreme skepticism about the possibility of authoritative discourse in social inquiry that characterizes the postmodern "posthumanists."

Two current phenomena bear on a number of the "footings" of qualitative approaches in social inquiry that have been discussed above; indeed, these new developments can be considered metaphoric in regard to the current situation—rather like a connected set of parables. The first of these phenomena is what might be termed a "cell phone revolution"— the ubiquity of access to video recording and still photography that can be rapidly, inexpensively, and widely shared and stored on the Internet. Audiovisual documentation of scenes and practices of everyday life is no longer the province of the professional videographer or cinematographer (or of the professional social scientist). These media devices, together with still photography, are being used routinely in everyday life by people who are functioning, whether deliberately or nondeliberately, as autoethnographers who are creating documents that show their perspectives on various aspects of their lives.

The other phenomenon that produces audiovisual records of mundane conduct in everyday life can be called a "surveillance revolution." Continuous video monitoring of street traffic and of street and alley life, shopping in stores, and mandated body camera recording and patrol car dashboard camera recording by soldiers and police produces large numbers of video documents

on a daily basis. (As cited earlier, a current example of use of police-recorded footage in the study of police–community relations is presented by Raymond et al. [2022].)

And as noted earlier, the widespread occurrence of video recording has normalized it, in a contemporary process that is akin to the normalization of amateur/family photographic documentation at the turn of the 20th century through widespread use of George Eastman's Kodak box camera. Video recording and its subsequent review is no longer exotic or professional. It no longer stands alongside everyday life but has become a part of it for many people.

Unedited video footage and "selfie" photographs don't simply speak for themselves—they are polysemous and multivocal; hence, their significance as records requires review and interpretation. In a seminal paper titled "Professional Vision," Goodwin (1994) showed how lay audience perceptions of video footage of the police beating of Rodney King as he lay face down on the ground were reinterpreted by a police expert witness in a jury trial to show how King's body movements in response to blows from police batons could be seen as possible evidence of his intent to stand up and attack the police officers—thus, the police officers who at first glance seemed to be brutally mistreating King were actually acting in self-defense. The professionally coded hermeneutic vision of the expert was accepted by the jury as evidence of reasonable doubt concerning the malevolent intent of the police officers. Since then, citizen-recorded video has increasingly appeared as evidence in court cases—the most extremely unambiguous example being a bystander's recording showing the 9 minutes of a Minneapolis police officer's knee pressure on George Floyd's neck, leading to his death by asphyxiation. Almost equally unambiguous was the footage recorded by one of the three perpetrators of Ahmaud Arbery's death in Georgia, showing that Arbery was unarmed and running away from his assailant when he was shot in the back and killed. In much broader scale, video footage and still photographs from cell phones recorded by attacking and defending participants during the mob assault on the U.S. Capitol on January 6, 2021, have been used to identify and charge persons who committed trespass or violence in that confrontation.

Forensic inquiry, courtroom litigation, and jury deliberation can be considered forms of qualitative inquiry—a combination of evidence collection, analysis, and narrative representation based on interpretive judgment. But the evidence derived from visual image review has differing epistemic status in a courtroom from what it might have in a scholar's study. In a trial, the crucial issue is determining the presence or absence of reasonable doubt concerning the culpability of actions and intentions of defendants. The scholar can entertain postmodern skepticism about the existence of "facts," but that does not play well in the venue of a courtroom. Yet as the example from Goodwin shows—in contrast to the examples of Floyd and Arbery—video footage by itself is often partial and ambiguous. In a sense, what is visible from video recording is not that much different from the information available from eyewitness testimony (which can be thought of as analogous to interview comments in a formal research study). In the court trial, the task of a jury and judge is to achieve closure by rendering a definite hermeneutic conclusion.

Legal reasoning and decision-making are no less liable to uncertainty and consequent disputation than are other forms of social inquiry. The same is true for political argument presently, in which accusations of "fake news" are frequent. The discourses of jurisprudence and of public policy debate—and the contentions within those discourses concerning credibility of evidence and warrant for inference—can be seen as running along parallel lines with the current situation of qualitative social inquiry more generally. What counts as "fact" and as reasonable interpretive judgment is bitterly contested in all these arenas.

CONCLUSION

Mark Twain is said to have said, "History doesn't repeat itself—at best it sometimes rhymes." If he was correct, then the proponents of postpositivist social science are in serious trouble. Such inquiry, grounded in what is assumed to be a seamless whole of science, aims to discover general laws of social process that are akin to the laws of physics—that is, an enterprise firmly grounded in prose and in literal meanings of things. That inquiry approach will continue to be controverted by the stubborn poetics of everyday social life—its rhyming; the nonliteral, labile meanings inherent in social action; and the unexpected twists and turns that belie prediction and control, let alone the situatedly privileged position of the human subject as observer/actor within the ongoing flow of everyday life. It may well be that social science will at last give up on its perennially failing attempts to assume that history actually repeats itself and therefore can be studied as if it did. One might think that contemporary qualitative social inquiry would be better equipped than such a prosaic social physics to take account of the poetics of social and cultural processes, and yet qualitative social inquiry expends considerable energy on internecine dispute, with differing approaches vying for dominance.

Kuhn's (1962) philosophy of science claimed that old paradigms defining "normal science" were eventually replaced by newer ones—in a story of revolutionary succession. Some have criticized this view as overly optimistic—since the proponents of "normal science" in any generation cling tenaciously to their existing beliefs, even in the face of contradictory evidence (see, e.g., the discussion in Lakatos & Musgrave, 1970). If it is difficult for a paradigm to be replaced in the physical sciences, this is even more the case for the human sciences. Because history doesn't repeat itself exactly, a crucial study that produces new knowledge forcing a paradigm shift usually can't be conducted in the first place, let alone be consistently "replicated." In consequence, in social inquiry, old paradigms don't die; they just go to the hospital and get fitted with a cardiac pacemaker. After that, they can live on for a long time with other paradigms, side by side. It follows that contestation and turf struggle are likely to continue, from inside and outside the multiple communities of practice in qualitative social inquiry.

Let me finish this discussion in first person: It does seem to me that the full-blown realist ethnographic monograph, with its omniscient narrator speaking to the reader with an apparent neutrality as if from nowhere and nowhen—a subject who stands apart from their description—is no longer a genre of reporting that can responsibly be practiced, given the duration and force of the critiques that have been leveled against it. Some adaptation, some deviation from the classic form seems warranted. It also seems to me that there should be viable places along the full spectrum of approaches in qualitative and postqualitative inquiry where we can practice our crafts without resorting to sniping at others, or to self-satisfied smugness with ourselves as we are protected within the silo of our own mutual citation network, or to nostalgia for a past imaginary that was indeed problematic. This requires adopting a certain degree of humility as we consider what any of our work is capable of accomplishing, whatever our particular approach and commitments might be. (In the interest of full disclosure, I should say here that temperamentally and ecclesiastically, I am an Episcopalian—one who wants to be Catholic and Protestant at the same time.)

At this writing, it is only 108 years since Malinowski set foot in the Trobriand Islands and 100 years since the publication of his first report on that fieldwork (Malinowski, 1922). I still believe that Malinowski's overall aim for ethnography was a noble one, especially as amended in the words that follow: "to grasp the points of view of those who are studied and of those who are studying (recognizing that these may be one and the same people)—their relations to life, their

visions of their worlds." I think it is fair to say that we have learned since the middle of the 20th century how hard it is to achieve such an aim partially, even to move in the direction of that aim. We know now that this is far more difficult than Malinowski and his contemporaries had anticipated. Yet it could still orient our continuing reach.

Since argument persists, others may well disagree.

DISCUSSION QUESTIONS

1. Describe and comment on the shift from objectivist qualitative research (akin to specimen collection by field biologists) to Malinowskian interpretivist qualitative research.

2. "Ethnography" means "writing about other people" while "autoethnography" means "writing about one's self." Is hermeneutical validity—description from "the native's point of view"—possible when research is done by outsiders rather than by insiders? Discuss the contrastive affordances and constraints of insider ethnography and outsider ethnography, considering the possibility that insider/outsider could be a false binary.

3. Comment on the possibilities of Indigenous epistemologies and ontologies in de/colonizing social inquiry, identifying affordances and limitations in such uses.

4. Discuss implications of participatory action research employing new information technologies—including cell phone video recording, wearable cameras, and social media dissemination networks—for futures of qualitative inquiry.

5. Summarize arguments for and against posthumanist and postqualitative approaches to social research.

NOTE

1. Some discussion here is adapted from my own previous writing on these topics, drawing especially on Erickson (1986, 2006). Because the literature on qualitative research methods is huge, the reader is also referred to Vidich and Lyman (1994) for an extensive review of classic realist ethnography in American sociology and anthropology; to Urrey (1984) for an extensive review of field research methods, primarily in British social anthropology; and to Heider (1982) for an extensive review of ethnographic film.