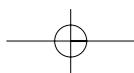
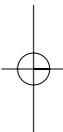
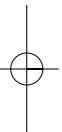


PART III

THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY



9

QUALITATIVE METHODOLOGY

NORMAN K. DENZIN

University of Illinois, Urbana–Champaign

Qualitative research is a field of inquiry in its own right. It cuts across disciplines, subfields, and subject matter.¹ A complex, interconnected family of terms, concepts, and assumptions surrounds the qualitative research orientation. These include the traditions associated with positivism, poststructuralism, and the many qualitative research perspectives or methods connected to cultural and interpretive studies.

In North America, qualitative research operates in a complex historical field that cross-cuts seven historical moments. These seven moments overlap and simultaneously operate in the present. They can be defined as the traditional (1900–1950), the modernist, or golden age (1950–1970), blurred genres (1970–1986), the crisis of representation (1986–1990) and postmodern, a period of experimental and new ethnographies (1990–1995), post-experimental inquiry (1995–2000), and the future, which is now (2000–). The future, the seventh moment, is concerned with moral discourse, with the development of a sacred texture. The seventh and eighth moments suggest that the social sciences and the humanities become sites for critical conversations about democracy, race, gender, class, nation, freedom, and community.

Successive waves of epistemological theorizing move across these moments. The traditional period is associated with the positivist, foundational paradigm. The modernist or golden age and blurred genres moments are connected to the appearance of postpositivist arguments. At the same time, a variety of new interpretive, qualitative perspectives were taken up, including hermeneutics, structuralism, semiotics, phenomenology, cultural studies, and feminism.² In the blurred genres phase, the humanities became

central resources for critical, interpretive theory and the qualitative research project broadly conceived. The researcher became a *bricoleur*, learning how to borrow from many different disciplines.

The blurred genres phase produced the next stage, the crisis of representation. Here, researchers struggled with how to locate themselves and their subjects in reflexive texts. A kind of methodological diaspora took place, a two-way exodus. Humanists migrated to the social sciences, searching for new social theory and new ways to study popular culture and its local, ethnographic contexts. Social scientists turned to the humanities, hoping to learn how to do complex structural and poststructural readings of social texts. The line between a text and a context blurred. In the postmodern, experimental moment, researchers continued to move away from foundational and quasi-foundational criteria. Alternative evaluative criteria were sought, those that were evocative, moral, critical, and based on local understandings.

North Americans are not the only scholars struggling to create postcolonial, nonessentialist, feminist, dialogic performance texts, texts informed by the rhetorical, narrative turn in the human disciplines (Delamont, Coffey, and Atkinson 2000). This international work troubles the traditional distinctions between science, the humanities, rhetoric, literature, facts, and fiction. As Atkinson and Hammersley (1994) observe, this discourse recognizes “the literary antecedents of the ethnographic text, and affirms the essential dialectic” underlying these aesthetic and humanistic moves (p. 255).

Moreover, this literature is reflexively situated in a multiple, historical, and national context. It is clear that

America's history with qualitative inquiry cannot be generalized to the rest of the world (Atkinson, Coffey, and Delamont 2001). Nor do all researchers embrace a politicized, cultural studies agenda that demands that interpretive texts advance issues surrounding social justice and racial equality.

Lopez (1998) observes that "there is a large-scale social movement of anti-colonialist discourse" (p. 226), and this movement is evident in the emergence of African American, Chicano, Native American, and Maori standpoint theories. These theories question the epistemologies of Western science that are used to validate knowledge about indigenous peoples. The Maori scholar Russell Bishop (1998) presents a participatory and participant perspective (Tillman 1998:221) that values an embodied and moral commitment to the research community one is working with. This research is characterized by the absence of a need to be in control (Bishop 1998:203; Heshusius 1994). Such a commitment reflects a desire to be connected to and a part of the moral community. The goal is compassionate understanding (Heshusius 1994).

These understandings are only beginning to enter the literatures on social problems and deviance. As they do, a blurring of the spaces between the hyphens that join researchers and those studied occurs. Definitions of sociological phenomena, including social problems and deviance, are thereby made problematic.

QUEERING THE INQUIRY

In the context of discussing the study of same-sex experience, Kong, Mahoney, and Plummer (2002) present compelling historical evidence to support the conclusion that "*the sensibilities of interviewing are altered with the changing social phenomena that constitute 'the interviewee'*" (p. 240, italics in original). Reviewing the interviewing of gays in North America and Europe over the past 100 years, they trace a movement from a "highly positivist mode of research through one where the boundaries become weaker, and on to a situation where interviewing has been partially deconstructed" (p. 240).

These authors distinguish three historical moments: (1) traditional, (2) modernizing, and (3) postmodern. Their analysis contrasts the three periods in terms of assumptions about interviewers, gays, lesbians, questions asked, approaches taken, wider cultural discourses, and politics. Interviewers are presumed to be objective and heterosexual in the traditional period, closeted in the modern period, and out in the postmodern moment. Same-sex experiences are approached clinically, in terms of pathologies in the traditional period, while they are normalized in the postmodern period, when discourses on disease give way to talk of liberation, politics, and postmodern ethics.

Kong et al. (2002:254) offer three conclusions relevant to the arguments presented in this chapter. Interviewing gays and lesbians today is very different from interviewing them at the end of the nineteenth century. With the arrival of postmodern understandings, new forms of interviewing and new kinds of findings are appearing. A form of reflexive, radical historicity should now be a part of all interpretive inquiry. Of equal importance, any form of inquiry, such as the interview, is itself a cultural form, in which questions and answers become self-validating.

READING HISTORY

Several conclusions can be drawn from this brief history, which is, like all histories, somewhat arbitrary. First, each of the earlier historical moments is still operating in the present, either as a legacy or as a set of practices that researchers continue to follow or argue against. The multiple, and fractured histories of qualitative research now make it possible for any given researcher to attach a project to a canonical text from any of the above-described historical moments. Multiple criteria of evaluation compete for attention in this field. Second, an embarrassment of choices now characterizes the field of qualitative research. There have never been so many paradigms, strategies of inquiry, or methods of analysis to draw upon and utilize. Third, we are in a moment of discovery and rediscovery, as new ways of looking, interpreting, arguing and writing are debated and discussed. Fourth, the qualitative research act can no longer be viewed from within a neutral or objective positivist perspective. Class, race, gender, and ethnicity shape the process of inquiry, making research a multicultural process.

QUALITATIVE RESEARCH AS A PROCESS

Any definition of qualitative research must work within this complex historical field. Qualitative research means different things in each of these moments. Nonetheless, an initial, generic definition can be offered.³

Qualitative research is multimethod in focus, involving an interpretive, naturalistic approach to its subject matter. This means that qualitative researchers study things in their natural settings, attempting to make sense of or interpret these things in terms of the meanings people bring to them. Qualitative research involves the studied use and collection of a variety of empirical materials—case study, personal experience, introspection, life story, interview, and observational, historical, interactional, and visual texts—that describe routine and problematic moments and meanings in an individual's life.

Three interconnected, generic activities define the qualitative research process. They go by a variety of

100 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

different labels, including theory, method, and analysis, and ontology, epistemology, and methodology. Behind these last three terms stands the personal biography of the gendered researcher, who speaks from a particular class, racial, cultural, and ethnic community perspective. The gendered, multiculturally situated researcher approaches the world with a set of ideas, a framework (theory, ontology) that specifies a set of questions (epistemology), which are then examined (analysis, methodology) in specific ways. That is, empirical materials bearing on the question are collected and then analyzed and written about. Every researcher speaks from within a distinct interpretive community, which configures, in its special way, the multicultural, gendered components of the research act. This community has its own historical research traditions, which constitute a distinct point of view. This perspective leads the researcher to adopt particular views of the “other” who is studied. At the same time, the politics and the ethics of research must also be considered, for these concerns permeate every phase of the research process.

RESISTANCES TO QUALITATIVE STUDIES

The academic and disciplinary resistances to qualitative research illustrate the politics embedded in this field of discourse. The challenges to qualitative research are many. Qualitative researchers are called journalists, or soft scientists. Their work is termed unscientific, or only exploratory, or entirely personal and full of bias. It is called criticism and not theory, or it is interpreted politically as a disguised version of Marxism or humanism (see Huber 1995; also Denzin 1997:258–61 for a review).

These resistances reflect an uneasy awareness that the traditions of qualitative research commit one to a critique of the positivist or postpositivist project. But the positivist resistance to qualitative research goes beyond the “ever-present desire to maintain a distinction between hard science and soft scholarship” (Carey 1989:99). The positive sciences (e.g., physics, chemistry, economics, and psychology) are often seen as the crowning achievements of Western civilization, and in their practices it is assumed that “truth” can transcend opinion and personal bias (Carey 1989:99). Qualitative research is seen as an assault on this tradition, whose adherents often retreat into a “value-free objectivist science” (Carey 1989:104) model to defend their position. They seldom attempt to make explicit and critique the “moral and political commitments in their own contingent work” (Carey 1989:104).

Positivists further allege that the so-called new experimental qualitative researchers write fiction, not science, and they have no way of verifying their truth statements. Ethnographic poetry and fiction signal the death of empirical science, and there is little to be gained by attempting

to engage in moral criticism. These critics presume a stable, unchanging reality that can be studied with the empirical methods of objective social science. The province of qualitative research, accordingly, is the world of lived experience, for this is where individual belief and action intersect with culture. Under this model, there is no preoccupation with discourse and method as material interpretive practices that constitute representation and description. Thus is the textual, narrative turn rejected by the positivist orientation.

The opposition to positive science by the postpositivists and the poststructuralists is seen, then, as an attack on reason and truth. At the same time, the attack by positive science on qualitative research is regarded as an attempt to legislate one version of truth over another.

POLITICS AND REEMERGENT SCIENTISM

The scientifically based research (SBR) movement initiated by the National Research Council (NRC) has created a new and hostile political environment for qualitative research. Connected to the No Child Left Behind Act of 2001, SBR embodies a reemergent scientism (Maxwell 2004), a positivist, evidence-based epistemology. Researchers are encouraged to employ “rigorous, systematic, and objective methodology to obtain reliable and valid knowledge” (Ryan and Hood 2004:80). The preferred methodology has well-defined causal models using independent and dependent variables. Causal models are examined in the context of randomized controlled experiments that allow replication and generalization (Ryan and Hood 2004:81).

Under this framework, qualitative research becomes suspect. There are no well-defined variables or casual models. Observations and measurements are not based on random assignment to experimental groups. Hard evidence is not generated by these methods. At best, case study, interview, and ethnographic methods offer descriptive materials that can be tested with experimental methods. The epistemologies of critical race, queer, postcolonial, feminist, and postmodern theories are rendered useless, relegated at best to the category of scholarship, not science (Ryan and Hood 2004:81; St. Pierre 2004:132).

Critics of the evidence movement are united on the following points. “Bush Science” (Lather 2004:19), and its experimental, evidence-based methodologies, represents a radical masculine backlash to the proliferation of qualitative inquiry methods over the last two decades (Lather 2004). The movement endorses a narrow view of science (Maxwell 2004), celebrating a “neoclassical experimentalism that is a throwback to the Campbell-Stanley era and its dogmatic adherence to an exclusive reliance on quantitative methods” (Howe 2004:42). There is “nostalgia for a simple and ordered universe of science that never was”

(Popkewitz 2004:62). With its emphasis on only one form of scientific rigor, the NRC ignores the need and value of complex historical, contextual, and political criteria for evaluating inquiry (Bloch 2004).

Neoclassical experimentalists extol evidence-based “medical research as the model for educational research, particularly the random clinical trial” (Howe 2004:48). But the random clinical trial—dispensing a pill—is quite unlike “dispensing a curriculum” (Howe 2004:48), nor can the “effects” of the educational experiment be easily measured, unlike a “10-point reduction in diastolic blood pressure” (Howe 2004:48).

Qualitative researchers must learn to think outside the box of positivism and postpositivism as they critique the NRC and its methodological guidelines (Atkinson 2004). We must apply our critical imagination to the meaning of terms such as *randomized design*, *causal model*, *policy studies*, and *public science* (Cannella and Lincoln 2004; Weinstein 2004). Furthermore, we must resist conservative attempts to discredit qualitative inquiry by placing it back inside the box of positivism.

MIXED-METHODS EXPERIMENTALISM

Howe (2004) observes that the NRC finds a place for qualitative methods in mixed-methods experimental designs. In such designs, qualitative methods may be “employed either singly or in combination with quantitative methods, including the use of randomized experimental designs (p. 49). Mixed methods are direct descendants of classical experimentalism. They presume a methodological hierarchy, with quantitative methods at the top, relegating qualitative methods to “a largely auxiliary role in pursuit of the technocratic aim of accumulating knowledge of ‘what works’” (pp. 53–54).

The mixed-methods movement takes qualitative methods out of their natural home, which is within the critical, interpretive framework (Howe 2004:54; but see Teddlie and Tashakkori 2003:15). It divides inquiry into dichotomous categories, exploration versus confirmation. Qualitative work is assigned to the first category, quantitative research to the second (Teddlie and Tashakkori 2003:15). Like the classic experimental model, it excludes stakeholders from dialogue and active participation in the research process. This weakens its democratic and dialogical dimensions and reduces the likelihood that the previously silenced voices will be heard (Howe 2004:56–57).

Howe (2004) cautions that it is not just the “methodological fundamentalists” who have bought into [this] approach. A sizeable number of rather influential . . . educational researchers . . . have also signed on. This might be a compromise to the current political climate; it might be a backlash against the perceived excesses of postmodernism; it might be both. It is an ominous development, whatever the explanation” (p. 57).

THE PRAGMATIC CRITICISMS OF ANTIFOUNDATIONALISM

Seale et al. (2004:2) contest what they regard as the excesses of an antimethodological, “any thing goes,” romantic postmodernism that is associated with this project. They assert that too often the approach valued produces “low quality qualitative research and research results that are quite stereotypical and close to common sense” (p. 2).

In contrast, Seale et al. (2004) propose a practice-based, pragmatic approach that places research practice at the center. Research involves an engagement “with a variety of things and people: research materials . . . social theories, philosophical debates, values, methods, tests . . . research participants” (p. 2). (Actually this approach is quite close to my own view of the *bricoleur* and *bricolage*.)

Seale et al.’s (2004) situated methodology rejects the antifoundational claim that there are only partial truths, that the dividing line between fact and fiction has broken down (p. 3). They believe that this dividing line has not collapsed, that we should not accept stories if they do not accord with the best available facts (p. 6). Oddly, these pragmatic procedural arguments reproduce a variant of the evidence-based model and its criticisms of poststructural, performative sensibilities.

I turn now to a brief discussion of the major differences between the qualitative and quantitative approaches to research.

QUALITATIVE VERSUS QUANTITATIVE RESEARCH

Qualitative implies an emphasis on processes and meanings that are not rigorously examined or measured (if measured at all) in terms of quantity, amount, intensity, or frequency. Qualitative researchers stress the socially constructed nature of reality, the intimate relationship between the researcher and what is studied, and the situational constraints that shape inquiry. Such researchers emphasize the value-laden nature of inquiry. They seek answers to questions that stress how social experience is created and given meaning. In contrast, quantitative studies emphasize the measurement and analysis of causal relationships between variables, not processes. Proponents claim that their work is done from within a value-free framework.

RESEARCH STYLES: DOING THE SAME THINGS DIFFERENTLY?

Of course, both qualitative and quantitative researchers “think they know something about society worth telling to others, and they use a variety of forms, media and means to communicate their ideas and findings” (Becker 1986:122). Qualitative research differs from quantitative

research in five significant ways (Becker 1996). These points of difference turn on different ways of addressing the same set of issues.

1. Uses of Positivism and Postpositivism

First, both perspectives are shaped by the positivist and postpositivist traditions in the physical and social sciences. These two positive science traditions hold naive and critical realist positions concerning reality and its perception. In the positivist version, it is contended that there is a reality out there to be studied, captured, and understood, while the postpositivists argue that reality can never be fully apprehended, only approximated (Guba 1990:22). Postpositivism relies on multiple methods as a way of capturing as much of reality as possible. At the same time, emphasis is placed on the discovery and verification of theories. Traditional evaluation criteria such as internal and external validity are stressed, as is the use of qualitative procedures that lend themselves to structured (sometimes statistical) analysis.

Historically, qualitative research was defined within the positivist paradigm, where qualitative researchers attempted to do good positivist research with less rigorous methods and procedures. Some midcentury qualitative researchers (Becker et al. 1961) reported participant observation findings in terms of quasi-statistics. As recently as 1999, two leaders of the grounded theory approach to qualitative research attempted to modify the usual canons of good (positivistic) science to fit their own postpositivist conception of rigorous research (Strauss and Corbin 1999).

Flick (1998) usefully summarizes the differences between these two approaches to inquiry. He observes that the quantitative approach has been used for purposes of isolating "causes and effects . . . operationalizing theoretical relations . . . [and] measuring and . . . quantifying phenomena . . . allowing the generalization of finding" (p. 3). But today, doubt is cast on such projects:

Rapid social change and the resulting diversification of life worlds are increasingly confronting social researchers with new social contexts and perspectives . . . traditional deductive methodologies . . . are failing . . . thus research is increasingly forced to make use of inductive strategies instead of starting from theories and testing them . . . knowledge and practice are studied as local knowledge and practice. (P. 2)

2. Acceptance of Postmodern Sensibilities

The use of quantitative, positivist methods and assumptions has been rejected by a new generation of qualitative researchers who are attached to poststructural, postmodern sensibilities. These researchers argue that positivist methods are but one way of telling a story about society or the social world. They may be no better or no worse than any other method; they just tell a different kind of story.

This tolerant view is not shared by everyone. Many members of the critical theory, constructivist, poststructural, and postmodern schools of thought reject positivist and postpositivist criteria when evaluating their own work. They see these criteria as irrelevant to their work and contend that it reproduces only a certain kind of science, a science that silences too many voices. These researchers seek alternative methods for evaluating their work, including verisimilitude, emotionality, personal responsibility, an ethic of caring, political praxis, multivoiced texts, and dialogues with subjects.

3. Capturing the Individual's Point of View

Both qualitative and quantitative researchers are concerned about the individual's point of view. However, qualitative investigators think they can get closer to the actor's perspective by detailed interviewing and observation. They argue that quantitative researchers are seldom able to capture the subject's perspective because they have to rely on more remote, inferential empirical materials.

4. Examining the Constraints of Everyday Life

Qualitative researchers are more likely to confront and come up against the constraints of the everyday social world. They see this world in action and embed their findings in it. Quantitative researchers abstract from this world and seldom study it directly. They seek a nomothetic or etic science based on probabilities derived from the study of large numbers of randomly selected cases. These kinds of statements stand above and outside the constraints of everyday life. Qualitative researchers, on the other hand, are committed to an emic, ideographic, case-based position, which directs their attention to the specifics of particular cases.

5. Securing Rich Descriptions

Qualitative researchers believe that rich descriptions of the social world are valuable, while quantitative researchers, with their etic, nomothetic commitments, are less concerned with such detail. They are deliberately unconcerned with such descriptions because such detail interrupts the process of developing generalizations.

These five points of difference described above (uses of positivism and postmodernism, acceptance of postmodern sensibilities, capturing the individual's point of view, examining the constraints of everyday life, securing thick descriptions) reflect commitments to different styles of research, different epistemologies, and different forms of representation. Each work tradition is governed by a different set of genres, each has its own classics, its own preferred forms of representation, interpretation, and textual evaluation. Qualitative researchers use ethnographic prose, historical narratives, first-person accounts, still photographs, life history, fictionalized facts, and biographical and autobiographical materials, among others.

Quantitative researchers use mathematical models, statistical tables, and graphs and usually write in an impersonal, third-person prose.

WORKING THE HYPHEN: THE “OTHER” AS RESEARCH SUBJECT

From its turn-of-the-century birth in modern, interpretive form, qualitative research has been haunted by a double-faced ghost. On the one hand, qualitative researchers have assumed that qualified, competent observers could with objectivity, clarity, and precision report on their own observations of the social world, including the experiences of others. Second, researchers have held to the belief in a real subject or real individual who is present in the world and able, in some form, to report on his or her experiences. So armed, the researchers could blend their own observations with self-reports provided by subjects through interviews, life story, personal experience, and case study documents.

These two beliefs have led qualitative researchers across disciplines to seek a method that would allow them to record their own observations accurately while also uncovering the meanings their subjects brought to their life experiences. This method would rely on the subjective verbal and written expressions of meaning given by the individuals studied, these expressions being windows to the inner life of the person. Since Dilthey ([1900] 1976), this search for a method has led to a perennial focus in the human disciplines on qualitative, interpretive methods.

Recently, as noted above, this position and its beliefs have come under assault. Poststructuralists and postmodernists have contributed to the understanding that there is no clear window into the inner life of an individual. Any gaze is always filtered through the lenses of language, gender, social class, race, and ethnicity. There are no objective observations, only observations socially situated in the worlds of the observer and the observed. Subjects, or individuals, are seldom able to give full explanations of their actions or intentions; all they can offer are accounts or stories about what they did and why. No single method can grasp the subtle variations in ongoing human experience. Consequently, qualitative researchers deploy a wide range of interconnected interpretive methods, always seeking better ways to make more understandable the worlds of experience that have been studied.

INTERPRETIVE PARADIGMS

All qualitative researchers are philosophers in that “universal sense in which all human beings . . . are guided by highly abstract principles” (Bateson 1972:320). These principles combine beliefs about *ontology* (What kind of being is the human being? What is the nature of reality?), *epistemology* (What is the relationship between the inquirer and the known?), and *methodology* (How do we

know the world or gain knowledge of it?) (see Guba and Lincoln 2000). These beliefs shape how the qualitative researcher sees the world and acts in it. The researcher is “bound within a net of epistemological and ontological premises which—regardless of ultimate truth or falsity—become partially self-validating” (Bateson 1972:314).

The net that contains the researcher’s epistemological, ontological, and methodological premises may be termed a paradigm (Guba 1990:17) or interpretive framework, a “basic set of beliefs that guides action” (Guba 1990:17). All research is interpretive and guided by a set of beliefs and feelings about the world and how it should be understood and studied. These beliefs may be taken for granted, only assumed, while others are highly problematic and controversial. Each interpretive paradigm makes particular demands on the researcher, including the questions that are asked and the interpretations that are brought to them.

At the most general level, four major interpretive paradigms structure qualitative research: (1) positivist and postpositivist, (2) constructivist-interpretive, (3) critical (Marxist, emancipatory), and (4) feminist-poststructural. These four abstract paradigms become more complicated at the level of concrete specific interpretive communities. At this level, it is possible to identify not only the constructivist but also multiple versions of feminism (Afrocentric and poststructural),⁴ as well as specific ethnic, Marxist, and cultural studies paradigms.

The positivist and postpositive paradigms work from within a realist and critical realist ontology and objective epistemologies and rely on experimental, quasi-experimental, survey, and rigorously defined qualitative methodologies. The constructivist paradigm assumes a relativist ontology (there are multiple realities), a subjectivist epistemology (knower and subject create understandings), and a naturalistic (in the natural world) set of methodological procedures. Findings are usually presented in terms of the criteria of grounded theory. Terms such as *credibility*, *transferability*, *dependability*, and *confirmability* replace the usual positivist criteria of internal and external validity, reliability, and objectivity.

FEMINIST, ETHNIC, MARXIST, CULTURAL STUDIES, AND QUEER THEORY MODELS

Critical theory is a materialist-realist ontology—that is, the real world makes a material difference in terms of race, class, and gender. Subjectivist epistemologies and naturalistic methodologies (usually ethnographies) are also employed. Empirical materials and theoretical arguments are evaluated in terms of their emancipatory implications. Criteria from gender and racial communities (e.g., African American) may be applied (emotionality and feeling, caring, personal accountability, dialogue).

Poststructural feminist theories emphasize problems with the social text, its logic, and its inability to ever fully

104 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

represent the world of lived experience. Positivist and postpositivist criteria of evaluation are replaced by other terms, including the reflexive, multivoiced text that is grounded in the experiences of oppressed people.

The cultural studies and queer theory paradigms are multifocused, with many different strands drawing from Marxism, feminism, and the postmodern sensibility. There is a tension between humanistic cultural studies that stress lived experiences and a more structural cultural studies project that stresses the structural and material determinants (race, class, gender) of experience. The cultural studies and queer theory paradigms use methods strategically—that is, as resources for understanding and for producing resistances to local structures of domination. Such scholars may do close textual readings and discourse analysis of cultural texts, as well as local ethnographies, open-ended interviewing, and participant observation. The focus is on how race, class, and gender are produced and enacted in historically specific situations.

BRIDGING THE HISTORICAL MOMENTS: INTO THE PRESENT

Two theses have organized the discussion to this point. First, in its relationship to the field of sociological inquiry, the history of qualitative research is defined more by breaks and ruptures than by a clear, evolutionary, progressive movement from one stage to the next. These breaks and ruptures move in cycles and phases, so that which is passé today may be in vogue a decade from now. Just as the postmodern, for example, reacts to the modern, someday there may well be a neomodern phase that extols Malinowski and the Chicago School and finds the current poststructural, postmodern moment abhorrent.

The second assumption builds on the tensions that now define qualitative sociological inquiry. There is an elusive center to this contradictory, tension-riddled enterprise, which seems to be moving further and further away from grand narratives, and single, overarching ontological, epistemological, and methodological paradigms. This center lies in the humanistic commitment of the researcher to always study the world from the perspective of the interacting individual. From this simple commitment flow the liberal and radical politics of qualitative sociological research on social problems. Action, feminist, clinical, constructionist, ethnic, critical, and cultural studies researchers are all united on this point. They all share the belief that a politics of liberation must always begin with the perspective, desires, and dreams of those individuals and groups who have been oppressed by the larger ideological, economic, and political forces of a society or a historical moment.

This commitment defines an ever-present, but always shifting, center in the discourses of qualitative research. The center shifts and moves as new, previously oppressed, or silenced voices enter the discourse. Thus, for example,

feminists and ethnic researchers have articulated their own relationship to the postpositivist and critical paradigms. These new articulations then refocus and redefine previous ontologies, epistemologies, and methodologies, including positivism and postpositivism. These two theses suggest that only the broad outlines of the future can be predicted, as the field confronts and continues to define itself in the face of four fundamental issues.

The first and second issues are what we have called the crises of representation and legitimization. These two crises speak, respectively, to the other and its representations in our texts and to the authority we claim for our texts. Third, there is the continued emergence of a cacophony of voices speaking with varying agendas from specific gender, race, class, ethnic, and Third World perspectives.

Fourth, throughout its history, qualitative sociological research has been defined in terms of shifting scientific, moral, sacred, and religious discourses. Since the Enlightenment, science and religion have been separated, but only at the ideological level, for in practice religion and the sacred have constantly informed science and the scientific project. The divisions between these two systems of meaning are becoming more and more blurred. Critics increasingly see science from within a magical, shamanistic framework (Rosaldo 1989:219). Others are moving science away from its empiricist foundations and closer to a critical, interpretive project that stresses morals and moral standards of evaluation (Clough 1998:136–37).

Three understandings shape the present moment; these are,

- The qualitative sociological researcher is not an objective, authoritative, politically neutral observer standing outside and above the social world (Bruner 1993:1).
- The qualitative researcher is “historically positioned and locally situated [as] an all-too-human [observer] of the human condition” (Bruner 1993:1).
- Meaning is “radically plural, always open, and . . . there is politics in every account” (Bruner 1993:1).

The problems of representation and legitimation flow from these three understandings.

THE CRISIS OF REPRESENTATION

As indicated, this crisis asks the questions, “Who is the Other? Can we ever hope to speak authentically of the experience of the Other, or an Other? And if not, how do we create a social science that includes the Other?” The short answer to these questions is that we move to include the other in the larger research processes that have been developed. For some, this means participatory or collaborative research and evaluation efforts. These activities can occur in a variety of institutional sites, including clinical, educational, and social welfare settings.

For other researchers, it means a form of liberatory investigation wherein the others are trained to engage in

their own social and historical interrogative efforts and are then assisted in devising answers to questions of historical and contemporary oppression that are rooted in the values and cultural artifacts that characterize their communities.

For still other social scientists, it means becoming coauthors in narrative adventures. And for still others, it means constructing what are called “experimental,” or “messy,” texts where multiple voices speak, often in conflict, and where the reader is left to sort out which experiences speak to his or her personal life. For still others, it means presenting to the inquiry and policy community a series of autohistories, personal narratives, lived experiences, poetic representations, and sometimes fictive and/or fictional texts that allow the other to speak for himself or herself. The inquirer or evaluator becomes merely the connection between the field text, the research text, and the consuming community in making certain that such voices are heard. Sometimes, increasingly, it is the “institutionalized other” who speaks, especially as the other gains access to the knowledge-producing corridors of power and achieves entry into the particular group of elites known as intellectuals and academics or faculty.

The point is that both the other and more mainstream social scientists recognize that there is no such thing as unadulterated truth, that speaking from a faculty, an institution of higher education, or a corporate perspective automatically means that one speaks from a privileged and powerful vantage point, and that this vantage point is one to which many do not have access, by dint of either social station or education.

Judith Stacey (1988) speaks of the difficulties involved in representing the experiences of the other about whom texts are written. Writing from a feminist perspective, she argues that a major contradiction exists in this project, despite the desire to engage in egalitarian research characterized by authenticity, reciprocity, and trust. This is so because actual differences of power, knowledge, and structural mobility still exist in the researcher-subject relationship. The subject is always at grave risk of manipulation and betrayal by the ethnographer (p. 23). In addition, there is the crucial fact that the final product is too often that of the researcher, no matter how much it has been modified or influenced by the subject. Thus, even when research is written from the perspective of the other, for example, women writing about women, the women doing the writing may “unwittingly preserve the dominant power relations that they explicitly aim to overcome” (Bruner 1993:23).

THE AUTHOR’S PLACE IN THE TEXT

The feminist solution clarifies the issue of the author’s place in the text. This problem is directly connected to the problem of representation. It is often phrased in terms of a false dichotomy—that is, “the extent to which the personal self should have a place in the scientific scholarly text”

(Bruner 1993:2). This false division between the personal and the ethnographic self rests on the assumption that it is possible to write a text that does not bear the traces of its author. Of course, this is incorrect. All texts are personal statements.

The correct phrasing of this issue turns on the amount of the personal, subjective, poetic self that is in fact openly given in the text. Bruner (1993) phrases the problem this way: “The danger is putting the personal self so deeply back into the text that it completely dominates, so that the work becomes narcissistic and egotistical. No one advocates ethnographic self-indulgence” (p. 6). The goal is to openly return the author to the text in a way that does “not squeeze out the object of study” (p. 6).

There are many ways to openly return the author to the qualitative research text. Fictional narratives of the self may be written. Performance texts can be produced. Dramatic readings can be given. Field interviews can be transformed into poetic texts, and poetry, as well as short stories and plays, can be written. The author can engage in a dialogue with those studied. The author may write through a narrator, “directly as a character . . . or through multiple characters, or one character may speak in many voices, or the writer may come in and then go out of the [text]” (Bruner 1993:6).

THE CRISIS OF LEGITIMATION

It is clear that critical race theory, queer theory, and feminist arguments are moving farther and farther away from postpositivist models of validity and textual authority. This is the crisis of legitimization that follows the collapse of foundational epistemologies. This so-called crisis arose when anthropologists and other social scientists addressed the authority of the text. By the authority of the text, I refer to the claim any text makes to being accurate, true, and complete. That is, is a text faithful to the context and the individuals it is supposed to represent? Does the text have the right to assert that it is a report to the larger world that addresses not only the researcher’s interests but also the interests of those who are studied?

This is not an illegitimate set of questions, and it affects all of us and the work that we do. And while many social scientists might enter the question from different angles, these twin crises are confronted by everyone.

COPING WITH THE PRESENT

A variety of new and old voices, critical theory, and feminist and ethnic scholars have also entered the present situation, offering solutions to the problems surrounding the crises of representation and legitimating. The move is toward pluralism, and many social scientists now recognize that no picture is ever complete, that what is needed is many perspectives, many voices, before we can achieve a

106 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

deep understanding of social phenomena and before we can assert that a narrative is complete.

The modernist dream of a grand or master narrative is now a dead project. The postmodern era is defined, in part, by the belief that there is no single umbrella in the history of the world that might incorporate and represent fairly the dreams, aspirations, and experiences of all peoples.

CRITICAL THEORISTS, CRITICAL PEDAGOGY

The critical theorists from the Frankfurt to the Annales world systems and participatory action research schools continue to be a major presence in qualitative research, and they occupy a central place in social theory (Freire 1998; Kincheloe and McLaren 2000; Denzin 2003). The critique and concern of the critical theorists have been an effort to design a pedagogy of resistance within communities of differences. The pedagogy of resistance, of taking back “voice,” of reclaiming narrative for one’s own rather than adapting to the narratives of a dominant majority, was most explicitly laid out by Paolo Freire (1998) working with adults in Brazil. Critical pedagogy seeks to overturn oppression and to achieve social justice through empowerment of the marginalized, the poor, the nameless, and the voiceless. This program is nothing less than the radical restructuring of society toward the ends of reclaiming historic cultural legacies, social justice, the redistribution of power, and the achievement of truly democratic societies.

FEMINIST RESEARCHERS

Poststructural feminists urge the abandonment of any distinction between empirical science and social criticism. That is, they seek a morally informed social criticism that is not committed to the traditional concerns or criteria of empirical science. This traditional science, they argue, rests a considerable amount of its authority on the ability to make public what has traditionally been understood to be private (Clough 1998:137; Olesen 2000; Lather 2004). Feminists dispute this distinction. They urge a social criticism that takes back from science the traditional authority to inscribe and create subjects within the boundaries and frameworks of an objective social science. Feminist philosophers question the scientific method’s most basic premises, namely, the idea that scientific objectivity is possible.

CRITICAL RACE AND QUEER THEORY SCHOLARS

There is yet another group of concerned scholars determining the course of qualitative social problems research: They are critical race (Ladson-Billings 2000) and

queer theory scholars (Kong et al. 2002), who examine the question of whether history has deliberately silenced, or misrepresented, them and their cultures.

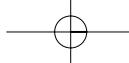
This new generation of scholars, many of them persons of color, challenge both historical and contemporary social scientists on the accuracy, veracity, and authenticity of the latter’s work, contending that no picture can be considered final when the perspectives and narratives of so many are missing, distorted, or self-serving to dominant majority interests. The result of such challenges has been threefold: (1) the reconsideration of the Western canon; (2) the increase in the number of historical and scientific works that recognize and reconstruct the perspectives of those whose perspectives have been previously written out of the present; and (3) an emphasis on life stories and case studies, stories that tell about lives lived under the conditions of racism and sexism.

BACK TO THE FUTURE

The press for a civic social science remains (Agger 2000). We want a civic sociology—by which we mean not just fieldwork located in sociology but rather an extended, enriched, cultivated social science embracing all the disciplines. Such a project characterizes a whole new generation of qualitative researchers: educationists, sociologists, political scientists, clinical practitioners in psychology and medicine, nurses, communications and media specialists, cultural studies workers, and researchers in a score of other assorted disciplines.

The moral imperatives of such work cannot be ignored. Not only do we have several generations of social science that have solved serious human problems, but many times, such work only worsened the plight of those studied. Beyond morality is something equally important: The mandates for such work come from our own sense of the human community. A detached social science frequently serves only those with the means, the social designation, and the intellectual capital to remain detached. We face a choice, in the seventh and eighth moments, of declaring ourselves committed to detachment, or solidarity with the human community. We come to know each other and we come to exist meaningfully only in community. We have the opportunity to rejoin that community as its resident intellectuals and change agents.

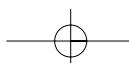
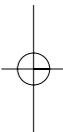
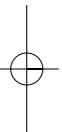
And as we wait, we remember that our most powerful effects as storytellers come when we expose the cultural plots and the cultural practices that guide our writing hands. These practices and plots lead us to see coherence where there is none or to create meaning without an understanding of the broader structures that tell us to tell things in a particular way. Erasing the boundaries between self, other, and history, we seek to learn how to tell new stories, stories no longer contained within, or confined to, the tales of the past. And so we embark together on a new project, a



project with its own as yet not fully understood cultural plots and cultural practices.

And what remains, throughout, will be the steady, but always changing, commitment of all qualitative social problems researchers. The commitment, that is, to study

human experience and its problems from the ground up, from the point of interacting individuals who together and alone make and live histories that have been handed down to them from the ghosts of the past.



10

QUANTITATIVE METHODOLOGY

KENNETH D. BAILEY

University of California, Los Angeles

HISTORY OF SOCIOLOGICAL QUANTIFICATION

Quantitative reasoning is widely applied in the discipline of sociology and quantification aids sociologists in at least seven main research areas: quantitative modeling, measurement, sampling, computerization, data analysis, hypothesis testing, and data storage and retrieval. But sociologists differ widely in their views of the role of quantification in sociology. This has apparently always been true to some degree. While Durkheim was a proponent of quantification, Weber was less enthusiastic. However, while Weber advocated the nonquantitative method *Verstehen*, both Weber and Durkheim saw the importance of method as well as theory, as both authored books on method (Weber 1949; Durkheim [1938] 1964). Today, the situation is much different, as a wide gulf exists between theory and method in twenty-first-century sociology, with only a few authors such as Abell (1971, 2004) and Fararo (1989) simultaneously developing theory and quantitative methodology designed to test theoretical propositions.

The most vocal proponent of quantification in sociology may have been Lundberg (1939), who was known as the unabashed champion of strict operationalism. Operationalism, as originally defined in physics by Bridgman (1948), is the belief that “in general any concept is nothing more than a set of operations, *the concept is synonymous with the corresponding set of operations*” (Bridgman 1948:5–6). George Lundberg (1939, 1947) took the application of operationalism in sociology to an extreme. In Lundberg’s view, one did not approach an already existing concept and then attempt to measure it. The correct

procedure in Lundberg’s view is to use measurement as a way of defining concepts. Thus, if one is asked what is meant by the concept of authoritarianism, the correct answer would be that authoritarianism is what an authoritarianism scale measures.

When he encountered objections to his advocacy of the use of quantification in sociology, Lundberg (1939, 1947) replied that quantitative concepts are ubiquitous in sociology, and need not even be symbolized by numerals, but can be conveyed verbally as well. For example, words such as “many,” “few,” or “several” connote quantitative concepts. In Lundberg’s view, quantification is embedded in verbal social research as well as in everyday thought and is not just an artificial construct that must be added to the research process by quantitative researchers.

After Lundberg (1939, 1947) and others such as Goode and Hatt (1952) and Lazarsfeld (1954) laid the foundation for quantitative sociology in the 1930s, 1940s, and 1950s, the field surged in the 1960s and 1970s. The 1960s saw increased visibility for quantitative sociology with the publication of books and articles such as Blalock’s (1960) *Social Statistics*, Kemeny and Snell’s (1962) *Mathematical Models in the Social Sciences*; White’s (1963) *An Anatomy of Kinship*; Coleman’s (1964) *Introduction to Mathematical Sociology, Foundations of Social Theory*; Duncan’s (1966) “Path Analysis: Sociological Examples”; Land’s (1968) “Principles of Path Analysis”; Blalock’s (1969) *Theory Construction: From Verbal to Mathematical Formulations*; and White’s (1970) *Chains of Opportunity*.

Quantitative methods became even more visible in the 1970s and 1980s with the publication of a host of mathematical and statistical works, including Abell’s (1971)

Model Building in Sociology; Blalock's (1971) *Causal Models in the Social Sciences*; Fararo's (1973) *Mathematical Sociology*; Fararo's (1989) *Meaning of General Theoretical Sociology*; Bailey's (1974b) "Cluster Analysis"; and Blalock's (1982) *Conceptualization and Measurement in the Social Sciences*.

QUANTITATIVE DATA COLLECTION

Specific quantitative techniques make rigorous assumptions about the kind of data that is suitable for analysis with that technique. This requires careful attention to data collection. For data to meet the assumptions of a quantitative technique, the research process generally entails four distinct steps: hypothesis formulation, questionnaire construction, probability sampling, and data collection.

Hypothesis Formulation

A hypothesis is defined as a proposition designed to be tested in the research project. To achieve testability, all variables in the hypothesis must be clearly stated and must be capable of empirical measurement. Research hypotheses may be univariate, bivariate, or multivariate, and some may contain auxiliary information, such as information about control variables. The vast majority of hypotheses used by quantitative sociologists are bivariate. The classical sequence is to formulate the hypotheses first, before instrument construction, sample design, or data collection. Hypotheses may be inductively derived during prior research (Kemeny and Snell 1962) or may be deductively derived (Bailey 1973). Increasingly, however, quantitative sociologists are turning to the secondary analysis of existing data sets. In such a case, hypothesis formulation can be a somewhat ad hoc process of examining the available data in the data bank or data set and formulating a hypothesis that includes the existing available variables.

For example, Lee (2005) used an existing data set and so was constrained to formulate hypotheses using the available variables. He presented three hypotheses, one of which stated that democracy is not directly related to income inequality (Lee 2005:162). While many quantitative studies in contemporary sociology present lists of formal hypotheses (usually five or less), some studies either leave hypotheses implicit or do not present them at all. For example, Torche (2005) discusses the relationship between mobility and inequality but does not present any formal hypotheses (p. 124).

Questionnaire Construction

In the classical research sequence, the researcher designed a questionnaire that would collect the data necessary for hypotheses testing. Questionnaire construction, as a middle component of the research sequence, is subject

to a number of constraints that are not always well recognized. First and foremost is the necessity for the questionnaire to faithfully measure the concepts in the hypotheses. But other constraints are also imposed after questionnaire construction, chiefly sampling constraints, data-collection constraints, and quantitative data-analysis constraints. The questionnaire constrains the sampling design. If the questionnaire is very short and easily administered, this facilitates the use of a complicated sample design.

However, if the questionnaire is complex, then sample size may need to be reduced. The construction of a large and complex questionnaire means that it is difficult and time-consuming to conduct a large number of interviews. It also means that money that could otherwise be spent on the sample design must now be used for interviewer training, interviewing, and codebook construction. In addition to such sampling and data-collection constraints, the chief constraint on instrument design is the type of quantitative technique to be used for data analysis.

That is, the questionnaire must be designed to collect data that meet the statistical assumptions of the quantitative techniques to be used. Questionnaires can quickly become long and complicated. Furthermore, there is a tendency to construct closed-ended questions with not more than seven answer categories. While such nominal or ordinal data are often used in regression analyses, they are marginally inappropriate for ordinary least squares (OLS) regression and other quantitative techniques that assume interval or ratio data. Clearly, one of the great advantages of conducting a secondary analysis of data that has already been collected is that it avoids dealing with the many constraints imposed on the construction of an original data-collection instrument.

Probability Sampling

Many extant quantitative techniques (particularly inductive statistics) can only be used on data collected with a rigorous and sufficiently large probability sample, generally a random sample of some sort. One of the questions most frequently asked of research consultants is, "What is the minimum sample size acceptable for my research project?" Based on the law of large numbers and other considerations, some researchers permit the use of samples as small as 30 cases (Monette, Sullivan, and DeJong 2005:141). There is clearly a trend in the sociological literature toward larger sample sizes, often achieved through the use of the secondary analysis of existing samples and the pooling of multiple samples.

Sociology had few if any research methods books of its own prior to the publication of the volume by Goode and Hatt (1952). Before 1952, sociological researchers relied primarily on psychology research books, such as Jahoda, Deutsch, and Cook (1951), which de-emphasized sampling by relegating it to the appendix. Psychology emphasized the experimental method, with a small number of

110 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

research subjects (often 15 or less), and de-emphasized surveys. Furthermore, in the mid-twentieth century, it was common for both psychology and sociology to use a “captive audience” sample of students from the researcher’s classes.

The chief research models for sociology before 1952 were psychology and (to a lesser degree) medicine. While psychology routinely used a small sample of subjects in experiments, samples in medical research were often quite small as well. If a researcher is conducting medical research, such as a study of pediatric obsessive compulsive disorder, it may be difficult to obtain more than 8 or 10 cases, as the onset of this syndrome is usually later in life. With psychology and medicine as its chief models before 1952, sample sizes in sociology tended to be small.

Over time, sample sizes in sociology have grown dramatically. The present emphasis is on national samples and multinational comparisons, as sociology moves away from the psychological model and toward the economic model. For example, Hollister (2004:669, table 1) did not collect her own data, but used secondary data with an N of 443, 399 to study hourly wages.

Data Collection

During the period 1950 to 1980 when social psychology was dominant in sociology, data collection was often a matter of using Likert scales of 5–7 categories (see Bailey 1994b) to collect data on concepts such as authoritarianism or alienation from a relatively small sample of persons.

Now that economics is becoming the dominant model (see Davis 2001), there are at least two salient ramifications of this trend. One is that an individual researcher is unlikely to possess the resources (even with a large grant) to collect data on 3,000 or more cases and so must often rely on secondary data, as did Joyner and Kao (2005). Another ramification is that researchers wishing to use these large economic data sets that are relatively prevalent must obviously use a different kind of data, and different quantitative techniques, than researchers did in an earlier era when psychology predominated. The psychological orientation resulted in data collection more conducive to analysis of variance, analysis of covariance, and factor analysis, in addition to multiple regression (OLS). Today things have changed, and the technique of choice for the large economic data sets is logistic regression.

MATHEMATICAL SOCIOLOGY

It is useful to divide the extant quantitative techniques in twenty-first-century sociology into inferential statistics (probability-based techniques with tests of significance) and mathematical models (techniques that lack significance tests and are often nonprobabilistic). Rudner (1966) makes a distinction between method and methodology. Although the two terms are often used interchangeably in

sociology and elsewhere, there is an important difference between them. According to Rudner, methods are techniques for gathering data, such as survey research, observation, experimentation, and so on. In contrast, methodologies are criteria for acceptance or rejection of hypotheses. This is a crucial distinction. Some mathematical models lack quantitative techniques for testing hypotheses, as these are not built into the model.

In contrast, inductive statistics, in conjunction with statistical sampling theory, provides a valuable means for sociologists not only to test hypotheses for a given sample but also to judge the efficacy of their inferences to larger populations. Tests of significance used in sociology take many forms, from gamma to chi-square to t -tests, and so on. Whatever the form or level of measurement, significance tests yielding probability, or “ p ,” values provide not only a way to test hypotheses but also a common element for community with researchers in other disciplines that also use significance tests.

Mathematical sociology has traditionally used methods such as differential and integral calculus (Blalock 1969: 88–109). Differential equations are frequently used to construct dynamic models (e.g., Kemeny and Snell 1962; Blalock 1969). However, one of the problems with mathematical models in sociology (and a problem that is easily glossed over) is that they are sometimes very difficult to apply and test empirically. Kemeny and Snell (1962) state that mathematical models are used to deduce “consequences” from theory, and that these consequences “must be put to the test of experimental verification” (p. 3). Since experimental verification in the strictest sense is relatively rare in sociology, this seems to be an Achilles heel of mathematical sociology.

To verify the predictions by comparing them with the experimental data, Kemeny and Snell (1962) use the statistical test chi-square. That is, the mathematical model proves inadequate for hypothesis testing and must be augmented by a statistical test (p. 62). Kemeny and Snell (1962) then “improve” the model by stating that there may be some subjects to which the model does not apply and “adding the assumption that some 20 per cent of subjects are of this type” (p. 62). Unfortunately, such “model simplification,” achieved by simply excluding a proportion of the population from the analysis, is rather common in quantitative sociology. Yamaguchi (1983) explains his failure to include women in the analysis by writing, “In this paper, I limit my analysis to non-black men to simplify the model” (p. 218).

The dilemma is real. If the sociological phenomenon is too complex, then the mathematical sociologist will not be able to solve all the inherent computational problems, even with a large computer. Fortunately, the future technological advances in computer hardware and software, along with the continued development of new mathematical techniques such as blockmodeling (Doreian, Batagelj, and Ferligoj 2005), ensure a bright future for mathematical sociology. While the challenges of social complexity are

real, the rewards for those who can successfully model this complexity with mathematics are great. For additional commentary and references on mathematical sociology in the twenty-first century, see Edling (2002), Iverson (2004), Lewis-Beck, Bryman, and Liao (2004), Meeker and Leik (2000), and Raftery (2005).

STATISTICAL SOCIOLOGY

While statistical methods extant in sociology can all be classified as probability based, they can be divided into tests of significance (such as gamma) and methods used for explanation (often in terms of the amount of variance explained), prediction, or the establishment of causality. Among these techniques, the most commonly used are multiple correlation, multiple regression, logistic regression, as well as analysis of variance (the dominant method in psychology) or analysis of covariance. Other methods used less frequently by sociologists include cluster analysis, factor analysis, multiple discriminant analysis, canonical correlation, and smallest space analysis (Bailey 1973, 1974a), and latent class analysis (Uggen and Blackstone 2004).

Which statistical technique is appropriate for a given analysis can depend on a number of factors, one of which is the so-called level of measurement of the quantitative data involved. S. S. Stevens (1951) divided data into four distinct levels—nominal, ordinal, interval, and ratio. It is important to stress consistent measurement at all four levels, as lack of attention to consistent measurement across studies in sociology is problematic for the field.

Nominal

The reality is that nominal variables can be very important in both sociological theory and statistics, but unfortunately they have been badly neglected by sociologists and often are created and treated in a haphazard fashion. This is unfortunate because discussions of classification techniques are readily available to sociologists in the form of work on cluster analysis and various classification techniques for forming typologies and taxonomies (McKinney 1966; Bailey 1973, 1994a). Carefully constructed classification schemas can form the foundation for all “higher” levels of measurement. A sociological model lacking adequate nominal categories can be the proverbial house of cards, ready to collapse at any moment.

The nominal level of measurement deals with nonhierarchical categories. Many of the most theoretically important and frequently used sociological variables lie at this level of measurement, including religion, sex, political affiliation, region, and so on. Much of the statistical analyses at the nominal level consist of simple frequency, percentage, and rate analysis (Blalock 1979). However, the chi-square significance test can be used at the nominal level, as can a number of measures of association, such as Tschuprow's T , V , C , Tau, and Lambda (Blalock

1979:299–325). Sociologists often dislike nominal categorical variables because it is felt that they are merely descriptive variables that do not possess the explanatory and predictive power of continuous variables, such as interval and ratio variables. But more important, nominal (and also ordinal) categorical variables are disliked because they generally do not fit into the classical multiple regression (OLS) models that (until the recent dominance of logistic regression) have been widely used in sociology.

In univariate cases with a large number of categories, or especially in multivariate cases with a large number of variables, and with each containing a large number of categories, the analysis can quickly become very complex, so that one is dealing with dozens if not hundreds of categories. As Blalock (1979) notes, there is often a tendency for researchers to simplify the analysis by dichotomizing variables (p. 327). Unfortunately, such attenuation results in both loss of information and bias.

Another problem with categorical data is that the printed page is limited to two dimensions. Thus, if one has as few as five categorical variables, and wishes to construct a contingency table showing their interrelations, this requires a five-dimensional table, but only two dimensions are available. The customary way to deal with this, even in computer printouts, is to print 10 bivariate tables, often leading to an unmanageable level of complexity.

Ordinal

Nominal and ordinal variables share some similarities and problems. Measures of association such as Spearman's r_s and tests of significance such as the Wilcoxon test are also available for ordinal variables (Blalock 1979). As with nominal variables, ordinal variables cannot be added, subtracted, multiplied, or divided (one cannot add rank 1 to rank 2 to obtain rank 3).

The ordinal level shares with the nominal level the problem of the desire to simplify. Sociologists often wish to reduce the number of ordered categories to simplify the research project, but unfortunately they often conduct this simplification in an ad hoc manner, without any statistical or theoretical guidelines for reducing the number of categories. Again, this leads to problems of attenuation and bias, as noted for the nominal level.

Interval and Ratio

A sea change has occurred in sociology in the last 40 years, as shown later in the review of *American Sociological Review* (ASR). During the 1950s and 1960s, American sociologists relied primarily on percentage analysis, often using nominal and ordinal measurement. Later in the twentieth century, quantitative researchers stressed the use of interval and ratio variables to meet the assumptions of OLS multiple regression analysis. Now, as seen below, there has been a major shift back to the use of nominal and ordinal variables in logistic regression.

112 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

Interval variables are continuous, with “arbitrary” zero points, while ratio variables have absolute or “nonarbitrary” zero points. Theoretically, only ratio variables, and only those found in nonattenuated fashion with a wide range of continuous values, should be used in multiple regression models, either as independent or dependent variables. Although textbooks such as Blalock (1979) say that only interval measurement is needed, in my opinion ratio is preferred and should be used whenever possible (p. 382). In reality, continuous variables are routinely used in regression without testing to see whether they can be considered ratio or only interval.

Furthermore, while such continuous variables may theoretically or potentially have a wide range of values, they often are empirically attenuated, with extremely high and low values (or perhaps even midrange values) occurring infrequently or rarely. Also, attenuated variables that are essentially ordinal, and contain only five values or so, are often used in surveys (e.g., Likert scales). While these Likert variables do not meet the technical requirements of multiple regression, either as dependent or independent variables, they are often used in regression, not only as independent variables but also as dependent variables.

As noted earlier, sociologists have traditionally struggled to meet the requirements of OLS regression, especially when encountering so many nominal and ordinal variables in everyday theory and research. For example, Knoke and Hout (1974) described their dependent variable (party identification) by saying, “The set of final responses may be coded several ways, but we have selected a five-point scale with properties close to the interval scaling our analysis requires” (p. 702). While this dependent variable may indeed be “close” to interval, it remains severely attenuated, possessing only five “points” or values compared with the hundreds or even thousands of potential values in some interval variables. In addition to using attenuated ordinal scales in regression (even though they clearly do not meet the assumptions of regression), sociologists often use nominal variables in regression. These are often used as predictors (independent variables) through the technique of “dummy variable analysis” involving binary coding.

As shown later by my review of *ASR*, the most common statistical technique in contemporary sociology is multiple regression in some form, including OLS and logistic regression. However, many of the variables used in sociology are nominal or ordinal. Those that are interval or ratio are often recoded as ordinal variables during data collection. The result is that between the existence of “naturally occurring” nominal and ordinal variables and the (often unnecessary) attenuation of nominal, ordinal, interval, and ratio variables, the range of empirical variation is greatly attenuated.

A common example is when an income variable with potentially dozens or even hundreds of values is reduced to five or so income categories to make it more manageable during the survey research process (see Bailey 1994b).

While it is true that respondents are often reluctant to provide their exact income, other alternatives to severe category attenuation are available. These include the use of additional categories (up to 24) or even the application of techniques for dealing with missing data. In addition, some common dependent variables, when studied empirically, are found to have small empirical ranges, but the adequacy of correlation and regression is formally assessed in terms of the degree of variance explained. Considering the cumulative effect of variables that are empirically attenuated, added to those variables that are attenuated by sociologists during the course of research, it is not surprising that explained variance levels are often disappointing in sociology.

A generic multiple regression equation for two independent variables is shown in Equation 10.1.

$$Y = a + b_1X_1 + b_2X_2 \quad [10.1]$$

The model in Equation 10.1 is quite robust and adaptable but should not be abused by using it with severely attenuated data. Although one cannot add additional dependent variables, additional independent variables are easily added. Also, the model can easily be made nonlinear by using multiplicative predictors such as X_1X_2 or X^n .

Assume that the dependent variable (Y) is annual income, and the predictors are, respectively, age and educational level. One could conduct an OLS regression analysis for a large data set and experience a fairly small degree of attenuation if the data were collected properly and the variables were not attenuated through unnecessary categorization. But now assume that a second regression analysis is computed on Equation 10.1, but this time the dependent variable is whether the person attends college or not, coded 1 or 0, and the independent variables are sex (coded 1 for female and 0 for male) and age (coded 1 for 20 or younger and 0 for 21 or older). Running OLS regression on this will yield very little in terms of explained variance. The analysis can be converted to logistic regression by computing the odds ratio and taking the natural log (logit) to make it linear. The limitations of this model are that little variance exists to be explained and the predictors are inadequate.

IMPLICATIONS

While many of the logistic regressions one sees in the sociological literature have many more predictors, many of these are often dummy variables (ordinal or ratio), and the wisdom of running regression on such data remains debatable. What accounts for the tremendous popularity of logistic regression, when many times the degree of variance explained remains decidedly unimpressive (see the discussion below)? Perhaps logistic regression is now a fad, or perhaps users do not see an adequate alternative. Why do they not just present correlation matrices? Why is regression needed? Perhaps because typologies using

nominal variables are said to provide description, correlation is said to provide explanation, and regression is said to provide prediction, with prediction considered to be the highest form of analysis (Blalock 1979).

The implications of the analysis to this point are clear: Sociologists have long struggled to deal with the analytical problems posed by the different levels of measurement, and they continue to do so. While the recent widespread adoption of logistic regression has surely changed the way that sociologists deal with nominal (and to a lesser extent ordinal) variables, for example, it is not clear that the fit between theory and method, or between empirical data and method, has been drastically improved. Changes are still needed, and some recommendations are presented below.

METHOD AND THEORY

As previously noted, method and theory have become sharply bifurcated within sociology over the past 40 years. While the *ASR* once published methods articles, now these articles are routinely segregated into journals, such as *Sociological Methodology*, *Sociological Methods and Research*, or the *Journal of Mathematical Sociology*. Thus, quantitative methods are not only separated from qualitative sociology (which has its own journals such as *Qualitative Sociology*) but also are separated from sociological theory (with its own American Sociological Association journal, *Sociological Theory*).

Kemeny and Snell (1962) state that one first inductively derives a theory through observation and empirical research and then uses quantitative models to deduce testable hypotheses from the theory. The procedure suggested by Kemeny and Snell (1962) is a sound one. The obvious problem with successfully using such an integrated theory/method research process in contemporary sociology is that the theory and quantitative methods knowledge segments are so segregated and widely divided that it is increasingly difficult for the individual researcher to have access to all of this separated literature. By segregating sociology into largely verbal theory (*Sociological Theory*) and quantitative sociology (the *Journal of Mathematical Sociology*), the process of developing theories and testing them is made more difficult than it should be.

In spite of the wide degree of artificial separation of theory and method in sociology, the quantitative area has changed in a manner that makes it more consistent with the needs of theory. To meet the goal of operationalizing sociological theory, the quantitative method area should minimally provide three main services:

1. Quantitative sociology must provide both diachronic (dynamic) models dealing with process and synchronic (cross-sectional) models dealing with structure. Until the last decade or so, statistical sociology provided mainly synchronic or cross-sectional models via OLS. Now many

logistic regression models are longitudinal as in event history analysis (Allison 1984).

2. The second service that quantitative method (including both statistical sociology and mathematical sociology) must provide is to talk increasingly in terms of actors rather than primarily in terms of equations or variables. While theory talks in terms of action by individuals or groups (agency), quantitative method talks in terms of change in variables (mathematics) or relationships among sets of variables (regression). A good example of the use of actor-oriented dependent variables in logistic regression is provided by Harknett and McLanahan (2004) who predict whether the baby's mother will take a certain action or not (marry the baby's father within 30 days).

3. Quantitative sociology must do a better job of raising R^2 s as variance explained in many regression analyses in sociology (whether OLS or logistic regression) remains unacceptably low. A lot of this may be due to attenuation of variables, both dependent and independent. As seen above, some of the attenuation is avoidable, and some unavoidable. Until recently, the dominant regression model was OLS regression, which did a poor job of incorporating nominal and ordinal variables. Logistic regression includes nominal variables aggressively, thus making it more compatible with theory that is replete with such nominal variables and providing a welcome means of bridging the theory-method gap. However, it is unclear that the incorporation of nominal variables (both dependent and independent) in logistic regression has raised the variance explained by any meaningful degree. It is important that we pay more attention to this problem and that we focus on R^2 values, not just on p values. That is, it is likely that there is actually more variance that can be explained empirically, but the techniques in use are not picking it all up. Perhaps sociology has lost sight of whether sociological models fit the data well, which is the primary point of prediction. To say it another way, if logistic regression is used in virtually every analysis in the *ASR*, it seems obvious that this method will fit the data better in some cases than in others. In the cases where it can be determined that the fit is not good, perhaps an alternative method of analysis should be considered.

HISTORICAL COMPARISONS

Perhaps most sociologists are at least vaguely aware of changes in quantitative techniques that have appeared in the sociological literature in the last 40 years, particularly the shift toward logistic regression. I decided that it would be helpful to illustrate these changes by conducting a review of the *ASR* over the last 40 years. While a full review of all issues was impossible due to time constraints, it seemed that a partial review would be illuminating. I compared the last full volume of the *ASR* that was available (2004) with the volumes 40 years before (1964), and 30 years before (1974), as shown in Table 10.1.

114 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

Table 10.1 Presence of Regression by Type (OLS or Logistic), *American Sociological Review*, 1964, 1974, 2004

Review, 1964, 1974, 2004

<i>ASR Vol.</i>	<i>Q^a</i>	<i>OLS^a</i>	<i>LR^a</i>	<i>Both^b</i>	<i>NQ^c</i>	<i>Total^d</i>
V. 29 1964	28 (70%)	2 (5%)	0 (0%)	0 (0%)	12 (30%)	40 (100%)
V. 39 1974	51 (86%)	25 (42%)	0 (0%)	0 (0%)	8 (14%)	59 (100%)
V. 69 2004	35 (95%)	3 (8%)	25 (68%)	4 (11%)	2 (5%)	37 (100%)

- a. Q = number of articles with quantitative analysis (at least some numbers or percentages), OLS = number of articles with least squares regression only (not logistic), and LR = number of articles with logistic regression only.
- b. Both = number of articles with both OLS and LR.
- c. NQ = number of articles without any quantitative analysis (no numbers).
- d. T = total number of articles. All percentages are percentages of this total, although some percentages reported in the text may use a different base.

Table 10.1 shows the presence or absence of quantitative analysis in every article of *ASR* in 1964 (Volume 29), 1974 (Volume 39), and 2004 (Volume 69). These volumes were not selected by scientific probability sampling but were arbitrarily chosen to reflect changes in quantitative methods. The first year (1964) shows the initial use of regression, 1974 shows the growth of OLS regression, and 2004 (the last full volume available) shows the dominance of regression, both the continuing presence of OLS and the predominance of logistic regression. Presidential addresses were omitted as they tended to be nonquantitative essays. I also omitted research notes, replies, and comments and included only the articles from the main research section of the journal.

The first row of Table 10.1 analyzes Volume 29 (1964) of *ASR*. It reveals that 70 percent of all articles (28 out of 40) were quantitative. The remaining 12 were verbal essays without any numbers. An article was counted as quantitative if it had raw scores or means. The predominant numerical method in 1964 was percentage analysis; however, there were two cases of regression analysis. These were OLS analyses with continuous dependent variables, although they were identified only as “regression analysis.” There were no instances of logistic regression. Although regression was soon to dominate sociological statistics, this trend was not yet evident in 1964.

However, by 1974, the trend toward the use of regression was clearly visible. The proportion of the articles that were quantitative in 1974 was 86 percent, up from 70 percent a decade earlier. Although there were still no logistic

regression analyses in *ASR* in 1974 (regression with categorical dependent variables), fully 49 percent of all quantitative articles (and 42 percent of all articles in the entire volume) were OLS regressions showing clear evidence of its upcoming dominance in sociological analysis.

It should be noted that in 1974, many of the OLS regression analyses were presented in the form of “path analysis,” with the “path coefficients” presented in path diagrams. While 70 percent of all *ASR* articles were quantitative in 1964 and 86 percent in 1974, by 2004 the proportion of quantitative *ASR* articles had climbed to a startling 95 percent, with logistic regression in some form accounting for the majority of these. Out of a total of 37 articles in Volume 69, only two were entirely verbal, lacking any numerical analysis at all.

Even more startling was the fact that in 2004, out of the 35 quantitative articles in *ASR*, 32, or 86 percent of all articles in the volume, and 91 percent of all quantitative articles were regressions. Still more surprising, of the 32 articles with regressions, only three had OLS regression only. The remaining 29 had logistic regression, with 25 of these containing logistic regression only, and with four more articles presenting both OLS and logistic regression in the same article. Four additional articles (not shown in Table 10.1) contained “hybrid” models, which used various combinations of OLS and logged dependent variables, or presented models said to be “equivalent to OLS,” and so on. Of the three quantitative articles that contained no regression, one contained both analysis of variances and analysis of covariance, while the other two contained only percentage analysis.

When logistic regression occurs in 29 out of 35 (83 percent) of quantitative articles and 29 out of 37 total articles (78 percent), it obviously has an amazing degree of dominance for a single technique. In fact, in the last four issues of Volume 29 (Issues 3, 4, 5, and 6), 19 of the total of 20 articles contained logistic regression of some sort (the other article was entirely verbal, with no quantitative analysis of any kind). This means that fully 100 percent of the quantitative articles (and 95 percent of all articles) in the June through December issues of the 2004 *ASR* (Volume 69) contained at least one logistic regression analysis. This dominance prompts the rhetorical question of whether one can realistically hope to publish in *ASR* without conducting logistic regression. It appears possible, but the odds are against it. If one wishes to publish in *ASR* without logistic regression analysis, the article should include OLS regression.

What accounts for the fact that in 2004, 95 percent of all published *ASR* articles were quantitative, and of these, 83 percent contained at least one logistic regression analysis? Could it be that quantitative sociologists in general are taking over the field of sociology, and sociologists should expect a wave of mathematical sociology articles to be published in *ASR*? I did not see any publications in Volume 69 containing articles that I would classify as mathematical sociology. I did see two models in 1974 that I would

classify as work in mathematical statistics (one stochastic model and one Poisson model), but none in 2004.

Comparing 1974 *ASR* articles with 2004 *ASR* articles, we see a sea change toward logistic regression. From the standpoint of quantitative methodology, I can certainly appreciate the heavy reliance that *ASR* currently has on logistic regression. While casual observers might say that “regression is regression” and that not much has changed in 30 years, in reality nothing could be farther from the truth. The 29 logistic regression analyses presented in Volume 69 of *ASR* differ from the 25 OLS regression analyses of Volume 39 in a number of important ways. The traditional OLS regression that was dominant in 1974 has the following features:

1. It uses a continuous (interval or ratio) dependent variable.
2. It uses predominantly continuous independent variables, perhaps with a few dummy variables.
3. It uses R^2 to evaluate explanatory adequacy in terms of the amount of variance explained.
4. It uses about 5 to 10 independent variables.
5. It usually reports values of R^2 (explained variance) in the range of .20 to .80, with most values being in the intermediate lower part of this range.

In contrast, the logistic regression that dominates twenty-first-century sociology has these features:

1. It uses categorical rather than continuous dependent variables (see Tubergen, Maas, and Flap 2004).
2. It often uses rather ad hoc procedures for categorizing dependent and independent variables, apparently without knowledge of proper typological procedures (Bailey 1994a) and without regard to the loss of information that such categorization entails, as pointed out by Blalock (1979). Some of these decisions about how categories should be constructed may be theory driven, but many appear to be arbitrary and ad hoc categorizations designed to meet the specifications of a computerized model.
3. It logs the dependent variable to “remove undesirable properties,” generally to achieve linearity, and to convert an unlogged skewed distribution to a logged normal distribution, more in keeping with the requirements of regression analysis (see Messner, Baumer, and Rosenfeld 2004).
4. It uses more categorical or dummy variables as independent variables, on average, than does OLS regression.
5. It uses larger samples.
6. It uses more “pooled” data derived through combining different samples or past studies. This has the advantage of getting value from secondary data. While it is good to make use of data stored in data banks, in some cases this

practice may raise the question of whether the data set is really the best one or is just used because it is available.

7. It uses more models (often three or more) that can be compared in a single article.
8. It uses more multilevel analysis.
9. It uses more “corrections” of various sorts to correct for inadequacies in the data.
10. It often does not report R^2 because it is generally recognized to have “undesirable properties” (see Bailey 2004), thereby providing no good way for evaluating the efficiency of the explication in terms of the amount of variance explained.
11. It generally reports statistically significant relationships with p values less than .05, and often less than .01, or even .001.
12. It presents more longitudinal analysis.

While the trends toward multilevel analysis, longitudinal analysis, and actor orientation are welcome, the plethora of categorical variables and the complexity of the presentations (often spilling over into online appendixes) are of concern. Also, while all computerized statistical programs are vulnerable to abuse, the probability that some of the “canned” logistic regression programs will be used incorrectly seems high due to their complexity. But the chief concern regarding the dominance of logistic regression is that while the recent logistic regressions appear more sophisticated than their traditional OLS counterparts, it is not clear that they have provided enhanced explanatory power in terms of variance explained. In fact, logistic regression in some cases may have lowered the explanatory efficacy of regression, at least when interpreted in terms of explained variance.

The binary coding of dependent and independent variables can obviously lead to extreme attenuation and loss of explanatory power, as noted by Blalock (1979). One of the most undesirable properties of R^2 for any dichotomous analysis is that the dichotomous dependent variable is so attenuated that little variance exists to be explained and so R^2 is necessarily low. If nothing else, the large number of cases when no R^2 of any sort is reported is certainly a matter of concern, as it makes it very difficult to compare the adequacy of OLS regressions with the adequacy of logistic regressions.

In lieu of R^2 , users of logistic regression generally follow one of three strategies: (1) They do not report any sort of R^2 (Hollister 2004:670), relying solely on p values. The p values of logistic regression often are significant due (at least in part) to large sample size, such as Hollister’s (2004:669, sample N of 443,399 in table 1). While large sample sizes may not guarantee significant p values, they make them easier to obtain than with the smaller sample sizes previously used in many traditional sociological studies; (2) they report a “pseudo R^2 ” (see Hagle 2004),

such as those reported by McLeod and Kaiser (2004:646) for their table 3, ranging in value from .017 to .112 (the highest reported in the article is .245 in table 5, p. 648); or (3) they report some other R^2 term, such as the Nagelkerke R^2 , as reported by Griffin (2004:551), in his table 4, with values of .065 and .079.

SUMMARY

In the middle of the twentieth century, sociology relied on careful percentage analysis as the backbone of its quantitative methodology, augmented by relatively rudimentary statistics, such as measures of central tendency, correlation coefficients, and tests of significance such as chi-square. Although sociologists were aware of multivariate statistics such as factor analysis and multiple discriminant analysis, the onerous computation that these methods required before computerization limited their use.

With the advent of mainframe computers in the 1960s and 1970s, sociologists could go to their university-computing center and run a variety of multivariate statistical analyses. Thus, by 1974, OLS regression became the dominant method. A major problem with OLS regression was that it could accommodate only a single interval-dependent variable, and the independent variables had to be intervally measured as well, except for “dummy” variables. Thus, many important theoretical variables, such as religion, race, gender, and so on, could not be properly accommodated in the dominant regression model.

But by 2004, all had changed. The sea change to logistic regression facilitated the use of multiple regression, as one no longer needed to limit the analysis to interval or ratio dependent variables. Also, the dependent variable could be logged. The advantages of logistic regression are great. These advantages include the facilitation of multi-level analysis (such as use of the individual and country levels) and the ease with which data can be pooled so that many surveys are used and sample sizes are large. Logistic regression makes good use of existing data sets and does a much better job of longitudinal analysis than OLS. Furthermore, the published logistic regressions are replete with categorical variables that were previously missing from OLS regression.

While the advantages of logistic regression are obvious, it may be debatable whether the dominance of this technique indicates that theory and method have merged in an ideal fashion in contemporary sociology. There are several reasons why. First, much sociological theory is not stated in terms of the binary-coded dichotomies favored in logistic regression. While the prediction of dichotomies is certainly theoretically significant in some cases, it would not seem to match the general significance of predicting the full range of values in an interval or ratio variable. That is, why limit the analysis to predicting 1 or 0, when it is possible to predict age from birth to death. Second, since sociological theory is generally not written in terms of logged

variables, it is difficult to interpret statistical analysis where the dependent variables are logged to normalize them.

In summary, the logistic regression analyses now dominating provide a number of benefits. These include, among others, advances in longitudinal analysis, in multi-level analysis, in the use of pooled data, in the presentation of more comparative models in each analysis, and in the presentation of more interaction analyses. But logistic regression sometimes appears to relinquish these gains by losing theoretical power when it is unable to provide impressive R^2 values. This is due in part to the excessive attenuation resulting from the widespread use of binary-coded dependent variables (often dichotomies).

PROSPECTS FOR THE 21ST CENTURY

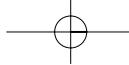
The future for quantitative sociology will include the continued use of logistic regression. There also will be further developments in blockmodeling and also in longitudinal methods, including event history analysis. There will also be continued interest in multilevel techniques (Guo and Zhao 2000) as well as in agent-based or actor modeling (Macy and Willer 2002). There will also be increased interest in nonlinear analysis (Meeker and Leik 2000; Macy and Willer 2002). In addition, there will be continued advances in regression analysis in such areas as fixed effects regression, including Cox regression (Allison 2005) and spline regression (Marsh and Cormier 2001).

Davis (2001) writes, “In sum, I believe the seeming technical progress of logistic regression (and its cousins) is actually regressive” (p. 111). In another analysis of the logistic regression model, Davis writes,

In short, despite the *trappings* of modeling, the analysts are not modeling or estimating anything; they are merely making glorified significance tests. Furthermore, these are usually merely wrong or deceptive significance tests because . . . they usually work with such large N s that virtually anything is significant anyway. (P. 109)

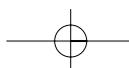
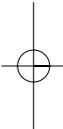
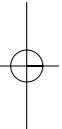
Davis recommends a return to path analysis, in part because it is easier to measure the success or failure of path analysis (p. 110).

Sociologists rely on logistic regression because the variables used are conducive to this technique. Davis (2001) also notes the shift within sociology from using psychology as a model to the present reliance on economics. He writes that in the 1950s psychology was the “alpha animal,” but now economics is a “Colossus” (p. 105). Quantitative researchers have long favored economic variables because they are easier to quantify. Furthermore, inequality research has benefited from the wide availability of economic coefficients such as the Gini (Lee 2005). Nevertheless, sociologists are now more likely to be citing *Econometrica* or *The World Bank Economic Review*, and the future influence of economics on sociology seems clear.



While the advantages of logistic regression are clear, there are other methods that deserve consideration as well. It is clear that sociologists will increasingly employ the methods of epidemiology, such as hazard and survival models and Cox regression (Allison 2005), and the methods and data sets of economics. But in addition, sociologists will

undoubtedly continue to collect their own data sets while employing the OLS regression and path analysis models. They will also use relatively neglected techniques such as factor analysis, analysis of variance, analysis of covariance, multiple discriminate analysis, canonical correlation, and smallest space analysis.



11

COMPARATIVE HISTORICAL SOCIOLOGY

WILLIAM R. WOOD

JOHN WILLIAMSON

Boston College

A subfield of sociological works exists, often grouped together under the name “comparative historical sociology” (CHS). These are the works of a sociology that emerged in the late 1960s and early 1970s, partially in response to perceived shortcomings of functionalism and crude Marxism, and partially as a return to classical sociological questions regarding the apparent contradictions and problems of modernity itself. Although a “comparative” approach is used in virtually all branches of social scientific inquiry, within CHS it has largely followed the works of Karl Marx and Max Weber with regard to the comparison of macrounits of analysis—the state, social class, capitalism, and culture. Its themes and major practitioners are well known; examples include Wallerstein’s (1974) study of the modern world-system, Moore’s (1966) study of democracies and dictatorships, Skocpol’s (1979) study of revolutions, and Mann’s (1986, 1993) study of the origins of social power. Although these names are not exhaustive, they are representative of a sociology that relies explicitly on the past to explain and understand the origins, auspices, and arrangements of social structures, institutions, and processes.

On the other hand, there exists outside of the field of CHS a multitude of subdisciplinary pursuits, all of which evidence a degree of overlap with the topical and methodological concerns of comparative historical work. “Historical sociology” is quite similar to, and is even labeled interchangeably with, CHS. Social history in its various forms, particularly the *Annales* School work under Fernand Braudel, the “new history” of E. P. Thompson, the

new “new history” of Perry Anderson, along with various “history from below” projects, all likewise share CHS’s interest in the formation and development of social class and nation-states. Feminist works such as Mies’s (1986) *Patriarchy & Accumulation on a World Scale* also have taken a comparative historical approach to the study of gender and social class. Even the work of scholars such as Philippe Ariès (1981), whose *The Hour of Our Death* epitomizes the French emphasis on the history of *mentalités*, or “attitudes,” is both comparative and historical insofar as it explicates differing social attitudes toward death and dying throughout Western history.

The issue, then, becomes where one should draw the line between CHS and other works demonstrating some degree of comparative sociohistorical investigation. In the case of CHS, this line was perhaps originally carved out between the grand theory of functionalism and the narrative singularity of historiography. As Stinchcombe (1978) has argued, “one does not apply theory to history; rather one uses history to develop theory” (p. 1). Thus, for Stinchcombe and many others in the field, CHS was, and remains, a fundamentally social-scientific endeavor, whose main purpose is not the narrative description of *wie es eigentlich gewesen ist* (“the way things actually were”) but rather the formulation of theoretical knowledge concerning historical processes and social structures. For many of its practitioners, CHS is not history, or even social history, per se, but rather represents the sociological analysis of history, particularly in relation to the rise of capitalism, social class, and the modern state.

This distinction between social science and history is not new. It is a tension that runs the gamut of sociological thinking, dating back to at least Comte's proposition of a "science of society," as well as to the succession of Enlightenment thinkers, who initiated the scientific study of economics, politics, and law. Marx and Durkheim both argued that their work was "scientific," although for Marx this "science" was intrinsically linked to the study of history. However, it was Weber who devoted the most serious consideration to the *methodenstreit* between the social and natural sciences—the question of whether sociology should be aligned nomothetically with the empirical sciences or idiographically with the traditions of hermeneutics and interpretation (Weber 1949).

More recently, within the last two decades, this tension between social science as either a nomothetic or an idiographic pursuit has become more pronounced. CHS has been criticized for its dependence on historical data, its proclivity for the use of small numbers of cases, and for its close association to qualitative research. This is also due, in part, to the rise of the so-called linguistic turn in the humanities and social sciences, which has affected not only CHS but also sociology in general in its claims to empirical knowledge and value-neutral methodologies. While much of the work in CHS still assumes Stinchcombe's proposition that history can be used to develop sociological theory, newer scholarship has questioned the limits of theoretical generalization within the field.

Arguably, it is not so difficult to trace the origins and seminal works within CHS. It is, however, decidedly more difficult to draw contemporary boundaries between this field of study and works within history, the humanities, culture studies, policy studies, international relations, and political science, which all appear to be moving with increasing ease between one another as disciplinary boundaries become more ambiguous. This chapter will look at the general contours of CHS: its origins, its major works, and its relationships to other fields of study. It will also look more closely at the debates regarding method, theory, and epistemology, giving special consideration to the longstanding tension between history and sociology.

ORIGINS

The foundations of CHS are present in its emergence as a distinct field within sociology in the 1960s and 1970s, as well as in the work of much of classical sociology that was itself vested in historical investigations of the rise of capitalism, the nation-state, and modernity. In this regard, CHS initially focused extensively, as it still does today, on the concepts of social class and the nation-state, where the influence of Marx and Weber are most present. Although a "comparative historical" approach can arguably be applied to a variety of phenomena, its emphasis on class and the state reflects basic concerns of Marx and Weber regarding the origins and role of social class, the rise of the modern

state, bureaucracy, industrialization, and revolution. Despite their dissimilarities, these two thinkers both believed that history itself provided an important explanatory role in their respective analyses of social change. The peculiar aspects of social organization related to capitalism, industrialization, bureaucracy, and modern rationality could only be located and analyzed in the past histories of modes of production, "primary" accumulation, the division of labor, technology, religion, and government.

Durkheim is often left out of the discussion of CHS and "the classics." While Durkheim's work was, in some sense, no less dependent on history than Marx's or Weber's, for Durkheim history could not define the function of a particular social fact, nor could it provide a positivistic framework necessary for the analysis of social organization. Mathieu Deflem (2000) characterizes this as "the distinction between causal explanation and functional analysis," where "causal-historical research and functional-synchronic analysis were divorced and the latter was often the privileged perspective," particularly within midcentury American sociology.

The use of history for explaining and understanding social change and organization was also present in the work of other well-known late-nineteenth- and early-twentieth-century scholars. Sombart's ([1902] 1928) *Der moderne Kapitalismus* continued in the tradition of Marx's analysis of the history of capitalism. The early work of the *Annales* School under Bloch and Febvre in the 1930s, as well as its later direction under Fernand Braudel, has been influential within CHS and world-systems theory specifically. Polanyi's (1944) *The Great Transformation* analyzed the rise and apparent failure of the "market society." Hannah Arendt's (1951) *The Origins of Totalitarianism* compared the rise of Soviet communism and German fascism and their relationship to anti-Semitism. These works deserve mention because they mitigate the notion that the close relationship between history and sociology was "rediscovered" in the waning light of functionalism in the 1960s and 1970s. By the middle of the last century, American sociology had become the predominant locus of sociology itself; the work of Talcott Parsons and other functionalists came to dominate almost every major research university in the United States. Yet even within Parsonian functionalism, as well as in the work of other midcentury scholars such as Robert Merton, history per se was not ignored. Rather, with its emphasis on the search for a general theory of social organization, functionalism largely eschewed history as a viable means of sociological explanation.

Immanuel Wallerstein (2000) has called the era of functionalist dominance between 1945 and 1960 the "golden age" of sociology, the time when "its tasks seemed clear, its future guaranteed, and its intellectual leaders sure of themselves" (p. 25). Yet somewhat rapidly, sociology moved from the certainty and dominance of midcentury functionalism to the uncertainty of a discipline united in name only. One consequence of this sociological fracturing

120 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

was a return to history, or, more specifically, a return to Marxist and other critical works that viewed social problems as immanently rooted in history itself—colonialism, capitalism, slavery, and war. Although Weber's work was also being reread, comparative historical works in the late 1960s and 1970s owed more to Marx than to Weber, influenced in part by new readings of Marx in Britain (E. P. Thompson, Perry Anderson, and Eric Hobsbawm) and France (Althusser and Braudel). While by no means homogeneous—for example, the disagreements between Thompson and Althusser—variations of Marxist analysis were by far the most prevalent within both “new history” as well as within the inception of the so-called second wave of historical sociology.

Although the works of Marxist historians were (and remain) influential within CHS, what separated “new history” and the *Annales* School from early CHS was the proposition that CHS could be empirical, and could generate generally applicable theory. This was evident in the use of comparative methodologies and particularly the development of the case studies approach. A principal concern of comparative historical sociologists was not so much the writing of history but rather the use of history for the development of empirically valid theories about large-scale social change: the transitions from feudalism to capitalism, agrarianism to industrialization, fiefdom to nation-state, and local culture to commodity culture.

However disparate in terms of individual works, the CHS that emerged from the late 1960s until the early 1980s was articulated largely as a “middle ground” between the grand theories of functionalism, teleological Marxism, and the perceived idiosyncratic tendencies of historiography. Tilly (1981) notes that such a “middle ground” was not an attempt to reconcile theory and history. On the contrary, it was a conglomeration of specialties that sought to “concentrate on human social relationships . . . deal with change over a substantial succession of particular times [and] . . . yield conclusions that are generalizable, at least in principle, beyond the particular cases observed” (p. 57). This approach is clear in the work of Wallerstein, Tilly, Skocpol, Stinchcombe and Moore, and others and remains a central position in comparative historical research today.

MAJOR THEMES AND WORKS

A majority of comparative historical works share important general features. Notably, CHS deals in macrosocial units of analysis. Charles Ragin (1987:8–9) makes a useful distinction between “observational units” and “explanatory units” of analysis within comparative work. This distinction is common throughout sociology, as Ragin (1987) notes:

For most noncomparative social scientists, the term [unit of analysis] presents no special problems. Their analysis and their explanations typically proceed at one level, the individual

or organizational level. This is rarely the case in comparative social science, where analysis often proceeds at one level (perhaps the individual level) . . . and the explanation is couched at another level (usually the macrosocial level). (P. 8)

Dependency theorists and neo-Marxists, for example, rejected the assumptions of early modernization and development theories by bringing attention to larger external factors involved in the purported “inability” of poorer nations to modernize. The sizable corpus of work on revolutions has documented the degree to which external and larger units of explanatory analyses are involved in the precipitation, as well as the successes or failures of revolutionary movements. Thus, early comparative historical work emerged out of a context in which an endogenetic model of social change was standard; a major focus of CHS has been to determine how and where larger social, economic, and political structures contribute to or determine historical processes and events.

Within CHS, the idea of an “explanatory unit of analysis” is not the same as the establishment of direct causality. Contrary to other comparative sociological approaches, for example, comparative cross-national analysis, CHS usually does not present or analyze casual determinacy through statistical methodologies. Even where quantitative analysis is sometimes used, the emphasis on outcomes is almost always on the identification of what Ragin (1987) calls “intersections of conditions,” and it is usually assumed that any several combinations of conditions might produce an outcome.

One reason for the emphasis on “intersections of conditions” is that CHS focuses extensively on the identification and development of historical “cases.” As they are used in comparative historical work, cases involve the identification of particular processes, institutions, or events as situated within a larger temporal setting. The development of cases thus requires extensive knowledge not only of the particular phenomenon being studied but also a broad understanding of the economic, social, and cultural milieu in which this phenomenon has occurred as well as its location within a temporal sequence of complex events and the identification of possible causal relationships.

The effort required in the development and identification of case studies explains, in part, why comparative historical work usually results in a small *n*. A second explanation for small *n* is that the macrosocial units of analysis that are of interest to comparative historical sociologists are often limited in number. Comparative historical works that use a small or single *n* are, therefore, more often qualitatively oriented, and rely on methodologies more suited to the development of rich and detailed description of individual cases, the identification of unforeseen or unanticipated phenomenon, and the proposal of general hypotheses that may be followed up through more detailed studies.

Another commonality within CHS is an interest in macrosocial changes over long periods of time, decades or

even centuries. Fernand Braudel's work, in particular, remains influential for his notion of the *longue durée*. In CHS, world-systems theory remains most closely aligned with the study of the *longue durée*. Even where Braudel's work may be criticized for its Marxist structuralist approach, the overarching notion that the study of large social structures and processes requires a long durational setting is common throughout comparative historical work.

Finally, most comparative historical works draw from a variety of disciplines, not only history and sociology but also economics, political science, legal studies, geography, and more recently race, gender, and culture studies. This is often necessary both for the development of suitable comparative cases as well as for the analysis of macrosocial structures and processes, where different disciplines provide a contextual framework not readily apparent within sociology. Paige's (1997) *Coffee and Power*, for example, draws from various disciplines—economics, political science, gender studies, and culture studies—in comparing the histories of El Salvador, Nicaragua, and Costa Rica over the last century, and proposes that each case cannot be understood outside of the complex political, social, and cultural relationships each country has had in relation to this commodity.

Thus, even within these aforementioned similarities, any attempt at grouping the wide disparity of works within CHS requires an artificial thematic, theoretical, or methodological unity not borne out in the disparity of works in the field. Much debate exists regarding not only the merits of individual works but also in their respective classifications. For our purposes, Charles Tilly's classification of the various levels of comparison within the study of comparative history remains conceptually useful. Tilly (1984:60–61) categorizes comparative historical works into four categories: world-historical, world-systemic, macro-historical, and micro-historical. Of these, we will look at the first three, as they constitute an overwhelming amount of work within CHS.

WORLD-HISTORICAL APPROACHES

In the category of world-historical approaches, Tilly includes works from Toynbee and Braudel as exemplifying “schemes of human evolution, the rise and fall of empires, and of successive modes of production.” Arguably, the work of Toynbee (1934), particularly his *A Study of History*, falls squarely into the category of “the rise and fall of empires,” as does the work of those such as Oswald Spengler (1926) and Samuel Huntington (1997). For varying reasons, all of these scholars, except Braudel, have played fairly minor roles within CHS—Spengler and Toynbee, perhaps for their almost total lack of materialist analysis, and Huntington, who as Matlock (1999) has argued, “makes the same error Toynbee did in assuming that the many disparate elements that make up his ‘civilizations’ comprise a coherent, interdependent whole” (p. 432).

Marx and Weber

Marx's theoretical connection between the forces and relations of production as a means by which to understand and methodologically approach social organization and power remains central within sociology, and particularly germane to comparative historical analyses. Marx's work has also proved fruitful in CHS in the extension of his notion of the capitalist mode of production to larger geographical regions, such as in world-systems analysis, and in the comparative historical interest in revolutions. Finally, Marx remains central within CHS by way of influence of the Italian Marxist, Antonio Gramsci. Gramsci's concept of “hegemony,” formulated in his analysis of the Italian working-class embrace of fascism, has been widely adopted, used, and critiqued in comparative historical work, particularly in Marxist work on the state.

In the case of Weber's influence on CHS, this is more difficult to trace to any single work or even particular theory, as his work was less organized than Marx's around a particular theme or organizing principle. The best known among Weber's ([1930] 2001) comparative works remains *The Protestant Ethic and the Spirit of Capitalism*, but this work has been less influential within CHS, and arguably sociology itself, than his other writings. In the case of CHS, Weber's work was also closely associated with that of Parsons's and structural functionalism. Outside of the more interpretive emphasis of Bendix and the pluralist approach of those such as Lipset, Weber was somewhat cast aside in favor of the reinvigoration of Marx that characterized much of CHS in the late 1960s and early 1970s.

By the later 1970s and 1980s, however, Weber's work was being widely read and used within CHS, including studies of nation-states and state policy, nationalism, and social movements. Contrary to Marx, for whom society was defined more or less as the forces and relations of production, Weber argued that class alone was not able to account for the variety of forms of social organization. “Status” and “party” were, in Weber's estimation, equally influential spheres of social life. This recognition alone has been most important for sociology, which now recognizes economics, politics, and culture as distinct and interrelated spheres of social organization and power.

Weber's influence on CHS, however, extends beyond his tripartite analysis of social organization. Weber's (1975:128) interpretive method (*Verstehen*) broadened the task of social analysis by proposing that sociology must elucidate not merely the causal sequence of events but also the meaning of social action. In Weber's estimation, people, institutions, and organizations act for a variety of reasons: class interest, obligation, honor, emotion, tradition, custom, or habit. Understanding the meaning of social action was therefore as important as the effects of such actions, insofar as they were both necessary components of causal explanation. As sociologists could rarely definitively know the actual motives of social actors, Weber stressed the need for “ideal types” of social action

122 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

(e.g., instrumental rational action, value-oriented action, affective action, and traditional action) against which specific cases could be juxtaposed. Comparative analysis was useful and necessary for Weber both for understanding the differences between different cases as well as for refining ideal types.

At the same time, Weber argued that the nature of an “interpretive” science mitigated the possibility of causal attribution when juxtaposed against that of the natural sciences. As Giddens (1971) notes, “Weber stresses that causal adequacy always is in a matter of degrees of probability . . . [T]he uniformities that are found in human conduct are expressible only in terms of the probability that a particular act or circumstance will produce a given response from an actor” (p. 153). Here, Weber’s work has seeped down thoroughly into CHS, which more often seeks “conjectural” explanations than “calculable” ones.

WORLD-SYSTEMS THEORY

Out of Tilly’s four categories, “world-systems approaches” denotes the most cohesive corpus of work within CHS. While Wallerstein’s (1974) *The Modern World-System* is generally regarded as the starting point of the world-systems approach, the last 30 years has seen the subsequent proliferation of works from many scholars. Wallerstein developed his concept of the “modern world-system” partially as a response to perceived deficiencies within modernization theory and partially in relation to Braudel’s notion of the *longue durée* and the “world economy.” In *The Modern World-System*, Wallerstein argued that contrary to the apparent “success” of capitalism in the West, and its apparent “failures” elsewhere, modern capitalism represented rather a single “world-system” based largely on the geographical division of labor between “core,” “semiperipheral,” and “peripheral” regions. For Wallerstein, there had been other “world-systems,” largely articulated under a single political entity, but the modern world-system is unique in that it constitutes “a world-economy [that] has survived for 500 years and yet has not come to be transformed into a world-empire” (p. 348).

This uniqueness is explained through the historical rise of Western capitalism. According to Wallerstein (1974), “capitalism has been able to flourish precisely because the world-economy has had within its bounds not one but a multiplicity of political systems” (p. 348). Wallerstein’s argument rests on the notion that within the modern world-system, capitalism relies on a particular geographical configuration of the division of labor but is at the same time not bound to any one geographical location. Arrighi (1997) notes,

Central to this account [is] the conceptualization of the Eurocentric world-system as a capitalist world-economy. A world-system [is] defined as a spatio-temporal whole, whose spatial scope is coextensive with a division of labor among its

constituent parts and whose temporal scope extends as long as the division of labor continually reproduces the “world” as a social whole. (Para. 5)

The division of labor under capitalism, while certainly present within early-modern Western European states, was for Wallerstein more pronounced as a division of labor and resources that began in the sixteenth century to define the respective core, semiperipheral and peripheral regions of Western Europe, Central Europe, Eastern Europe, and the Americas. Where Northwestern Europe was successful in amassing capital for purposes of industrial production, largely through war and colonization, it was also able to coerce or force semiperipheral and peripheral regions into the production of foodstuffs and cheap textiles, as well as the exportation of raw materials. While limited movement between these regions has occurred, most notably in the case of the United States as the now dominant “core” region, for Wallerstein the movement within regions is secondary to the arrangement of the system itself.

Wallerstein’s analysis has been expanded on in a proliferation of works both critical and complementary to his theory of the modern world-system. One of the best known is Arrighi’s (1994) *The Long Twentieth Century*. Arrighi follows Wallerstein’s logic of a global world-system but emphasizes the ebb and flow of finance capital in what he calls “systemic cycles of accumulation.” Arrighi identifies four major systemic cycles of accumulation, dating from the sixteenth-century Italian city-states (particularly Genoa), moving to Holland in the eighteenth century, Britain in the nineteenth century, and finally the United States in the twentieth century. For Arrighi, the study of the movement and growth of capitalism must take into account not only the division of labor or the periodic stability of production but also the periods of crises and instability by which capitalism is able to move expansively from one region to another. Profitability in trade and production, argues Arrighi, periodically reaches geospatial limits, at which point capital moves toward high finance, war, and eventual relocation into newer and larger spheres of trade and production.

Other world-systems scholars have argued that the world-system existed prior to the rise of European capitalism. Janet Abu-Lughod’s (1989) *Before European Hegemony*, for example, suggests that the world-system as conceptualized by Wallerstein is actually a subset of a larger world economy that encompassed parts of China, Southeast Asia, Africa, and Europe from the twelfth to the fourteenth century. Andre Gunder Frank and Barry Gills (1993) have also argued that Wallerstein’s world-system is itself part of a larger world-system, but unlike Abu-Lughod, they see this world-system dating back not to the eleventh or twelfth century but 5,000 years. For Frank and Gills, the conceptualization of a larger and more truly global world-system represents more than an attempt to “reorient” Wallerstein’s unit of analysis on an even larger

scale. It also questions major assumptions within world-systems theory, and indeed much of classical sociology itself, regarding (1) the analysis of capital accumulation as a peculiarly European phenomenon; (2) the notion that “core,” “semiperiphery,” and “periphery” are relatively new or exist only within the European development of capitalism; and (3) whether or not cycles of expansion and contraction within European capitalism are in fact only part of an interrelated world-system “that extend[s] back many centuries before 1942” (pp. 3–4).

MACRO-HISTORICAL APPROACHES

In Tilly’s categorization of differing levels of comparative analysis, macro-historical approaches fall in between world-systems approaches and micro-historical approaches. Tilly (1984) argues that “at this level, such large processes as proletarianization, urbanization, capital accumulation, statemaking, and bureaucratization lend themselves to effective analysis” (pp. 63–64). In describing different units of analysis, Tilly is also making an argument that the “macro-historical” approach deals with the largest units of analysis from which empirically verifiable arguments can be derived from comparative case studies. Although this point remains contentious, it is the case that the large majority of work in CHS focuses on the processes taking “states, regional modes of production, associations, firms, manors, armies, and a wide variety of [other] categories” as their units of analysis (p. 63).

Nation-States

Virtually all historical comparative works engage various aspects of nation-states in the study of different forms of government, social class, revolutions, militarism, social welfare, civic society, social citizenship, and cultural studies. States are used both as descriptive and explanatory units of analysis. Over the last half-century, the most well-known approaches to the study of the state are structural-functionalist theories, including pluralism and early modernization theory; elitism, Marxism, and class-centered theories; the state-centered approach, and institutionalism or new institutionalism.

Pluralist and Modernization Theories of the State

Pluralist theories of the state such as those put forth by Parsons (1966, 1969, 1971) and Smelser (1968) have tended to view the liberal democratic state and particularly the United States as a neutral mechanism for the “equilibration” of competing actors and groups. Social class has on occasion been identified as an important or central interest group, but pluralist approaches have more frequently emphasized the ability of the free market and representative democracy to mitigate the concentration of power. A variation of pluralist theory known as “elite

pluralism” or “polyarchy” concedes that elites maintain a disproportional amount of power and influence within liberal democracies but views competition among different elite groups as prohibitive of the creation of a single ruling class.

Pluralist theory has been largely confined to analyses of modern Western states. Its functionalist correlate for the study of nonindustrial Western nations is found in early modernization theory (also called development theory). Here, nation-states are assumed to develop in a similar unilinear fashion, and modernization theorists have argued that a “dichotomy” exists between traditional and modern states. The question for modernization and development theorists such as Rostow (1960), Almond and Powell (1966), and Eisenstadt (1966) was thus how to “encourage” policies of industrialization and democratization similar to those that had occurred in the West.

Marxist and Class-Centered Theories of the State

Marxist theories of the state became quite popular by the 1960s in both Europe and North America. The well-known “Miliband-Poulantzas,” often referred to as the “instrumentalist-structuralist” debate, seen as crucial at that time, was between Marxists who viewed the state as more of a direct or subjective extension of class interests (e.g., as an “instrument for the domination of society”; Miliband 1969:22) and those who viewed the state as a distinct set of structures and practices through which the logic of capitalism was naturalized and reproduced. While the instrumentalist position was quite popular, structuralist theory has fared better within sociological analyses of the state, particularly in its ability to analyze the state less as the subjective extension of the elite than as an objective relation of economic, political, and social structures or “state apparatuses.” Structural Marxists, for example, have investigated (1) the manner in which capitalism was reproduced in “institutional form[s] of political power” (Offe and Ronge 1975:139), (2) the use of social welfare to stabilize class conflict (Gough 1979), and (3) the successes and failures of states to mediate fiscal crises (O’Conner 1973) and legitimization crises (Habermas 1973; Offe 1973). The work of Offe, in particular, recognized important contradictions between state institutions, as well as circumstances where states acted against the interests of elites.

Where structural Marxism has fared better is within works that are more historically oriented. The structural Marxism of Althusser and the anthropological structuralism of Lévi-Strauss, on the other hand, have largely fallen out of favor for their tendency toward transhistorical or functionalist analysis of deeply rooted social structures that were seen as totalizing or teleological by other Marxists (e.g., Anderson 1974; Thompson 1963).

Perry Anderson’s (1974) *Lineages of the Absolutist State*, along with the work of Moore (1966), represented a decidedly different class-centered approach to the study of states, suggesting that history was far more important in

124 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

understanding the development of modern states than most structural Marxists had allowed for. These two works were central in the development of the “comparative historical” method. Both Moore’s and Anderson’s work cast significant doubt on the idea that states followed anything like a normative or unilinear progression of development. Anderson argued that contrary to the idea that an emerging bourgeoisie had merely supplanted the landed feudal aristocracies of Europe, absolutist monarchies had rather helped to foster the bourgeoisie. For Anderson, however, this did not occur at the same level throughout Eastern and Western Europe, and particularly in England. A large part of Anderson’s analysis was therefore directed toward explaining the “lineages” of absolutist states from relatively similar feudal relations to decidedly different modern economic and political paths.

Barrington Moore’s (1966) *Social Origins of Dictatorship and Democracy* set the stage for a generation of comparative historical work on nation-states. His general thesis is often summed up as “no bourgeoisie, no democracy.” In each case study, Moore argued that the relative strength of the bourgeoisie was decisive in the formation and outcome of different revolutions or revolutionary movements. Moore then linked these different revolutionary typologies to the development of differing forms of modern governments—democracy, fascist dictatorship, or communist dictatorship.

For Moore, however, the presence or absence of a strong bourgeoisie was important *within* a sequence or ordering of specific historical events. In this sense, Moore’s was one of the first comparative historical works that analyzed cases both structurally and temporally. As Mahoney (2003) notes, “Since the publication of *Social Origins*, nearly all comparative historical scholars have come to theorize about the ways in which the temporal ordering of events and processes can have a significant impact on outcomes” (p. 152). More generally, Moore’s work suggested that class conflict itself was not given to any one specific historical trajectory or outcome.

State-Centered Theory

By the late 1970s and early 1980s, comparative historical sociologists were questioning the usefulness of Marxist analyses. If there had existed any thematic or theoretical unity in the field under its earlier Marxist cohesion, the 1980s (1) witnessed not only the demise of any such cohesion but the beginnings of a proliferation of different approaches to the study of states that rejected earlier assumed groupings of capitalism and the state as cohesive or binomial components of “society-centered” approaches, (2) questioned the limitations of class conflict and the division of labor as an analytical approach to the study of modern states, and (3) analyzed the “agency” and efficacy of states, elites, and institutions.

The single biggest shift in the historical comparative study of states was the development of the state-centered

approach of Evans, Giddens, Mann, Reuschmeyer, Skocpol, and Tilly in the early 1980s. Although varied in their respective emphasis on different aspects of state formation and activity, this approach was a redress of what Skocpol called “society-centered” functionalist, pluralist, and Marxist approaches to the study of the state that, as Skocpol (1985) argued, tended to view states as “inherently shaped by classes or class struggles [that] function to preserve and expand modes of production” (pp. 4–5).

State-centered theorists drew heavily from Max Weber’s work on bureaucracy and political sociology. Contrary to Marx, Weber had developed a comprehensive and systematic theory of the state, one that agreed with Marx’s analysis of class divisions but rejected Marx’s primacy of class itself as determinate or even central in the formation or logic of modern states. Weber ([1919] 1958) argued rather that “sociologically the state cannot be defined in terms of its ends . . . Ultimately, one can define the modern state sociologically only in terms of the specific *means* peculiar to it, as to every political association, namely, the use of force” (pp. 77–78). The primary goal of the state was, in Weber’s analysis, sustained sovereignty over a particular territory through the monopoly of the legitimate use of physical force.

Using Weber’s work, proponents of the state-centered approach thus argued that states themselves should be considered as “weighty actors” able to “affect political and social processes through their policies and patterned relationships with social groups” (Skocpol 1985:1). Jessop (2001) summarizes nicely the major assumptions and research foci of the state-centered approach:

- (1) The geo-political position of different modern states within the international system of nation-states . . .
- (2) the dynamic of military organization and the impact of warfare in the overall development of the state;
- (3) the distinctive administrative powers of the modern state . . .
- (4) the state’s role as a distinctive factor in shaping institutions, group formation, interest articulation, political capacities, ideas, and demands beyond the state . . .
- (5) the distinctive pathologies of government and the political system—such as bureaucratism, political corruption, government overload, or state failure; and (6) the distinctive interests and capacities of “state managers” (career officials, elected politicians, etc.) as opposed to other social forces. (P. 153)

The state-centered approach opened up or expanded on several avenues of comparative historical research, including the study of economic policy (Evans 1985; Reuschmeyer and Evans 1985), revolutions (Farhi 1990; Goodwin 1997; Skocpol 1979; Wickham-Crowley 1991, 1992), and militarism and war (Giddens 1987; Mann 1988; Tilly 1985).

Institutionalism

Popular throughout the 1980s, state-centered theory largely merged with or moved toward what is called

historical institutionalism. In the late 1980s and early 1990s, two new institutional approaches, rational choice theory and historical institutionalism, emerged as interdisciplinary pursuits within political science, organizational studies, economics, and sociology. Within CHS, historical institutionalism is closely aligned with the state-centered approach insofar as it recognizes the state as a potential locus of action. However, historical institutionalists such as Hacker, Immergut, Pierson, Skocpol, Steinmo, Thelen, and others have moved away somewhat from the notion of the state as actor, toward the investigation of how institutions themselves are both agents and objects within larger networks of structurally limited possibilities. A central focus of historical institutionalism is the emphasis on historically contingent institutional “paths” or “path dependency.”

Pierson (2000) describes path dependency as “increasing returns” where “the costs of switching from one alternative to another will in certain social contexts increase markedly over time” (p. 251). Path dependency thus seeks to explain the “initial conditions” or “critical junctures” that precipitate specific institutional paths, recognizing that small events or actions can lead to large outcomes. Historical institutionalists also recognize that while paths may become more stable or determined through positive feedback, outcomes are not predetermined. Emphasis is placed on the “timing” or “sequence” of events in an attempt to explain institutional movement or development.

Historical institutionalism also argues that questions of power and legitimacy are almost inexorably linked to institutional processes. Comparative historical sociologists and political scientists have used this approach extensively when explaining why similar institutional structures and choices vary widely between states in the case of social welfare (King 1992; Orloff 1993; Pierson 1994; Skocpol 1992), social health care policies (Immergut 1992), taxation (Steinmo 1993), and labor movements and democratization (Collier and Collier 1991; Mahoney 2002).

Social Class and Labor

A key theme in the comparative study of social class has been the historical formation of modern classes. Researchers interested in “transition periods” in Europe and the United States have developed different theories about the movement from feudalism to capitalism and from agrarianism to industrialism. Hobsbawm (1965) argued that a “general crisis” within seventeenth-century Europe had been central to the development of European capitalism. Brenner (1977) proposed that levels of peasant organization and revolt could explain the emergence of variant forms of capitalism in Europe, particularly the early development and force of industrialism in England. The work of Moore and Anderson (discussed above) was also central in transitional literature. E. P. Thompson’s (1963) *The Making of the English Working Class* was a redress to structural Marxism (specifically Althusser), and this work continues to be influential for his thesis that class is not merely a

structural category but rather “an active process, which owes as much to agency as to conditioning” (p. 9).

Comparative historical sociologists such as Tilly have argued that the nineteenth century represented a substantial shift in the formation of social class. Tilly’s (1975, 1978) work emphasizes the change in later-nineteenth-century Europe from collective “reaction” to more deliberative or purposive collective action such as labor organization strikes. As Eder (2003) notes, “What changes in 1848, the year chosen by Tilly as a convenient time marker, are the claims and the action repertoire. Claims become more proactive; new rights are claimed, rather than old rights defended” (p. 279).

The comparative study of organized labor in Western twentieth-century states has looked at general patterns of labor strength and organization between states, as well as produced several notable comparative works on specific labor movements and unions (see Haydu 1988; Taylor 1989; Tolliday and Zeitlin 1985). Voss’s (1993) work on the Knights of Labor rejects the “American exceptionalism” explanation for the conservatism of American labor movements and concludes that the fall of the Knights of Labor shifted the direction of American labor unions toward a decidedly different and more conservative course. Stepan-Norris and Zeitlin’s (2002) *Left Out: Reds and America’s Industrial Unions* argues that the post-World War II decline of unions can be traced to the anticommunist purging that effectively crippled many unions. Kimeldorf’s (1988) *Reds or Rackets* explored how longshoremen’s unions on the East and West Coasts of the United States developed, respectively, toward conservative and radical political affiliation.

Comparative studies of class have also looked at the changing structures of labor itself in the West, particularly in the later part of the twentieth century. The world of Mills’s “white-collar” managers and the division between the managerial and working classes has given way to a complex arrangement of labor sectors and relationships. Myles and Turegun (1994) argue,

By the 1970s virtually all class theorists—Marxist and Weberian—had converged on the centrality of two broad strata for understanding the class structure of advanced capitalist societies: the growing army of mid-level corporate officials engaged in the “day-to-day” administration of the modern firm . . . and the professional and technical “knowledge” workers who have become virtually synonymous with postindustrialism. (Pp. 112–13)

Moreover, as Myles and Turegun note, the rise of the latter group has been categorized alternatively as “the service class” (Goldthorpe 1982), as part of “new petite bourgeoisie” (Poulantzas 1975), or as “knowledge workers” (Wright 1978).

The division of bourgeoisie/proletariat or owner/worker has thus become more complex with the rise of managerial and “middle” classes, and comparative historical sociologists have sought explanations for differences or

varieties of class formation—largely in comparative studies of states. Katzenstein (1984, 1985) has identified differences between liberal (e.g., the United States and Britain), statist (e.g., France), and corporatist (e.g., Germany, Austria, and smaller European states) systems of capitalism as crucial for the development of class and the relationship between labor and capital. Others such as Zysman (1983) and Arrighi (1994) have emphasized the central role of financial systems and finance capital in the structuring of industry, labor markets, and social class.

The comparative historical study of class and labor in other regions besides Europe and the United States is still limited but has increased somewhat more recently, partially in relation to the rise of global commodity chains and the rapid change in labor relations under structural adjustment policies and flexible accumulation. Bonacich et al. (1994), Candland and Sil (2001), and Silver (2003) have all looked at global production schemes or changes in global labor trends and relations. Studies of labor relations in Latin America include Collier and Collier's (1991) case studies of eight Latin American countries and the relationship between labor movements and political developments in the twentieth century; Bergquist's (1986) *Labor in Latin America*, which looks at the experiences of workers in the export-oriented economies of Argentina, Columbia, Chile, and Venezuela; Huber and Stafford's (1995) *Agrarian Structure and Political Power*; and Murillo's (2001) *Labor Unions, Partisan Coalitions, and Market Reforms in Latin America*. Studies of labor in Asia include Frenkel's (1993) edited volume *Organized Labor in the Asia-Pacific Region: A Comparative Study of Trade Unionism in Nine Countries*; Gills and Piper's (2002) edited volume *Women and Work in Globalizing Asia*; and Hutchison and Brown's (2001) *Organizing Labour in Globalizing Asia*. Comparative historical work on Africa is perhaps not surprisingly the most underrepresented within the field, the work of Michael Burawoy (1972, 1981) being the notable exception.

Revolutions

In many respects, because the study of revolutions and states in CHS are so closely tied to one another, the movement of research and theory about revolutions parallels research on the state itself. Midcentury American thought on revolutions tended to follow a functionalist analysis, using variants of early modernization theory to explain revolutions as disequilibria between traditional and modern forms of social organization. However, as Goldstone notes (2003:58–59), large *n* studies attempting to link “the strains of transition” to revolutions have been only partially successful at best. The most notable finding that came out of these studies, argues Goldstone (2003), was the realization that “different countries were different in important ways, and that revolutions themselves were different in how they unfolded, their levels of violence, and which elites and groups were involved” (p. 59).

The assumption of unilinear development from premodern to modern society is not unique to functionalist analysis of revolutions, however. In the case of historical materialist accounts of revolution, as Comninel (2003) notes, “The classic formulation of this transformation has been as ‘bourgeois revolution’—a historically progressive class of capitalist bourgeois taking political power from an outmoded landed class of feudal aristocrats” (p. 86). Moore's (1966) work, however, cast significant doubt on both orthodox Marxist and functionalist depictions of any unilinear progression from premodern to modern states, and the role that revolutions play in this transformation. While Moore argued that class conflict, and particularly the strength of peasant movements, was central to the potential for and shape of revolutions in his case studies (Russia, France, Germany, Japan, the United States, Great Britain, and India), his analysis also showed that varying forms of class conflict led to very different types of revolutions and subsequently to different types of modern states. Tilly's (1978) work *From Mobilization to Revolutions* also centered on class conflict as central to revolutionary movements, although for Tilly, revolutionary conditions did not emerge from class exploitation alone. Rather, revolutions were a form of “collective action” that required specific political opportunities, access to resources, and an organizational structure capable of attracting support and mounting a sustained challenge.

Skocpol's (1979) *States and Social Revolutions* challenged what she has called in various places “society-centered” analysis of states and revolutions. In this seminal work, Skocpol argued that the success of revolutions in France, Russia, and China were as much or more the result of external forces—markets and militarism—than of internal political instability. Moreover, Skocpol argued that in each case, successful revolutions depended on other structural factors as well, namely, competition or conflict between rulers and elites, and the organizational ability of revolutionaries.

More recently, Skocpol's work, and social-structural theory in general, has become less popular in light of research on the numerous revolutions and revolutionary movements that have occurred within the last half-century. If anything, the differences between revolutions in Eastern Europe and the former Soviet Union, Iran, the Philippines, Central America, and Asia have made comparative historical scholars cautious toward theorizing too broadly about the causes of revolutions. Yet the current lack of any single dominant theoretical approach to the study of revolutions has been greeted by a deluge rather than a dearth of work in the area. As Goldstone (2003) notes,

The elements of revolutionary process [have been] expanded to include international pressures, fiscal strain, intraelite conflict, a wide range of popular protest and mobilization, underlying population on resources, and coordination between opposition elites and popular protest to produce revolutionary

situations, as well as the pivotal role of revolutionary ideologies in guiding outcomes. (P. 69)

Social Movements

Social movements have been of keen interest to comparative historical sociologists not only for determining the conditions under which such movements may emerge but also in understanding why they succeed or fail in their respective aims. By “aims,” the study of collective action recognizes that very often such action constitutes more than mob violence or disorganized reaction to external political, social, and cultural pressures. Prior to the American civil rights movement and subsequent social movements, much of the thinking on the topic was centered around functionalist and behaviorist theories in North America and Marxist theory within Europe. The civil rights movement, along with the antiapartheid movement, environmental movements, and other social movements, were clear indications, however, for sociologists that collective action could not be adequately explained as spontaneous reaction to the short-term breakdown of social norms (functionalism), or merely as response to material inequalities or oppression.

More recent approaches include resource mobilization and political process theories. These approaches argue that social inequality is endemic throughout social relations, and that collective actions and social movements cannot be explained solely by inequality (e.g., “relative deprivation”) or oppression itself. Rather, resource mobilization and political processes theories argue that social movements are created and engendered by “opportunity structures” and access to resources otherwise unavailable to potential collective actors. “New” social movement theory has argued that modern social movements differ from earlier forms of collective action in that the contested terrain encompasses not only class conflict and material inequality but the symbolic production of meaning and identity (Canel 1997; Cohen 1985; Melucci 1980, 1985).

METHODOLOGICAL AND EPISTEMOLOGICAL CONSIDERATIONS

Within the last two decades or so, there has been significant debate regarding the role of method and theory in comparative historical analysis. These debates encompass not only particular critiques of various works and theories but more generally the historical comparative claim to theoretical knowledge, the reliability of causal explanation in comparative historical work, and the purported division in comparative historical work between sociology and history. Moreover, these debates can be linked to the “linguistic turn” that has occurred throughout the social sciences and humanities, particularly in relation to the various postmodernist and poststructuralist critiques of epistemology and knowledge/power relations.

In the case of methodology and subsequent claims to theory generation, Jeffery Paige (1999:782) has characterized the polarities of this debate in CHS as one of “advancing general theories of society,” on the one hand, and “explaining historical conjectures,” on the other. This division is a revival of the *methodenstreit* confronted by Max Weber, focusing on the question of whether to situate sociology nomothetically, which is within the realm of empirical sciences, or idiographically, within the realm of hermeneutics and interpretation.

Although CHS takes history as its “field of study,” its earlier practitioners generally sought to situate the field on the *other* side of the *methodenstreit*. Calhoun (1998) notes that

rather than emphasizing sociology’s substantive need for history—the need for social theory to be intrinsically historical—Skocpol and Tilly among others argued that historical sociology should be accepted because it was or could be comparably rigorous to other forms of empirical sociology. (P. 850)

Part of this “rigor” lay in the notion that CHS could speak *scientifically* about history, not only by distinguishing causal sequences of events but also by generating broader theories about society itself through the study of history.

With a few exceptions, this view of CHS was the predominant view through the mid-1970s. By the late 1970s and early 1980s, however, the nomothetic/idiographic assumption was being questioned both within CHS as well as within sociology itself. Philip Abrams’s (1982) *Historical Sociology* was one of the first serious critiques of this assumption, where Abrams argued that “in terms of their fundamental preoccupations, history and sociology are and always have been the same thing” (p. x). While the notion that there are *no* differences between history and sociology was and remains perhaps not as widely held as critics of this position decry, comparative historical analysis in the last two decades has undoubtedly seen a growing divide along the lines of “historical conjecture” and “general theory.”

Sociologists such as Goldthorpe (1991), Burawoy (1989), Kiser and Hechter (1991), and others have moved to counter the growing “historicism” within sociology, something that Goldthorpe (1991) has called “mistaken and—dangerously—misleading” (p. 225). Instead, Goldthorpe argues,

History may serve as a “residual category” for sociology, marking the point at which sociologists, in invoking “history,” thereby curb their impulse to generalize or, in other words, to explain sociologically, and accept the role of the specific and of the contingent as framing—that is, as providing both the setting and the limit—of their own analyses. (P. 212)

Here the debate becomes as much epistemological as methodological. The question becomes “What counts as

128 • THE SCIENTIFIC APPROACH TO THE STUDY OF SOCIETY

legitimate knowledge within comparative historical analysis?" This is a difficult question and one that has plagued not only CHS but also sociology and the social sciences in general. Currently, nothing like the cohesion of functionalism in sociology or the dominance of Marxism exists within CHS. Some like Kiser and Hechter see the concomitance of sociology and history as a dangerous vacuum. Others see this as the movement away from a confining and limiting sociology.

Much of the current work in CHS arguably falls somewhere in the middle. Some, such as Mahoney and Rueschemeyer's (2003) edited work *Comparative Historical Analysis in the Social Sciences*, seek to show that historical comparative work can be empirically rigorous and that the field has been successful not only in individual projects but also in the accumulation of knowledge in the field itself. Paige (1999:785), on the other hand, has argued that many "second-generation" comparative historical sociologists have developed "neither case-specific conjectural explanation, nor universal theory, but rather historically conditional theory," which Paige defines as the practice of "examining anomalies in theoretical frameworks presented by particular time-place conjunctures." Yet these historically conditional theories resemble less a graduated continuum than myriad trajectories of method and approach to theory.

Moreover, it is not only CHS that has changed but the discipline of history as well. Thus, within comparative historical analysis, the question of the relationship of history to sociology is hardly settled. The turn toward history within sociology itself, the overlap between sociological and historical work, the emergence of differing methodological strategies, and the growing interdisciplinary nature of the field have created a decidedly complex and contentious blurring of the boundaries of the field. If anything, it is questions of method and epistemology that appear most daunting for the future of comparative historical studies.

THE FUTURE OF COMPARATIVE HISTORICAL SOCIOLOGY

At this point, conclusions are difficult. Part of this stems from the possibility that the field itself has become unwieldy. This would not be surprising except for the fact that so few historical comparative courses are taught when compared to other sociological subfields. Outside of a

dozen or so "classic" works in the field, lists of readings for "comparative historical" courses vary widely, as does the inclusion or exclusion of methods, and works in the history of the field itself.

The literature in the field has in fact become subsets of literatures that have largely moved toward specialization, as well as being connected with other disciplines and fields of study. For some, the emergence of subspecializations runs the risk of "turning [students] into skilled technicians" competent in specific methodologies but "crippling" their ability "to think like social scientists" (Wallerstein 2000:33). For others, the overlap with other disciplines and fields of study is seen as a corruption or regression away from the goal of empirical research and the construction of general theory (Burawoy 1989; Goldthorpe 1991; Kiser and Hechter 1991). For yet others, the movement of comparative historical analyses into other areas such as feminist and culture studies is indicative of the "domestication" of the field itself, where CHS has lost its once "critical edge" to other disciplines and fields (Calhoun 1996).

In many ways, CHS is today less diverse or "transdisciplinary" than merely divided along differing thematic, methodological, theoretical, and epistemological positions. There seems to be much hope in "trans- or "postdisciplinary" approaches. There is also decidedly less actual work that can be pointed to as examples of what such work should look like, particularly in several major sociological journals that for the last decade or so have played host to a series of various attacks on and defenses of what CHS is or is not.

However, it is not at all clear that these growing divisions are as dangerous as many claim, or that CHS as a meaningful rubric has not outlived its usefulness. Its initial growth in the United States and Europe was as much a social as an academic movement, a type of collective identification against the perceived shortcomings of sociology and its inability to address problems of social injustice, exploitation, and war. As this collective identity has faded, so too has the notion that comparative historical work must be grounded in these larger theoretical concerns. In this sense, a truly transdisciplinary approach must begin not with greater emphasis on interdisciplinary research but with the more reflexive question of whether or not the field has outgrown its conceptual boundaries. It must confront the fact that today the landscape of the field resembles a contested and contentious division of comparative historical "sociologies."